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Abstract

Political scientists since Anthony Downs (1957) have hypothesized that more policy information leads to a higher voter turnout. To empirically test this hypothesis, we conducted an Internet-based randomized field experiment during Japan's 2004 Upper House election. Japan's 2004 election is ideal for testing our hypothesis because political parties prepared formal "manifestos" that include proposals for major policy issues. We find that voters are less likely to abstain when they receive policy information about both ruling and opposition parties through their official party websites. The information effects are larger among those voters who were planning to vote but were undecided about which party to vote for. Our findings also shed light on the role of the Internet as an important source of information for voters.

Methodologically, this paper illustrates how to design and analyze Internet-based randomized field experiments, which are becoming increasingly common in the social sciences. Our experimental approach avoids the problem of endogenous information acquisition, which is inherent when using observational studies to estimate the causal effects of information on voting behavior. Furthermore, we employ a randomized block design to ensure efficient randomization, and apply a Bayesian statistical model to account for noncompliance and nonresponse, the two prevailing problems of field experiments.

Estimating the Causal Effects of Policy Information on Voter Turnout: An Internet-based Randomized Field Experiment in Japan*

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Abstract

Political scientists since Anthony Downs (1957) have hypothesized that more policy information leads to a higher voter turnout. To empirically test this hypothesis, we conducted an Internet-based randomized field experiment during Japan's 2004 Upper House election. Japan's 2004 election is ideal for testing our hypothesis because political parties prepared formal "manifestos" that include proposals for major policy issues. We find that voters are less likely to abstain when they receive policy information about *both* ruling and opposition parties through their official party websites. The information effects are larger among those voters who were planning to vote but were undecided about which party to vote for. Our findings also shed light on the role of the Internet as an important source of information for voters.

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1 Introduction

Over the past decades, political scientists have emphasized information as a key determinant of voting behavior (e.g., Alvarez, 1998; Ansolabehere and Iyengar, 1995; Ferejohn and Kuklinski, 1990; Grofman, 1993; Huckfeldt and Sprague, 1995; Iyengar and Kinder, 1987). In *An Economic Theory of Democracy*, Anthony Downs (1957) first theorized the effects of information (or lack thereof) on voter turnout, and this pioneering work inspired the studies of later scholars. In particular, one of Downs’s main arguments – *the less information a voter has, the more likely he is to abstain* – led to the development of various formal models that attempt to explain the causal relationship between information and turnout (e.g., Feddersen and Pesendorfer, 1996, 1999; Ghirardato and Katz, 2002; Matsusaka, 1995; Palfrey and Rosenthal, 1985).

In this paper, we empirically test this information hypothesis through an Internet-based randomized field experiment we administered during Japan’s 2004 Upper House election. We find that voters are less likely to abstain when they receive policy information about *both* ruling and opposition parties through their official party websites. The information effects are larger among those voters who were planning to vote but were undecided about which party to vote for. Our findings also shed light on the role of the Internet as an important source of information for voters.

The contributions of this paper are both substantive and methodological. First, our experiment is one of few randomized field experiments that directly estimates the causal effect of policy information on voter turnout. Our study also examines the role of the Internet as an important source of information for voters by utilizing real policy information available through the official party websites. Japan’s 2004 Upper House election is not only ideal for testing our hypothesis but also interesting in its own light. In particular, as a conscious effort to move away from personal politics, major political parties in Japan have recently begun to prepare “manifestos” that explicitly state formal proposals for major policy issues. The findings of this paper bear important policy implications on the influence of such policy proposals on the voting behavior of the Japanese electorate.

Secondly, our experimental approach avoids the problem of *endogenous information acquisition* that is inherent when estimating the causal effect of information on voting behavior in observational studies. Specifically, those voters with a strong intention to vote may be more likely to acquire policy information. By exposing randomly selected voters to exogenous information, we minimize this potential bias. Furthermore, we illustrate how to design, conduct, and analyze Internet-based randomized field experiments, which are becoming increasingly common in the social sciences.¹ In particular, we employ a randomized block design to ensure efficient randomization. We then apply a Bayesian statistical model to account for the two prevailing problems of field experiments, *noncompliance* and *nonresponse*. We hope that our methodological approach will serve as a template for future randomized field experiments.

The rest of the paper is organized as follows. In Section 2, we discuss the existing theoretical and empirical literature on information and voter turnout. In Section 3, we present the design of our Internet-based randomized field experiment. In Section 4, we describe our statistical methods that can be used to analyze randomized field experiments with noncompliance and nonresponse. The results of our statistical analysis are shown and discussed in Section 5. Finally, Section 6 concludes.

2 Information and Voter Turnout

In this section, we first discuss theoretical and empirical studies on the relationship between information and voter turnout. We then explain motivations for our use of a randomized field experiment.

¹For example, an NSF-funded project, Time-Sharing Experiments for the Social Sciences (TESS), offers researchers opportunities to conduct Internet-based experiments. See <http://www.experimentcentral.org>

2.1 Theoretical Studies

In *An Economic Theory of Democracy*, Downs (1957) first theorized how information affects voters' decision-making from a rational choice perspective. His model, which is later formalized by Matsusaka (1995), assumes that a citizen has some preferences about a given policy issue, but is uncertain as to whether a party of his choice is the "right" party that brings out the most desirable policy outcomes.² Thus, information increases voters' confidence in their choice by helping them evaluate some objective characteristics of parties and their proposed policies.

The pioneering work of Downs has inspired many scholars who then developed formal models to explain why information leads to a higher voter turnout. For example, Ghirardato and Katz (2002) present a decision-theoretic model and show how the quality of information (i.e., "ambiguity") affects a voter's turnout decision. They demonstrate that a citizen abstains from voting when candidates' policy positions are ambiguous; i.e., when "One candidate looks better than the other in some scenario he envisions, while the opposite happens in another scenario" (p. 3). Here, information is shown to reduce a voter's ambiguity about candidates and hence to increase the probability of voting. Another recent example is the game-theoretic model of Feddersen and Pesendorfer (1996, 1999) which focuses on the asymmetric information across voters. The authors show that uninformed voters, even when they prefer one policy over another, abstain from voting and delegate their votes to other voters who are more informed and therefore are more likely to make a correct policy choice.

Although the assumptions and logic of these formal models are different, their basic implications have been largely consistent with Downs's original conjecture – *more policy information leads to a higher voter turnout*. Our study tests this essential hypothesis. However, in this paper, we do not test the different causal mechanisms derived from these formal models. Instead, we leave that task for future research. This limitation arises in part because some of the causal variables

²For comprehensive reviews of the Downsian model of voter turnout, see Blais (2000), Feddersen (2004) and Grofman (1983).

are difficult, if not impossible, to manipulate in a real election. For example, we must manipulate parties' policy positions (to test the Ghirardato–Katz model), vary the types and number of voters (to test the Feddersen–Pesendorfer model), and measure the levels of voters' confidence and their subjective evaluations of party differentials (to test the Downs–Matsusaka model). Given the difficulties of manipulating such factors in a real election, we decide to vary the *amount* of policy information each voter receives and measure how it affects a voter's turnout decision. We expose randomly selected voters to policy information through the use of official party websites right before the election.

2.2 Empirical Studies

While there exist many empirical studies that examine the relationships between information and voter turnout, we believe that our experimental study makes unique contributions to the literature. First, this is one of the few randomized field experiments that directly estimates the causal effects of *policy information* on voter turnout in a real election (see Iyengar and Jackman, 2003; Wantchekon, 2003, for notable exceptions). Downs (1957) emphasized the importance of policy information and argued that party policies determine party differential, which in turn influences on voting behavior (p. 266). The formal models reviewed above also assume rational voters who evaluate the policies of parties and candidates. Therefore, in order to examine the empirical implications of these theoretical models, we estimate the causal effect of information about party policies on voter turnout.

Of course, as Downs himself observed, most citizens are not well-informed about policies.³ Indeed, most of the the existing empirical literature shows that voters rely on various short cuts and cues (Lupia, 1994; Popkin, 1991, 1993). They include party labels (e.g., Campbell *et al.*, 1960), candidate characteristics (e.g., McDermott, 1997, 2005; Sigelman *et al.*, 1995), past economic performance (e.g., Fiorina, 1981; Key, 1966), very recent experiences (e.g., Achen and Bartels, 2004),

³Even before Downs, others made a similar point (see e.g., Berelson *et al.*, 1954; Lippmann, 1922).

media coverage and televised advertisement (e.g., Ansolabehere and Iyengar, 1995; Ansolabehere *et al.*, 1994), and opinions of other voters (e.g., Huckfeldt and Sprague, 1995). While the importance of these factors is almost indisputable, it neither denies the importance of policy information nor precludes the need for empirically evaluating the impact of such information.

The second important feature of our experiment is the use of the Internet. In many democracies, including Japan, South Korea, and the United States, the Internet has become an important source of information for voters and an essential tool for parties and candidates to reach voters (e.g., Dulio *et al.*, 1999; Gibson *et al.*, 2003a,b). Accordingly, in Japan and elsewhere, facilitating and/or regulating information flow from parties to voters via the Internet is the focus of an important policy debate. Our study also contributes to the emerging literature which examines the question of how the Internet affects voters' political attitudes, opinions, and behavior (e.g., Johnson and Kaye, 2003; Lupia and Philpot, Nd; Weber *et al.*, 2003).

Finally, Japan is an interesting and important case when examining the causal effect of policy information. Recently, as an attempt to mobilize votes based on their policy proposals, major Japanese political parties have begun to prepare "manifestos" that explicitly state their formal policy proposals on major issues. Meanwhile, the recent decline in voter turnout is an often-expressed concern in Japan (e.g., Horiuchi, 2002). How much do these manifestos attract voters and revive their interests in politics? Does manifesto-based electioneering encourage voters to cast their ballots based on party policies rather than on their personal connections to a particular candidate? These questions are particularly important in the context of Japanese politics because Japan has recently undergone significant electoral and political reforms in order to minimize the influence of personal politics and to promote policy-based electoral campaigns and voter participation.

2.3 Use of a Randomized Field Experiment

To test the information hypothesis stated in Section 2.1, we designed and conducted an Internet-based randomized field experiment.⁴ While randomized field experiments face ethical and practical limitations, there is a major advantage when estimating the causal effect of information on voter turnout. Specifically, a field experimental study allows us to manipulate the quantity and/or quality of information each respondent receives during a real election. Survey researchers have analyzed various questions that are designed to measure the amount of information voters possess.⁵ While this literature has yielded considerable insight about the possible *association* between voting and information, they face a common methodological problem that hinders the estimation of the *causal effect* of information on voting behavior. That is, those voters who have a strong intention to vote may be more likely to acquire the information. Our experimental approach is designed to address this problem of *endogenous information acquisition*.

In our experiment, we employ a randomized block design to ensure efficient randomization (e.g., Cox and Reid, 2000) and apply the Bayesian causal inference framework of Imbens and Rubin (1997) to properly analyze randomized field experiments even when some unavoidable problems occur. Recent political scientists have seriously considered the above limitation of observational studies and have conducted randomized experiments. They examined, for example, the effects of televised political advertisement and various canvassing techniques (e.g., Ansolabehere and Iyengar, 1995; Ansolabehere *et al.*, 1994; Gerber and Green, 2000).⁶ Although these studies present

⁴See Harrison and List (2004) for various definitions of field experiment.

⁵These measures include factual test questions (e.g., Lupia, 1994), interviewers' subjective evaluations of each respondent's level of information (e.g., Bartels, 1996), indirect proxies such as levels of education and involvement in social activities (e.g., Rosenstone and Hansen, 1993; Wolfinger and Rosenstone, 1980), and other information indexes (e.g., Palfrey and Poole, 1987). See Luskin (1987) and Zaller (1992, pp.333–344) for comprehensive reviews.

⁶See Kinder and Palfrey (1993); McDermott (2002) for reviews of experimental studies in the political science literature.

a promising approach, even a well-designed experiment is not always free from complications, especially when it is conducted outside of a laboratory setting (see e.g., Imai, 2005). In particular, an experimenter cannot force everyone to answer all of the questions (i.e., nonresponse) and to receive a randomly assigned treatment (i.e., noncompliance). Ignoring these complications typically yields incorrect inferences.

Finally, our study builds on two existing studies and makes methodological improvements. Lassen (2005) examines the impact of policy information on voter turnout using data from a Copenhagen referendum on decentralization. His study is based on an observational study where the treatment assignment (i.e., the selection of districts for a pilot project of decentralization) is assumed to be nearly random.⁷ Wantchekon (2003) conducted a unique randomized field experiment in Benin to examine the effect of campaigns with different policy platforms on voting behavior (but not on turnout). Both studies successfully show the utilities of experimental approaches and address important substantive issues, yet they face a common methodological challenge. In particular, while their unit of analysis is an individual, the unit of treatment assignment is a group of individuals – villages in Wantchekon (2003) and districts in Lassen (2005). As a result, researchers do not have information as to whether individuals actually received the information. Such a mismatch between the units of analysis and the treatment requires additional assumptions and statistical adjustment when estimating the causal effect of the information on voting behavior (see e.g., Frangakis *et al.*, 2002).⁸ As in many other field experiments, the studies also may suffer from potential bias due to nonresponse. We design and analyze our experiment so as to minimize these potential sources of bias.

⁷See Angrist (1990) and Ho and Imai (2004, 2005) for natural experiments where the treatments are actually randomized.

⁸This common problem in field and natural experiments has not been properly dealt with in political science (see Imai, 2005).

3 Experimental Design

In June and July of 2004, we conducted an experiment through a Japanese Internet survey firm, *Nikkei Research*.⁹ Our experiment consisted of three separate surveys, as depicted in Figure 1; a screening survey, a pre-election survey, and a post-election survey. In the following, we explain the implementation of each survey and our experimental design in detail.

3.1 Screening Survey

Between June 24th and 29th, two weeks prior to the election, we conducted a screening survey and asked 6,000 respondents to answer several questions about themselves and their vote intention in the upcoming election.¹⁰ We collected the data on each respondent's prefecture of residence, age, gender, highest education completed, and party preference.¹¹ In addition, we asked the respondents whether they were planning to vote in the upcoming election (planning to vote, not planning to vote, or undecided), and, if so, which party and candidate they were planning to vote for, and how much confidence they had in their plan (a four-point scale). Note that both the Upper and Lower House elections in Japan adopt a combination of a plurality system and a proportional representation system. The wording of our survey questions closely followed the standard protocol from large-scale Japanese national surveys, such as those of the Japan Election Study (JES) projects. Furthermore, the survey was designed so that respondents must answer all

⁹The company has the sampling pool of roughly 40,000 Internet users, throughout Japan, who have agreed to receive occasional electronic mails asking them to participate in on-line surveys. Those who fill out a survey questionnaire have a chance to win a gift certificate in the amount of approximately five to ten dollars.

¹⁰We asked *Nikkei Research* to randomly select the equal number of male and female monitors, all of whom are aged between 20 to 59. Note that in Japan, unlike in the United States, every eligible voter (20 years old and above) is automatically registered.

¹¹For party preference, we first asked respondents to indicate whether they supported a particular party or a candidate, and then asked to rate their support for each of the five major parties on a four-point scale.

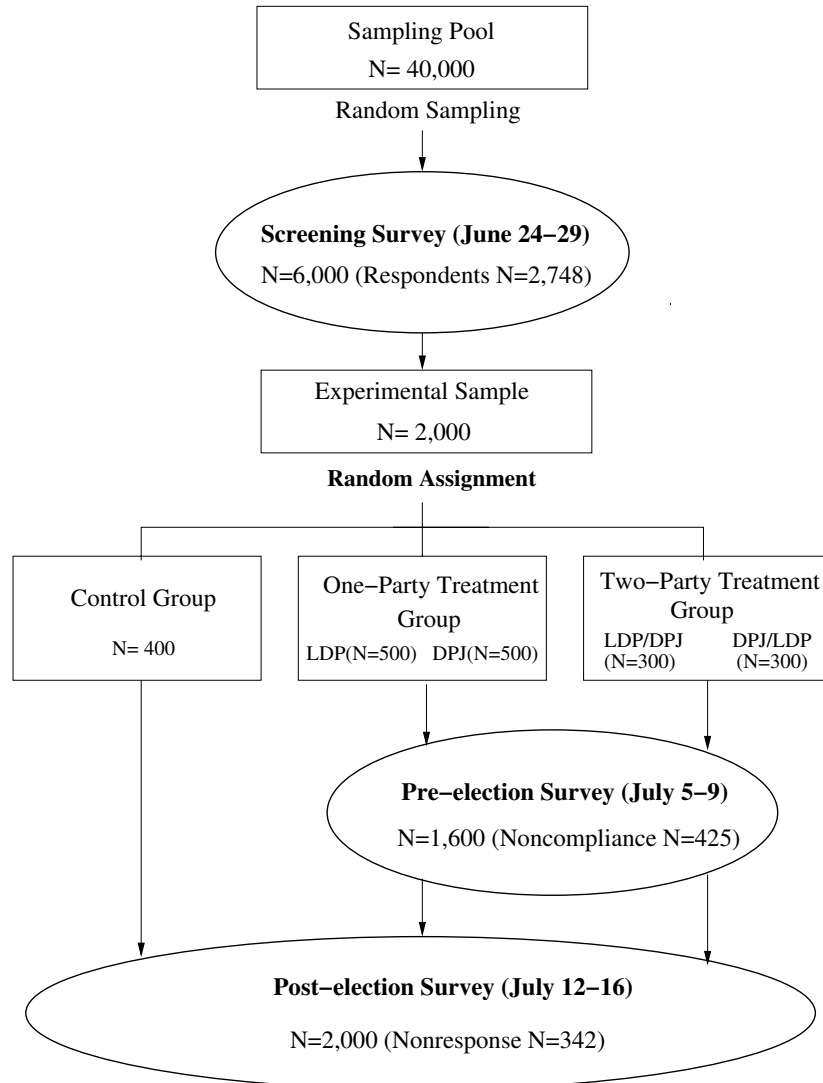


Figure 1: The Experimental Design. The experiment uses three surveys. The screening survey measures the pre-treatment covariates, and the pre-election survey administers the randomized treatments. Finally, the post-election survey measures the outcome variable.

the questions in order to complete the survey.¹²

Out of 6,000 individuals who received an electronic mail asking them to fill out the screening survey, 2,748 individuals completed the survey. From this group, we then randomly selected

¹²Since the Internet survey firm does not produce sampling weights, we ignore the problem of unit nonresponse here.

2,000 eligible voters as our experimental sample.¹³ With a few exceptions of over-represented urban prefectures, the comparison of the 2000 Census and our sample shows no clear evidence of geographical sampling bias. Yet, as is the case for many other experimental studies, our sample is not fully representative of the Japanese electorate. First, the individuals in our sample were those who had Internet access, voluntarily registered themselves as a monitor for the survey firm, and agreed to fill out the screening survey. While the lack of representativeness (as well as the unavailability of sampling weights) is a clear drawback for the use of the Internet survey in our experiment, we note that from a policy perspective we are often interested in examining the effectiveness of a party website for Internet users. Second, our sample is likely to contain active Internet users with some interest in politics. This means that, as seen later, the rates of noncompliance and nonresponse are much lower than typical field experiments. Although one ideally wants a representative sample of a target population, the high response and compliance rates bring their own advantages; our results rely less on statistical assumptions, than would be possible with a representative sample. These issues reflect a usual trade-off between internal and external validity in empirical research.¹⁴

3.2 Pre-election Survey

From July 5th to 9th, we administered the pre-election survey. The Upper House election was held on July 11th, two days after the closing of the survey. Before sending an electronic mail soliciting their participation in the survey, we randomly assigned the treatments to the voters of our experimental sample. In particular, we considered two types of treatments and randomly divided the sample into three groups, i.e., the two treatment groups and the control group. The voters in the *one-party treatment group* were asked to visit the designated website of either the Liberal Democratic Party (LDP) or the Democratic Party of Japan (DPJ), while those in the *two-*

¹³The sample size is reduced to 2,000 given the financial constraint.

¹⁴Experimental studies typically gain internal validity at the cost of external validity of observational studies.

	Randomized blocks						Total
	I	II	III	IV	V	VI	
	planning to vote male	female	not planning to vote male	female	undecided male	female	
One-party treatment group							
DPJ website	194	151	24	33	36	62	500
LDP website	194	151	24	33	36	62	500
Two-party treatment group							
DPJ/LDP websites	117	91	15	20	20	37	300
LDP/DPJ websites	117	91	15	20	20	37	300
Control group							
no website	156	121	19	26	29	49	400
Block size	778	605	97	132	141	247	2000

Table 1: Randomized Block Design of the Japanese Election Experiment: Six randomized blocks were formed on the basis of the two covariates, gender (male or female) and the answer to the question, “Are you going to vote in the upcoming election?” (“planning to vote”, “not planning to vote”, or “undecided”). Within each block, the complete random assignment of the treatments was conducted such that the size of each treatment and control group is equal to the predetermined number, which is listed in the right column. The total sample size is 2,000.

party treatment group were asked to visit the websites of both parties.¹⁵ None of the individuals in the control group were asked to participate in the pre-election survey. Our experimental design with two different treatment conditions allows us to examine not only whether exposure to policy information influenced voter turnout, but also how different levels of exposure affected turnout.

In order to randomly divide the sample into the two treatment and control groups, we applied a randomized block design as shown in Table 1. We formed six blocks on the basis of the gender and voter intention variables, which we obtained from the screening survey. We chose the voter

¹⁵Many formal models assume two major parties (or candidates) competing in an election. Japan’s 2004 Upper House election fits such a two-party competition model fairly well, because in this election, most voters were confronted with the choice to vote for the LDP, which has been in power since its foundation in 1955 (except during a short period from 1993 to 1994), or for the DPJ, which is the largest opposition party formed by amalgamation of various parties. In fact, in this election, 82 percent of seats were won by these two major parties. Other small opposition parties (e.g., Japanese Communist Party), as well as a small ruling party (i.e., New Komeitō), play minor roles in government formation and policy implementation.

intention variable because we thought it was an important predictor of voters' turnout decision and their compliance status.¹⁶ Within each of six randomized blocks, we conducted the complete randomization of treatments such that the total number of voters is 1,000, 600, and 400 for the one-party and two-party treatment groups, and the control group, respectively. Within the one-party treatment group, a random half of the voters were instructed to visit the DPJ website, whereas the other half was instructed to visit the LDP website. Similarly, for the two-party treatment group, a random half of the voters were instructed to visit the DPJ website first before visiting the LDP website, while the order was reversed for the other half.

One advantage of a randomized block design over a simple randomization design is that it reduces the observed and unobserved differences between the treatment and control groups and thus yields more efficient estimates (e.g., Cox and Reid, 2000).¹⁷ Table 4 in Appendix A illustrates the overall balance of the observed covariates, showing that none of the t -statistics is significant at conventional levels. That is, there are only small differences between the treatment and control groups in terms of the observed covariates including age, education, party preferences, etc. Finally, with the randomized block design, we can estimate the causal effects for each subgroup more efficiently.

For both the LDP and DPJ, we used the official website showing their party platform. Specifically, we selected the particular section of the two parties' official "manifesto" that describes their formal policy proposals on pension reform.¹⁸ Our focus on pension reform was motivated by the fact that it was one of the two major issues in this election along with the government's policy

¹⁶The definition of compliance will be fully discussed in Section 4.

¹⁷Other types of experimental designs with a similar idea include matched-pair design using optimal matching (Greevy *et al.*, 2004) and propensity score matching (Hill *et al.*, 1999)

¹⁸In Japan, parties and candidates are not allowed to change the contents of their websites during a campaign period. This regulation, though it may not be desirable from a normative perspective, is convenient for our experimental study, as we need not consider the possibility that different voters within the same treatment group viewed different web contents.

towards Iraq.¹⁹ The LDP website presented their pension reform policies by explaining in detail the legislation that had been passed in the Diet approximately one month before the election. This legislation was widely considered to constitute a set of minor changes to the current pension system. The LDP website gave little information about plans for further reforms, such as the possible future integration of various pension systems which has been widely debated. It also did not mention other unresolved and controversial issues.²⁰ In contrast, at the very beginning of its party manifesto, the DPJ emphasized the need for the national integration of pension systems and proposed the abolishment of the special pension for Diet members. While the DPJ website proposed major reforms, however, it did not specify the content of the reforms and did not explain how such proposals would be implemented.

Before being instructed to visit the website in the pre-election survey, voters were presented with a few brief questions about the pension reform. These were general questions that were intended to prepare voters before being exposed to the policy information. A translation of the complete questionnaire appears in Appendix B. After answering these questions, voters were instructed to click a direct link which took them to the designated party website. The instructions also included a friendly warning, which mentioned they would be asked about their opinions on the website after visiting it. We designed the survey so that voters had to visit the website in order to go to the next question. In addition, we also obtained information as to whether visitors actually opened the designated website in their browser even when the voters decided not to go to the next question. Finally, to complete the survey, voters were asked to answer several brief

¹⁹According to a poll conducted by *Asahi Shimbun* and the University of Tokyo, 53 percent of candidates and 61 percent of voters regarded pension reform as one of the most important issues in this election (*Asahi Shimbun*, evening edition, June 24, 2004).

²⁰These issues include the question of whether to abolish the special pension scheme available for Diet members, the political scandal where many Diet members had not been paying for their national pension premiums, and the politically sensitive question of whether to raise the consumption tax to pay for the ever increasing burden of the pension system.

questions about the website they had just visited. For those voters who were assigned to the two party websites, the same set of questions was presented after they visited each website. At the end of the survey, voters were also given a chance to freely write their opinions about the website. Although filling in this open-ended question was optional, nearly 80 percent of those who participated in the pre-election survey wrote some comments and/or opinions. The voters indicated a high level of interest in the pension reform, the upcoming election, and/or the party websites.

3.3 Post-election Survey

Finally, we conducted a post-election survey between July 12th and 16th to measure the outcome variable for all 2,000 experimental subjects. We used the same questionnaire for everyone, asking whether they had voted in the election. We kept the survey short in order to minimize the unit nonresponse, and as a result, more than 80 percent of the respondents completed the survey. In the next section, we demonstrate how to statistically adjust for this nonresponse problem when estimating causal effects in randomized field experiments with noncompliance.

4 Statistical Method for Randomized Experiments

In this section, we present a statistical method that we use to estimate the causal effect of policy information on voter turnout. First, we describe the two main complications of our experiment, noncompliance and nonresponse, which are absent in the ideal prototype of classical randomized experiments, but nevertheless are common for non-laboratory experiments. Second, we describe our Bayesian model that addresses these complications under certain assumptions. Finally, we briefly explain how to estimate the model and draw causal inferences.

4.1 Randomized Experiments with Noncompliance and Nonresponse

In political science, experimenters often do not have full control over their human subjects and hence are likely to face additional complications that call for statistical adjustments. In particular, the problems of noncompliance and nonresponse frequently threaten the internal validity of non-laboratory experiments. In the context of our experiment, some voters did not visit the designated website even when they were instructed to do so (i.e., noncompliance). Moreover, a few voters did not fill out the post-election survey, and therefore, the outcome variables were not recoded for them (i.e., nonresponse). Since these two problems typically do not occur at random, ignoring them in estimation may severely bias causal inferences.

To analyze randomized experiments with noncompliance and nonresponse, we begin by describing the formal statistical framework of Angrist, Imbens, and Rubin (1996) (see also Frangakis and Rubin, 2002). Let Z_i be the treatment assignment indicator variable, which is equal to 1 if voter i is instructed to visit a party website and is equal to 0 otherwise. Next, let T_i represent the (actual) treatment indicator variable, which is equal to 1 if voter i actually visits the website and is equal to 0 otherwise. Following our experimental design, we conduct three separate analyses with different binary treatment variables. First, we examine the causal effect of browsing one party website and two party websites, separately. In addition, we also estimate the causal effects of visiting at least one party website for different subgroups in the sample that we select on the basis of observed covariates.

For each analysis, then Z_i and T_i can be defined accordingly. If a voter logged on to the pre-election survey questionnaire website but did not visit the designated website, we set $T_i = 0$ for the voter. There are 133 such individuals, 63 of which belong to the one-party treatment group. This corresponds to about 10 percent of those voters who logged onto the survey website. In the two-party treatment group, there are 18 voters who visited only one designated website and did not complete the pre-election survey. We set $T_i = 0$ for these voters. However, everyone who visited the two party websites completed the survey (i.e., no drop out after visiting both websites). Because

of the drop-outs, we conduct sensitivity analyses using three different definitions of compliance (logged on to the survey website, visited the party websites, and completed the survey). The results do not appear to be sensitive to these definitions.²¹

The overall compliance rate (i.e., the proportion of those respondents in the sample who would take the treatment only when they are assigned to one), as shown in Section 5, is about 70 percent, which is quite high when compared to typical field experiments.²² As we demonstrate below, a high compliance rate is crucial for successful statistical analysis of randomized experiments. It reduces the degree to which estimated causal effects rely on model assumptions, which are often difficult to verify from observed data.

Finally, following Rubin (1974), we define two potential outcomes, $Y_i(1) \equiv Y_i(T_i = 1)$ and $Y_i(0) \equiv Y_i(T_i = 0)$. $Y_i(1)$ is the outcome (i.e., a binary variable of turnout) observed for voter i if the voter visited the designated website and $Y_i(0)$ is the outcome which would be observed if voter i did not visit the website. This means we only observe $Y_i = T_i Y_i(1) + (1 - T_i) Y_i(0)$. Moreover, there exists the nonresponse problem because some respondents did not fill out the post-election survey.²³ To introduce a formal notation, let R_i represent the indicator variable which is equal to 1 if Y_i is observed and equal to 0 if it is missing. In our data, there are 342 respondents among 2,000 individuals who did not fill out the post-election survey (75 of them belong to the control group, 113 of them are members of the one-party treatment group, and the others belong to the two-party treatment group). The attrition rate is roughly equal across the treatment and control groups.

²¹ Although it is possible to define a multi-value treatment and conduct appropriate analyses (see e.g., Imai and van Dyk, 2004), we take a sensitivity analysis approach here for simplicity.

²²For example, the compliance rate of Gerber and Green (2000)'s experiment is as low as 25 percent (Imai, 2005).

²³Note that by our design of the screening survey, we do not have any missing data for the covariates.

4.2 Defining Causal Effects

Given this setup, we define the causal effect of policy information on turnout for voter i as the difference between the two potential outcomes: $Y_i(1) - Y_i(0)$.²⁴ A fundamental problem of causal inference is that we can observe only one of the two potential outcomes, while the calculation of a causal effect requires both of them (Holland, 1986). Furthermore, two assumptions are implicit in this formulation. First, we assume *no interference among units* (Cox, 1958; Rubin, 1990). That is, for all $i \neq j$, we assume that voter i 's treatment assignment status, Z_i , does not affect voter j 's treatment status, T_j , and that the potential outcomes for voter j , $Y_j(1)$ and $Y_j(0)$, are not affected by the treatment status of voter i , T_i , or her treatment assignment status, Z_i . In our experiment, this assumption seems reasonable because the voters in our sample are unlikely to communicate with each other about the experiment.²⁵

Second, we assume *no direct effect of treatment assignment*. That is, for voter i , the treatment assignment status Z_i is assumed to affect the voter's potential outcomes, $Y_i(1)$ and $Y_i(0)$, only through the actual treatment status, T_i .²⁶ In our experiment, the assumption would be violated if a respondent changes her decision to vote or abstain because she was instructed to visit a party website even though she did not actually complete the pre-election survey. This scenario is a potential concern for 113 voters who logged onto the pre-election survey but did not complete it. Therefore, we conducted sensitivity analyses by applying various definitions of compliance. The results support the main conclusion of this paper.

Finally, we define two types of individuals in our experiment – compliers and noncompliers. Compliers refer to the voters who visit the designated party website *only when* instructed to do

²⁴Other causal effects such as $Y_i(1)/Y_i(0)$ are also possible.

²⁵We emphasize that this assumption is often implicitly invoked. In principle, one can relax the assumption by directly modeling the dependence between the potential outcomes of voter i and the treatment status of voter j . Doing so, however, significantly complicates the analysis and requires additional assumptions.

²⁶In the literature, this assumption is called *exclusion restriction*.

		Treatment Assignment	
		$Z_i = 1$	$Z_i = 0$
Actual Treatment	$T_i = 1$	Complier $Y_i(1)$ is observed; $C_i = 1$	
	$T_i = 0$	Noncomplier $Y_i(0)$ is observed; $C_i = 0$	Complier or Noncomplier $Y_i(0)$ is observed; $C_i = ??$

Figure 2: Compliers and Noncompliers in the Japanese Election Experiment. The figure classifies compliers and noncompliers by treatment assignment, Z_i , and actual treatment, T_i . We assume that noncompliers solely consist of never-takers. From the observed data, Z_i and T_i , one can identify compliers and noncompliers all but in one case where $Z_i = T_i = 0$. The upper right cell is empty because we assume that always-takers and defiers do not exist in this experiment.

so (i.e., $(T_i = 1, Z_i = 1)$ and $(T_i = 0, Z_i = 0)$), while noncompliers are those who do not follow the instructions. There are three types of noncompliers (Angrist *et al.*, 1996); always-takers, who do visit the party website regardless of whether they are instructed to do so (i.e., $(T_i = 1, Z_i = 1)$ and $(T_i = 1, Z_i = 0)$), never-takers, who do not visit the party website regardless of the instruction (i.e., $(T_i = 0, Z_i = 0)$ and $(T_i = 0, Z_i = 1)$), and defiers, who visit the website *only when* they are not instructed to do so (i.e., $(T_i = 1, Z_i = 0)$ and $(T_i = 0, Z_i = 1)$). We use C_i as the complier indicator variable, which is equal to 1 if respondent i is a complier and equal to 0 if he is a noncomplier. In our analysis, we assume that there are neither always-takers nor defiers. This assumption seems reasonable in our experiment.

Figure 2 summarizes our assumption about the types of noncompliers. It shows that from the observed data, i.e., treatment assignment Z_i and actual treatment T_i , we can identify the compliance status of the voters in the treatment group (the left column of the figure). However, for the voters in the control group (the lower right cell of Figure 2), we need to infer their compliance status using the observed compliance pattern of the treatment group. As we shall see below, randomization of treatment assignment makes such inference possible because it guarantees that voters in the control group are similar in their observed and unobserved characteristics.

Our quantities of interest can be defined separately for these two types of voters (Angrist

et al., 1996). By definition, noncompliers never receive the treatment and so their treatment effect cannot be inferred from the observed data. Therefore, we may focus on the causal effect of the treatment for compliers who are in the upper left and lower right cells of Figure 2. The estimand, the sample complier average causal effect (CACE)²⁷, is defined as,

$$\text{CACE} = \frac{\sum_{i=1}^N C_i [Y_i(1) - Y_i(0)]}{\sum_{i=1}^N C_i}. \quad (1)$$

In our experiment, CACE defines the causal effect of policy information on voting behavior *for those who would visit the website if they are told to do so*. It is important to note that CACE does not equal the usual sample average treatment effect, $\text{ATE} = \frac{1}{N} \sum_{i=1}^N [Y_i(1) - Y_i(0)]$, which is the causal effect for the entire sample. From the perspective of policymakers who want to increase voter turnout by using the Internet, CACE might be of greater interest than ATE because political parties can not force every voter to visit their party website. Hence, it is important to estimate the causal effect for those voters who are likely to be exposed to policy information via the Internet.

Another quantity of interest is the intention-to-treat effect, which represents the average causal effect of *treatment assignment*, Z_i , rather than the actual treatment, T_i . In our experiment, this is the causal effect of being asked to visit the party website rather than the effect of actually visiting the website. Unlike CACE, ITT effect does not directly correspond to the effect of actual treatment. However, it represents the effectiveness of the treatment assignment for the whole sample, rather than for a subsample. Despite this important difference, CACE and ITT effect are closely related because CACE represents ITT effect for compliers (this follows from the definition of compliers). Furthermore, ITT effect (for the whole sample) is the weighted average of ITT effect for compliers and noncompliers. As a result, ITT effect equals CACE multiplied by the fraction of compliers in the sample, $\sum_{i=1}^N C_i/N$. The relationship implies that a low compliance probability leads to a larger difference between CACE and ITT effect.

²⁷Another estimand of interest is its population counterpart (see Imbens, 2004)

4.3 Model, Estimation, and Inference

We now describe our model based on the assumptions explicitly stated above. In particular, we adopt the Bayesian causal inference framework of Imbens and Rubin (1997) because as demonstrated by Barnard *et al.* (2003) and others, this framework allows one to deal with noncompliance and nonresponse in a relatively straightforward manner (see also Hirano *et al.*, 2000; Frangakis *et al.*, 2002). Particularly, we apply the modeling approach of Barnard *et al.* (2003) to our experiment.

Our model consists of two parts. First, we specify the conditional probability of being a complier given each voter’s observed covariates, i.e., $\Pr(C_i = 1 \mid X_i)$. Note that the compliance status is unknown for the voters in the control group. Thus, their compliance status will be imputed based on the model and covariates. We use the following binary probit model with the latent variable U_i ,

$$C_i = 1 \quad \text{if} \quad U_i > 0 \quad \text{where} \quad U_i \sim \mathcal{N}(\delta_k + X_i^\top \xi, 1), \quad (2)$$

where δ_k is the intercept specific to each of the randomized blocks $k = 1, \dots, 6$, and ξ is the vector of coefficients. From this model, one can estimate the fraction of compliers in the sample, $\sum_{i=1}^N C_i/N$, by imputing the unknown compliance status for the voters in the control group.

Next, we model turnout given the compliance status, the treatment status, T_i , and the observed covariates, X_i , i.e., $\Pr(Y_i = 1 \mid C_i = c, T_i = t, X_i)$ for $t = 0$ or 1 . Again, we use the binary probit model defined via the latent variable, W_i ,

$$Y_i = 1 \quad \text{if} \quad W_i > 0 \quad \text{where} \quad W_i \sim \mathcal{N}[\alpha_k + C_i \{\beta_0 T_i + \beta_1 (1 - T_i)\} + X_i^\top \gamma, 1]. \quad (3)$$

where β_0 and β_1 are the intercepts specific to compliers with and without the treatment, respectively. The base category is noncompliers.

Finally, the two parts of the model in equations 2 and 3 are combined to form the following

complete-data likelihood function,

$$\begin{aligned} & \prod_{i=1}^N \left[\Phi(\delta_k + X_i^\top \xi) \Phi(\alpha_k + \beta_0 T_i + \beta_1(1 - T_i) + X_i^\top \gamma)^{Y_i} \right. \\ & \quad \times \left. \{1 - \Phi(\alpha_k + \beta_0 T_i + \beta_1(1 - T_i) + X_i^\top \gamma)\}^{1 - Y_i} \right]^{C_i} \\ & \quad \times \left[\{1 - \Phi(\delta_k + X_i^\top \xi)\} \Phi(\alpha_k + X_i^\top \gamma)^{Y_i} \{1 - \Phi(\alpha_k + X_i^\top \gamma)\}^{1 - Y_i} \right]^{1 - C_i}, \end{aligned} \quad (4)$$

where Φ is the cumulative probability function of the standard normal distribution (see also Hirano *et al.*, 2000). We cannot directly evaluate this likelihood because the compliance status is not observed for the voters in the control group. Therefore, we use the Markov chain Monte Carlo (MCMC) algorithm described below, to conduct a Bayesian analysis.

In our experiment, approximately 17 percent of the voters did not fill out the post-election survey. For those voters, the values of the outcome variables are missing. However, deleting the observations with nonresponses may severely bias our causal inferences (e.g., Frangakis and Rubin, 1999). We deal with the nonresponse problem by modeling the missing data mechanism concerning the outcome variable, Y_i . Specifically, we assume that the pattern of missing data is conditionally independent of potential outcomes given the compliance status, the treatment status, and the observed covariates, i.e., $R_i \perp Y_i(0), Y_i(1) \mid C_i, T_i, X_i$ where \perp denotes the independence.²⁸ That is, by using C_i , T_i , and X_i , we predict the missing values of the outcome variable via the model specified in equation 3. In our experiment, X_i includes many of the basic predictors of voting behavior among Japanese voters. Therefore, this assumption seems quite reasonable.

To complete our model in the Bayesian framework, we assign two independent conjugate prior distributions on $(\alpha, \alpha_1, \alpha_0, \beta)$ and (δ, γ) , both of which are multivariate Normal distributions. We then sample from the joint posterior distribution via a MCMC algorithm; we sample iteratively from $p(C_i^{\text{mis}} \mid \delta_k, \xi, \alpha_k, \beta_0, \beta_1, \gamma, T_i, X_i, Z_i), p(U_i \mid \delta_k, \xi, C_i, X_i), p(\delta_k, \xi \mid X_i, U_i), p(Y_i^{\text{mis}} \mid \alpha_k, \beta_0, \beta_1, \gamma, C_i, T_i, X_i), p(W_i \mid \alpha_k, \beta_0, \beta_1, \gamma, C_i, T_i, X_i, Y_i)$ and $p(\alpha_k, \beta_0, \beta_1, \gamma \mid C_i, T_i, X_i, W_i)$ where

²⁸This assumption is called *latent ignorability* because the missing mechanism depends on the partially observed variable, C_i (Frangakis and Rubin, 1999).

C_i^{mis} and Y_i^{mis} represent the missing values of compliance status and outcome variable. The usual calculation shows that all the conditional distributions are standard. In our implementation, we use the marginal data augmentation algorithm of Imai and van Dyk (2005) to exploit the latent variable structure and speed up the convergence. Our inference is based on 50,000 draws after discarding the initial 10,000 draws. We use independent diffuse prior distributions; i.e., Normal distribution with mean zero and variance 100. The MCMC algorithm produces the posterior draws for the unobserved compliance status and the missing values of the outcome variable, as well as the model parameters. Given these posterior draws, the average causal effect and its uncertainty estimate can be calculated. The Gibbs sampling algorithm is implemented by our own C code, which we make publicly available with an easy-to-use R interface.

5 Estimated Causal Effects of Policy Information

In this section, we present the results of our statistical analysis. Our main finding is that voters are less likely to abstain when they receive policy information about *both* ruling and opposition parties through their official party websites. Moreover, the information effects are larger among those voters who were planning to vote but were undecided about which party to vote for.

First, Table 2 compares the causal effect of one-party treatment with that of two-party treatment by presenting the posterior summaries of quantities of interest; ITT effect, CACE, proportion of compliers in the sample. Also, the estimated turnout rates for the control group are shown separately for compliers, noncompliers, and the entire sample. They serve as the baseline turnout of no exposure to the designated party websites.²⁹ The estimated baseline turnout rate is about 70 percent, which is almost 15 percentage points higher than the official turnout rate of the 2004 Upper House election. This gap arises in part because our sample is not representative of the

²⁹These turnout rates for the control group are estimates because we do not observe the outcome variable for some voters and the compliance status is unknown for every voter in the control group.

	Summary of Posterior Distributions			
	mean	s.d.	2.5%	97.5%
One-party Treatment				
Intention-to-Treat (ITT) Effect	0.008	0.022	-0.034	0.052
Complier Average Causal Effect (CACE)	0.011	0.029	-0.045	0.069
Fraction of Compliers	0.751	0.007	0.736	0.764
Turnout for the Control Group				
Compliers	0.706	0.023	0.661	0.752
Noncompliers	0.675	0.057	0.559	0.783
All	0.698	0.009	0.680	0.715
Two-party Treatment				
Intention-to-Treat (ITT) Effect	0.031	0.022	-0.013	0.074
Complier Average Causal Effect (CACE)	0.044	0.032	-0.019	0.106
Fraction of Compliers	0.704	0.012	0.681	0.726
Turnout for the Control Group				
Compliers	0.685	0.026	0.634	0.738
Noncompliers	0.727	0.053	0.617	0.825
All	0.697	0.009	0.680	0.715

Table 2: Estimated Causal Effects of Policy Information on Voter Turnout for One-party and Two-party Treatments. The figures represent the numerical summaries of posterior distributions for each quantity of interest separately for the one-party treatment and two-party treatment conditions: standard deviation, and 95 percent credible interval. The estimated turnout rates for the control group are shown separately for compliers, noncompliers, and the entire sample, as the baseline turnout of no exposure to the designated party websites.

Japanese electorate. It is also possible that self-reported turnout is biased (e.g., Burden, 2000; Campbell *et al.*, 1960; Silver *et al.*, 1986). The magnitude of such self-reporting bias is minimal, however, unless the degree of mis-reporting is affected by the actual treatment or the treatment assignment. Finally, the estimated model parameters for the two-party treatment effect appear in Appendix C.

As expected from the hypothesis, the posterior means of CACE and ITT effects estimates are all positive. The causal effects of one-party treatment are small and there is relatively large uncertainty. The probability of voting increases (from 70.6 percent) by only 1.1 percentage points on average among those voters who *actually visited* one party website (CACE), and the increase is on average less than one percentage point (from 69.8 percent) for those who were *asked to visit* one website (ITT effect). Since the estimated proportion of compliers is considerably high (75.1

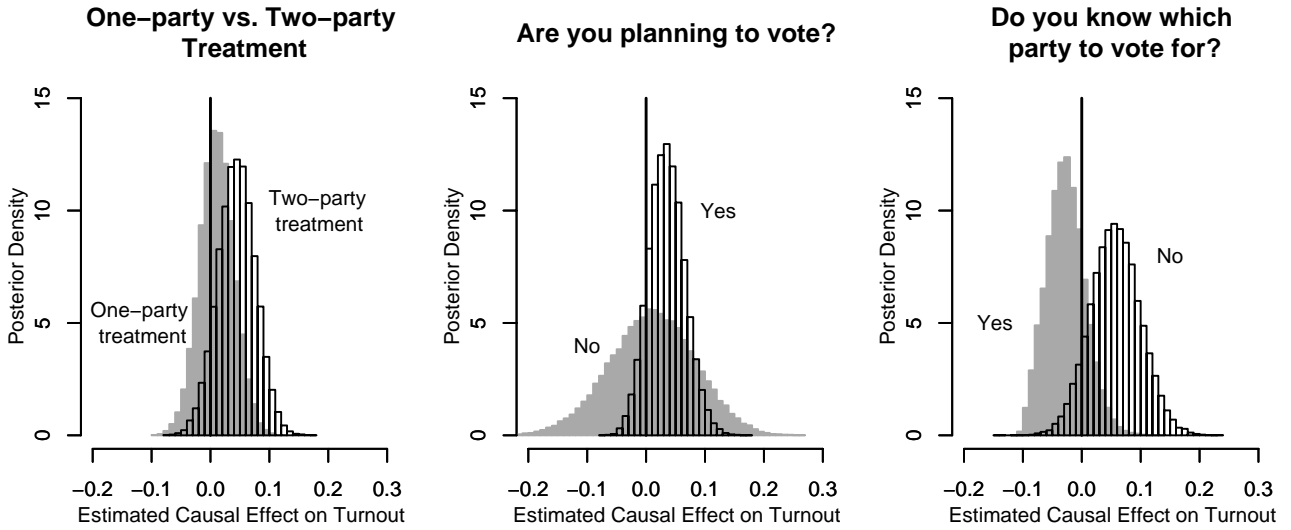


Figure 3: Histograms of Posterior Simulation Draws for the Estimated Complier Average Causal Effects (CACE) on Turnout. The left panel compares the one-party treatment effect (shaded histogram) with the two-party treatment effect (unshaded histogram). The middle panel compares CACE using two subgroups – those voters who were planning to vote (unshaded) and those who were not (shaded). The right panel compares the causal effects for another set of two subgroups – those who knew which party to vote for (shaded) and those who did not (unshaded). The vertical lines represent the zero causal effect.

percent in the one-party treatment and 70.4 percent in the two-party treatment), the estimated ITT effects and CACE are somewhat similar in our study. Large posterior standard deviations mean that one cannot statistically distinguish these small positive estimates from zero.

In contrast, the causal effects of the two-party treatment are stronger. The turnout probability increases (from 69.7 percent) by 3.1 percentage points on average if a voter is asked to visit the websites of both LDP and DPJ. The causal effect among compliers is even larger, showing that those who actually visited the two party websites were on average 4.4 percentage points more likely to vote than those who did not.³⁰ Although the 95 percent Bayesian credible intervals include zero for both ITT effect and CACE, approximately 91 percent of posterior draws take positive values. This suggests that exposing voters to the policy information of both parties has a positive effect

³⁰If we also regard those voters who have logged on the pre-election survey as compliers, the estimated effect is slightly smaller. For the two-party treatment effect, for example, we estimate 2.7 percentage points for ITT effect (standard deviation 2.8) and 3.5 percentage points for CACE (standard deviation 3.5) on average.

on their turnout. The left panel of Figure 3 presents the histograms of posterior simulation draws for one-party and two-party treatments, graphically illustrating the difference between the two.

Our findings offer support for the hypothesis that policy information increases voter turnout, in particular, when being exposed to the policy information of *both* ruling and opposition parties (as opposed to that of just one party). One possible interpretation is that voters can only improve their understandings of policy differences of the two parties when they *compare* policy proposals.

Finally, we examine whether the size of causal effects differs across certain types of voters. Using some of the pre-treatment variables measured in the screening survey, we analyze different subgroups related to vote intention. Table 3 presents the results of this analysis for four subgroups of interest. Because of the limited sample sizes, we estimate the effect of visiting at least one party's website by pooling the one-party and two-party treatment groups. First, we compare those voters who said they were planning to vote with those who said they were undecided or not planning to vote, based on one of the pre-treatment variables used to define randomized blocks.³¹ The results indicate that visiting the designated website increases turnout by 3.5 percentage points (from 86.0 percent) on average among those who said they were planning to vote. On the other hand, there is very little effect on those who said they were undecided or not planning to vote. While the small sample size makes the finding somewhat inconclusive, our treatment had little effect for those who did not have a strong intention to vote in the first place. However, we regard the effect size quite large for those voters who were planning to vote given that the baseline turnout without exposure to the party websites is estimated to be greater than 85 percent. The middle panel of Figure 3 shows the histograms of posterior distributions of CACE for the two subgroups. Approximately 88 percent of posterior draws take positive values for the subgroup of those who said they were planning to vote.

We also compare the group of voters who knew which party to vote for with that of those

³¹We pool those voters who were undecided and those who were not planning to vote because of limited sample sizes for these two groups.

	Summary of Posterior Distributions			
	mean	s.d.	2.5%	97.5%
Planning to Vote				
Intention-to-Treat (ITT) Effect	0.026	0.023	-0.017	0.073
Complier Average Causal Effect (CACE)	0.035	0.031	-0.023	0.097
Fraction of Compliers	0.750	0.006	0.738	0.761
Turnout for the Control Group				
Compliers	0.860	0.021	0.819	0.903
Noncompliers	0.852	0.051	0.743	0.942
All	0.858	0.009	0.838	0.874
Undecided / Not Planning to Vote				
Intention-to-Treat (ITT) Effect	0.007	0.050	-0.094	0.103
Complier Average Causal Effect (CACE)	0.011	0.072	-0.136	0.149
Fraction of Compliers	0.694	0.009	0.674	0.712
Turnout for the Control Group				
Compliers	0.311	0.057	0.197	0.423
Noncompliers	0.373	0.091	0.200	0.556
All	0.331	0.022	0.293	0.374
Knew which Party to Vote For				
Intention-to-Treat (ITT) Effect	-0.021	0.025	-0.065	0.032
Complier Average Causal Effect (CACE)	-0.027	0.032	-0.085	0.042
Fraction of Compliers	0.769	0.008	0.753	0.783
Turnout for the Control Group				
Compliers	0.926	0.024	0.882	0.972
Noncompliers	0.897	0.061	0.769	1.000
All	0.919	0.010	0.897	0.938
Didn't Know which Party to Vote For				
Intention-to-Treat (ITT) Effect	0.041	0.030	-0.018	0.100
Complier Average Causal Effect (CACE)	0.057	0.042	-0.025	0.141
Fraction of Compliers	0.713	0.006	0.700	0.725
Turnout for the Control Group				
Compliers	0.556	0.033	0.492	0.620
Noncompliers	0.604	0.065	0.476	0.728
All	0.570	0.013	0.543	0.594

Table 3: Vote Intention and Estimated Causal Effects of Policy Information on Voter Turnout. The figures represent the numerical summaries of posterior distributions for each quantity of interest separately for the one-party treatment and two-party treatment conditions: standard deviation, and 95 percent credible interval. The estimated turnout rates for the control group within each subsample are shown separately for compliers, noncompliers, and the entire subsample, as the baseline turnout of no exposure to the designated party websites.

who did not two weeks prior to the election. The results show that visiting the designated party websites had a slightly negative effect on the former group. In fact, 80 percent of posterior draws for CACE take negative values. In contrast, the policy information raises turnout by 5.7 percentage points on average among those who did know which party to vote for. Again, although a relatively large posterior standard deviation prevents us from drawing a definitive conclusion, the right panel of Figure 3 show that 91 percent of posterior draws exceeds zero for this subgroup. While the sample of our experiment is too small to conduct a direct test and draw a definitive conclusion, the independent analyses of these subgroups suggest that policy information may have a positive impact on the turnout of those who are willing to vote and yet are undecided about which party to vote for.

6 Concluding Remarks

In this paper, we empirically tested the information hypothesis that the more information voters receive, the more likely they are to vote. We designed an Internet-based randomized field experiment during Japan's 2004 Upper House election and estimated the causal effects of policy information on voter turnout. The results offer support for the information hypothesis. We found that viewing the manifestos of *both* ruling and opposition parties through their official party websites increases voter turnout by more than four percentage points on average. We also found some suggestive evidence that the size of information effects varies with voters' party preference and vote intention.

These findings bear important implications. First, political scientists since Downs have shown that voters are typically uninformed and hence use short cuts and cues, rather than policy information, when making their voting decisions. However, we found that policy information influences decision making of voters especially when voters are willing to vote but have not decided which party to vote for. Second, our findings suggest that official party websites and the Internet in general may provide an important source of information for voters and an essential tool for political

parties to reach potential voters. In the context of Japanese politics, we found that manifesto-based and policy-oriented electioneering does influence voting behavior. Thus, more active campaigning and policy discussion via the Internet have the potential to increase voter participation in the future.

We believe that there are a variety of possibilities for further research on the causal relationship between information and voting behavior. Given our main finding that more information leads to a higher turnout, future research might investigate *why* and *how* information increases turnout. Existing studies in the literature suggest a number of possible causal mechanisms. Information may affect voters' behavior through different factors such as partisanship, perceived party differentials, confidence in vote choice, and political sophistication (e.g. Brody and Page, 1972; Conover and Feldman, 1989; Krosnick, 1988; Markus and Converse, 1979; Merrill *et al.*, 2001; RePass, 1971; Sniderman *et al.*, 1982). While we found that policy information received right before the election influenced voting behavior, it is also possible that information given earlier during campaign periods affect voters' decision making in different ways.

Finally, we demonstrated that randomized field experiments can be used to effectively test the information hypothesis by avoiding the endogenous information acquisition problem that is common in the analysis of social surveys. Randomized block designs as well as other similar methods (e.g., matched-pair designs) can ensure efficient randomization. Our Bayesian statistical method overcomes non-compliance and non-response problems, which are frequently encountered in field experiments. We were able to enhance the validity of the statistical assumptions about possible nonresponse and noncompliance by measuring a number of important pre-treatment covariates through the screening survey and by minimizing the number of non-compliers. We hope that this paper serves as a methodological template for future empirical inquiry on the causal relationship between information and voting.

Appendices

A Balance of Observed Covariates After Randomization

The figures in Table 4 represent t -statistics testing mean differences for each pretreatment variable across six randomized blocks. Each of the four columns shows the overall balance of the variables separately for each treatment group compared with the control group. No t -statistic indicates statistically significant mean differences at 0.05 level, implying that the pretreatment variables are well balanced for all treatment assignments.

B Pre-election Survey

We prepared different sets of questionnaires for two treatment groups. The voters in the one-party treatment group were asked to visit the designated website of either the LDP or the DPJ. Those in the two-party treatment group were asked to visit the websites of both parties (the LDP website before visiting the DPJ website or the DPJ website before the LDP website). Below, we only show the questionnaire for the one-party treatment group using the LDP website. The other questionnaires are almost identical except that Screens C and D are repeated for the two-party treatment group.

Screen A Please answer questions starting from the one below: Do you agree, partially agree, partially disagree or disagree with each of the following items? Please select the response that best reflects your position. (Please select one only)

- I am interested in the current developments in pension reforms.
- Recently, I have had negative feelings towards people who avoid paying national pension premiums.
- The national pension system needs to be fundamentally reformed.

- The positions on national pension reforms are considerably different between ruling parties and opposition parties.
- When voting in the recent Upper House Election, I will take into consideration of each party's pension policy.

Screen B In the upcoming Upper House Election, each party's pension reform proposal receives much attention. We would like you to first read the Liberal Democratic Party's reform plan published in their website, and then express your thoughts using questions such as "Are the explanations on reform proposals easy to understand?", "What is your perception on the 'readiness' of the system to carry out reforms?", and "Do the proposals take into consideration the positions of various people?".

Screen C We now ask you to take a look at the Liberal Democratic Party's proposal.

1. If you click on the button below, the Liberal Democratic Party's website pops up [in a different window].
2. If you scroll down the page, you will see a passage titled "To put the pension system on a firmer basis" (about 1 page). Please read this passage carefully.
3. When you finish reading, click on the "to questions" button at the bottom of this page and continue onto the next page. (If you do not open the Liberal Democratic Party's website, you cannot open the question page.)

Screen D We would like to ask for your thoughts on the Liberal Democratic Party's website and the party's pension reform proposal. (If you want to browse the Liberal Democratic Party's website one more time, please click here.) Do you agree, partially agree, partially disagree or disagree with each of the following items? Please select the response that best reflects your position. (Please select one only)

- I thought the explanations regarding the pension reform proposal were easy to understand.
- I received the impression that the proposal is fairly drastic.
- I could perceive the political party’s preparedness and responsibility towards the pension reform.
- There seemed to be support to unify pensions from people of various occupations.
- I thought that adequate measures were formulated to address the financial aspect of the pension system.
- I thought the proposed pension system would take into consideration women and those who have been injured.

Screen E Lastly, please write your own opinions about the pension reform and the party websites that convey these reform proposals. (Please make specific points, and do not use *hankaku katakana*.)

C Estimated Model Parameters

Table 5 shows the posterior summaries for each of the estimated parameters of compliance and outcome models in our Bayesian model with the two-party treatment. The covariates used in the estimation include each respondent’s age, whether her highest education completed is college or above, whether she knew which party to vote for in the 2004 Upper House election (4 point scale), and whether she thought the LDP, New Kōmeitō, JCP (Japan Communist Party), or SDP (Social Democratic Party) was her preferred party (4 point scale). These variables are taken from the screening survey. We also added the aggregate voter turnout rate in the prefecture of the respondent’s residence in the previous Upper House election in 2001.

	One party treatment		Two party treatment	
	LDP	DPJ	LDP/DPJ	DPJ/LDP
Age	-0.27	0.09	-0.97	-1.11
Age ²	-0.19	0.09	-0.89	-1.04
Gender (Male=1, Female=2)	0.06	0.06	0.09	0.09
Highest education				
Junior highschool	-0.27	-0.68	-0.13	0.35
Highschool	0.39	-0.47	0.46	0.57
Vocational school	-1.21	-0.12	0.44	-0.39
Two-year college	-0.44	0.47	-0.09	0.85
College	1.27	0.43	-0.20	-1.00
Graduate school	-1.09	-0.43	-0.85	0.09
Four point scale variables about voter preferences				
Preferred party or candidate	0.14	-0.27	-0.32	-0.28
LDP is a preferred party	-0.77	0.26	0.24	0.39
DPJ is a preferred party	-0.24	0.90	-0.30	-0.18
Komeito is a preferred party	-0.21	0.64	0.77	0.50
JCP is a preferred party	-0.27	1.00	-0.30	0.83
SDP is a preferred party	0.37	1.23	-0.78	-0.46
Planning to vote	-0.08	-0.08	0.02	0.02
Proportional representation: Planning to vote for				
LDP	0.24	0.49	-1.31	-0.79
DPJ	-0.08	-1.16	-0.14	-0.70
Komeito	0.35	0.32	0.61	0.87
JCP	0.93	1.57	0.81	0.46
SDP	-0.26	-0.95	0.36	0.66
other party	-1.23	0.19	0.75	0.83
Confidence (4 point scale)	-0.60	0.04	0.19	0.17
Electoral district: Planning to vote for				
LDP candidate	0.99	0.89	-1.35	-0.06
DPJ candidate	-0.22	-0.46	-0.08	-0.46
Komeito candidate	-0.62	-0.29	0.15	-0.51
JCP candidate	0.11	0.66	0.64	-0.85
SDP candidate	-1.49	-1.80	-0.34	-1.08
Independent candidate	-0.70	-0.31	0.38	1.29
Other candidate	1.66	0.80	1.81	1.35
Confidence (4 point scale)	0.68	0.08	0.20	0.44
Not planning to vote	0.07	0.07	0.17	0.17
Undecided	0.04	0.04	-0.17	-0.17

Table 4: Overall Balance of Pretreatment Variables Across Randomized Blocks. The figures represent t -statistics for all pre-treatment covariates comparing each treatment group with the control group.

	Summary of Posterior Distributions			
	mean	s.d.	2.5%	97.5%
Compliance Model Parameters				
Intercept for Block I	0.945	1.061	-1.173	2.995
Intercept for Block II	0.824	1.065	-1.296	2.894
Intercept for Block III	1.040	1.079	-1.112	3.138
Intercept for Block IV	0.709	1.060	-1.403	2.766
Intercept for Block V	0.422	1.076	-1.725	2.514
Intercept for Block VI	0.924	1.072	-1.208	3.010
Age	-0.001	0.006	-0.013	0.011
Highest education completed: college or above	0.019	0.118	-0.212	0.251
Knew which party to vote for	0.169	0.131	-0.088	0.428
LDP is a preferred party	-0.036	0.134	-0.298	0.225
DPJ is a preferred party	0.009	0.141	-0.270	0.284
New Kōmeitō is a preferred party	0.092	0.160	-0.223	0.405
JCP is a preferred party	-0.280	0.164	-0.604	0.041
SDP is a preferred party	0.172	0.168	-0.155	0.503
Aggregate turnout in 2001	-0.005	0.017	-0.039	0.029
Outcome Model Parameters				
Compliers with treatment	-0.184	0.182	-0.543	0.172
Compliers without treatment	-0.380	0.261	-0.887	0.139
Intercept for Block I	-0.124	1.118	-2.327	2.038
Intercept for Block II	-0.442	1.109	-2.628	1.709
Intercept for Block III	-2.112	1.124	-4.330	0.060
Intercept for Block IV	-2.114	1.117	-4.314	0.045
Intercept for Block V	-1.438	1.112	-3.618	0.709
Intercept for Block VI	-1.355	1.117	-3.549	0.805
Age	0.024	0.007	0.011	0.037
Highest education completed: college or above	0.033	0.120	-0.202	0.269
Knew which party to vote for	0.554	0.146	0.267	0.838
LDP is a preferred party	-0.192	0.136	-0.461	0.072
DPJ is a preferred party	0.157	0.147	-0.133	0.447
New Kōmeitō is a preferred party	-0.108	0.162	-0.422	0.213
JCP is a preferred party	-0.204	0.170	-0.541	0.127
SDP is a preferred party	0.205	0.173	-0.129	0.543
Aggregate turnout in 2001	0.010	0.018	-0.025	0.045

Table 5: Posterior Summaries for Each of the Estimated Parameters in the Two-Party Effect Model. The columns show the mean, standard deviation, and 95 percent credible interval of the posterior distributions.

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