# Voting for Democracy: Campaign Effects in Chile's Democratic Transition

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**Abstract**

Elections in authoritarian regimes often present conditions under which campaigns should affect voting behavior, typically in favor of the democratic opposition. However, scholars of both voting behavior and democratization have largely ignored the effect of campaigns in cases of electoral authoritarianism. In this paper I examine the effect of televised campaign advertising on vote choice in the 1988 plebiscite that inaugurated Chile's transition to democracy. Based on a matching analysis of post-electoral survey data, I show that the advertising of the opposition's "No" campaign made Chileans more likely to vote against dictator Augusto Pinochet, whereas the advertising of the government's "Yes" campaign had no discernible effect. In Chile, as well as instances of "democratization by elections" in the Philippines, Serbia, and Ukraine, opposition campaigns may have played an important causal role in the eventual change of political regime.

# 1 Introduction

The predominant form of non-democratic rule in the modern world is electoral authoritarianism—a regime type in which autocrats submit to the polls on a regular basis while ensuring that the rules of the game are rigged in their favor. Such contests may be fought on an unlevel playing field, but they are also distinct from sham elections where dictators run unopposed and have their reelection rubber-stamped. Incumbents and opposition candidates both campaign for votes in authoritarian elections, and sometimes the opposition scores a surprising victory, eventually leading to a democratic transition. The effects of these campaigns on voting behavior is thus a question of great substantive as well as theoretical importance. Does the intensity of government propaganda make voters more likely to favor the status quo in an authoritarian election? Or can an opposition campaign, despite the uphill battles that it faces, convince citizens to vote for democracy?

The question of campaign effects under electoral authoritarianism has been largely ignored by scholars of voting behavior. Research on campaign effects in established democracies has a long pedigree, but studies of new democracies are fewer in number, and those examining authoritarian elections are virtually nil. Studies of public opinion under authoritarian rule have examined sources of ongoing support for these regimes, but not the instances in which their leaders sometimes submit to elections (Geddes and Zaller, 1989; Kennedy, 2009; Stockmann and Gallagher, 2011). Yet, as I will argue below, elections held under authoritarian rule generate a relatively strong expectation of campaign effects in favor of the democratic opposition as long as it is allowed to communicate its message to the public. Where adequate data are available, authoritarian elections present a ripe opportunity for examining the effects of campaign persuasion on individual voters.

For its part, the study of electoral authoritarianism and transitions to democracy has also largely ignored the role of campaigns. Even the recent literature on "democratization by elections" (Lindberg, 2009) typically assumes that authoritarian incumbents lose elections because a long-hostile public has been waiting to throw them out. Yet dictators might also be defeated at the polls because an opposition or civil society campaign convinces initially apathetic or ambivalent voters to take a chance on democratization. Whether the process ends with a dictator conceding defeat,

or attempting to steal the election and being driven from power by mass protests, such transitions might never have gotten underway if not for an effective campaign. Examples include the defeat of Chile's Augusto Pinochet, examined in detail in this paper, as well as transitions in the Philippines, Serbia, and Ukraine, which I discuss in a comparative section. The role of campaigns in democratization is an aggregate-level question—did the campaign shift enough votes to alter the outcome of the election—but individual-level analysis can play an important role in answering it.

The bulk of this paper examines the effect of televised campaign advertising on individual voting behavior in the 1988 plebiscite that inaugurated Chile's transition to democracy—one of the first examples of democratization involving a dictator's defeat in "stunning elections" (Huntington, 1991). In this up-or-down vote on Pinochet, the government and opposition were each granted free television advertising time in the month before the election—one of the few elements of a level playing field. Using matching, I compare groups of survey respondents who internalized each side's advertising message to different degrees but are similar in other respects that could influence their vote. I find that receiving the message of the opposition's "No" campaign made Chileans more likely to vote against Pinochet, whereas internalizing the claims of the government's "Yes" campaign had no effect on voting behavior.

My approach to the analysis of campaign effects improves on many prior observational studies. Previous efforts have often been frustrated by the use of simple linear additive regression models that do not distinguish the effects of cross-cutting campaign communications (Zaller, 1996). By using a matching technique that makes no functional form assumptions and a treatment indicator that separately measures receipt of each side's campaign message, I am able to avoid these pitfalls. The matching procedure also passes several placebo tests, arguing against the presence of an unobserved confounder, and the findings are robust to alternative estimation methods or assumptions about the voting behavior of non-respondents. While causal inference with observational data always requires more assumptions than with experimental or semi-experimental research designs, the present study should compare favorably to other survey-based analyses of campaign effects.

In the final section of the paper, I shift to an aggregate level of analysis and a comparative per-

spective, examining the implications of campaign effects for the outcome of Chile's plebiscite as well as for other cases of democratization by elections. Combined with other evidence, such as the opposition's steady growth in the polls precisely during the period of televised campaign advertising, campaign effects on individual voting behavior in Chile do indeed suggest that the campaign contributed to the opposition's electoral victory. If campaign effects could be demonstrated in other cases of democratization by elections, a similar story might be told elsewhere. Examining the electoral defeats of Ferdinand Marcos in the Philippines, Slobodan Milošević in Serbia, and Viktor Yanukovych in Ukraine, I show that an opposition victory in these contests was not as inevitable as it is often portrayed in hindsight. Rather, candidates' fluctuation in the polls and journalists' predictions of the outcome suggest that campaigns may have shifted voters' intentions in these elections, sometimes at the last minute.

## 2 Campaign Effects and Democratization by Elections

There are strong reasons to expect that when the opposition to an authoritarian regime is allowed to campaign in an election—even one that falls short of the "free and fair" standard—its efforts will affect voters' behavior at the polls. A longstanding tradition of research on political behavior has found that partisan attachments and preexisting stores of relevant political information tend to render voters resistant to persuasion by campaigns or the media (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Converse, 1962; Zaller, 1992). Both resistance factors should be particularly weak in cases of elections held under authoritarian rule. With respect to partisan sentiments, political parties may have been in recess, or competing on only a highly constrained basis, for many years prior to the current election. Voters may be largely unfamiliar with the parties presenting candidates for office, much less have any particular party loyalty that would guide their vote choice and make them resistant to campaign stimuli.[1] In making a choice among lesser-known options, the campaign should weigh more heavily.

A second factor—the shift from one-sided to two-sided information flow during authoritarian

elections—suggests not only that campaign effects should be present but that they should favor the opposition. During non-electoral periods, authoritarian regimes typically dominate the flow of political information to the public, conceding little space to the opposition. Prior to elections, autocrats usually allow the opposition a greater capacity to communicate its message—whether via television advertising, door-to-door canvassing, public rallies, or some other medium—in order to make the contest appear legitimate. Citizens who receive the messages of both sides are thus exposed to a set of government-sponsored arguments they have probably heard repeatedly over the years, along with a novel, countervailing message from the opposition.[2] This opposition message, which might include discussion of human rights violations or other formerly taboo topics, approximates a "new issue" on which opinions have not yet stabilized and even those on the other side of the ideological fence are more liable to be moved toward support (Zaller, 1992).

The presence of a new, countervailing message from the opposition might also render the government's message ineffective or even cause it to backfire. Studies of public opinion under authoritarian rule have found that, among the best educated citizens, support for authoritarian policy positions typically declines at the highest levels of exposure to government propaganda (Geddes and Zaller, 1989; Kennedy, 2009; Stockmann and Gallagher, 2011). The interpretation for this pattern is that, in regimes with effective control over the mass media, only the well educated have access to alternative information sources that help them process political information critically. A campaign environment, with competing opinions from both sides, provides this capacity much more broadly, so high levels of exposure to the official line might reduce support for authoritarian rule even at lower levels of education.

A final reason to expect campaign effects in electoral authoritarian regimes concerns likely differences between the strategies of the government and opposition. In many elections, the persuasive efforts of competing candidates tend to cancel out in the aggregate, meaning that voters equally exposed to these multiple appeals experience little net change in their opinions (Bartels, 1993; Zaller, 1996). Such an expectation is particularly strong when candidates employ similar strategies—e.g., meeting an attack with a counterattack, focusing on proposals for job creation, or

appealing to hope and optimism. Strategic homogeneity of this sort is less likely in authoritarian regimes, especially those that have not held regular elections in the past. Candidates and parties will have had fewer opportunities to learn from each other's prior successes and failures, and local communities of campaign professionals—if they even exist—are less likely to have developed best practices and standard operating procedures (Boas, 2010). Divergent campaign strategies may differ in terms of their persuasiveness as well, so voters equally exposed to the messages of both sides may still experience some net change in opinion and behavior.

Despite the likelihood of electoral campaigns affecting voting behavior in authoritarian regimes, the role of campaigns has not received extensive attention in the literature on authoritarianism and transitions to democracy, including recent work on democratization by elections (Lindberg, 2006, 2009). Lindberg's (2006) study of Africa argues that the holding of repeated elections in hybrid regimes creates institutions, rights, and societal expectations that further democratization. Because the focus is on the cumulative effects of electoral *procedures*, campaign persuasion (and indeed, the results of any single election) fall largely outside his scope. Other research in this vein examines the effects of single, crucial elections that trigger processes of democratization. Here, campaigns could potentially matter, but attention has been focused elsewhere. For example, Schedler's (2009) model of democratization by elections acknowledges that the opposition must successfully campaign for votes, but he focuses almost entirely on the struggle over electoral rules.

The more common assumption about the causes of dictators' electoral defeats is that, through hubris or the reluctance of underlings to pass along bad information, they mistakenly assume they will win the votes of a secretly hostile public. Diamond (2009, xiv), for instance, argues that authoritarian incumbents such as Marcos, Milošević, and Pinochet "delude themselves into believing the fawning reports from their intelligence agencies that the people are behind them." Other analysts have made similar claims (Thompson and Kuntz, 2004, 2006; Guerrero and Mangahas, 2004; Constable and Valenzuela, 1989; Birch, 2002). Such arguments imply that the election was over even before the campaign began—that poor governing performance, human rights abuses, or some other flaw of authoritarian rule turned public opinion against the regime, and that it was virtually

assured of losing a fair election, regardless of how effectively the opposition persuaded voters.

Analysis of democratization by elections in post-Soviet states stands as an exception to the above characterization, in that several studies have looked centrally at the causal role of opposition and civil society campaigns. Bunce and Wolchik's (2009) "electoral model" of opposition strategies includes a crucial role for campaigns that influence public opinion. In their explanation for successful transitions from semi-authoritarianism in six cases in the region and the maintenance of authoritarian rule in five others, Bunce and Wolchik (2010) argue that vigorous campaigns are crucial for convincing an ambivalent public to participate in elections and vote against authoritarian incumbents. Accounts by independent civil society activists from some of these countries also highlight the role of non-partisan voter turnout drives in motivating groups such as youth—who sympathized with the opposition but were inclined toward abstention—to go to the polls (Forbrig and Demeš, 2007).[3]

Recent studies of post-Soviet states have focused welcome attention on the causal effect of campaigns in transitions from authoritarian rule. Yet the evidence they offer—such as the presence of vigorous opposition campaigns in five of six successful transitions, and their absence in all five failures—is strictly at the aggregate level. Without also examining data on individual voting behavior, there is room for uncertainty about whether campaigns actually affected people in the ways that they posit. Ukraine's 2004 "Orange Revolution" is a case of democratization by elections following a strong opposition campaign, but as argued below, it is possible that individual campaign effects in this election tended, on balance, to benefit the authoritarian candidate. On its own, evidence of an individual-level effect does not constitute proof an an aggregate-level effect. Taken jointly, however, both forms of evidence allow for the strongest inferences about the causal role of campaigns in the outcome of an election, and of any subsequent regime transition.

# 3 Chile's 1988 Plebiscite

The above-mentioned theoretical expectations of campaign effects on individual voting behavior apply to the 1988 plebiscite that inaugurated Chile's transition to democracy. Chile's plebiscite was a special election organized by the military government of Augusto Pinochet, in which voters were asked to choose between an additional eight-year term for the dictator (the "Yes" option) and open presidential elections to be held the next year (the "No" option). The "Yes" campaign was run by the military government and supported by several right-wing parties; the "No" campaign was run by the *Concertación de Partidos por el No*, an umbrella coalition of opposition forces on the center-left. The "No" option was ultimately victorious in the October 5 plebiscite by a margin of 55% to 43%, triggering an open presidential election in December 1989 and the inauguration of a civilian president in 1990.

Existing analyses of Chile's transition to democracy have tended to place little emphasis on the Concertación's electoral victory over Pinochet. Instead, scholars typically describe the transition as a elite-dominated affair involving the negotiation of pacts between opposition forces and the military (Cavarozzi, 1992; Hartlyn, 1998; Huntington, 1991; Karl, 1990; O'Donnell, 1992). Such characterizations overlook the fact that there *was* no transition to democracy in Chile—pacted or otherwise—until after Pinochet's electoral defeat. In the mid-1980s, moderate sectors of the opposition announced several proposals for a negotiated transition—on increasingly favorable terms for the military regime—but were repeatedly rebuffed. Pinochet's defeat in the plebiscite transformed a process intended to legitimize authoritarian rule into one that would install a new democratic regime, and it shifted the political dynamic enough to allow for limited negotiation between the two sides (Godoy Arcaya, 1999). The important causal role of the plebiscite in Chile's transition establishes this as a case of democratization by elections, even if elite negotiations were crucial at later stages.

Studies of Chile's transition that do focus on the plebiscite typically minimize the importance of the campaign, attributing the outcome to political miscalculation by an unpopular but egotistical dictator (e.g., Constable and Valenzuela, 1989). Yet prior to the election, an opposition victory

appeared much less certain. Writing a year before the plebiscite, Chilean academic Cea (1987, 669) predicted that "if the President runs, we may expect him to win by a landslide." In January 1988, a "No" campaign memo warned that "if the opposition leadership insists on behaving as it has until now [i.e., public bickering], it is possible that we will lose" (Comité Técnico, 1988, 3). Surveys done by the Concertación's campaign strategists in the six months before the election showed the "No" option in the lead, but with a large group of undecided voters that could swing the outcome either way (Figure 1). Given these polling numbers, opposition leaders took the campaign very seriously, consistently seeking to win over fence-sitters, reinforce the decisions of "No" leaners, and convince wavering "Yes" supporters to switch sides.

Chile's 1988 plebiscite is a case where we would expect the opposition's campaign efforts to be successful at influencing individual voters. A first reason is that partisan cues provided a weak basis for deciding between the two options. At the time of the plebiscite, there had been no partisan electoral competition in Chile since 1973; the Pinochet government had spent fifteen years denigrating parties and party politics; and a number of parties that participated in the plebiscite campaign did not exist in the pre-1973 democracy. As a result, there was little party identification. In a survey conducted four months before the election, respondents were asked whether they had a favorable or unfavorable impression of major political parties, a standard even lower than identifying with them. An average of 55% was unfamiliar with each party or had no opinion of it.[4]

Moreover, identification with one of the existing parties at the time of the 1988 plebiscite did not automatically map onto a "Yes" or "No" vote as it might in a normal presidential election. National Renewal, one of the two major right-wing parties, had opposed the choice of Pinochet (rather than a civilian) as the government's candidate, and some prominent figures close to the party called for a "No" vote in the plebiscite (Allamand, 1999). On the center-left, one of the Concertación's most important members, the Christian Democratic Party (PDC), had opposed the 1970–1973 government of Socialist Salvador Allende and initially supported Pinochet's coup. Since a victory for the "No" option would only schedule a competitive presidential election for the following year, there was no certainty about the effect of one's plebiscite vote on the composition of an eventual

government. A PDC-sympathizing voter on the center-left and an RN partisan on the center-right might both prefer to vote "Yes" in the plebiscite if they suspected a Socialist would win an open election, but "No" if they believed a co-partisan would prevail.

A second reason to expect campaign effects in Chile's 1988 plebiscite concerns an abrupt change in one key aspect of the political information environment. Since Pinochet's 1973 coup, news coverage had either ignored the political opposition entirely or cast them as "extremists" and "terrorists" whenever there was a protest or some other unruly event to denounce. Biased news coverage of the opposition continued during the plebiscite campaign as well. Major newspapers and radio stations were all sympathetic to Pinochet; only a few outlets with limited circulation or listenership sided with the opposition (Tironi and Sunkel, 2000). On television, Chileans' most important source of political information according to the survey analyzed below, news coverage during the campaign overwhelmingly favored the "Yes" option (Hirmas, 1993, 84).

Despite the biased news media, a crucial feature of Chile's plebiscite campaign gave the opposition a chance to convey its political message widely for the first time in fifteen years. Regulations specified that broadcast television channels would set aside free airtime for campaign advertising (the *franja de propaganda electoral*, or "electoral advertising filmstrip") during the month before the election. Each day, a thirty-minute bloc was divided equally between the government and opposition and broadcast simultaneously on all television stations nationwide. Despite being aired during periods of generally low viewership, the *franja* ended up being extremely popular and widely watched—it obtained a rating of 65%, ten points higher than the popular show *Sábado Gigante* (Hirmas, 1989, 117). As a result, millions of Chilean citizens who had heard only the government's point of view for over a decade were exposed to a novel countervailing message. In the words of one Chilean scholar, the change was like "a group of people in a dark room suddenly emerging into the light" (Méndez et al., 1989, 115).

A final reason to expect campaign effects in Chile's 1989 plebiscite concerns stark differences in tone between the "Yes" and "No" messages, meaning that their individual persuasive effects would be less likely to cancel out among voters equally exposed to both. The "Yes" campaign

engaged in numerous attacks on the opposition, focusing on the policy failures of the previous Allende government and casting its opponents as violent, undemocratic, and dominated by extreme leftists. In contrast, the "No" campaign projected a positive, forward-looking message of national reconciliation, epitomized by its slogan "Chile, happiness is coming." It sought to convey fewer attacks, and to focus its limited criticism on Pinochet as an individual rather than the military regime and its supporters. Content analysis of television advertising from this election underscores these differences: 38% of the "Yes" campaign's advertising time consisted of negative appeals, versus 19% for the "No" campaign.[5]

While this paper does not test hypotheses about the effects of specific positive versus negative appeals, strategic differences between the two sides do increase the likelihood of detecting campaign effects, even among voters equally exposed to both messages. Research has found, for instance, that negative advertising deemed irrelevant and uncivil (e.g., because it dredges up the distant past and treats its target with excessive disrespect) can backfire on its sponsor and be counterproductive (Fridkin and Kenney, 2011). Such a characterization seems more likely to apply to the "Yes" than the "No" campaign's advertising, suggesting a net effect in favor of the "No" among voters equally exposed to both.

# 4 Analysis of Campaign Advertising Effects

To assess the effect of television advertising on vote choice in Chile's 1988 plebiscite, I conduct an analysis of post-electoral survey data using matching, a technique that requires fewer assumptions than standard regression analysis. Matching seeks to simulate the context of an experiment by comparing observations that received a treatment (such as exposure to a particular campaign message) to a subset of those that did not receive it (the control group). This subset is chosen such that the distribution of pretreatment covariates—in this case, demographics and other characteristics that cannot be affected by the campaign—is similar across groups. In contrast to regression, control observations that had little chance of receiving the treatment are dropped from the analysis

and cannot affect the causal estimate. If the matching procedure achieves balance across groups with respect to all covariates that affect both treatment assignment and the outcome, simple mean differences offer an unbiased estimate of the average treatment effect on the treated (ATT). Unlike regression, matching is a non-parametric procedure that does not require any modeling assumption about how these observed covariates are related to the treatment and the outcome of interest.

The data for this analysis are drawn from a post-electoral survey of 1700 Chileans in 29 cities nationwide, representing 62% of the total population.[6] The survey was administered by the Centro de Estudios de la Realidad Contemporánea (CERC), with face-to-face interviews conducted from October 20–27, 1988, two to three weeks after the plebiscite. In addition to standard demographic and political behavior variables, the survey contains a number of specific questions about media consumption in general and during the plebiscite campaign in particular. It is thus well suited to an analysis of campaign effects.[7]

The outcome variable is a response to the following question: "Could you tell me how you voted in the plebiscite, for the 'Yes' or for the 'No'?" The question was asked of all survey respondents, including those who may have abstained or never registered to vote. This particular measure appears to suffer from social desirability bias, as is common in post-electoral surveys, particularly after dramatic, transitional elections. When missing values are omitted, 25.7% of respondents reported a "Yes" vote, 62.2% reported a "No" vote, and 12.1% reported a blank/null vote or abstention. The corresponding figures for the population from which the sample was drawn are 35.6%, 51.7%, and 12.6%.[8]

The difference between the reported and actual distribution of votes may largely be due to the high rate of non-response to this survey question. Over a fifth of all respondents refused to answer the question about their plebiscite vote. Social desirability bias could take the form of outright lying, but it might also lead "Yes" voters to refuse to answer the question. The sensitivity of the analysis to assumptions about the distribution of missing values is discussed in detail below.[9]

The treatment of interest is respondents' receipt of each side's television advertising message, something that that is often difficult to probe in surveys. Analysis of campaign and media effects

often relies on self-reports of exposure, but these frequently suffer from both random and systematic measurement error, which leads to biased effect estimates (Bartels, 1993; Zaller, 1996). First, respondents might answer that they watched the *franja* because the television set was always on in the evenings, not because they paid attention to the programming. Second, it is common to misremember one's frequency of exposure (e.g., 2–3 times a week versus 4–5 times a week). Third, and most problematic, social desirability bias might lead respondents to intentionally misreport their exposure. At the time the survey was administered, the crucial role of the *franja* in the electoral outcome was already being discussed, and the average non-viewer might not want to admit that she had missed out on this historic event. Moreover, social desirability bias could differ between "Yes" and "No" voters, with the latter being particularly likely to overstate their exposure.

A common solution to problems with self-reported exposure is to use a measure of political information as an alternative treatment indicator, assuming that such knowledge is gained primarily through media exposure (Zaller, 1996). However, since such survey questions typically tap general political knowledge, such as the name of one's senator, this approach works best for examining media effects on a broad level rather than the effect of one or the other side's television advertising.

In the present analysis, I am able to use a measure of political knowledge that is unusually direct in its ability to gauge receipt of each side's advertising message. The CERC survey contains a battery of questions that mention specific scenes from the *franja* and ask respondents whether they correspond to the "Yes" or "No" campaigns. These include a) "the widow of the policeman assassinated by terrorists;" b) "the young man beaten by police at the protest;" c) "the dark tunnel;" d) "Doña Yolita, who doesn't have enough money to buy tea;" and e) "the mother of Cazzely" (a well known soccer star).[10] It would be difficult to answer these questions correctly without having gained some exposure to the message of each side's campaign advertising.

In addition to direct viewership of the *franja*, it is possible to have gained knowledge about particular scenes through discussions with friends and family. Hirmas (1989, 109) reports that the *franja* became "the topic of everyday conversation" during the campaign. Thus, these survey questions are likely to also measure knowledge gained through "hot talk," or communication within

social networks (Baker, Ames and Renno, 2006). This dual measurement is an inferential benefit when examining campaign advertising effects. If the true effect of advertising on vote choice depends partly on direct exposure to the message and partly on secondary social communication, a measure that can tap both of these pathways should provide a more accurate causal estimate.

To construct a treatment indicator based on knowledge of specific scenes in the *franja*, I examined how many questions a respondent correctly answered about the "Yes" and "No" campaigns. I excluded the "Doña Yolita" question because this scene was copied by the "Yes" campaign after it appeared in the "No" *franja*, making the correct response ambiguous. Therefore, a respondent could at most answer two questions correctly for both the "Yes" and "No" campaigns. Table 1 shows how many respondents fall into each category. A plurality of respondents answered all of these questions correctly; a large number also answered none of them correctly. Of particular interest are the shaded cells on the off-diagonal, corresponding to those who got more questions right for one side's television advertising than they did for the other. These respondents show evidence of a "reception gap" (Zaller, 1996) with respect to the two campaign messages, which should allow us to isolate their individual effects.

To analyze the effect of campaign advertising on vote choice in the plebiscite, I conducted a matching analysis based on three categories of the treatment variable: full knowledge of the *franja* (four correct answers), greater knowledge of the "Yes" message, and greater knowledge of the "No" message. The "Yes" message category, which contains the fewest respondents, is taken as the treatment group; the other two are control groups to be matched to it. I also attempted to match the treatment group to respondents who answered no questions correctly, but I was unable to obtain a set of matches that achieved satisfactory covariate balance. Those with zero knowledge of the *franja* were simply too different from the treatment group to allow for valid inferences. In addition, I excluded the 157 respondents who correctly answered one question about each side's television advertising, both because this group is less theoretically interesting than the others and because it is similar in size to the "Yes" message category, which would complicate matching.

It is important to specify what types of causal inferences can and cannot be made as a result

of this matching procedure. In estimating the average treatment effect on the treated (ATT), I am looking at respondents who fell into the "Yes" message category and examining the effect of this reception gap on their vote choice. Thus, I can consider the counterfactual of how these treated respondents might have voted if they been exposed equally to both messages, or primarily to the "No" message. I cannot make direct inferences about respondents in the other two categories because they constitute larger and more varied groups than those who primarily received the message of the "Yes" campaign. Luckily, those in the "Yes" message category are similar to other respondents as a whole in terms of basic demographics (descriptive statistics are in the Appendix). However, they do differ significantly from each control group on one or more of these covariates, as well as on various measures of media consumption—hence the need for matching.

Matching also requires the stable unit treatment value assumption (SUTVA)—namely, that the potential outcomes for any one unit are independent of any other unit's assignment to treatment or control (Rubin, 1978). This assumption has implications both for the internal validity of the estimated effects and for their interpretation. The main threat to internal validity would involve contamination between subjects, which might occur if multiple survey respondents were sampled from a single home, neighborhood, or workplace. Survey design—choosing no more than one individual per home, and a median of 10 at the neighborhood level—should mitigate this concern.[11] SUTVA also means that, while our estimates inform us about the average effect of any one voter's receipt of the "Yes" vs. "No" advertising messages, it cannot tell us what would have happened if *everyone* had been simultaneously treated or not treated with that knowledge. Thus, while the estimated effect of campaign advertising on individual vote choice has implications for its aggregate-level effect on the electoral outcome, one should keep in mind that they are distinct.

I began the matching process by using logistic regression to estimate the propensity (or p-score) of being assigned to treatment versus each of the two control groups. Standard demographic variables (*Education*, *Family Income*, *Age*, *Religiosity*, *Male*, *Employed*, *Urban*) were all entered in the two p-score models, along with indicators for each of Chile's regions outside of the Santiago metropolitan area. I also included covariates measuring television viewing habits and sources of

political information, which could affect treatment assignment as well as vote choice. These include frequency of watching television news (*TVnews*) and an indicator for whether television is the respondent's primary source of information about current events (*TVinfo*). In order to capture political predispositions, I included indicators for whether the respondent's preferred newspaper or radio station was sympathetic to the opposition (*Opposition Paper*, *Opposition Radio*) and whether he or she usually watched the least biased (*UCTV*) or most biased (*TVN*) television station.[12] These media consumption variables are arguably pre-treatment, since they concern general habits and were measured in the survey prior to any question about the campaign. All multi-category variables were treated as interval-level; I also included their squared terms.[13]

To pair respondents from each control group to those in the treatment group, I conducted genetic matching (Sekhon, 2011) using the linear predictors from each p-score model, along with the actual covariates, quadratic terms, and several first-order interaction terms that were unbalanced in initial matching runs. Before matching, a number of covariates differed significantly between treatment and control groups, as shown in Table 2. After matching, balance was very good for both control groups. For the 28 covariates and squared terms, the minimum p-value from paired difference-in-means t-tests and bootstrapped Kolmogorov-Smirnov (KS) tests was .287 for the "No" control group and .121 for the "Both" control group. For 75 first-order interaction terms, p-values were no lower than .107 for the "No" control group and .091 for the "Both" control group (reported in the Appendix).[14]

Before proceeding to examine the effect of message reception on vote choice, it is worth stating the hypotheses to be tested in terms of the specific treatment effects to be estimated. Since those who primarily received the "Yes" message are taken as the treatment group, separate ATTs will be estimated with respect to each of the two control groups: those who primarily received the "No" message and those who received both messages. These two treatment effects can be referred to as $ATT_{yes/no}$ and $ATT_{yes/both}$. They will be estimated for each of four outcomes: a "No" vote, a "Yes" vote, a blank vote or abstention, and non-response.

A first possible hypothesis is that the televised appeals of the "Yes" and "No" campaigns were

equally effective in shifting voters' opinions. If we were comparing only Chileans with full knowledge of the *franja* to those with no knowledge, this hypothesis would predict a zero net effect because of the "canceling out" of the separate messages. However, one would still expect to find evidence of campaign advertising effects on voting behavior among voters with reception gaps. Those who primarily received the "Yes" message should be much more likely to vote "Yes" and less likely to vote "No" than those who primarily received the "No" message. Voters who received both messages should occupy a middle category, so $ATT_{yes/no}$ should be approximately double the magnitude of $ATT_{yes/both}$ for the "Yes" and "No" voting outcomes.

A distinct set of hypotheses is suggested by the differences between the "Yes" and "No" campaign strategies and the fact that the "No" campaign presented voters with new information after years of pro-government propaganda. If the "No" campaign's televised appeals shifted voters' opinions, but those of the "Yes" campaign had no effect, we would expect those who primarily received the "Yes" message to vote differently from the two control groups because the latter both received the "No" campaign's message. However, the two control groups should vote similarly to one another because they differ only in their receipt of the ineffectual "Yes" campaign message. Hence, $ATT_{yes/no}$ and $ATT_{yes/both}$ should be approximately equal.

A final possibility, also consisted with a net effect in favor of the "No" vote, is that the "No" campaign's televised appeals worked as intended while those of the "Yes" campaign generated a backlash. Several Chilean analysts (e.g., Tironi and Sunkel, 2000, 184) have suggested a double effect of this sort. If this were true, we would expect those with full knowledge of the *franja* to be least likely to vote "Yes" and most likely to vote "No," since the individual effects of each side's advertising would compound one another. Thus, $ATT_{yes/both}$ would be significantly larger in magnitude than $ATT_{yes/no}$. The latter might even equal zero if the "No" persuasive effect and "Yes" backlash effect were similar in size.

The relationship between message reception and vote choice after matching offers strong evidence that television advertising did affect vote choice in Chile's 1988 plebiscite. To begin with, the null hypothesis of zero effect can be clearly rejected. As shown in Table 3, of the four esti-

mated treatment effects on the "Yes" and "No" votes, two are significant at the .05 level and one at the .1 level, using the standard errors proposed by Abadie and Imbens (2006) to account for the uncertainty of the matching procedure.

Comparisons between the two ATTs are somewhat less conclusive for distinguishing among the various hypotheses about the effect of each side's advertising, but on balance they support the interpretation that the "No" advertising worked as intended while the "Yes" *franja* had no effect. For the "No" voting outcome, $ATT_{yes/both}$ is larger in magnitude than $ATT_{yes/no}$, which might argue in favor of a backlash effect for the "Yes" campaign advertising. For the "Yes" voting outcome, $ATT_{yes/no}$ is more than three times the magnitude than $ATT_{yes/both}$, which might argue in favor of the "canceling out" hypothesis, or even that the "Yes" campaign advertising was more effective that that of its opponents. However, in neither case is the difference in ATTs statistically significant. Hence, these results are consistent with the hypothesis that only the "No" campaign advertising had an effect on vote choice.

The fact that $ATT_{yes/both}$ is large and highly significant for the non-response outcome suggests that social desirability bias is complicating the analysis. To examine the sensitivity of these results to various assumptions about the true vote of non-respondents, I conducted a simulation. For treatment or control group $j$, let $p_j$ be the probability that true "No" voters refused to answer the vote question, and $rp_j$ the probability that true "Yes" voters refused to answer, where $r$, the ratio of "Yes" to "No" non-response propensities, is assumed constant across groups. As shown in the Appendix, the true number of "Yes" and "No" votes for group $j$ can be expressed as a function of the reported voting frequencies and the single unknown parameter $r$. Letting $r$ take on 41 values from $\frac{1}{3}$ to 3, I randomly reassigned the appropriate number of non-respondents for each treatment or control group to the "Yes" and "No" vote categories, and then calculated the resulting ATTs, differences in ATTs, and standard errors. I repeated this simulation 500 times, generating mean values of these statistics for each value of $r$.

Based on this simulation, support for the tentative conclusion advanced above—that the "Yes" campaign's advertising was ineffective, while the "No" campaign's advertising shifted vote intentions—

is strengthened by any reasonable assumption about the relative non-response propensities of "Yes" and "No" voters. Given the evidence of social desirability bias with respect to the vote choice question, the most likely value for $r$ is at the upper end of its range—i.e., non-response is more likely among "Yes" than "No" voters.[15] With $r \geq 1.8$, treatment effects on the "No" vote retain their previous level of significance, and those for the "Yes" vote are substantially larger and more significant than before (at the .001 level for $ATT_{yes/no}$ and the .05 level for $ATT_{yes/both}$). Moreover, the differences in treatment effects are smaller and similarly insignificant. For the "Yes" voting outcome, $ATT_{yes/no}$ is only about 1.3 times the size of $ATT_{yes/both}$ rather than 3 times as large. Full results are in the Appendix.

Because some imbalance remains between treatment and control groups even after matching, and any number of unobserved confounders might also be present, it is important to conduct placebo tests. A placebo test examines a relationship between two covariates that is theoretically expected to equal zero; a non-zero result suggest the presence of a confounder. In general, two types of placebo tests are available—those testing alternative treatments and those testing alternative outcomes. The matched dataset passes two placebo tests of each type, lending credence to the claim that treatment effect estimates do not contain significant bias.

For placebo tests involving an alternative treatment, I examine the effect of watching soap operas (*TVsoaps*) or televised movies/serials (*TVmovie*) on vote choice. Entertainment programs on Chilean television did not suffer from overt political bias at the time of the plebiscite, so they should have no causal effect on voting behavior. However, watching television in general—either the *franja* or these alternative programs—might be correlated with some unobserved trait that also influences how people vote. Thus, if viewership of these television programs has a significant relationship with vote choice both before and after matching, the after-matching relationship must be due to an unobserved confounder, which could also be biasing estimates of the *franja* treatment effect. Prior to matching, viewership of both soap operas and movies/serials has a strong bivariate relationship with vote choice, with more frequent viewers being significantly less likely to vote "No" and more likely to vote "Yes" in the plebiscite. After matching, the relationship between

vote choice and each of these variables disappears, as shown in the Appendix.[16]

A second type of placebo test considers the effect of the television advertising treatment on an alternative outcome. The CERC survey asked respondents about their level of agreement with the following statements: “it is better for the family that the woman should concentrate on the home and the man on his job” (*Woman-Home*) and “the responsibility for children basically lies with the woman” (*Woman-Kids*). The topic of gender roles was virtually never mentioned in the *franja*, so exposure to one or the other side’s campaign message should not have any causal effect on attitudes about women’s role in society.[17] Any relationship after matching would have to be attributed to an unobserved confounder—most likely, ideology—that could also bias the estimated effect of the *franja* on vote choice. Prior to matching, the treatment group and the “Both messages” control group were highly unbalanced with respect to these two covariates, as reported in the last lines of Table 2. After matching, the imbalance disappeared.

As a robustness check, I also estimated a multinomial logistic model of vote choice using the matched dataset. Since perfect balance between treatment and control groups could not be obtained, ATT estimates are likely to contain some degree of bias, however small. In such cases, estimating a parametric model on the matched dataset can reduce bias (Ho et al., 2007). The model uses the same 4-category outcome variable, with indicator variables for the two control conditions and all of the matched covariates and quadratic terms entered on the right-hand side. The results of this robustness check (reported in the Appendix) are reassuring. When predicted voting probabilities based on this model are calculated for every individual in the matched dataset, and averaged over the different treatment and control conditions, they are almost identical to the distribution of actual votes after matching.[18] No predicted probability differs from the actual proportion in the matched dataset by more than .035, and no ATT estimate derived from these predicted probabilities differs from the standard ATT estimate by more than .018.

The extensive series of tests passed by the present analysis should lend confidence to its conclusions. Dealing with social desirability bias on the outcome variable by randomly reassigning missing data to the “No” and “Yes” voting categories strengthens the findings under the most

realistic range of assumptions about non-response propensities. Four placebo tests—two involving alternative treatments, and two involving alternative outcomes—argue against omitted variable bias. Finally, predicted probabilities from a multinomial logistic model of vote choice, which could reduce any remaining bias due to imperfect covariate balance after matching, are nearly identical to the simple voting proportions in the matched dataset.

# 5 Discussion and Comparative Perspectives

The results of this analysis offer strong evidence that the "No" campaign's television advertising affected voters in Chile at an individual level. The question of aggregate-level effects remains. Did the Concertación win over large numbers of voters because of its television advertising, thereby contributing to its electoral victory? If television advertising shifted a bloc of votes toward the "No" option, it must also have influenced voters at an individual level. In other words, individual effects are a necessary condition for the existence of any aggregate effect on the electoral outcome, and evidence of the former is valuable for making an argument about the latter. Yet individual effects are not sufficient for demonstrating an aggregate-level effect; other evidence at the aggregate level must also be considered.

Combined with evidence of individual effects, fluctuations in vote intention during the month prior to the plebiscite suggest that the "No" campaign's television advertising was indeed an important component of its electoral victory. In the poll standings shown in Figure 1, the only period in which support for the "No" vote increased at the expense of the "Yes" vote is between September 3 and September 25—a period corresponding almost exactly to the first three weeks of the *franja*. Taken on its own, this last-minute fluctuation in the polls could be attributed to any number of other causes, such as increased mass mobilization in the final stretches or a crystallization of voters' opinions irrespective of campaign stimuli. However, in conjunction with the evidence of an individual-level effect on voting behavior, it seems likely that the "No" campaign's television advertising contributed to the shift.

In addition to television advertising, other aspects of the "No" campaign may also have contributed to its electoral victory. Outside of the *franja*, the opposition was generally unable to campaign on a level playing field—permits for public rallies, for instance, were often denied for the most desirable locations—but it was nonetheless able to get its message to the public through additional means. The overall significance of the "No" campaign for democratization in Chile, therefore, is likely even larger than the role of television advertising in its electoral victory.

Chile's 1988 plebiscite is not the only case of democratization by elections where a campaign may have affected individual voting behavior as well as the overall result. As discussed above, dictators' defeats at the polls are often described as inevitable outcomes, given a supposed bottled-up public hostility to their regimes. Yet retrospective assessments of this sort are often belied by pre-electoral poll standings and news coverage that depict the race as much more competitive or show that voters' opinions are shifting during the campaign. Close scrutiny of several other "stunning elections" suggests that opposition or civil society campaigns may have had similar effects on voters' opinions as in Chile. Television advertising may not have played as crucial a role; rallies, mass mobilization, and face-to-face campaigning were probably more significant. Regardless of the most relevant medium, however, these cases indicate the broader importance of examining campaign effects in elections held under authoritarian rule.

One possible case of campaign effects involves the Philippines' February 1986 election. After Marcos announced the election in November, opposition forces quickly converged around Corazon Aquino as a consensus candidate. But opposition unity alone did not seem to ensure her victory. Independent surveys done in early- to mid-January gave Marcos a lead of 3 to 8 percentage points (Henares, 1992); a January 13 news report described Marcos's victory as "almost certain" (Howard, 1986, 5). Yet Aquino closed the gap in the final weeks, taking a narrow lead in a survey conducted days before the election (Henares, 1992). Ultimately, Aquino's unexpectedly strong performance forced Marcos to rely on more egregious forms electoral fraud than he had intended, paving the way for the mass protests that ultimately forced him to flee the country (Thompson, 1995, 150). Had Aquino's campaign been less effective, Marcos might have won the election legitimately, or

through more concealable fraud, and could plausibly have remained in power.

Serbia's 2000 election is a similar case. As in Chile, it was widely believed at the beginning of the campaign that the public was apathetic and skeptical of the possibility of change, which would favor the reelection of dictator Milošević (Birch, 2002, 502). Indeed, Milošević led the polls early in the race, but lost ground to Vojislav Koštunica, the candidate of a united opposition coalition, by late July (Birch, 2002, 502). During the last month before the September 24 election, Koštunica gained five percentage points in the polls while Milošević declined by two (Penn, Schoen and Berland Associates, Inc., 2000). Thompson and Kuntz (2004, 166) argue that choosing Koštunica as a candidate "was crucial to the opposition's success" because of his nationalist credentials and honest image; communicating these characteristics to voters over the course of the campaign may have accounted for some of his growing support. Koštunica's strong showing surely helped galvanize his supporters to protest against the regime when fraudulent initial results were released, which ultimately forced Milošević to concede defeat. The outcome might well have differed if his campaign had been less successful.

Examining individual-level campaign effects in cases of democratization by elections is also important because net effects might not always favor the opposition. Bunce and Wolchik (2009) suggest that the campaign for Ukraine's 2004 election benefited Viktor Yushchenko, the opposition candidate who ultimately won after mass protest over fraudulent results forced a re-run of the second round. However, evidence suggests that the campaign may actually have favored Viktor Yanukovych, the candidate hand-picked to succeed the authoritarian incumbent, at the expense of Yushchenko. In contrast to Aquino and Koštunica's come-from-behind victories, Yushchenko had a clear lead three months before the election but saw Yanukovych gain significant ground over the course of the campaign (Razumkov Centre, 2004, 4). Though he had only an 8% approval rating as incumbent Prime Minister (McFaul, 2007, 54–55), Yanukovych ultimately garnered 44% of the vote (to Yushchenko's 52%) in the non-fraudulent final contest. His aggressive and dirty campaign, which sought to inflame regional tensions and peg his opponent as a pawn of the West (Kuzio, 2005), may well have won votes at Yushchenko's expense. In this case, the campaign may actually

have narrowed the race enough for the government to attempt a stolen election—necessitating a popular uprising, rather than a simple concession speech, to bring about democratization. Analyzing data on individuals could help shed light on this question of aggregate-level effects.

Journalists' predictions and shifts in poll standings are only suggestive of campaign effects on voting behavior in the Philippines, Serbia, or Ukraine; they do not constitute proof. A tightening race might just as easily reflect the crystallization of voters' opinions as the persuasive effects of candidates' electoral appeals. However, closer examination of these three cases does show that the fate of the regime was not solely determined by a dictator's decision to hold elections. If the outcome of each election played a key causal role in democratization, it quite possible that the campaign, by influencing voting behavior, did so as well.

# 6 Conclusion

Research on campaign effects has a long pedigree in established democracies, but elections held under authoritarian rule have remained outside of its scope. Yet authoritarian elections in which the opposition is allowed to communicate its message to the public—even on largely unequal terms—are likely cases for campaigns affecting voting behavior. Partisan loyalties in the electorate are often weak or nonexistent under authoritarian rule, meaning that there is more room for the campaign to shift opinions. Authoritarian elections bring two-sided information flow to a public more accustomed to government propaganda, presenting voters with new, countervailing arguments that are more likely to have an influence. Campaign strategies may also differ significantly between government and opposition, meaning that persuasive effects are less likely to cancel out even among those equally exposed to the appeals of both sides. In many instances, campaign effects should favor the opposition, though as Ukraine's 2004 election suggests, the government candidate may also reap the greater benefit.

If campaign effects on individual voting behavior are likely in cases of electoral authoritarianism, campaigns may also have played a causal role in many instances of democratization by

elections. Thus far, the study of transitions from electoral authoritarianism has focused primarily on other factors, such as the strategic positioning of opposition and regime elites. It is often assumed that dictators who submit to the polls were bound to lose any election that was moderately free and fair. Research on democratization by elections in the post-Soviet world has paid welcome attention to the role of campaigns, but only at the aggregate level. Without also examining data on individual behavior, there is still room for uncertainty as to whether the campaign contributed to the opposition's victory.

Examining the 1988 plebiscite that inaugurated Chile's transition to democracy, I show that the effect of televised campaign advertising did in fact benefit the opposition in this election. Using survey respondents' answers to questions about television advertising as a measure of their receipt of each side's message, I argue that the "No" campaign's televised appeals made Chileans more likely to vote against Pinochet, whereas the "Yes" campaign's advertising had no discernible effect. This finding seems intuitively plausible: it is easy to envision voters tuning out yet another round of pro-government propaganda, while being moved by the first televised message from the opposition in fifteen years. Yet no prior study has sought to document campaign effects on voting behavior in this election. Perhaps as a result, scholars have tended to conclude that Pinochet was bound to lose any free and fair election, rather than giving a creative, hard-fought, and ultimately successful opposition campaign the credit it deserves.

Going forward, scholars have a chance to examine campaign effects in other electoral authoritarian regimes. As shown by the discussion of the Philippines, Serbia, and Ukraine, survey data from transitional elections often exist, and in some cases they might permit the analysis of campaign effects. Even more promising is the opportunity to conduct new surveys—specifically designed to gauge the impact of the campaign—during future authoritarian elections. The practical challenges are greater than in democracies, but the desire for international legitimacy often encourages a certain openness, and pollsters are less challenging to perceived national sovereignty than the foreign election observers who often visit. Hopefully, future research will establish opposition campaigns as an important causal factor in many cases of democratization by elections.

# Notes

[1]A similar argument has been made regarding party identification and campaign/media effects in new democracies (Baker, Ames and Renno, 2006; Boas, 2005; Greene, 2011; Lawson and McCann, 2005)

[2]It is possible that those who tire of government propaganda stop paying attention to the medium, such as television, that will also convey the opposition's campaign message. The novelty of two-sided information flow might revive their engagement, but even if it does not, the severity of this concern can be assessed empirically by research designs (such as the present) that separately measure receipt of each side's campaign message.

[3]Studies of post-Soviet states have also examined another type of campaign—demonstrations of support for the opposition, and pressures on regime elites, by international actors such as non-governmental organizations. As the present paper focuses on voting behavior, I do not examine campaigns to influence the actions of elites, though in some cases they were certainly consequential in the fall of authoritarian rule.

[4]Data are from the Centro de Estudios Público's May–June 1988 national survey.

[5]These figures are drawn from a larger content analysis project examining presidential election campaign advertising in Chile, Peru, and Brazil over several decades. Coding was done by the author; intercoder reliability was tested by having undergraduate research assistants recode a random sample of advertisements. For Chile (elections from 1988–2006), coders agreed with 96% of my coding decision for negative vs. positive/neutral appeals. More details are in Boas (2010, 669–671).

[6]The survey used a multistage cluster sample. Census subdistricts and houses were first chosen randomly; individuals were then selected based on quotas of sex, age, and employment.

[7]The other major survey covering this campaign, a pre-electoral survey conducted by the Centro de Estudios Públicos and analyzed in Méndez et al. (1989), includes many fewer media-related questions and does not allow for a fine-grained measure of campaign message reception.

[8]These figures are based on electoral returns and June 1988 voting-age population estimates in

Chateau and Rojas (1989) for the specific *comunas* sampled by the survey.

[9]It is also possible that some “No” voters refused to answer out of fear. However, respondents with missing values on the vote question were significantly *less* likely to say that reprisals against “No” voters had taken place after the election. Hence, it seems reasonable to believe that the non-response category contains more “Yes” than “No” voters.

[10]Scenes a) and c) are from the “Yes” *franja*; b), d), and e) are from the “No” *franja*.

[11]This level of clustering is similar to that of the U.S. National Election Studies.

[12]Opposition newspapers include *La Epoca* and *Fortín Mapocho*; the pro-opposition radio station is *Radio Cooperativa* (Tironi and Sunkel, 2000). Hirmas (1993) offers data on television bias.

[13]For the small number of missing values on the interval-level variables (4% of all observations for *Family Income*; less than 1.5% for all others), I substituted mean values. Missing values on indicator variables received a score of zero.

[14]These include all possible first-order interactions of covariates in the p-score model, except for those involving the regional indicators, as well as the unlikely interaction of *Opposition Paper* and *Opposition Radio* with the government television station *TVN*.

[15]Choosing $r = 2.5$ comes closest to approximating the distribution of valid votes in the sampled population. However, voting proportions in the matched dataset should not necessarily equal those in the sampled population, since certain categories of voters (e.g., those with no knowledge of the franja) were excluded entirely, and some respondents were dropped from each control group to obtain balance. Hence, it is most appropriate to consider a range of likely values for $r$.

[16]The loss of significance is not an artifact of the smaller post-matching sample size; coefficient estimates would still be insignificant even if their standard errors had not changed.

[17]Out of 6.5 hours of campaign advertising analyzed, less than a minute touched upon this issue.

[18]An alternative approach, calculating predicted probabilities for a “typical” voter (e.g., mean values on interval variables and modal values on indicator variables), would not be directly comparable to the proportions reported in Table 3.

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**Figure 1:** Vote Intention in Chile's 1988 Plebiscite. Data are drawn from internal surveys by CIS, a consortium of think tanks conducting research for Chile's "No" campaign.

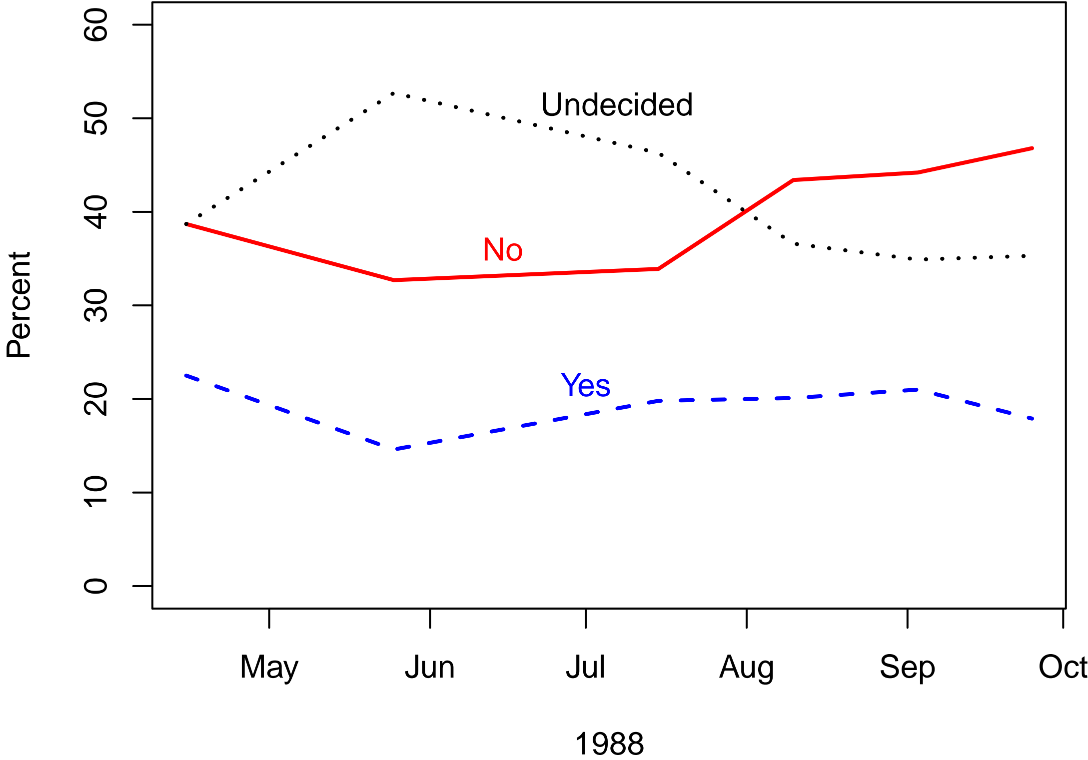

**Table 1:** Number of Scenes Correctly Identified. Shaded cells indicates respondents with a reception gap.

| | | "No" *Franja* | | |
|---|---|---|---|---|
| | | **0** | **1** | **2** |
| "Yes" *Franja* | **0** | 301 | 151 | 136 |
| | **1** | 52 | 157 | 337 |
| | **2** | 19 | 87 | 460 |

**Table 2:** Balance Statistics Before and After Matching. 'Std. mean diff.' is the mean difference of the treated and control observations divided by the standard deviation of the treated observations. P-values are from bootstrapped Kolmogorov-Smirnov (KS) tests or mean difference t-tests (two-sample before matching, paired after matching). Chile's Regions 11–12 were not sampled by the survey. *Urban* is an indicator variable for residing in Santiago, Valparaíso, Viña del Mar, or Concepción.

| | "Yes" vs. "No" | | | | "Yes" vs. Both | | | |
|---|---|---|---|---|---|---|---|---|
| | 100 × Std. mean diff. | | Min. p-val., KS or t-test | | 100 × Std. mean diff. | | Min. p-val., KS or t-test | |
| **Covariate** | **Before** | **After** | **Before** | **After** | **Before** | **After** | **Before** | **After** |
| Education | 6.39 | 2.36 | 0.34 | 0.29 | 49.29 | 2.75 | 0 | 0.33 |
| Education$^2$ | 12.00 | 2.62 | 0.17 | 0.29 | 48.68 | 3.67 | 0 | 0.33 |
| Family Income | 3.40 | 3.71 | 0.46 | 0.53 | -27.97 | 0.01 | 0 | 0.91 |
| Family Income$^2$ | 7.88 | 5.19 | 0.36 | 0.32 | -26.37 | -2.23 | 0 | 0.74 |
| Age | 21.53 | 1.34 | 0.01 | 0.56 | 45.83 | 4.74 | 0 | 0.24 |
| Age$^2$ | 22.50 | 1.84 | 0.01 | 0.56 | 44.61 | 6.22 | 0 | 0.12 |
| Religiosity | -4.00 | 1.34 | 0.65 | 0.85 | 9.31 | -1.30 | 0.31 | 0.81 |
| Religiosity$^2$ | -1.31 | 3.57 | 0.68 | 0.64 | 8.41 | -2.27 | 0.35 | 0.79 |
| Male | -1.84 | 1.26 | 0.84 | 0.88 | -11.10 | 0 | 0.23 | 1 |
| Employed | -3.02 | -2.56 | 0.74 | 0.74 | -5.18 | 1.28 | 0.58 | 0.56 |
| Urban | 7.89 | -1.28 | 0.38 | 0.71 | -6.27 | -2.56 | 0.50 | 0.16 |
| TVnews | 6.45 | 3.30 | 0.47 | 0.72 | 10.85 | -1.98 | 0.03 | 0.80 |
| TVnews$^2$ | 7.67 | 3.67 | 0.39 | 0.69 | 14.86 | -0.17 | 0.03 | 0.82 |
| TVinfo | 18.40 | -6.48 | 0.04 | 0.46 | 40.24 | -2.59 | 0 | 0.81 |
| Region 1 | -8.44 | 0 | 0.36 | 1 | 1.93 | 0 | 0.83 | 1 |
| Region 2 | 1.06 | 0 | 0.90 | 1 | 5.80 | 0 | 0.52 | 1 |
| Region 3 | -10.17 | 0 | 0.31 | 1 | -16.64 | 0 | 0.14 | 1 |
| Region 4 | 12.15 | 0 | 0.15 | 1 | 11.98 | 0 | 0.16 | 1 |
| Region 5 | 3.82 | 0 | 0.67 | 1 | 8.78 | 3.46 | 0.33 | 0.53 |
| Region 6 | -6.03 | 0 | 0.52 | 1 | -19.48 | -4.62 | 0.07 | 0.32 |
| Region 7 | -27.30 | 0 | 0.01 | 1 | -25.54 | 5.64 | 0.03 | 0.32 |
| Region 8 | -1.85 | -1.68 | 0.84 | 0.78 | 8.99 | 0 | 0.32 | 1 |
| Region 9 | 0.68 | 0 | 0.94 | 1 | 6.88 | 0 | 0.43 | 1 |
| Region 10 | 1.16 | 2.88 | 0.90 | 0.32 | 0.29 | 11.51 | 0.98 | 0.16 |
| Opposition Paper | -21.31 | 2.30 | 0.02 | 0.32 | -86.87 | 0 | 0 | 1 |
| Opposition Radio | -11.94 | 1.73 | 0.19 | 0.32 | -45.26 | 3.46 | 0 | 0.73 |
| UCTV | -21.38 | -1.27 | 0.02 | 0.32 | -37.15 | 0 | 0 | 1 |
| TVN | 13.55 | 1.30 | 0.13 | 0.76 | 28.57 | -3.91 | 0 | 0.58 |
| **Placebo tests** | | | | | | | | |
| Woman-Home | -7.04 | 11.95 | 0.42 | 0.24 | -37.38 | 1.41 | 0 | 0.71 |
| Woman-Kids | -7.91 | -5.60 | 0.36 | 0.57 | -22.55 | 9.40 | 0.01 | 0.35 |

**Table 3:** Message Reception and Plebiscite Vote. Top panel contains row proportions. Entries in parentheses are Abadie-Imbens standard errors for the ATT estimates and conventional standard errors for their differences. N = 158 for each group. $^{\dagger}$ $p < 0.1$; $^{*}$ $p < .05$; $^{**}$ $p < .01$.

| | | Plebiscite Vote: | | | |
|---|---|---|---|---|---|
| | | "No" | "Yes" | Blank/none | No response |
| Message Reception: | "Yes" (treatment) | 0.386 | 0.291 | 0.063 | 0.259 |
| | "No" (control) | 0.475 | 0.190 | 0.152 | 0.184 |
| | Both (control) | 0.532 | 0.259 | 0.082 | 0.127 |
| Treatment Effect: | $ATT_{yes/no}$ | -0.089† (0.054) | 0.101* (0.049) | -0.089* (0.039) | 0.076 (0.053) |
| | $ATT_{yes/both}$ | -0.146* (0.063) | 0.032 (0.050) | -0.019 (0.029) | 0.133** (0.051) |
| | $ATT_{yes/no}$ - $ATT_{yes/both}$ | 0.057 (0.073) | 0.070 (0.061) | -0.070 (0.043) | -0.057 (0.065) |