Abstract: American political consultants generally believe that negative advertising is an effective campaign tool, even though academic research on this question has been inconclusive. In Chile, the opposite belief prevails, based upon a reading of the plebiscite that inaugurated its transition to democracy. In this election, the democratic opposition defeated General Pinochet with a predominantly positive campaign, in contrast to the military government’s relentless attacks. Based upon content analysis of television advertising and a matching analysis of post-electoral survey data, this paper analyzes campaign effects on vote choice in Chile’s 1988 plebiscite. I find that the effect of televised campaign advertising benefited the democratic opposition over the incumbent military government. However, the most likely explanation for this effect contradicts the conventional wisdom about this campaign. Voters appear to have been influenced not by the difference in tone of the two sides, but rather by the difference in credibility of their claims.
What is the effect of positive campaign advertising on political behavior? Though political scientists have amassed a large body of research examining the relationship between tone of advertising and outcomes such as turnout and vote choice, this literature has generally posed hypotheses in terms of the effect of negative messages. To a large extent, this orientation of research programs may be due to American political consultants’ preference for attack ads and the widespread belief that negative advertising is effective even though nobody likes it. Empirical studies that have come to different conclusions have tended to frame their findings in terms of debunking this conventional wisdom, rather than affirming any prior expectation that positive campaign advertising has a unique effect.

This paper examines the effect of televised campaign advertising on vote choice in Chile, a country where a very different conventional wisdom prevails about the effectiveness of negative versus positive campaign appeals. In Chile’s 1988 plebiscite, which asked voters to choose between a transition to democracy and continued authoritarian rule under Augusto Pinochet, the democratic opposition employed a predominantly positive campaign message while the incumbent military government chose to viciously attack its opponents. The dramatic victory of the “No” option in this plebiscite has had long-lasting effects on campaign strategies, convincing many politicians and political consultants in Chile that negative messages are simply counterproductive. The Chilean conventional wisdom is largely predicated on the belief that the positive message of the “No” campaign, particularly as conveyed through its television advertising, was a crucial factor in its victory over Pinochet. No existing research, however, has sought to address the political behavior hypothesis implied by this common belief—that exposure to the “No” campaign’s television advertising had the causal effect of making voters less likely to support Pinochet.
There are at least two distinct reasons why campaign advertising effects might have favored the opposition in Chile’s 1988 plebiscite, only one of which supports the conventional wisdom. A first possible hypothesis focuses on the tone of advertising, arguing that voters were repulsed by the military government’s negativity and attracted by the opposition’s positive appeals. For voters equally exposed to both messages, these individual effects should compound one another, producing an especially strong tendency to vote “No.”

A second hypothesis focuses less on the positive tone of the opposition’s campaign message and more on the plausibility of its claims. The government’s campaign advertising threw out a number of exaggerated, over-the-top charges that an opposition victory would spell the return of communism and economic chaos. The message of the “No” campaign was arguably much more believable—including the specific and tangible criticisms of Pinochet that it did advance. If voters tended to discount the implausible message of the government’s campaign, and were affected only by the credible claims of the opposition, one would expect a different pattern of behavior than if they were actively repulsed by Pinochet’s negative advertising.

In this paper, I argue that campaign advertising effects favored the “No” option in Chile’s 1988 plebiscite because of the plausibility of its claims, not because of voters’ inherent distaste for negativity and preference for positive appeals. The argument draws upon content analysis of television advertising to characterize campaign strategies and an analysis of post-electoral survey data to identify their effects on voting behavior. Using matching, I compare groups of survey respondents who differ in terms of their reception of each side’s advertising message but are similar in other respects that could influence their vote in the election. I find that respondents who differ only in their reception of the government’s negative campaign message do not vote
differently from one another. Only the credible claims of the “No” campaign are shown to have an effect on vote choice.

The findings of this study underscore a series of recent arguments that campaign effects are more likely in new democracies than in established ones. They also suggest that the relative effectiveness of competing campaign appeals depends upon more characteristics of the message than simply its positive or negative tone.

**Campaign Effects and Tone of Advertising: Old versus New Democracies**

For many years, political communication research in the United States expressed skepticism that media exposure during electoral campaigns exerts much influence on public opinion or vote choice (Lazarsfeld, Berelson, and Gaudet 1948; Berelson, Lazarsfeld, and McPhee 1954; Campbell et al. 1960; Patterson and McClure 1976; Chaffee and Hochheimer 1985; Finkel 1993). Demographic variables and existing partisan attachments were thought to play an overwhelming role in structuring political behavior, with campaigns and the media exerting only “minimal effects” at the margin. Recent research on new democracies, however, has questioned the generalizability of this finding. In this section I examine the contextual factors underlying the minimal effects finding in the United States and consider why campaigns should exert much greater impact on political behavior in new democracies. With respect to negative versus positive advertising, one cannot make a general prediction for the direction of this effect, but it has the potential to be quite large in new democracies.

When considering the likelihood of media effects in new democracies, it is important to recognize that the minimal effects finding in the United States depended upon a context in which
preexisting political opinions inoculated most citizens against campaign appeals. Resistance to persuasion derives from two key aspects of the political environment in this and other advanced democracies (Campbell et al. 1960; Zaller 1992; Iyengar and Simon 2000). The first condition consists of stable party identification among the electorate and meaningful partisan cues during campaigns, such that knowing which candidate is a Republican and which is a Democrat allows most voters to choose between them. A second feature is the availability of balanced sources of political information, or the ability to select from a multiplicity of sources and find one that matches pre-existing preferences. In such a context, exposure to campaign appeals tends to reinforce rather than alter political opinions.

Both the partisan and political information criteria are much less likely to apply to new democracies, leading to a greater expectation of media effects (Boas 2005; Lawson and McCann 2005; Baker et al. 2006; Greene 2009). By definition, citizens in new democracies have less experience evaluating political candidates than their counterparts in advanced democracies, and levels of party identification tend to be lower. Party systems are also typically volatile or in flux, especially during transitional elections. Even voters who sympathize with a party may not find a co-partisan among the ranks of serious contenders, leading them to consider other candidates whose formal party affiliation offers little informational short-cut. With respect to sources of political information, the mass media in new democracies are often biased in their coverage of politics, because of conservative ownership, the lingering influence of authoritarianism, and an often underdeveloped journalism profession (Lawson and Hughes 2005). Such bias is even more likely in the case of liberalizing authoritarian regimes (Lawson 2002). In addition, ownership of media outlets is often highly concentrated in new democracies, so citizens have fewer distinct options from which to choose (Lawson and Hughes 2005). If existing political opinions have
primarily been informed by one-sided sources of information, they will tend to be more fragile than those in advanced democracies and more liable to shift if exposed to counterarguments during a campaign.

An additional feature underlying the minimal effects finding in advanced democracies is that candidates’ persuasive efforts are likely to cancel out in the aggregate, such that voters exposed to these competing messages experience little net change in their opinions (Bartels 1992, 2006; Zaller 1996). The “canceling out” expectation is particularly strong when candidates employ similar campaign strategies and have an equivalent capacity to convey their appeals to the electorate, such as spending roughly the same amount of money on television advertising.¹

In contrast to advanced democracies, new and transitional democracies present a context in which the appeals of competing candidates are inherently less likely to cancel out. Given the inherent uncertainty involved in campaigning after a lengthy episode of authoritarian rule, candidates’ strategies in initial post-transition elections are likely to differ substantially from one another (Boas 2009). If one of these distinct appeals is inherently more persuasive, we would expect to see a non-zero net effect of exposure to competing messages during the campaign. Candidates’ resources are also likely to differ in new democracies, leading to different levels of investment in television advertising or other aspects of media campaigning (Greene 2009). In such contexts, aggregate exposure to campaign messages will tend to generate “reception gaps” for a large number of voters, with a detectable net effect on their opinions (Zaller 1996; Moreno 2004).

Even in advanced democracies, the strategies of competing candidates may differ in important ways, generating the possibility of net effects on political behavior or public opinion.

¹ The “canceling out” problem is partly a methodological one: if equal and opposite campaign effects sum to zero in the aggregate, one should be able to detect these separate effects by using more fine-grained measures, finding sufficient variation in message reception, and employing better theoretical models (Zaller 1996).
One major topic of research in this realm has concerned the effect of negative versus positive campaign advertising. The prevailing opinion among American campaign consultants, repeatedly confirmed by surveys of practitioners and studies of the industry, is that negative advertising is effective at swaying vote choice even though viewers generally dislike it (Sabato 1981: 165-166; Kern 1989: 25-26; Perloff and Kinsey 1992; Thurber and Nelson 2000: 192; Mark 2006; Grossman 2009). However, academic research on the effects of negative advertising in the United States has generally failed to support this conventional wisdom. Though the conclusions of individual studies have varied, two recent meta-analyses of the literature found that, on average, negative advertising has no different effect than positive advertising on vote choice, turnout, and a variety of other outcomes (Lau et al. 1999; Lau, Sigelman, and Rovner 2007).

The scant evidence of a general effect of negative advertising does not mean that it never exerts an effect on political behavior. Some studies have found significant positive effects, some have found significant negative effects, and some have found no effect, so a valid conclusion is that the impact of negative advertising is likely to vary according to contextual variables or the nature of the advertising itself. In a study of U.S. Senate elections, for instance, Kahn and Kenney (1999) found that the effect of negative campaigning on turnout depended upon the believability of the criticism: attacks judged legitimate encouraged voters to go to the polls, while unfounded mudslinging convinced them to stay home. Similar variation seems likely with respect to the impact of negative advertising on vote choice.

Substantial variation in the effect of negative versus positive advertising seems even more likely in the diverse new democracies of the world, where institutional context and the nature of negative appeals are likely to differ substantially across countries as well as individual elections. As in the United States, the average effect of advertising tone on vote choice or turnout
in new democracies might well be zero. However, the variance of this effect should be larger, both because of broader contextual variation and because of the factors, highlighted above, that facilitate media effects in general. Indeed, the few existing studies of negative advertising in new democracies have found frequently large effects that, taken as a whole, vary in direction (Chang 2003; Moreno 2004; Desposato 2007).

In sum, because of weak partisan cues, unbalanced information sources, and a reduced likelihood that the strategies of competing candidates should “cancel out,” media effects should generally be larger in new than in established democracies. Whether one expects negative or positive advertising to be more effective should depend upon contextual factors associated with a particular case as well as the nature of the specific campaign messages.

**Chile’s 1988 Plebiscite: A Likely Case for Campaign Effects**

Based on the above criteria, the 1988 plebiscite that inaugurated Chile’s transition to democracy should generate a strong expectation of campaign advertising effects on vote choice. The absence of strong party identification and biased mass media environment under Pinochet’s authoritarian regime meant that citizens were unlikely to have formed stable preferences that would resist persuasion during the campaign. Moreover, a law allocating free television advertising time to both the government and opposition meant that citizens were likely to hear contrasting arguments during the month before the vote. Finally, there were dramatic differences in tone between both sides’ television advertising, allowing for the possibility that the persuasive effects of competing messages would not cancel out in the aggregate.
Chile’s 1988 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” option was supported by the apparatus of the military government, as well as several right-wing parties that had been in recess for most of the dictatorship period. The “No” campaign was supported by the 17-member Concertación de Partidos por el No (Coalition of Parties for the No, henceforth Concertación), a large umbrella group bringing together opposition forces on the center and the left. Registering to vote was optional, but 92% of eligible voters ended up on the electoral list. Voting was mandatory once registered; abstainers faced a fine, and only 2.5% of registered voters did so (Chateau and Rojas 1989). The “No” option was ultimately victorious in the October 5 plebiscite by a margin of 55% to 43%, triggering an open presidential election for December 1989 and the inauguration of a civilian president in March 1990.

A first reason to expect media effects in Chile’s 1988 plebiscite is that partisan cues provided a weak basis for deciding between the two options. Many parties from Chile’s pre-1973 democracy reemerged prior to the plebiscite, so voters were not presented with an entirely unfamiliar set of political organizations supporting each side. However, there had been no partisan electoral competition of any sort since 1973, and the Pinochet government had spent fifteen years actively denigrating parties and party politics. It seems reasonable to assume, therefore, that levels of party identification were low at the time of the 1988 election. Even some of Chile’s most important parties were unfamiliar to large numbers of voters. Partisan reorganization on the right meant that instead of the single National Party that had competed

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2 I am not aware of any surveys from the late 1980s that actually inquired about party identification in Chile.
prior to 1973, there were two new options, National Renewal (RN) and the Independent Democratic Union (UDI) (Pollack 1999). In the survey analyzed below, 35.8% of respondents were unable to place UDI on a left-right ideological scale, and 26.3% could not identify RN.

Because of the nature of the plebiscite—a referendum on an existing government rather than a contest to form a new one—an even more relevant question might be whether large numbers of votes were “up for grabs” in the election. In internal surveys conducted for the “No” campaign during the 6 months before the election, the percentage of respondents stating a “Yes” or “No” vote intention ranged from 47% to 65%. A survey by the Centro de Estudios Públicos (CEP) in April-May 1988 found that only 65.9% of voters had made up their minds.\(^3\) During the final months of the plebiscite campaign, therefore, at least a third of voters were liable to lean one way or another based on the persuasive arguments presented during the campaign.

In addition to weak partisan cues and a large number of undecided voters, the one-sided nature of political information in Chile’s mass media meant that citizens were unlikely to have formed stable opinions prior to the plebiscite campaign. For most of the dictatorship period, news coverage tended to ignore politics entirely, characterizing government functionaries as apolitical technocrats and mentioning the opposition only when denouncing the violent acts of “extremists” or “terrorists” (Portales 1989). In the 1980s, several new print media and radio stations emerged that were critical of the government and sided with the opposition (Tironi and Sunkel 2000). However, television was the major source of political information for most of the population at the time of the plebiscite, and it remained firmly under the control of the military.

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\(^3\) The CEP survey did not ask respondents how they intended to vote, meaning that this question is less likely to suffer from non-response bias.
regime. Until the 1990s, all television stations in Chile were either owned by the government or by universities; the coverage of the latter was easily influenced through Pinochet’s appointment of university presidents (Tironi and Sunkel 2000).

In the lead-up to the 1988 plebiscite, the content of Chilean television was highly favorable to Pinochet and the military government. Broadcast channels began to give some space to the opposition, most notably inviting its representatives to participate in a new series of political talk shows (Portales 1989). However, the presence of “No” supporters on Chilean television was hardly comparable to that of the government. A study of television news broadcasts from April to August 1988 found that the opposition received only 1% of all coverage on the state-owned network, while the government and pro-government groups received 82-86%. On the Catholic University channel, typically considered the least biased, the opposition received 4-6% of coverage, versus 45-53% for the government and its supporters (Hirmas 1993: 84). The imbalance was similar with respect to politicians’ presence on political talk shows (Portales 1989).

Pinochet also dominated Chilean television during commercial breaks. From January to September, the government broadcast a total of 8458 short spots trumpeting its achievements and warning of the dangers of the opposition returning to power (Hirmas 1989: 114-115). Meanwhile, “No” proponents were not allowed to purchase any airtime for political advertising.

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4 In the post-electoral survey analyzed below, 51% of respondents listed television as their most important source of information about current events. This accords with the results of other surveys from around the same time (Hirmas 1993: 84).

5 Qualitative analysis of the tone of coverage found it to be systematically pro-government as well (Portales 1989).

6 The electoral law did not actually permit any paid campaign advertising on television for either the government or the opposition. However, campaign advertising was defined narrowly as a message urging a particular vote in the election. A variety of pro-government messages could be broadcast without explicitly referring to the plebiscite.
In sum, the context of Chile’s 1988 plebiscite is such that we should expect more pronounced campaign effects than in most advanced democracies. Partisan cues provided a relatively weak basis for voters to make a decision, and levels of party identification were likely quite low at the time of the campaign. The mass media environment had also been markedly biased in favor of the government for years before the election. These two conditions mean that voters were unlikely to have formed stable political opinions that would naturally inoculate them against persuasive appeals during the campaign.

**The Franja: A Level Spot in an Unlevel Playing Field**

While Chilean citizens were generally exposed to one-sided political information prior to the 1988 plebiscite, one crucial aspect of television provided an equal opportunity for the opposition. An law passed in April 1988 specified that all broadcast television channels would set aside free airtime for campaign advertising (known as the *franja de propaganda electoral*, or “electoral advertising filmstrip”) during the month before any election. In the case of the 1988 plebiscite, the thirty minutes were to be divided equally between the government and supporters of the “No” option. Each day, a single program for each side would be aired simultaneously on all broadcast channels nationwide. The *franja* was not completely free of regulation; programs had to be submitted forty-eight hours in advance and approved by a review board. However, only one out of twenty-seven opposition broadcasts was censored in such a fashion, and the “Yes” campaign refrained from airing its own program the following day in order to maintain equity. The government had intentionally scheduled the *franja* for periods of low television viewership—10:45 to 11:15 p.m. on weekdays, and 12 noon to 12:30 p.m. on weekends.
However, the programs ended up being extremely popular and widely watched—they obtained a rating of 65%, ten points higher than the popular show *Sábado Gigante* (Hirmas 1989: 117).

As a result of the *franja*, television viewers accustomed to pro-government propaganda were potentially exposed to *some* countervailing arguments during the final month of the campaign. Those who paid attention to the opposition’s message may have been particularly likely to change their minds about how to vote.

**Divergent Campaign Messages**

A final key characteristic of Chile’s 1988 plebiscite concerns the stark difference in strategy between the two sides. The “No” campaign conveyed a positive message of national reconciliation in its free television advertising and limited its criticisms to tangible shortcomings of the Pinochet regime, whereas the “Yes” campaign launched a series of vicious, over-the-top ideological attacks against its center-left opponents. If one of these distinct appeals was inherently more persuasive than the other, the effects of competing messages on vote choice would be unlikely to cancel out in the aggregate.

The initial instinct of a number of opposition politicians was to use their unprecedented access to television to advance a hard-hitting, confrontational attack on Pinochet and the military regime (Sunkel 1992: 43; Puryear 1994: 156). A confrontational approach to the plebiscite campaign would have been a natural extension of opposition parties’ political strategy during much of the 1980s, which consisted of supporting a series of anti-regime protests led by labor unions and shantytown-based popular associations (Garretón 1989; Oxhorn 1994; Roberts 1998; Posner 2004). Negative campaigning also made sense given the nature of the plebiscite—the
vote was for or against Pinochet alone, and the Concertación had no alternative candidate that it could promote in a positive fashion.

Despite the many incentives to run a negative campaign, the Concertación ultimately opted to “go positive” in the 1988 plebiscite, conveying a non-confrontational and forward-looking appeal that all Chileans come together in support of democracy. Based on a series of surveys and focus groups, campaign strategists determined that Chileans were generally afraid of the heightened social conflict that might accompany an opposition victory, and that they would respond most positively to a message of national reconciliation (Tironi 1990: 19-25). The “No” campaign, therefore, ultimately adopted a positive and upbeat tone, epitomized by its slogan “Chile, happiness is coming.” While not shying away from negative statements about Pinochet, the campaign kept its criticism focused on the dictator as an individual rather than the military regime as a whole, stressing that Pinochet’s supporters would have a place in a future democratic Chile (Correa 1989: 162).

In stark contrast to the positive orientation of the “No” campaign, the government’s “Yes” campaign was an attempt to instill fear in the population of the consequences of an opposition victory. It sought to do so by painting the opposition as ideologically extremist and dominated by undemocratic leftist groups, such as the Communist Party and Movement of the Revolutionary Left, that had utilized violent tactics against the regime in recent years. In general, it portrayed the “No” option as a wolf in sheep’s clothing whose victory would bring nothing but chaos. Many of the advertising segments conveying these sorts of messages did so with a particularly apocalyptic tone or by parodying features of the “No” campaign—e.g., replacing its slogan “Chile, happiness is coming” with “Chile, the Marxists are coming.” The “Yes” campaign
also sought to remind voters of the economic crisis during the Allende government that Pinochet had overthrown, arguing that such crisis would return with the opposition back in power.

Data on the content of television advertising during the plebiscite campaign confirm these differences in tone. As part of a larger study of presidential campaign strategies in Latin America (Boas 2009), I conducted a content analysis of half of the episodes in Chile’s 1988 franja for both the “Yes” and “No” campaigns. Among other things, this content analysis involved classifying segments of advertising as to whether they conveyed criticism. As summarized in Table 1, the “Yes” campaign devoted nearly twice as much of its total advertising time to criticism as did the “No” campaign.

<table>
<thead>
<tr>
<th>CATEGORY</th>
<th>“No” Campaign PERCENT</th>
<th>“Yes” Campaign PERCENT</th>
</tr>
</thead>
<tbody>
<tr>
<td>Criticism</td>
<td>19.3</td>
<td>38.1</td>
</tr>
<tr>
<td>Policy</td>
<td>10.8</td>
<td>20.5</td>
</tr>
<tr>
<td>Campaign procedures</td>
<td>5.2</td>
<td>0.7</td>
</tr>
<tr>
<td>Ideological</td>
<td>0</td>
<td>5.2</td>
</tr>
<tr>
<td>Image of chaos</td>
<td>0</td>
<td>5.2</td>
</tr>
<tr>
<td>Other</td>
<td>3.3</td>
<td>6.5</td>
</tr>
<tr>
<td>Positive or neutral content</td>
<td>80.7</td>
<td>61.9</td>
</tr>
</tbody>
</table>

Content analysis data also underscore a second key difference between the advertising of both sides: the nature and plausibility of the criticism that they did advance. When the “No” campaign attacked Pinochet, it generally did so by criticizing the government’s current or prior policies and their effects on tangible issues such as unemployment and poverty, or by pointing out the non-level playing field in the current election. The “Yes” campaign also sought to criticize prior policies, primarily those of the Allende government. However, its claim that an opposition victory would bring back food shortages and hyperinflation was arguably less
plausible for voters who had not experienced such conditions in fifteen years. Moreover, a large share of the government’s attacks simply charged that the opposition was dominated by ideological extremists, or sought to associate the “No” option with an image of chaos, rather than advancing tangible critiques. Claims that an opposition victory would spell the return of communism were clearly hyperbolic at a time when Marxism was on the decline worldwide, and the opposition was presenting itself in moderate, social democratic terms.

Hypothesized Advertising Effects

Existing arguments about campaign advertising effects in Chile’s 1988 plebiscite have overwhelmingly claimed that they worked to the net benefit of the “No” option (Méndez et al. 1989: 93-94; Angell et al. 1992: 57; Sunkel 1992: 45; Aylwin 1995; Boeninger 1997: 323; Tironi 1998: 108-111; Tironi and Sunkel 2000: 184; Fermandois and Soto 2005: 386; Wiley 2006: 672). However, most of these claims are impressionistic in nature, and the few that are based on an analysis of survey data have not examined the indicators that would allow one to directly address the effect of television advertising on vote choice. In short, there have been no prior studies that could substantiate the claims commonly made about advertising effects in this election (Dussaillant 2005: 42). I argue that two distinct hypotheses about voting behavior can be derived from these existing arguments—one of which focuses on the negative versus positive tone of advertising in the plebiscite, and another that focuses on the difference in credibility of each side’s message.

While most claims about the franja in the plebiscite do not specify why media effects should have benefited the opposition, several arguments have pointed specifically to the contrast in tone between the two sides. Tironi and Sunkel (2000: 184), for instance, suggest that both the
“Yes” and the “No” campaigns’ television advertising drove undecided voters toward support for the “No” option—the former because voters were alienated by attacks, and the latter because they were attracted by a positive message of national reconciliation. The authors acknowledge the difficulty of empirically separating out these distinct effects but imply that they should compound one another for voters who received both messages. Under the “positivity” hypothesis, therefore, we should expect Chileans who never watched the *franja* to be most likely to vote “Yes,” while those who followed it most closely would be most likely to vote “No.” Meanwhile, those who primarily paid attention to only one side’s advertising should fall into a middle category.

A second possible hypothesis about the effect of television advertising on vote choice focuses on the difference in the plausibility of each side’s claims. Examining the results of a survey conducted just prior to the October 5 election, Méndez et al. (1989: 93-94) argues that television advertising benefited the opposition in part because respondents judged its *franja* to be more believable by a 52% to 24% margin.⁷ The post-electoral survey analyzed below reports an even larger believability advantage for the “No” campaign: 65% to 20%. Given the prior finding that voters’ response to attack advertising depends on whether they believe the charges, one can hypothesize that television advertising worked to the benefit of the “No” campaign because of this difference in credibility.

The “credibility” hypothesis implies a different relationship between message reception and vote choice than the “positivity” hypothesis. If the believability of each side’s claims is what matters for voting behavior, the government’s hyperbolic attacks on the opposition should not be

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⁷ Méndez et al. (1989) also note that respondents found the “No” campaign’s advertising to be more entertaining, motivating, dynamic, optimistic, and so on. Aesthetic preferences of this sort, however, do not provide a convincing predictor of the direction of media effects. After all, the conventional wisdom among American political consultants is that negative advertising works even though people generally dislike it.
counterproductive, merely ineffective. Thus, whether Chileans paid attention to the negative message of the “Yes” campaign should make no difference in their plebiscite vote. Those who primarily received the message of the “No” campaign should vote similarly to those who paid attention to both messages, and those who were primarily exposed to the advertising of the “Yes” campaign should be indistinguishable from those who never watched at all.

Analysis of Campaign Advertising Effects

To address the above hypotheses about the causal effect of television advertising on vote choice in Chile’s 1988 plebiscite, the ideal research design would involve an experiment. By randomly assigning subjects to different groups that were exposed to varying levels of advertising from the “Yes” and “No” campaigns, a researcher could be confident that any difference in their vote intention was due to the experimental treatment rather than confounding factors such as income, education, or prior political opinions. Random assignment would ensure an expectation of equivalence between the different groups with respect to these potential confounders. Such a design might have limited external validity, but it would allow for the most confident causal inferences, at least among experimental subjects.

When experimental data are unavailable, researchers interested in the question of media effects are forced to make causal inferences from observational data, such as pre- and post-electoral surveys. In the real world, people are not randomly assigned to watch campaign advertising more or less frequently from one another. Rather, they make their own decisions about media exposure, based on a number of factors that are also known to influence vote choice. Simply comparing the voting behavior of those who received different campaign stimuli is of
little use for causal inference because such voters almost certainly differ with respect to income, education, political opinions, and so on.

The traditional approach to this problem of self-selection would be to estimate a multivariate statistical model of vote choice. Such a model would examine the effect of exposure to television advertising, controlling for other variables that are correlated with both the treatment and the outcome and cannot be considered consequences of the treatment itself (as could, for instance, interest in the campaign). This modeling approach requires two particularly strong assumptions. The first is that the decision to expose oneself to different levels of campaign advertising does not depend on any unobserved variable that also has an effect on vote choice (the assumption of selection on observables, or no confounding). The second is that the statistical model has correct functional form, corresponding to the true causal relationship in the real world.

In place of a statistical model, my approach for analyzing survey data on Chile’s 1988 election involves matching, a technique that requires fewer assumptions than standard regression analysis. Matching seeks to simulate the context of an experiment by comparing observations in the treatment group to a subset of those in the control group, such that the distribution of pretreatment covariates 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 bias the causal estimate. If the matching procedure achieves balance across groups with respect to all covariates that affect treatment assignment as well as the outcome, simple mean differences offer an unbiased estimate of the average treatment effect for the treated (ATT). Matching still requires the assumption of no confounding, or selection on observables—namely, that the covariates on which one has achieved balance are the only ones that influence treatment
assignment as well as the outcome. However, it is a non-parametric procedure that does not require any modeling assumption about how these other variables are related to the treatment and the outcome of interest (Sekhon 2009).

Data and Indicators

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. The survey was administered by the Center for the Study of Contemporary Reality (CERC), with face-to-face interviews conducted from October 20-27, 1988, two to three weeks after the plebiscite. I obtained the data directly from CERC; the questionnaire is reproduced in Piñuel Raigada (1990). 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 more suited to an analysis of campaign effects than most electoral surveys in Latin America.

The outcome variable for this analysis 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. When missing values are omitted, the percentage of “No” votes among survey respondents is significantly higher than in the population from which the sample was

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8 Sampling proceeded in three stages. The first stage randomly sampled census subdistricts, and the second stage randomly sampled houses according to roads and the last digit of the street address. Individuals within houses were chosen based on census-derived quotas of sex, age, and employment status.
drawn (Table 2). Social desirability bias on vote choice questions tends to be larger than normal following dramatic, transitional elections such as this one.\footnote{In the Mexico 2000 Panel Study’s post-electoral cross-section, for instance, the self-reported share of valid votes for winning candidate Vicente Fox was inflated by 8.4 percentage points. The bias here is even starker—14.8 percentage points when only valid votes are considered—but Chile’s plebiscite was also a more dramatic and unexpected moment of regime change than the culmination of Mexico’s prolonged democratization process.}

### Table 2: Plebiscite Vote: Survey Self-Reports versus Electoral Returns

<table>
<thead>
<tr>
<th></th>
<th>“Yes”</th>
<th>No</th>
<th>Blank/none</th>
</tr>
</thead>
<tbody>
<tr>
<td>Survey</td>
<td>25.7%</td>
<td>62.2%</td>
<td>12.1%</td>
</tr>
<tr>
<td>Population sampled</td>
<td>35.6%</td>
<td>51.7%</td>
<td>12.6%</td>
</tr>
<tr>
<td>Nationwide</td>
<td>38.6%</td>
<td>49.1%</td>
<td>12.2%</td>
</tr>
</tbody>
</table>

Source: Author’s calculations based on comuna-level electoral returns and June 1988 voting-age population estimates in Chateau and Rojas (1989). Blank/none category includes those who cast a blank or null ballot, did not vote, or were never registered. Missing values deleted.

To a large extent, the difference between the reported and actual distribution of votes may be due to the high rate of non-response to this survey question. Over a fifth of all respondents—387 out of 1700—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; either phenomenon could produce the results above. It is also possible that some “No” voters refused to answer the question out of fear, given that they were still living under an authoritarian regime. 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. The sensitivity of the analysis to different assumptions about the distribution of missing values is discussed in greater detail below.
To measure the treatment of interest for this analysis, the most obvious approach would be to use a simple self-report of exposure to television advertising during the plebiscite campaign. The CERC survey does ask respondents how often they watched the *franja* during the campaign, but using this variable as a treatment indicator is problematic for several reasons. The survey question cannot identify respondents who primarily watched the advertising of their preferred option and may have turned off the television during the other side’s 15 minutes. Moreover, even among viewers who did not discriminate in such a fashion, measuring exposure to television advertising is not the same as measuring receipt of each side’s message (Zaller 1996). Survey respondents might claim to have watched the *franja* every day simply because the television set was always on in the evenings, not because they were paying close attention to the programming.

In addition to the imprecision of self-exposure questions, such indicators are known to contain both random and systematic measurement error, which leads to biased effect estimates (Achen 1983; Bartels 1993). In particular, a question about exposure to the *franja* during the plebiscite is likely to suffer from social desirability bias, leading viewers to systematically overestimate how often they watched the advertising. At the time the survey was administered, the crucial role of the *franja* in the electoral outcome was already being widely 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.

The typical solution to problems with self-reported exposure would be 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 “what is the name of your senator;” this approach usually works best for examining media effects on a broad level rather than the effect of specific messages contained in 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 tap 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).\(^\text{10}\) It would be difficult to answer these questions correctly without having gained some exposure to the message of each side’s campaign advertising. For those who did watch and pay attention, however, each of these scenes was a particularly memorable one that would be unlikely to be forgotten.

It is also possible to have gained knowledge about particular *franja* scenes through discussions with friends and family, just as a television viewer might ask acquaintances about the episode of a popular program that she missed the night before. Indeed, Hirmas (1989: 109) reports that the *franja* became “the topic of everyday conversation” during the campaign. Thus, these survey questions are likely to measure knowledge gained through “hot talk,” or communication within social networks, in addition to directly watching the *franja* (Baker et al. 2006). This dual measurement is arguably an inferential benefit when examining campaign advertising effects. If the true effect of advertising on vote choice depends partly on direct

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\(^{10}\) Scenes a) and c) are from the “Yes” *franja*; b), d), and e) are from the “No” *franja*.
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.

Table 3: Number of Scenes Correctly Identified

<table>
<thead>
<tr>
<th>“Yes” Franja</th>
<th>“No” Franja</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>301</td>
</tr>
<tr>
<td>1</td>
<td>52</td>
</tr>
<tr>
<td>2</td>
<td>19</td>
</tr>
</tbody>
</table>

Reception gap: N = 624
Reception gap: N = 158

To construct a treatment indicator based on knowledge of specific scenes in the franja, I examine how many questions a respondent correctly answered about the “Yes” and “No” campaigns. I exclude 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 can at most answer two questions correctly for both the “Yes” and “No” campaigns. Table 3 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 messages—624 in favor of the “No” campaign and 158 in favor of the “Yes” campaign.

11 “Don’t know,” “didn’t see it,” and non-response to the question are treated the same as an incorrect answer. Thus, no observations are dropped due to missing data on the treatment variable.
Matching Procedure

To analyze the effect of campaign advertising on vote choice in the plebiscite, I conduct a matching analysis based on three categories of the treatment variable: full knowledge of the \textit{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 and passed the placebo tests described below. Those with zero knowledge of the \textit{franja} were simply too different from the other three groups to allow for valid inferences. I also exclude 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 (Sekhon 2009). In estimating the average treatment effect for 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. Therefore, I can consider the counterfactual of how these treated respondents might have voted if they had fallen into the “Both” or “No” message control groups. I cannot, however, 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 do not differ significantly from other survey respondents on basic demographics (Table 4).
Table 4: Treatment Group versus Other Respondents

<table>
<thead>
<tr>
<th></th>
<th>Mean, “Yes” message</th>
<th>Mean, other respondents</th>
<th>P-value, difference</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>40.8</td>
<td>38.1</td>
<td>.058</td>
</tr>
<tr>
<td>Education (0-6)</td>
<td>3.19</td>
<td>3.03</td>
<td>.227</td>
</tr>
<tr>
<td>Family Income (1-9)</td>
<td>3.15</td>
<td>3.18</td>
<td>.837</td>
</tr>
<tr>
<td>Religiosity (0-3)</td>
<td>1.14</td>
<td>1.15</td>
<td>.900</td>
</tr>
<tr>
<td>Employed (%)</td>
<td>41.4</td>
<td>42.5</td>
<td>.738</td>
</tr>
<tr>
<td>Male (%)</td>
<td>46.8</td>
<td>49.4</td>
<td>.545</td>
</tr>
<tr>
<td>Urban (%)</td>
<td>58.9</td>
<td>56.5</td>
<td>.576</td>
</tr>
</tbody>
</table>

Note: P-values are for t-tests assuming equal variance. Urban is an indicator variable for residing in Santiago, Valparaíso, Viña del Mar, or Concepción.

Despite sharing similar demographic characteristics with other survey respondents as a whole, those in the “Yes” message category do differ significantly from each control group on one or more of these covariates, as well as on various measures of media consumption. To identify a subset of respondents in each control group that is similar to the treated group, I begin by using logistic regression to estimate the propensity (or p-score) of being assigned to treatment. The demographic variables in Table 4 are all entered in the p-score model, along with indicators for each of Chile’s regions outside of the Santiago metropolitan area.\(^\text{12}\) I also include a series of variables measuring television viewing habits and sources of political information, which could plausibly affect treatment assignment as well as vote choice. Among these are frequency of watching television news (TVnews, with four categories ranging from “never” to “always”) 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 include indicators for whether the respondent’s preferred newspaper or radio station is sympathetic to the opposition (Opposition Paper, Opposition Radio) and whether he or she usually watches the

\(^{12}\) These indicators cover regions I-X; no voters were sampled from regions XXI or XXII.
least biased (UCTV) or most biased (TVN) television station. These media consumption variables can arguably be considered pre-treatment, since they concern general habits and were measured in the survey prior to any question about the campaign. All multi-category variables in the p-score model are treated as interval-level; I also include their squared terms.

To pair respondents from each control group to those in the treatment group, I conducted genetic matching using the R package Matching (Sekhon forthcoming; Diamond and Sekhon 2008). I began with one-to-one matching with replacement on the linear predictors from each p-score model, along with the actual covariates and quadratic terms. I then checked for balance on 75 first-order interaction terms, added unbalanced interactions to the balance matrix, and repeated the procedure. After several iterations for each control group, I was able to obtain good balance. 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, as reported in Table 5. Slightly better balance could have been obtained here, but at the expense of many unbalanced interaction terms. For the 75 interaction terms, p-values were no lower than .107 for the “No” control group and .091 for the “Both” control group.

For the small number of missing values on these covariates (4% of all observations for Family Income; less than 1.5% for all others), I substituted mean values prior to matching and then checked balance on indicator variables for missing data. After matching, treatment and

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13 Opposition newspapers include La Epoca and Fortín Mapocho; the pro-opposition radio station is Radio Cooperativa (Tironi and Sunkel 2000).

14 I used the standard GenMatch() loss function, which involves maximizing the minimum p-value across the covariates from a) a paired t-test for difference in means between treatment and control groups, and b) a bootstrapped Kolmogorov-Smirnov (KS) test for difference in empirical distributions.

15 These include all possible first-order interactions of covariates in the p-score model, except for those involving the regional indicator variables, as well as the unlikely interaction of Opposition Paper and Opposition Radio with the government television station TVN.
**Table 5: Balance Statistics Before and After Matching**

<table>
<thead>
<tr>
<th>Covariate</th>
<th>100 × Std. mean diff.</th>
<th>Min. p-val., KS or t-test</th>
<th>100 × Std. mean diff.</th>
<th>Min. p-val., KS or t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Before</td>
<td>After</td>
<td>Before</td>
<td>After</td>
</tr>
<tr>
<td>Education</td>
<td>6.39</td>
<td>2.36</td>
<td>0.34</td>
<td>0.29</td>
</tr>
<tr>
<td>Education²</td>
<td>12.00</td>
<td>2.62</td>
<td>0.17</td>
<td>0.29</td>
</tr>
<tr>
<td>Family Income</td>
<td>3.40</td>
<td>3.71</td>
<td>0.46</td>
<td>0.53</td>
</tr>
<tr>
<td>Family Income²</td>
<td>7.88</td>
<td>5.19</td>
<td>0.36</td>
<td>0.32</td>
</tr>
<tr>
<td>Age</td>
<td>21.53</td>
<td>1.34</td>
<td>0.01</td>
<td>0.56</td>
</tr>
<tr>
<td>Age²</td>
<td>22.50</td>
<td>1.84</td>
<td>0.01</td>
<td>0.56</td>
</tr>
<tr>
<td>Religiosity</td>
<td>-4.00</td>
<td>1.34</td>
<td>0.65</td>
<td>0.85</td>
</tr>
<tr>
<td>Religiosity²</td>
<td>-1.31</td>
<td>3.57</td>
<td>0.68</td>
<td>0.64</td>
</tr>
<tr>
<td>Male</td>
<td>-1.84</td>
<td>1.26</td>
<td>0.84</td>
<td>0.88</td>
</tr>
<tr>
<td>Employed</td>
<td>-3.02</td>
<td>-2.56</td>
<td>0.74</td>
<td>0.74</td>
</tr>
<tr>
<td>Urban</td>
<td>7.89</td>
<td>-1.28</td>
<td>0.38</td>
<td>0.71</td>
</tr>
<tr>
<td>TVnews</td>
<td>6.45</td>
<td>3.30</td>
<td>0.47</td>
<td>0.72</td>
</tr>
<tr>
<td>TVnews²</td>
<td>7.67</td>
<td>3.67</td>
<td>0.39</td>
<td>0.69</td>
</tr>
<tr>
<td>TVinfo</td>
<td>18.40</td>
<td>-6.48</td>
<td>0.04</td>
<td>0.46</td>
</tr>
<tr>
<td>Region 1</td>
<td>-8.44</td>
<td>0</td>
<td>0.36</td>
<td>1</td>
</tr>
<tr>
<td>Region 2</td>
<td>1.06</td>
<td>0</td>
<td>0.90</td>
<td>1</td>
</tr>
<tr>
<td>Region 3</td>
<td>-10.17</td>
<td>0</td>
<td>0.31</td>
<td>1</td>
</tr>
<tr>
<td>Region 4</td>
<td>12.15</td>
<td>0</td>
<td>0.15</td>
<td>1</td>
</tr>
<tr>
<td>Region 5</td>
<td>3.82</td>
<td>0</td>
<td>0.67</td>
<td>1</td>
</tr>
<tr>
<td>Region 6</td>
<td>-6.03</td>
<td>0</td>
<td>0.52</td>
<td>1</td>
</tr>
<tr>
<td>Region 7</td>
<td>-27.30</td>
<td>0</td>
<td>0.01</td>
<td>1</td>
</tr>
<tr>
<td>Region 8</td>
<td>-1.85</td>
<td>-1.68</td>
<td>0.84</td>
<td>0.78</td>
</tr>
<tr>
<td>Region 9</td>
<td>0.68</td>
<td>0</td>
<td>0.94</td>
<td>1</td>
</tr>
<tr>
<td>Region 10</td>
<td>1.16</td>
<td>2.88</td>
<td>0.90</td>
<td>0.32</td>
</tr>
<tr>
<td>Opposition Paper</td>
<td>-21.31</td>
<td>2.30</td>
<td>0.02</td>
<td>0.32</td>
</tr>
<tr>
<td>Opposition Radio</td>
<td>-11.94</td>
<td>1.73</td>
<td>0.19</td>
<td>0.32</td>
</tr>
<tr>
<td>UCTV</td>
<td>-21.38</td>
<td>-1.27</td>
<td>0.02</td>
<td>0.32</td>
</tr>
<tr>
<td>TVN</td>
<td>13.55</td>
<td>1.30</td>
<td>0.13</td>
<td>0.76</td>
</tr>
</tbody>
</table>

**Placebo Tests**

<table>
<thead>
<tr>
<th></th>
<th>100 × Std. mean diff.</th>
<th>Min. p-val., KS or t-test</th>
<th>100 × Std. mean diff.</th>
<th>Min. p-val., KS or t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td>Woman-Home</td>
<td>-7.04</td>
<td>11.95</td>
<td>0.42</td>
<td>0.24</td>
</tr>
<tr>
<td>Woman-Kids</td>
<td>-7.91</td>
<td>-5.60</td>
<td>0.36</td>
<td>0.57</td>
</tr>
</tbody>
</table>

Note: 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).
control groups were balanced with respect to missing data, except for missing values of income.\textsuperscript{16}

Finally, it should be noted that matching ameliorates (though does not completely eliminate) the apparent social desirability bias with respect to vote choice. In the matched dataset, with missing values excluded, 30.5\% of respondents reported a “Yes” vote, 57.3\% reported a “No” vote, and 12.2\% reported a blank vote or abstention. These figures are much closer to the electoral returns for the sampled population reported in Table 2. The improvement comes solely from discarding unmatched control observations, not from the prior step of excluding respondents who answered zero questions or one question correctly about each side’s advertising. This excluded group’s vote distribution is actually much closer to the true electoral returns. Because there is some evidence of social desirability bias even after matching, and because the rate of non-response for the vote question is still high in the matched dataset (19\%), I retain non-response as a category of the outcome variable, rather than dropping the corresponding observations or imputing missing values.

**Four Placebo Tests**

Because some unbalance remains in treatment and control groups even after matching, and any number of unobserved factors might also be influencing selection into treatment, it is important to conduct placebo tests before examining treatment effects (Sekhon 2009). A placebo test examines a relationship between two covariates that should equal zero (as with the effect of a sugar pill in a medical study); a non-zero result suggest that some unobserved or unbalanced covariate is still affecting the probability of selection. In general, two types of placebo tests are

\textsuperscript{16} Multiple imputation robustness check to come.
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 estimates of the treatment effect will be unbiased.

### Table 6: Alternative Treatment Placebo Test

<table>
<thead>
<tr>
<th>Plebiscite Vote:</th>
<th>“No” (vs. “Yes”)</th>
<th>Blank/none (vs. “Yes”)</th>
<th>No response (vs. “Yes”)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Pre-matching</strong> (N=1242)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. TVsoaps</td>
<td>-0.15 **</td>
<td>0.039</td>
<td>-0.052</td>
</tr>
<tr>
<td></td>
<td>(0.061)</td>
<td>(0.094)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>2. TVmovie</td>
<td>-0.21 **</td>
<td>-0.15</td>
<td>-0.071</td>
</tr>
<tr>
<td></td>
<td>(0.083)</td>
<td>(0.13)</td>
<td>(0.099)</td>
</tr>
<tr>
<td><strong>Post-matching</strong> (N=474)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. TVsoaps</td>
<td>-0.095</td>
<td>0.11</td>
<td>-1.1</td>
</tr>
<tr>
<td></td>
<td>(0.089)</td>
<td>(0.13)</td>
<td>(0.81)</td>
</tr>
<tr>
<td>4. TVmovie</td>
<td>-0.089</td>
<td>0.13</td>
<td>-0.041</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.18)</td>
<td>(0.15)</td>
</tr>
</tbody>
</table>

Note: Each line reports coefficients and standard errors from a bivariate multinomial logistic regression of vote choice on the covariate plus an intercept (not shown). ** p < .01.

For placebo tests involving an alternative treatment, I examine the effect of watching soap operas (TVsoaps) or televised movies/serials (TVmovie) on vote choice. Prior to matching, viewership of these types of television programming 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 (Table 6). It makes no sense to interpret the relationship between soap opera or movie watching and vote choice in causal terms, however. Entertainment programs on Chilean television are not known to have contained overt political bias at the time of the plebiscite, so the association must be due entirely to self-selection. After matching, the
significant relationship between vote choice and each of these variables disappears. It should be noted that the loss of significance is not an artifact of the smaller post-matching sample size; insignificant results would be obtained even if the standard errors for these coefficients had not changed.

A second type of placebo test considers the effect of the television advertising treatment on an alternative outcome. The CERC survey asks respondents whether they strongly agree, agree, disagree, or strongly disagree 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). While the plebiscite campaign dealt extensively with topics such as the economy, political violence, and human rights, the topic of women’s gender roles was virtually never mentioned.17 Hence, exposure to one or the other side’s campaign message should not have any causal effect on attitudes about women’s role in society. Prior to matching, the treatment group and the “Both messages” control group are highly unbalanced with respect to these two covariates, as reported in the last lines of Table 5. After matching, the imbalance disappears, even though no covariates related to social attitudes were used in the matching procedure.

In combination with the balance statistics reported above, the fact that that matching procedure passes two different placebo tests should lend confidence to our causal inferences of the effect of message reception. It seems unlikely that any imbalance remaining after matching, or the failure to match on unobserved covariates, will introduce serious bias into estimates of the ATT.

17 Out of the six and a half hours of campaign advertising analyzed for this project, only one scene of less than sixty seconds duration from the “No” franja touched upon this issue.
Before proceeding to examine the effect of message reception on vote choice, it is worth stating our relevant hypotheses 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}, respectively, and they will be estimated for each of four outcomes: a “No” vote, a “Yes” vote, a blank vote or abstention, and non-response.

Each hypothesis implies different treatment effects on the probability of a “Yes” vote and a “No” vote, or has different implications for how ATT_{yes/no} and ATT_{yes/both} compare to one another (Table 7). The null hypothesis is that none of the treatment effects are significantly different from zero. Under a “canceling out” hypothesis ($H_1$), we would expect those who primarily received the “Yes” message to 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 larger in magnitude than ATT_{yes/both} for both the “Yes” and “No” voting outcomes. Under the “credibility” hypothesis ($H_2$), we should also expect the two ATTs to be negative for the “No” vote and positive for the “Yes” vote, because the treatment group differs from both control groups in terms of not having received the convincing “No” message. However, ATT_{yes/no} and ATT_{yes/both} should be similar in size for both voting outcomes because whether one internalized the implausible claims of the “Yes” campaign should be irrelevant to the vote. Finally, under the “positivity” hypothesis ($H_3$), ATT_{yes/no} should be zero for both voting outcomes. The “Yes” campaign’s negative message and
the “No” campaign’s positive appeals are both thought to have pushed voters toward the “No”
option, so one type of reception gap versus the other should not make any difference.

Table 7: Hypotheses

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>ATT estimate</th>
<th>“No” vote</th>
<th>“Yes” vote</th>
<th>Effect comparison</th>
</tr>
</thead>
<tbody>
<tr>
<td>H₀: zero effect</td>
<td>ATT&lt;sub&gt;yes/no&lt;/sub&gt;</td>
<td>0</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ATT&lt;sub&gt;yes/both&lt;/sub&gt;</td>
<td>0</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td>H₁: canceling out</td>
<td>ATT&lt;sub&gt;yes/no&lt;/sub&gt;</td>
<td>-</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ATT&lt;sub&gt;yes/both&lt;/sub&gt;</td>
<td>-</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td>H₂: credibility</td>
<td>ATT&lt;sub&gt;yes/no&lt;/sub&gt;</td>
<td>-</td>
<td>+</td>
<td>ATT&lt;sub&gt;yes/no&lt;/sub&gt; = ATT&lt;sub&gt;yes/both&lt;/sub&gt;</td>
</tr>
<tr>
<td></td>
<td>ATT&lt;sub&gt;yes/both&lt;/sub&gt;</td>
<td>-</td>
<td>+</td>
<td></td>
</tr>
<tr>
<td>H₃: positivity</td>
<td>ATT&lt;sub&gt;yes/no&lt;/sub&gt;</td>
<td>0</td>
<td>0</td>
<td></td>
</tr>
<tr>
<td></td>
<td>ATT&lt;sub&gt;yes/both&lt;/sub&gt;</td>
<td>-</td>
<td>+</td>
<td></td>
</tr>
</tbody>
</table>

The relationship between message reception and vote choice after matching provides
strong evidence that television advertising effects benefited the “No” option in the plebiscite—
most likely because of the credibility of its campaign message. To begin with, the null
hypothesis of zero effect can be clearly rejected. As shown in Table 8, of the four estimated
treatment effects on the “Yes” and “No” votes, two are significant at the .05 level and one at the
.1 level, using Abadie-Imbens (2006) standard errors to account for the uncertainty of the
matching procedure. The significance of ATT<sub>yes/no</sub> for both the “Yes” and “No” votes argues
against the “positivity” hypothesis; there is no evidence that the government’s negative
advertising campaign alienated voters to the same degree that the opposition’s positive appeals
attracted them. On their own, the direction and significance of ATT<sub>yes/no</sub> and ATT<sub>yes/both</sub> are
consistent with both the “credibility” and the “canceling out” hypothesis. However, the
difference between these two treatments effects is not significant for any of the four outcomes, a
result that clearly favors $H_2$ over $H_1$. On its own, the implausible argument of the “Yes” campaign appears not to have influenced voters’ decisions; only the “No” campaign’s credible message had a significant impact.

### Table 8: Message Reception and Plebiscite Vote

<table>
<thead>
<tr>
<th>Message Reception:</th>
<th>Plebiscite Vote:</th>
<th>“No”</th>
<th>“Yes”</th>
<th>Blank/none</th>
<th>No response</th>
</tr>
</thead>
<tbody>
<tr>
<td>“Yes” (treatment)</td>
<td></td>
<td>0.386</td>
<td>0.291</td>
<td>0.063</td>
<td>0.259</td>
</tr>
<tr>
<td>“No” (control)</td>
<td></td>
<td>0.475</td>
<td>0.190</td>
<td>0.152</td>
<td>0.184</td>
</tr>
<tr>
<td>Both (control)</td>
<td></td>
<td>0.532</td>
<td>0.259</td>
<td>0.082</td>
<td>0.127</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Treatment Effect:</th>
<th>ATT&lt;sub&gt;yes/no&lt;/sub&gt;</th>
<th>ATT&lt;sub&gt;yes/both&lt;/sub&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.089† (0.054)</td>
<td>-0.146* (0.063)</td>
</tr>
<tr>
<td></td>
<td>0.101* (0.049)</td>
<td>0.032 (0.050)</td>
</tr>
<tr>
<td></td>
<td>-0.089* (0.039)</td>
<td>-0.019 (0.029)</td>
</tr>
<tr>
<td></td>
<td>0.076 (0.053)</td>
<td>0.133** (0.051)</td>
</tr>
</tbody>
</table>

Note: 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. † $p < 0.1$; * $p < .05$; ** $p < .01$.

### Sensitivity to Assumptions about Non-Response

The above interpretation of results looks primarily at treatment effects on the “Yes” and “No” votes, but it is important also to consider the “No response” category and the likelihood that a large number of “Yes” voters refused to answer the survey question. To examine the sensitivity of these results to various assumptions about the true vote of non-respondents, I conducted a simulation. I assumed that true “No” voters refused to answer the vote question with probability $p$, and true “Yes” voters refused to answer with probability $rp$, where $r$ and $p$ are
constant across treatment and control conditions. The true number of votes corresponding to each voting outcome and treatment or control condition (or equivalently, the number of “Yes” and “No” votes hidden in the non-response category) can thus be expressed as a function of the unknown parameter $r$ and the reported voting frequencies. Letting $r$ range from $1/3$ to 3, I randomly reassigned the appropriate number of non-respondents to “Yes” and “No” vote categories for each treatment or control condition 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 41 distinct values of $r$.

Based on this simulation, support for the “credibility” hypothesis is strengthened by any reasonable assumption about the 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. With $r$ equal to 1.8 or higher, 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 difference in treatment effects is smaller, and similarly insignificant, for both the “Yes” and “No” outcomes. Results continue to support the “credibility” hypothesis even under unrealistic assumptions about non-response propensities.

With $r = 1/3$, ATT$_{yes/no}$ for the “Yes” outcome is still positive and significant at the .05 level, arguing against both the null and “positivity” hypotheses. Moreover, there is still no significant difference between ATT$_{yes/no}$ and ATT$_{yes/both}$, contradicting the “canceling out” hypothesis.

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18 Choosing $r = 2.5$ comes closest to approximating the distribution of valid votes in the sampled population. However, since certain categories of respondents (e.g., those with no knowledge of the franja) were excluded from the analysis, voting proportions in the matched dataset should not necessarily equal those in the sampled population. Hence, it is most appropriate to consider a range of likely values for $r$. 

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Robustness Check: Results from a Parametric Model

As a robustness check on the above results, I also estimate a multinomial logistic model of vote choice for the matched dataset. If treatment and control groups are perfectly balanced with respect to relevant covariates, mean differences between groups offer unbiased estimates of the true ATT. Perfect balance is rarely achieved, however, so 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). “Preprocessing” one’s data prior to regression analysis has the advantage of lessening model dependence, or the severity of the functional form assumption. The variance of the causal estimates obtained from different parametric model specifications will be much less if good balance has been achieved beforehand. Ideally, they should differ little from the ATT estimates as well.

The model that I estimate 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 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.\footnote{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 proportions reported in Table 4.} 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 analysis presented in this paper should lend confidence to its conclusions. Four placebo tests—two involving alternative treatments, and two
involving alternative outcomes—argue against omitted variable bias. Dealing with the apparent social desirability bias on the outcome variable by randomly reassigning missing data to the “No” and “Yes” voting categories does not weaken the results. Indeed, under the most realistic range of assumptions about non-response propensities, doing so actually strengthens the findings. 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 for the matched dataset.

Conclusion

Based on a matching analysis of post-electoral survey data, this paper has shown that campaign advertising effects on vote choice did occur during Chile’s 1988 plebiscite, and that the persuasive efforts of both sides did not cancel out among voters who paid equal attention to the two messages. This finding alone is an important one, contributing to the growing cross-national evidence that substantial media effects are more likely in new and transitional democracies than in established ones (Boas 2005; Lawson and McCann 2005; Baker et al. 2006; Greene 2009). It also confirms a commonly repeated claim among analysts of Chilean politics that has not previously been subjected to empirical testing. The persuasiveness of its television advertising may not be the only or even the principal reason that the “No” option was victorious in Chile’s 1988 plebiscite, but it undoubtedly helped.

Yet television advertising does not appear to have favored the “No” option in the plebiscite for the most commonly stated reason among analysts of Chilean politics—the contrast between its positive tone and the negative appeals of the government’s “Yes” campaign. On the
contrary, evidence suggests that the “No” franja was more effective because the arguments that it advanced—not only the forward-looking and positive ones, but also its specific criticisms of Pinochet—were more believable. Rather than alienating potential supporters and pushing them toward the “No” option, the “Yes” campaign’s negative advertising appears to have been largely irrelevant to voters’ decisions. On average, those who paid attention to both messages did not vote any differently than those who primarily paid attention to the “No” franja. Voters appear to have discounted the hyperbolic claims of the government’s fear campaign; they were moved only by the plausible arguments advanced by the opposition.

The finding that negative advertising was not inherently counterproductive in Chile’s 1988 plebiscite challenges an important conventional wisdom that has influenced the conduct of electoral campaigns in Chile’s new democracy. In contrast to the common belief among American political consultants, campaign professionals in Chile argue that aggressive criticism almost always backfires on the candidate, and that positive, forward-looking appeals are most effective. This “lesson learned” is routinely traced back to the experience of the 1988 plebiscite. Eugenio Tironi (2002: 78), a key strategist for the “No” campaign and arguably Chile’s most influential political communication consultant during the ensuing decades, expresses the conventional wisdom quite clearly:

Organizing a campaign around a denunciation of the system, the government, or one’s opponent generates more rejection than support—which is proven in Chile ever since the campaign for the “No” in the 1988 plebiscite, whose stroke of genius was precisely to avoid falling into that temptation.

This study suggest that a very different lesson could be drawn from Chile’s 1988 plebiscite: criticism and positive appeals can both be effective as long as they advance tangible, believable claims. Scrutinizing an opponent’s proposals or prior policy record is not a commonly employed campaign strategy in Chile, where candidates tend to focus instead on their empathy
with the electorate and ability to identify with its concerns (Boas 2009). But specific, policyoriented criticism is not the same as hyperbolic attacks intended to induce fear in the electorate, and voters may not respond to them in the same way. Political discourse can be fair and hardhitting without becoming dirty—a balance that the “No” campaign in the 1988 plebiscite arguably achieved. Indeed, normative arguments can be made in favor of negative advertising, which, in the United States, has been shown to be more informative about substantive policy issues than the typically more vacuous positive ads (Geer 2006).

A study of one electoral campaign, of course, cannot settle the question of what types of electoral appeals are generally most effective at winning votes, either in Chile or in other new democracies. Additional research will be necessary to continue to shed light upon this important question. What seems certain is that the effectiveness of positive versus negative advertising is likely to vary across cases, in response not only to the political context of the election and the country in which it is conducted, but also to the nature of the messages themselves.
References


Tironi, Eugenio. 1990. *La invisible victoria: Campañas electorales y democracia en Chile.* Santiago, Chile: Ediciones SUR.


