

How Do Voters Perceive Changes to the Rules of the Game? Evidence from the 2014 Hungarian Elections

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Abstract

Voters often rely on partisan attachments as they evaluate new policy proposals, but does partisanship also color their interpretation of incumbent efforts to entrench themselves in power by changing the “basic rules of the political game”? We explore this question by taking advantage of a rare instance where a single party held a supermajority sufficient to unilaterally amend the constitution and overhaul the electoral system. We embedded a randomized experiment in a panel survey around the 2014 Hungarian elections, providing respondents with different information about recent changes to the Hungarian electoral rules. While respondents were largely pessimistic about the reforms, providing information yielded no significant effects on their views on the elections legitimacy. But when information was presented alongside partisan cues, respondents became more negative in their views. Subgroup analysis shows that this effect is concentrated entirely among those not supporting the incumbent. Partisan differences in opinion dwarf any treatment effects we were able to induce. We provide evidence that these findings are unlikely the result of a well-informed populace. Rather, we provide the first experimental evidence that partisan-motivated reasoning applies not only to public policy under fixed institutions but also to changes to the institutional rules of a political system. Incumbents can exploit strong partisan attachments to reduce political competition.

At the heart of most theories of democratic accountability is the idea that voters retrospectively evaluate politicians and their actions (Achen and Bartels, 2016; Downs, 1957; Fiorina, 1981; Healy and Malhotra, 2013). This sanctioning role of voters is always important, but it plays a distinctive role where democratic incumbents seek to use transitory majorities to alter not only policy but also political institutions in ways that perpetuate their grip on office (Acemoglu, 2003; Aghion, Alesina and Trebbi, 2004; Calvo and Micozzi, 2005; Greif and Kingston, 2011). In most such instances, coalition politics and constitutional rules may impose limits, including a need to bargain with other parties in the legislature (Benoit and Hayden, 2004). Existing scholarship on political information, partisanship, and voter behavior focuses on voters' evaluations of policy or politicians, holding institutions fixed (Ashworth and Bueno De Mesquita, 2014). However, there are important instances in which formal constitutional rules are themselves at stake. If the incumbent successfully removes counter-majoritarian checks, the constraining effect of constitutional rules matters less and the sanctioning role of voters as a potential restraint on power becomes especially critical (Acemoglu, 2017). To punish politicians' transgressions of democratic norms voters must be aware of institutional changes, come to view them as illegitimate, and coordinate in their punishment strategy. In this paper we focus on the second of these: when do voters perceive institutional reforms that entrench an incumbent as illegitimate, i.e., as a violation of norms, outside the bounds of acceptable democratic political competition?

To address this question we take advantage of an extraordinary set of events in Hungary, a democratic country since the collapse of communism in 1990 and a member of the European Union. Like in many other newly democratizing countries, in Hungary efforts to entrench incumbent power are taking place not through overt actions such as a coup, the suspension of parliament, imprisonment of opposition leaders, and major electoral fraud, but rather through incremental and technical changes to existing laws that nominally follow legal procedures. In the wake of the global financial crisis and a series of government scandals, the center-right opposition party, Fidesz, won the 2010 elections in a landslide, securing a parliamentary super-majority sufficient to amend the

constitution at will. Since 2010 Fidesz has used its super-majority to try to establish a monopoly on political power. It has weakened checks on executive rule, re-regulated the media in ways that make it more difficult for opposition voices to be heard, and introduced a series of changes to the electoral system that systematically favor the incumbent.

In this paper we focus on the last of these Fidesz actions. We employ an original panel survey experiment conducted around the 2014 Hungarian national parliamentary elections, the first under the newly reformed electoral system. The experiment shows that informing respondents about the electoral reforms has no detectable average effect on survey respondents evaluations of the legitimacy of the elections before the election was held. But when presented with the same information alongside a partisan cue, respondents on average became more negative in their views of the reforms, but the effect is modest at about 0.15 standard deviations of the control group outcomes. Subgroup analysis shows that these treatment effects appeared only among those already planning on voting against the incumbent. By contrast, Fidesz partisans (those who voted for Fidesz in 2010 and 2014) and new Fidesz voters were not moved.

After the election, we leveraged the known election outcome to provide an additional informational treatment emphasizing the consequences of the reforms. Here we find that voters, on average, became more negative in their assessments of the elections legitimacy, although effect sizes remained modest at about 0.15 standard deviations of the control group outcomes. However, the difference between non-Fidesz and Fidesz voters widened slightly: non Fidesz-voters viewed the reforms even less favorably, while Fidesz voters, on average, remained unaffected. However, among Fidesz voters, there was one exception to this pattern. New Fidesz voters – those voting for Fidesz for the first time in 2014 - now expressed greater skepticism than Fidesz partisans about the legitimacy of Hungarian elections. This last finding suggests that had credible, easy-to-understand claims about the reforms' consequences been available pre-election then some Fidesz voters may have been more skeptical about the reforms. The complexity of the Hungarian electoral system combined with Fidesz's media reforms, however, precluded this possibility. The voters who found

the reforms illegitimate were those unwilling to vote for Fidesz in the first place and virtually all of our treatment effects are concentrated among this group. Although Fidesz lost vote share in 2014 it ultimately paid no price for its reforms in terms of its share of seats in the parliament. To understand why, we need to examine the role of information and partisanship in how voters assess election legitimacy.

1 Information and Partisan-Motivated Reasoning

We rely on two approaches to voter learning and behavior in generating the hypotheses we investigate here. The first emphasizes that, if voters have adequate information about politicians' actions, they will perform their democratic "duty" and punish overreaching politicians. A second perspective asserts that, even given adequate information, "facts don't speak for themselves" because voters perceive the world through a variety of cognitive and emotional lenses. These lenses can influence how voters interpret political information. Partisan attachment is among the most well-documented forces structuring how voters think. The "strong" version of this argument holds that voters will interpret information in ways that conform with their pre-existing partisan commitments rather than update their beliefs about political actors. A weaker version of the argument holds that "the influence of communication effects is not independent from partisanship...people's fundamental political dispositions systematically condition the impact of short-term communications based on the content that is communicated." (Malhotra and Margalit, 2010:854)

1.1 Informing voters

There remains a vigorous debate across economics and political science about the meaning of and requirements for electoral accountability in democracies (Ashworth, 2012; Duggan and Martinelli, 2017). Regardless of whether voters are purely retrospective or they use past behavior to sort "good types" from "bad," voters must have both well-formed preferences and the right information if

they are to serve as a credible check on leaders' behavior (Besley, 2006; Delli Carpini and Keeter, 1996). Voters do respond to new information, at least in certain contexts (Bullock, 2011; Kendall, Nannicini and Trebbi, 2015). If voters do not have sufficient information about the actual or likely consequences of specific policies, accountability is difficult to sustain. Problems are most likely to arise when the identity of responsible government agents is unclear and when policies are complex and play out over extended periods of time. A free and independent media is therefore crucial for providing voters with information that may be critical of incumbent politicians (Besley and Prat, 2006; Chang, Golden and Hill, 2010; Enikolopov, Petrova and Zhuravskaya, 2011; Ferraz and Finan, 2008). Voters are more informed and engaged where media is free and active (Leeson, 2008; Snyder Jr. and Strömberg, 2010).

This logic would seem to apply to Hungary, where incumbent politicians have altered institutional rules to reduce the threat of replacement. In passing their competition-reducing reforms, Fidesz's core strategy was one of obfuscation while at the same time scrupulously following democratic procedures. Obfuscation took two forms. First, the reforms were technical changes to an already complicated electoral system (discussed below). Second, consistent with Gehlbach and Sonin (2014), Fidesz imposed numerous restrictions on the media and its ability to offer critical political information. As a consequence Hungarian voters might be simply unaware of the government's reforms or, more likely given the already complicated electoral system, unable to discern the reforms' consequences for electoral outcomes. Given better information, voters would have taken a more negative view of the incumbent's actions. This leads to a first hypothesis:

1. Informing voters about the electoral system reforms will cause them to view the reforms as illegitimate or unfair.

1.2 Partisanship

Politics is complex and demanding. Sorting cause from effect and attributing blame and credit are difficult, even for experts who pay close attention. Simple facts may not have immediate interpretations in the minds of most voters (Campbell et al., 1960). Partisan attachment is one widely documented factor shaping voters' interpretation of political information. Party labels and party endorsements can serve as heuristics in low information environments allowing voters to invest less cognitive effort in forming opinions (Bartels, 2002; Huckfeldt et al., 1999; Mondak, 1993; Schaffner and Streb, 2002). In the case of uninformed voters, for example, partisan cues—signals of how certain political elites regard a particular policy or rule change—can stand in for voter knowledge of the policy or rule change (e.g., Lupia, 1994). This can have perverse effects (Lim, 2015).

More recent work has focused on how partisan identities shape the acquisition and interpretation of information. The core insight is that voters are often *motivated* reasoners, gathering and interpreting information in ways that conform with their pre-existing beliefs and attachments (Bolsen, Druckman and Cook, 2014; Epley and Gilovich, 2016; Leeper and Slothuus, 2014). As a result, voters might seek out, avoid, or weigh information differently in order to form opinions consistent with their preexisting partisan identity (Taber and Lodge, 2006). Where voters are better informed, partisanship still colors voters' perceptions of facts (Gerber and Huber, 2009; Shapiro and Bloch-Elkon, 2008), evaluations of an incumbent's performance and attribution of responsibility for poor outcomes (Tilley and Hobolt, 2011), and perceptions of electoral fraud (Beaulieu, 2014; Bowler and Donovan, 2016).¹ Supporters of the incumbent party are more willing to ignore or forgive corruption than voters with weaker or different partisan attachments (Anduiza, Gallego

¹But see Bullock et al. (2015). When given a financial incentive to be correct there is a reduction in partisan bias in factual reporting and an increase in admitted ignorance among survey respondents.

and Muñoz, 2013; Blais, Gidengil and Kilibarda, 2017; Eggers, 2014; Faller, 2015).² Partisans may become better informed about topics consistent with their existing commitments and remain ignorant about challenging facts; media exposure may not help ameliorate this (Jerit and Barabas, 2012). And even if partisans of different stripes can agree on the objective situation, they attribute blame selectively (Bisgaard, 2015).

Particularly relevant to this study, there is a widely-documented “winner-loser gap” in which supporters of the electoral loser are consistently more negative in their assessments of electoral integrity and fairness. The gap has been witnessed around the world (Anderson et al., 2005), including the USA (Sances and III, 2015), Africa (Moehler and Lindberg, 2009), Mexico (Cantú and García-Ponce, 2015), Belarus, Russia, and Ukraine (McAllister and White, 2015; Rose and Mishler, 2009). Doherty and Wolak (2012) use an experimental framework to shed light on the cognitive process at work here: when the fairness of a political process is ambiguous, people’s evaluations are more likely to reflect their pre-existing attitudes.

Although Hungary is a relatively new democracy, the party system has been stable enough for partisan attachments to form (Brader and Tucker, 2010). Moreover, since the ruling party began its attempt to establish a political monopoly, elite polarization has dramatically increased, which ought to magnify the effect of partisanship (Druckman and Slothuus, 2015; Körösiényi, 2013). Finally, unlike in long-standing democracies, in Hungary the ruling parties and the opposition parties now disagree not just on policy, but on the fundamental rules of the democratic game, leaving fights between parties as fights over the political regime itself. This leads to our second hypothesis:

2. Providing voters information about which party sponsored and which parties opposed the reforms will affect their perception of the legitimacy of the election, but that perception will itself vary with respondents’ partisan attachments. In particular:

- (a) Information about Fidesz support for and other parties’ opposition to the reforms will

²See also Olken (2009).

induce Fidesz supporters to view elections held under the new rules in a more positive light than information provided without partisan context.

- (b) Information about Fidesz support for and other parties' opposition to the reforms will induce Fidesz non-supporters to view elections held under the new rules in a more negative light than information provided without partisan context.

In our pre-analysis plan, we also proposed that voters' abilities to handle complex political calculations could condition the impact of this information on their attitudes (Ahlquist et al., 2015). Voters with more education or a greater sense of personal political efficacy will be less affected by new information simply because they have already incorporated more information into their existing opinions.

Before discussing our research design and findings, we provide the historical context to Fidesz's 2010 landslide victory.

2 Hungary after Communism

Hungary held its first post-communist national parliamentary elections in the spring of 1990.³ Parties spanning the political spectrum contested the election, with the rightist Hungarian Democratic Forum (MDF) ultimately forming a governing coalition. Together with its coalition partners, MDF presided over the initial stages of Hungary's adoption of market economics and democratic politics. Fidesz joined the liberal Alliance of Free Democrats and the Hungarian Socialist Party (MSZP), the successor to the communist party, in opposition.

As elsewhere in Eastern Europe, the first years of transition were difficult, and the 1994 national parliamentary elections saw a strong swing back toward the Socialists (MSZP). The So-

³See Benoit and Schiemann (2004) for an analytic account of the origins of Hungary's 1990 electoral system.

cialists captured an absolute parliamentary majority, which precipitated a transformation in the Hungarian party system. The parties of the former rightist ruling coalition faded into irrelevance, and the political space they held came to be occupied by Fidesz, which won the 1998 parliamentary elections. Between 1994 and 2002, leftist governments dominated by MSZP and a rightist government dominated by Fidesz alternated in power, as Hungary completed the reforms necessary for entry into the European Union.

MSZP eked out a narrow victory in the 2006 elections, making it the first party since the fall of communism to win two consecutive terms of office. But after a series of gaffes and scandals involving the Socialist Prime Minister Ferenc Gyurcsány, Fidesz and other parties organized massive protests and called for his resignation. The government survived, but MSZP never recovered its popularity, losing the 2010 election by a wide margin.

The 2010 elections were a watershed in Hungarian politics because Fidesz, in coalition with the small Christian Democratic People's Party, captured over two-thirds of the seats in parliament. This parliamentary super-majority gave Fidesz unprecedented power to remake the Hungarian political system without opposition input.

2.1 Centralization of Power under Fidesz

Fidesz has entrenched itself by altering the electoral system in subtle ways.⁴ The old system provided for a parliament of 386 members, some of whom were elected from regional party lists and others from single-member constituencies. The old system was exceedingly complex, with two rounds of voting and a system whereby parties losing in single-member districts were compensated with seats assigned through party lists. The new law simplifies the elections to one round, reduces the number of electoral constituencies from 176 to 106, and shrinks parliament to 199 seats. A little over half of these are now single-member districts (SMD) and highly gerrymandered

⁴See Hegedus (2013) and Boda and Medve-Bálint (2015) for detailed discussions.

in Fidesz's favor. The reforms changed the votes-to-seats calculations such that the parties of those winning single-member district seats now receive a PR seat bonus for the surplus votes they received rather than compensating losing parties in proportion to the wasted SMD votes. Fidesz also passed legislation allowing ethnic Hungarians in neighboring countries to acquire Hungarian citizenship without living in Hungary. These individuals are now eligible to vote for party lists in parliamentary elections. The net effect is to reduce the proportionality between votes received and seats won for the top vote-getters while retaining the centrifugal features of mixed and proportional systems. Fidesz's reforms, unconstrained by any opposition veto power or coalition partners, are largely consistent with Benoit (2004)'s predictions about seat-maximizing changes to electoral rules. But Fidesz went much further than just altering electoral rules.

Until Fidesz came to power in 2010, the primary check on the government's power in Hungary was the powerful Constitutional Court, which had both the authority and will to review and reject legislation that it deemed unconstitutional. Several of Fidesz Prime Minister Orbán's early initiatives aimed at altering this Constitutional Court. The Fidesz government attempted to impose a mandatory retirement age for justices in order to purge the Court of its older and more leftist and liberal judges, a blow that was softened after only after the European Union objected. The government also altered the appointment process to the court, routing nominees through a new Judicial Council controlled by Fidesz appointees. With no opposition participation, the Fidesz government drafted and then passed a new constitution that restricted the Court's authority, in particular its ability to review new constitutional amendments. In cases where the Court did attempt to reject a piece of Fidesz legislation, the government has simply used its super majority to amend the constitution to incorporate the new law.

In 2011 Fidesz passed a controversial media law with two key features. First, the law restricts the freedom of expression when it "encourages acts of crime," "violates public morals or the moral rights of others," or "incites hatred against any nation, community, national, ethnic, linguistic or other minority or majority as well as any church or religious group." These restrictions are

draconian by American standards but not outrageously out of line with European norms. However, the second feature of the legislation provides for the creation of a Media Council tasked with adjudicating disputes about whether particular content violated the speech codes. This Council is dominated by Fidesz partisans who serve terms of nine years. Given the further stipulation that media owners be “fit and proper,” Fidesz thus has the ability to suspend any organization it deems in violation of the rules.

These reforms were controversial both inside and outside Hungary. Domestic opponents protested, and the OSCE and EU among others made critical noises about the apparent democratic drift in Hungary. In this environment Hungary held its first parliamentary elections under the new rules in April 2014. These elections were the voters’ first opportunity to punish Fidesz for these self-serving but legal changes. Fidesz still won a plurality of the popular vote with just over 44% of the list vote, down from 52% in 2010, and retained its super-majority (67%) of seats in parliament.

3 Research design

We investigate our hypotheses using an original panel survey with embedded informational experiments.⁵ Our goal is to learn whether and how information about the reforms affects respondents’ views about the reforms’ consequences for the election’s legitimacy. The panel nature of the survey is important for three reasons. First, it allows to investigate the effects of our treatments *pre-election*, when voters’ attitudes matter most for determining the actual outcome and the notion of a “winner-loser gap” is less relevant. Second, following up after the election allows us to incorporate the actual results of the election in our experimental treatments. Third, the panel structure allows us to examine within-subject opinion change.

⁵The analysis reported in this paper was pre-registered with EGAP as registration number 20150127. Wherever our reported analysis deviates from the pre-registered plan we make note and provide justification in footnotes.

In cooperation with a larger research team based at Central European University we fielded a two-wave panel survey immediately before (31 March–4 April 2014) and after (28 April–5 May 2014) the Hungarian parliamentary elections of 6 April 2014. The questionnaire was administered by the polling firm Median in collaboration with Kutatocentrum. Our sample comprises regular respondents in Kutatocentrum’s active online panel, representative of the adult online resident citizen population of Hungary.⁶ The pre-election survey had a sample of 3000 Hungarians aged 18 and above, with respondents selected using quota sampling by gender, age group, settlement type and region. For the second wave we re-contacted the pre-election survey respondents after the election, sending additional email reminders until reaching 1500 of the original 3000.⁷ The online appendix presents survey participation rates,⁸ as well as the marginal distributions of gender, age, and regional residence variables for the pre-election survey sample and the post-election survey samples compared to their population proportions in the 2011 Hungarian census. The sample marginal dis-

⁶Respondents were incentivized to take the survey by entering them into a quarterly lottery for 200,000 HUF (about US\$815, approximately the average monthly wage). The sample may have included respondents who lived abroad by the time of the survey, but it was not intended to include newly naturalized citizens with dual citizenship or without a residence in Hungary.

⁷The respondents who participated in the second wave are 1.3 years older on average, slightly more likely to have a university degree and slightly less likely to have only a secondary degree, and have higher average income than those who participated only in the first wave. They are also less likely to identify as a Fidesz supporter and more likely to state they voted for a non-Fidesz party in 2010 than those who did not participate in the second wave. But they do not differ in gender balance, sense of efficacy, region, or religiosity. Restricting the analysis to only these respondents who participated in both waves does not substantially change any of the results. See online appendix.

⁸Participation rates are calculated as the number of surveys completed divided by the number of invitations with clicked survey links.

tributions are very similar to 2011 population values in both survey waves. The analysis reported here does not employ survey weights.

3.1 Outcomes

The dependent variable for this study is a question that asked respondents to report how they perceived the fairness of the 2014 election in light of the election reforms. We denote responses to this question for wave $t \in \{1, 2\}$ as Y_t . This question’s English translation reads: “How do you think the changes in the electoral system affect the fairness and legitimacy of Hungarian elections?” The possible responses were “Big effect for the better” (2), “small effect for the better” (1), “no effect at all” (0), “small effect for the worse” (-1), and “big effect for the worse” (-2).

All 3,000 respondents in the pre-election survey answered this question. Fifteen hundred of the original 3,000 respondents answered this same question again in the post-election survey. For descriptive purposes we construct the within-subject change in opinion, $\Delta Y_i = Y_{i2} - Y_{i1}$. This change variable, which takes on integer values between -4 and +4, is only measured for the 1500 respondents who participated in both survey waves.

3.2 The experiment

As part of the survey, we randomly assigned respondents with equal probability to one of three experimental conditions: *control*, *information*, and *information + partisan cue*.⁹ The treatment consists of displaying a text preamble immediately prior to answering the outcome question just described. Conditions differ in the information provided about the electoral reforms.

While we present different preambles in the pre- and post-election surveys we were unable to re-randomize treatment assignment between the pre- and post-election waves. Respondents who participated in both survey waves remained in the same experimental condition for both periods,

⁹See Bullock (2011) on “cues.”

e.g., those who saw no preamble (control condition) in the pre-election wave also saw nothing in the post-election wave. Below we discuss the consequences of this design for identifying treatment effects on within-subject opinion change, but for the moment, we can think of the post-election survey wave as presenting a compound treatment. We denote the assigned treatment condition for respondent i in wave t as D_{it} . For respondents in both waves $D_{i1} = D_{i2}$, so we will refer to D_i unless clarification for wave is necessary.

In the pre-election survey, the information treatments in English translation were as follows:

Control ($D_{i1} = 0$) no preamble

Information ($D_{i1} = 1$) “Since the last election there have been several changes made to the Hungarian electoral system. For instance, parliament has been shrunk from 386 to 199 members; constituency boundaries were changed; and Hungarians living abroad can now cast ballots in the election.”

Information + Partisan Cue ($D_{i1} = 2$) “Since the last election *the ruling party, Fidesz, has implemented* several changes to the Hungarian electoral system. For instance, Fidesz shrank parliament from 386 to 199 members; changed constituency boundaries; and Hungarian citizens living abroad can now cast ballots in the election. *The opposition rejected some of these changes.*”

The only difference (italicized above, but not in the survey interface) between *information* and *information+partisan cue* was the additional information that the ruling party Fidesz implemented the reforms and that the opposition parties rejected some of these reforms.

The pre-election treatments are designed to avoid appearing partisan or as an attempt to persuade respondents. Consequently the treatment is quite weak in the sense that we provide only limited facts with no context or analysis to aid interpretation. This provides a difficult test for Hypothesis 1 in the pre-election setting. The panel structure and post-election survey are therefore particularly interesting. Since the results of the election were known for the post-election survey

wave we could provide additional information on the actual *consequences* of the electoral reforms for the partisan composition of the legislature without delving into the details of the reforms or appearing partisan. The English translation for the post-election preambles are:

Control ($D_{i2} = 0$) no preamble

Information + Consequences ($D_{i2} = 1$) “There were several changes made to the Hungarian electoral system and constituency boundaries for the elections just held on 6 April. Under these new rules Fidesz won 67 percent of the parliamentary seats with 44 percent of the domestic vote in 2014, while in 2010, under the old rules, Fidesz needed 53 percent of the domestic vote to win 68 percent of the seats.”

Information + Consequences + Partisan Cue ($D_{i2} = 2$) “There were several changes made to the Hungarian electoral system and constituency boundaries for the elections just held on 6 April. Under these new rules *that the Fidesz majority adopted in parliament in spite of protest from the opposition*, Fidesz won 67 percent of the parliamentary seats with 44 percent of the domestic vote in 2014, while in 2010, under the old rules, Fidesz needed 53 percent of the domestic vote to win 68 percent of the seats.”

Again note that the only difference (italicized above, but not in the survey) between the *information+consequences* and the *information+consequences+partisan cue* treatments was that the latter included a clause referring to the opposition’s rejection of Fidesz’s changes.

We find some minor imbalance on some of covariates (Table 1). The *information* and *information + partisan cue* groups have a slightly greater proportion of women and Fidesz non-supporters (people who had supported Fidesz in 2010 but did not plan to do so in 2014; categorizations are described below) and have slightly lower income than the control group. We always report analyses with and without control variables, including gender and income. We interact the treatment variables with non-support and other attachment towards Fidesz, so the analyses are comparisons across treatment groups *within* each of these orientation categories.

[Table 1 about here]

3.3 Covariates

We hypothesized that different subpopulations will exhibit different baseline responses for our outcomes and that they will respond to our treatments differently. Specifically, we proposed in our pre-analysis plan that there would be heterogeneous effects based on party support and education/political efficacy.

Fidesz was the dominant party during this period and responsible for the election reforms. Opposition parties to the left were in disarray, so we concentrate on partisan attachment to Fidesz. We operationalize attachment using two pre-election, pre-treatment questions. The first asks which party/bloc respondents supported with their party list vote in the 2010 election and the second asks which party/bloc they plan to support with their party list vote in the upcoming election.¹⁰ Based on these questions we find substantial volatility in party support; for example 27% of the 905 respondents who voted for Fidesz in 2010 planned to defect in 2014. We therefore define four categories of partisan attachment: Fidesz non-supporters (those not voting for Fidesz in either election, including non-voters); Fidesz defectors (those who reported having voted for Fidesz 2010 but who do not plan to do so in 2014, including abstainers); Fidesz converts (those who did not vote for Fidesz in 2010 but who planned to do so in 2014), and Fidesz “partisans” (those voting for Fidesz in both elections).

We viewed education as one proxy for respondents’ political efficacy and pre-treatment in-

¹⁰In the pre-analysis plan we proposed measuring Fidesz partisanship using the 2014 vote intention question only, under the assumption that party list voting behavior would be relatively stable across elections. This turned out not to be the case (see the cross-tabs in the appendix). Other commentators also expressed concern that 2014 vote intention could be endogenous to the reforms. As a result we altered our measurement strategy as described.

formation levels. We use the question described in the online appendix to generate a 3-category partition of education, with categories {no secondary degree, secondary degree, university degree}. We use the political efficacy question described in the same appendix to generate an ordered, three category variable with answers in $\{-1,0,1\}$. We use the neutral response (0) as the reference category.

In addition to the models with only the treatment indicators as regressors, we fit several models with pre-specified covariates to more precisely estimate treatment effects. These are income, age (and its square), gender, and a dummy variable indicating whether the respondent lives in one of the three main regions of the country (West, Central, East). Finally we account for whether respondents planned to vote using the pre-election, pre-treatment question about their vote intentions. Details for all these variables are in the supplemental materials.

The research design for our causal analysis involves both a within- and across-subjects design. All hypotheses are assessed by simple differences-in-means and regression analysis.¹¹

4 Results

4.1 Election legitimacy

Respondents in our sample were decidedly negative in their views of the electoral reforms, at least in relating these reforms to the legitimacy of the 2014 election. Figure 1a displays the response distribution for this question by treatment status for both the pre- and post-election waves. To make the top and bottom panels as comparable as possible, this figure includes only those respondents who were in both survey waves. “Big effect for the worse,” the most extreme value on our scale, was the modal response in both waves, with the neutral “no effect” as the next most common

¹¹The supplemental materials describes results from randomization inference tests of corresponding sharp null hypotheses from the pre-analysis plan.

response. We also see that there appear to be differences across treatment groups; those in the *information+partisan cue* treatment are the most likely to be in the most negative category. This treatment appears to shift people from the neutral category to the most negative value.

[Figure 1 about here]

To better visualize the differences across treatment groups and survey waves we treat the Y_t as continuous variables and plot the group-specific means and (unadjusted) 95% confidence intervals in Figure 1b. Across all treatment groups, average opinions about the reforms became significantly more negative after the election. The information treatments move the respondents' average answers in a negative direction, although the *information+partisan cue* treatment is the only one that reaches conventional significance thresholds.

[Table 2 about here]

To take another view, Table 2 presents results from one-sided tests for differences-in-means across all treatment categories. The p -values are corrected for multiple comparisons using Holm's method (Holm, 1979). We find that *information+partisan cue* significantly affects perceived legitimacy of the election, shifting the average response negatively by about 0.17 on our scale when compared to the control group. By way of scale, this effect is quite modest: it is about 0.13 standard deviations of the outcomes for those assigned to control. Interestingly, providing information on only the content of reforms does *not* have a significant effect here. The difference between *information* and *information+partisan cue* borders on conventional significance thresholds. It appears that simply describing the reforms has little effect, but even a simple partisan context for these descriptions causes our subjects to respond. This is evidence against hypothesis 1.

4.1.1 Heterogeneous treatment effects

In addition to the modest treatment effects described above, there are large differences in both baseline responses and treatment effects between those respondents who supported the incumbent

party and those who did not. We found no meaningful subgroup differences in treatment effects by education or political efficacy, which were specified in the pre-analysis plan. We interpret the apparent findings for education (described below) as an artifact of education being a predictor of Fidesz support (supplementary materials).

[Figure 2 about here]

Figure 2 displays the partisan differences in starkest fashion for both the pre- (2a) and post-election (2b) responses. We display means of the electoral legitimacy outcome variable by treatment group and Fidesz attachment. Vertical bars represent 95% confidence bars, adjusted to account for multiple comparisons. Focusing for the moment on the pre-election results, those planning on casting votes for Fidesz, especially “partisans,” are, unsurprisingly, far more upbeat about the effects of the reforms on electoral legitimacy. We find significant treatment effects for *information + partisan cue* among both non-supporters and Fidesz defectors. We also find a significant effect for the *information* treatment among defectors. Contrary to H2a, our treatments had no effects among Fidesz converts or partisans. All our pre-election treatment effects are concentrated among those already planning on not voting for Fidesz. With the changes to election rules and boundaries that gave more weight to its supporters’ votes, Fidesz faced relatively little downside electoral risk with their reforms.

[Table 3 about here]

To investigate subgroup differences more comprehensively, we estimate three pairs of regression models with ordinary least squares (Table 3). In each pair the odd-numbered model omits the covariates: age, age squared, gender, income, region, and intention to vote.

Model 1 regresses pre-election legitimacy on the two treatment indicators, and the results in the first pair echo the overall results from the previous section: there is a modest but significant treatment effect in the expected negative direction for the *information+partisan cue* treatment, and

although the estimate for average *information* treatment effect is signed as expected, we cannot reject the null hypothesis that it is 0. Model 3 adds indicator variables for Fidesz non-supporter, convert, and partisan, with defector as the omitted category. We see that Fidez partisans and converts have much more positive views of the legitimacy of the elections in the pre-election survey than defectors (the omitted category) or non-supporters, and that adding these variables dramatically improves model fit. The estimated treatment effects are attenuated by the inclusion of these variables, which appears to be due to the relative over-representation of Fidesz non-supporters among the *information+partisan cue* treatment group. Among the covariates (estimates omitted for space considerations) we find that female respondents were more positively disposed towards the reforms than males and that those in Central Hungary (largely Budapest residents) were more negatively disposed toward the reforms than respondents in the East and West. The other covariates showed no significant relationship in Models 2 and 4.

Model 5 interacts these three types of orientations towards Fidesz with the treatment indicators, and the results mirror Figure 2a. We find a non-zero average treatment effect for the *information+partisan cue* treatment *only* among those who do not plan to vote for Fidesz (non-supporters and defectors, the omitted category). Among defectors (the omitted category) we find a significant negative treatment effect for the *information* that is not visible among non-supporters. We also find that the treatment effect for *information + partisan cue* is stronger among defectors than non-supporters, although the significance here is borderline. Among Fidesz converts and partisans, we find that the treatments have no effect on average. To get a better handle on the magnitude of treatment effects can compare them to the size of the overall gap in opinion between control group partisans and nonsupporters. We again see that effects are modest: the treatment effect among the non-supporters is about 7% of the partisan-nonsupporter opinion gap whereas among defectors is about 22% of the gap.

4.1.2 Post-election opinion

The information we provided respondents prior to the election was quite limited. One could reasonably question whether more direct statements about the reforms' consequences would have generated a discernible effect. We leverage the panel nature of our study to address this question. After the election we can provide a neutral description of the reforms' impact on actual outcomes. Perhaps this information (along with experiencing the election) affected respondents' views in ways that the weaker pre-election intervention did not.

Figure 1 indicated that, on average, respondents in all experimental conditions held more negative views about the reforms after the election. The difference in average responses across all respondents between the survey waves is -0.23. But before proceeding to analysis of the post-election data, recall our three methodological concerns. First, because treatment assignment was not re-randomized for the post-election survey wave, we essentially have one experiment. We are unable to determine whether it was the pre-election preamble, the post-election preamble, or the combination of both that caused any observed differences between treatment groups and controls post election.¹² Second, pre-election opinions of election legitimacy (Y_1 , the outcome in the previous subsection) was (necessarily) measured after the treatment (D_1). Nevertheless opinion change includes this Y_1 , implying post-treatment bias were we to regress opinion change on treatment indicators. Third, events other than the election occurred between the survey waves. We therefore cannot unambiguously claim that the election caused any opinion change among the control group. But we can still use the control group to estimate treatment effects and we can use the election results to create a stronger post-election treatment.

In light of these challenges we analyze the post-election data using just Y_2 (rather than ΔY_i) as the response. The estimated parameters for our experimental treatments therefore represent

¹²That said, we think it implausible that a couple sentences read in the context of a survey would continue to have an effect nearly a month later, as reflected in the dashed line in the figure.

the total effect of *both* our pre- and post-election preambles on post-election opinion. With this strategy we cannot isolate the specific effect of D_2 on Y_2 nor can we identify a causal effect of D_1 or D_2 individually on ΔY_i . But we can compare parameter estimates from our analysis of Y_2 to those from our analysis of Y_1 to determine whether our two interventions moved post-election opinion (relative to controls) more than the first portion of the intervention moved pre-election opinion.

[Table 4 about here]

Table 4 displays difference-in-means estimates of the average treatment effects on post-election opinion. The *information+partisan cue* treatment is again the only intervention that shows a significant effect relative to the control group. Moreover, the post-election point estimate is 23% larger than in the pre-election analysis (0.21 versus 0.17). Just providing information, even the stronger information intervention that was possible after the election, yielded no discernible effect on average. But contextualizing this information with partisan cues caused respondents to become significantly more pessimistic. This effect was statistically detectable even though all treatment groups became more negative, on average, after the election and many respondents were already at the bottom of our scale.

Figure 2b displays the mean post-election response by treatment group and Fidesz attachment status. In the post-election data we again see a clear differentiation by Fidesz attachment for both baseline opinion and treatment effects. Among the control group, the partisan gap became wider, largely as a result of both non-supporters and defectors becoming more negative about the reforms after the election. We continue to see no average treatment effects among the partisans and small but significant negative treatment effects for both the *information* and *information+partisan cue* treatments among non-supporters. The point estimate for the *information+partisan cue* treatment is larger than for *information* alone, but the difference between the two does not reach standard significance thresholds.

Comparing Figure 2a and Figure 2b for defectors and converts is particularly interesting. Before the election we see negative treatment effects for both treatments among defectors yet after the election we see none at all. Among converts it is the reverse pattern, although the post-election treatment effects do not cross conventional thresholds given the one-sided hypothesis. Upon closer inspection, however, we see that this divergence is largely driven by changes among the *control* group. Among defectors the average control group opinion changed by -0.4 between the two waves. Among converts, the control group average became 0.37 units more positive between the waves. In contrast the opinions of the treatment group respondents were relatively stable, becoming only slightly more negative, on average. Among the control group we see the well-documented “winner-loser” gap emerging among converts and defectors. Those converts receiving either treatment, however, saw a reduced “winner’s bump” in their post-election evaluation of the reforms. Defectors receiving either treatment came to view the reforms negatively even before the election. The outcome of the election helped the control group defectors “catch up.”

[Table 5 about here]

In Table 5 we repeat the regression analysis reported in Table 3 for the post-election data which sampled only 1500 of the original 3000 respondents. Results are again quite similar in that we find significant negative treatment effects for our *information+partisan cue* treatment. As with the difference-in-means analysis just reported, the treatment effect is also 20-25% larger in the post-election setting than in the pre-election data. When we control for Fidesz attachment in Models 3 and 4, our model fit improves dramatically, and we detect a statistically significant negative effect of our *information* treatment.

The big change from the pre-election analysis is the lack of any discernible conditional treatment effects by Fidesz attachment. We can compare how Fidesz non-supporters, Fidesz converts, and Fidesz partisans differ on average from Fidesz defectors in the control condition by examining the coefficients on these variables in Tables 3 and 5. There is a much smaller gap between partisans

and converts post-election than pre-election, and a much larger gap between converts and defectors (the omitted category) post-election than pre-election. We also find that the gap between defectors and non-supporters is much smaller post-election than pre-election. These comparisons remain even if we restrict the sample to only those who participated in both waves of the survey.

Overall, information on its own, even when it clearly depicts the pro-incumbent consequences of the reforms, had a harder time moving opinion than information couched in partisan terms. The partisan cues also had little effect on Fidesz supporters. We conclude that there is little support for H1 or H2a. However, the effects of both treatments on defectors pre-election and on converts post-election offer qualified support for H2b. It appears that only these “persuadable” voters are susceptible to being swayed by these treatments

To explore this further, Figure 3 displays within-subject opinion change by treatment status and baseline Fidesz attachment. The points depict the proportion of respondents in a given treatment-Fidesz attachment combination taking particular values of ΔY_i . For example, just under 20% of the Fidesz converts in the *information + partisan cue* condition moved +1 unit on the legitimacy question whereas 40% of the converts in the control condition had a similar change. The figure shows that respondents’ opinions about the reforms’ effects on election legitimacy were fairly stable: overall 62% reported the same opinion after the election, which is reflected in the spikes at 0 in all four subfigures. This is not surprising, however, as our response scale is bounded, and 38% of both-wave respondents put themselves in the lowest category (“big effect for the worse”) in the pre-election wave. If these respondents became more pessimistic after the election our survey is not able to detect it. Fidesz non-supporters were the least likely to move their opinions, followed by Fidesz partisans. In the control condition, defectors and converts were mirror images of one another; a substantial proportion of converts became one unit more positive after the election while about 35% of defectors became one or two units more negative.

[Figure 3 about here]

4.2 Were citizens already aware of the electoral reforms?

One possible interpretation of our findings is that Hungarian voters were, in fact, already well informed about the reforms so the information treatment meant little. All we did was provoke a partisan reaction. After all, when asked in our survey about whether they had “heard about any changes in the electoral system since the last parliamentary elections in 2010” fully 89% of our survey respondents said that they had. When we asked them to predict what percent of the party list vote Fidesz would win, the modal response was “40-50%.” Fidesz’s actual party list vote share was 45%. Nevertheless we do not think the data are consistent with a world in which the respondents were well-versed about Fidesz’s reforms and their likely consequences for electoral outcomes.

The question asking about “awareness” unfortunately does not tell us how much subjects understood about the complicated, multi-faceted reforms to an already complex electoral system. The responses also likely reflect some desirability bias because subjects were unable give a response of “don’t know” to this question.¹³ Fidesz’s reforms also targeted the media, so it is unclear what information voters would have already received, even if they “heard about” the reforms.

Presumably these well-informed respondents would also know about the reforms’ partisan provenance. Had these respondents actually been well-informed then we would expect null effects for *both* the *information* and *information+partisan cue* treatments. The fact that we instead observe a significant effect for the *information+partisan cue* treatment leads to the interpretation that a substantial fraction of respondents had a hazy understanding of the politics that generated the reforms. A well-informed population is also inconsistent with the treatment effects and opinion change that we observe after the election. We repeated the analysis in Tables 3 and 5 using only the subset of respondents reporting that they had heard about the reforms and our results are nearly identical, i.e., our results are not driven by the 10% who claimed not to know (online appendix).

¹³That survey respondents are reticent to admit ignorance is well known (Bishop, Tuchfarber and Oldendick, 1986; Schuman and Presser, 1980).

Finally, we might worry that some voters defected from Fidesz *because* of the reforms. Fidesz defectors, therefore, should arguably be those paying the closest attention. Yet they are the subgroup where we find the biggest treatment effects, even in the *information only* condition. While some were surely aware and concerned, all our evidence is inconsistent with a world in which the voters were well-informed of the election reforms and cast votes accordingly.

5 Conclusion

An underappreciated problem in democracies is that democratically-elected governments can exploit large but transitory majorities to entrench themselves in power by altering *ex post* the basic rules of the political game in their favor. When do voters sanction such undemocratic actions? To answer this question we took advantage of a rare empirical context in Hungary in which a single democratically-elected party had the legal authority to rewrite constitutional rules in its own interest. We fielded what is, to our knowledge, the first pre/post-election panel survey experiment around such large changes to the electoral system, assessing for the first time how voters themselves perceive rule changes that unambiguously advantage the incumbent party.

Institutional rules, usually taken as given in analyses of democratic accountability, can themselves become the subject of partisan-motivated reasoning. Before the election, providing information on just the content of the electoral reforms had no effect, on average, on respondents views of the legitimacy of major electoral reforms. Only when respondents were informed which parties supported or opposed the reforms did their views of the elections legitimacy move. But partisan differences in opinion about the reforms dwarfed any treatment effects. After the election there is some evidence that our intervention partially mitigated the “winner effect.” Nevertheless, voting for the incumbent party were far more likely to think that the reforms improved election fairness and legitimacy and those voting for the losing challenger thought the opposite. The effect of partisan cuing was visible only among respondents who were already voting against the incumbent.

Our findings are, however, not just a restatement of the familiar “winner-loser” gap in perceptions of election legitimacy. Survey respondents were divided along partisan lines even before the election took place. Regardless of partisan attachment, those who changed their opinions on the reforms became, on average, more pessimistic after the election. This suggests that respondents’ evaluation of the reforms and their consequences were shaped not just by being on the “losing” or “winning” team, as the conventional wisdom might expect, but also, in part, by preexisting partisan attachments. Further work in other contexts and with different types of party systems and proposed reforms will be needed to determine these results’ generalizability.

These deleterious effects of partisanship challenge a long-standing literature arguing that strong parties and mass partisanship protect new democracies from the depredations of elected officials who might abuse their authority, and are thus a precondition of democratic consolidation (Lupu, 2015; Mainwaring and Torcal, 2006; Mainwaring and Scully, 1995; Rose and Mishler, 1998). Beginning in the 1990s, students of the post-communist world recognized that their countries of study were particularly vulnerable to the problem of weak parties: after forty years of one-party dictatorship, new parties were weak and in general lacking deep roots in society. As a consequence, constitution designers sought to compensate for the expected lack of partisan attachments in new democracies by introducing electoral systems that would reduce parliamentary fragmentation and boost the more popular parties at the expense of proportionality. Similar efforts to avoid fragmentation proliferated in other parts of the world, where constitutional reforms were passed to achieve the dual goals of accountability and effectiveness (Hicken, 2009).

What the designers of institutions did not foresee was that too much executive dominance without sufficient countervailing legislative or judicial power might tempt well-organized ruling parties to transform transitory victory into long-lasting rule. When political power becomes concentrated in the hands of a hegemonic political party, strong partisanship means that strategic politicians can take advantage of voters’ informational constraints or cognitive biases to alter institutions in their favor. Where strong partisanship binds a sufficiently large minority to the hegemonic party, voters

will not throw out the hegemony-seeking incumbent even in fully democratic elections. Voters' sanctioning power over political misbehavior is thus thwarted when loyalty to party undermines faith in the political system as a whole. Partisan-motivated reasoning among voters extends beyond policy to institutional reforms, highlighting a basic vulnerability for democratic political institutions.

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Table 1: Balance across treatment groups. Covariate means by treatment group and p -values for two sample t -tests.

	Control (Mean)	Info (Mean)	Info - Control (p -val)	Info+Party (Mean)	Info+Party - Control (p -val)	Either (Mean)	Either - Control (p -val)
Fidesz support in 2010	0.31	0.28	0.22	0.28	0.12	0.28	0.11
Fidesz non-supporter	0.60	0.64	0.13	0.64	0.07	0.64	0.06
Fidesz convert	0.08	0.07	0.67	0.06	0.13	0.07	0.28
Fidesz partisan	0.23	0.21	0.28	0.22	0.43	0.21	0.28
Secondary school	0.45	0.43	0.39	0.44	0.52	0.44	0.39
University	0.41	0.41	0.75	0.41	0.83	0.41	0.76
Efficacy: Low	0.38	0.37	0.61	0.41	0.26	0.39	0.71
Efficacy: High	0.32	0.32	0.92	0.31	0.53	0.32	0.68
Age	47.92	47.19	0.30	47.48	0.53	47.33	0.33
Female	0.51	0.54	0.13	0.55	0.08	0.54	0.06
Income	207.18	198.79	0.12	196.57	0.05	197.68	0.04
Eastern Region	0.39	0.39	0.93	0.40	0.65	0.39	0.84
Western Region	0.31	0.31	0.73	0.31	0.73	0.31	0.69
Planned turnout	0.91	0.93	0.25	0.91	0.92	0.92	0.48
LSQ23: I am religious	0.45	0.47	0.18	0.45	0.99	0.46	0.44
in my own way							
LSQ23: I can't say if	0.06	0.05	0.21	0.06	0.77	0.06	0.38
I was religious or not							
LSQ23: I am not religious	0.26	0.29	0.12	0.27	0.60	0.28	0.23
LSQ23: I have a different conviction,	0.14	0.11	0.02	0.14	0.98	0.13	0.17
I am definitely not religious							

The third, fifth, and seventh columns report p -values from t -tests comparing the difference between means reported in the second, fourth, and sixth columns, respectively, and the control mean reported in the first column.

Table 2: Effects of treatments on Hungarians' assessments of 2014 election fairness and legitimacy, pre-election survey. Estimates are differences in group means.

	Estimate	Std. Error	<i>t</i> -value	Pr(< <i>t</i>)
<i>info</i> - control	-0.06	0.06	-1.02	0.15
<i>info+partisan cue</i> - control	-0.17	0.06	-2.89	0.01
<i>info+partisan cue</i> - <i>info</i>	-0.11	0.06	-1.87	0.06

n = 3000. One-sided *p*-values corrected for multiple comparisons using Holm's method.

Table 3: OLS regression for perceived effect of Hungarian electoral reforms on the legitimacy of the 2014 election, pre-election survey.

	1	2	3	4	5	6
Info	-0.06 (0.06)	-0.08 (0.06)	-0.01 (0.05)	-0.03 (0.05)	-0.36* (0.17)	-0.39* (0.16)
× non-supporter					0.39* (0.18)	0.40* (0.17)
× convert					0.55* (0.24)	0.54* (0.24)
× partisan					0.30 (0.20)	0.33* (0.19)
Info+Partisan Cue	-0.17* (0.06)	-0.19* (0.06)	-0.12* (0.05)	-0.13* (0.05)	-0.42* (0.17)	-0.45* (0.16)
× non-supporter					0.29 (0.18)	0.30* (0.17)
× convert					0.56* (0.25)	0.61* (0.24)
× partisan					0.38* (0.19)	0.39* (0.19)
Fidesz non-supporter			-0.55* (0.07)	-0.54* (0.07)	-0.77* (0.12)	-0.76* (0.12)
Fidesz convert			0.98* (0.10)	0.89* (0.10)	0.62* (0.17)	0.53* (0.16)
Fidesz partisan			1.36* (0.08)	1.42* (0.08)	1.14* (0.13)	1.18* (0.13)
Covariates	No	Yes	No	Yes	No	Yes
adj. <i>R</i> ²	0.00	0.03	0.37	0.40	0.37	0.40

Omitted category is Fidesz defector. *n* = 3000.

Standard errors in parentheses. Intercept estimated but not reported.

Covariates are age and its square, gender, income, region, and turnout intention

* indicates significance at *p* < 0.05 (one-tailed test)

Table 4: Effects of treatments on Hungarians' assessments of 2014 election fairness and legitimacy, post-election survey. Estimates are differences in group means.

	Estimate	Std. Error	t value	Pr(<t)
<i>info</i> - control	-0.10	0.08	-1.15	0.18
<i>info+partisan cue</i> - control	-0.21	0.08	-2.50	0.02
<i>info+partisan cue</i> - <i>info</i>	-0.11	0.08	-1.35	0.18

$n = 1500$. One-sided p -values corrected for multiple comparisons using Holm's method.

Table 5: OLS regression for perceived effect of Hungarian electoral reforms on the legitimacy of the 2014 election, post-election survey.

	1	2	3	4	5	6
Info	-0.10 (0.08)	-0.09 (0.08)	-0.12* (0.06)	-0.12* (0.06)	-0.005 (0.221)	0.02 (0.22)
× non-supporter					-0.13 (0.23)	-0.16 (0.23)
× convert					-0.36 (0.34)	-0.34 (0.34)
× partisan					-0.04 (0.26)	-0.07 (0.26)
Info+Partisan Cue	-0.21* (0.08)	-0.21* (0.08)	-0.20* (0.06)	-0.21* (0.06)	-0.15 (0.22)	-0.17 (0.22)
× non-supporter					-0.09 (0.23)	-0.09 (0.23)
× convert					-0.25 (0.35)	-0.20 (0.35)
× partisan					0.10 (0.26)	0.11 (0.26)
Fidesz non-supporter			-0.57* (0.10)	-0.56* (0.10)	-0.50* (0.16)	-0.48* (0.16)
Fidesz convert			1.20* (0.14)	1.15* (0.14)	1.41* (0.24)	1.33* (0.24)
Fidesz partisan			1.52* (0.11)	1.56* (0.11)	1.49* (0.18)	1.54* (0.17)
Covariates	No	Yes	No	Yes	No	Yes
adj. R^2	0.00	0.01	0.44	0.45	0.44	0.45

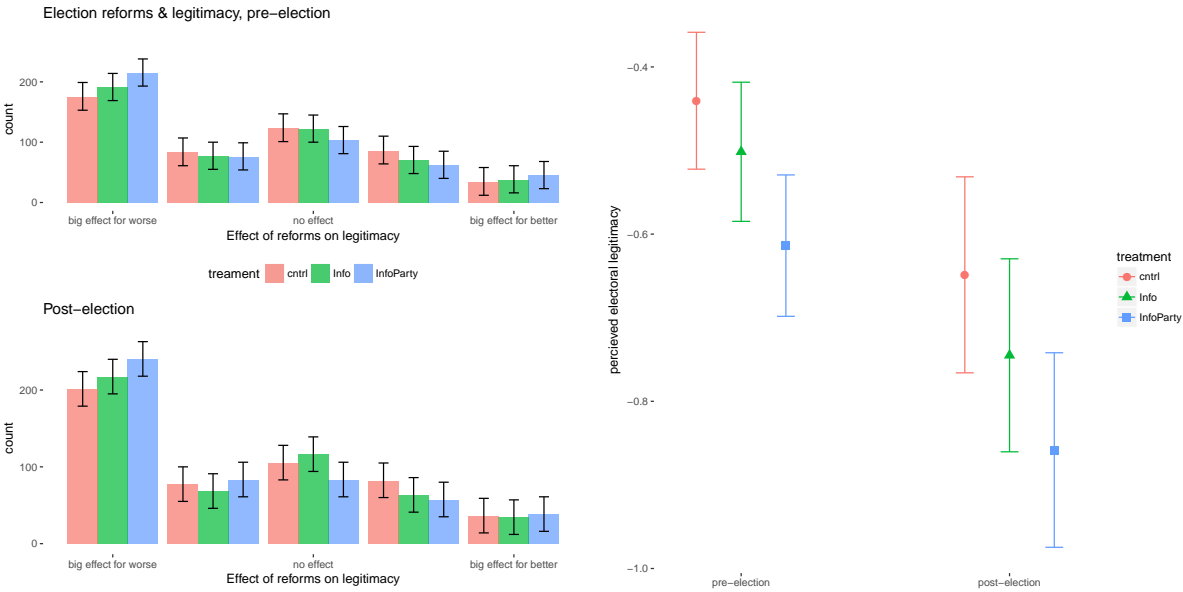
Omitted category is Fidesz defector. $n = 1500$.

Standard errors in parentheses. Intercept estimated but not reported.

Covariates are age and its square, gender, income, region, and turnout intention

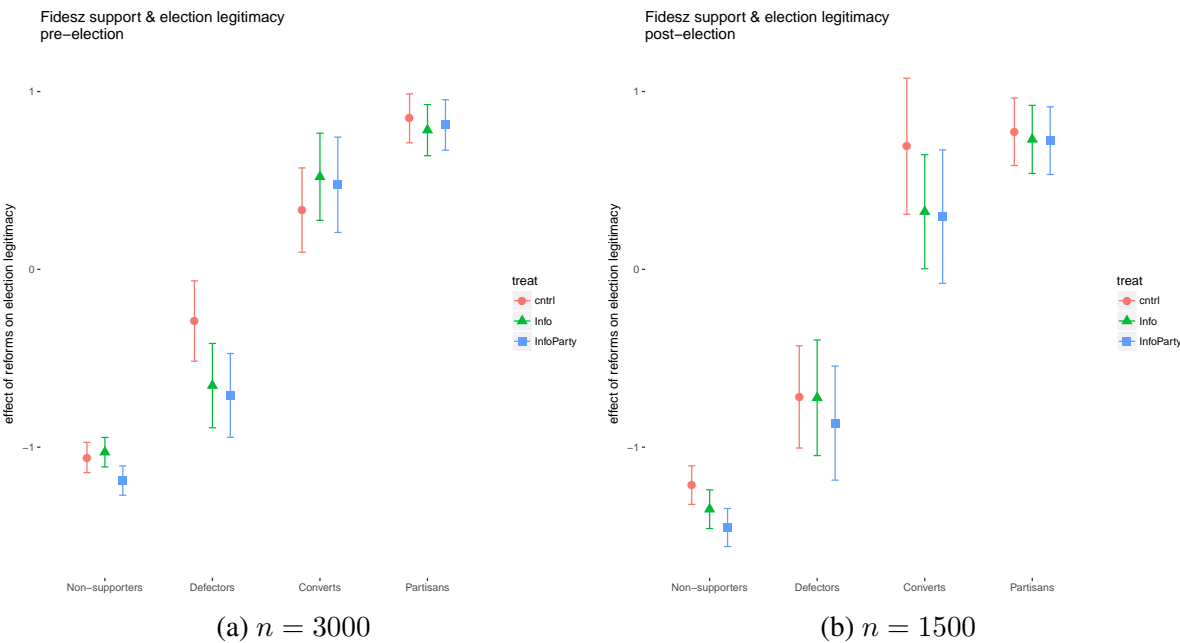
* indicates significance at $p < 0.05$ (one-tailed test)

Figure 1: Election legitimacy



(a) The distribution of responses to the election legitimacy question by treatment status, pre- and post-election. These plots include only the 1500 respondents who were in both waves. (b) Mean responses and unadjusted 95% confidence intervals for the election legitimacy question by treatment status, pre-election ($n = 3000$) and post-election ($n = 1500$).

Figure 2: Heterogeneous treatment effects by Fidesz partisan attachment. Within-group means with 95% confidence intervals adjusted for multiple comparisons.



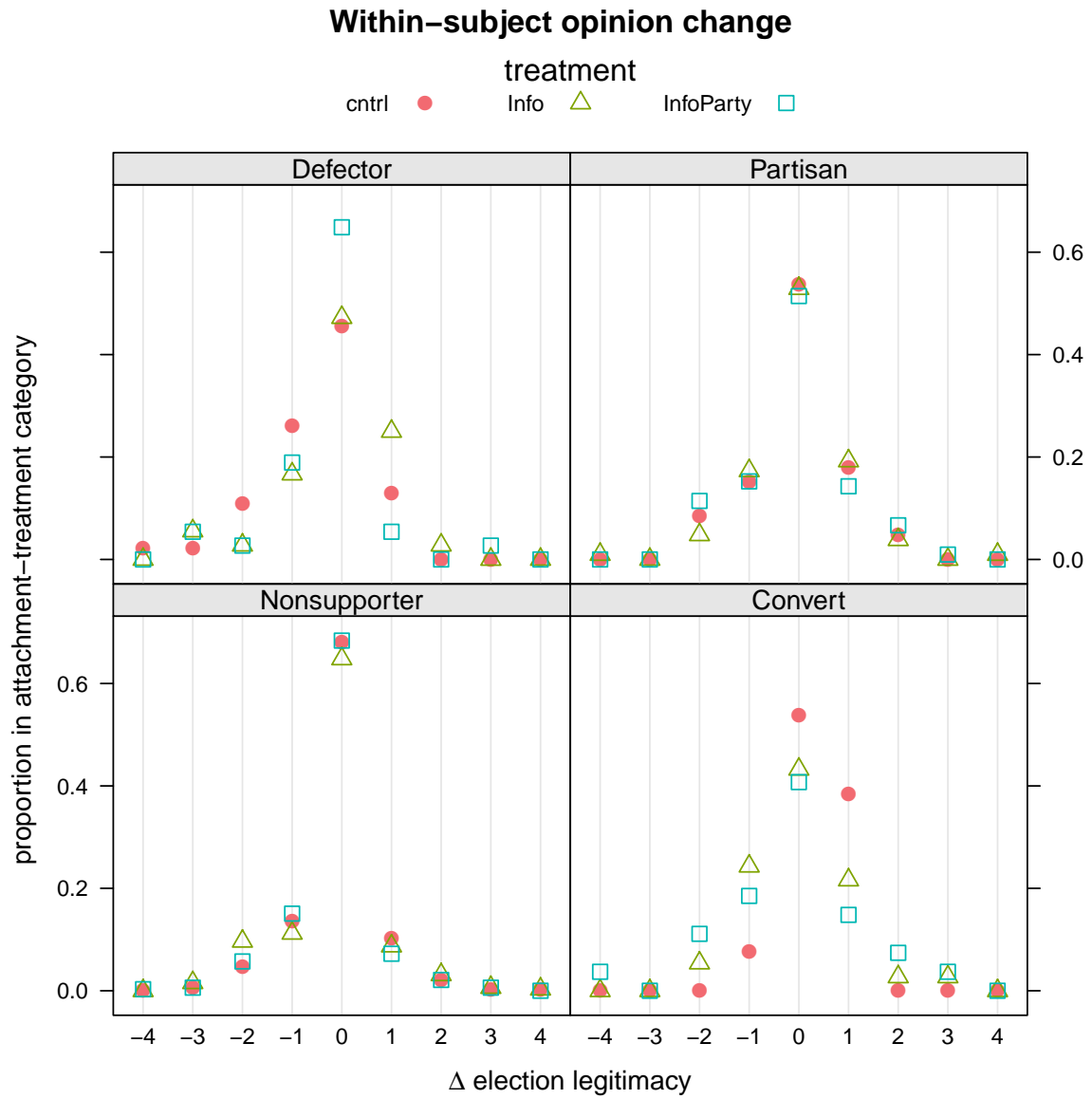


Figure 3: The distribution of opinion change about the legitimacy of the election by treatment status and Fidesz attachment.