Understanding the Effect of Political Advertising on Voter Turnout: A Response to Krasno and Green

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Krasno and Green have argued that political advertising has no impact on voter turnout. We remain unconvinced by their evidence, given concerns about how they measure the advertising environment, how they measure advertising tone, their choice of modeling techniques and the generalizability of their findings. These differences aside, we strongly agree that political advertising does little to undermine voter participation.

There has been much talk about a possible decline in televised political advertising as campaigns supposedly shift their advertising dollars to the internet in future election campaigns. Still, televised advertising for the 2008 presidential election started early (in January of 2007), candidate fundraising is going strong, and there is preliminary evidence that the advertising air war in 2008 will be heavier than ever (Gilbert 2007). When this essay is published, the airways will surely have already been filled with the back and forth of television advertising in the fight for each party's 2008 presidential nomination. Furthermore, there can be little doubt that this is just the tip of the iceberg—that hundreds of millions of dollars and hundreds of thousands of ads will flood targeted markets over the summer and fall of 2008. Many pundits and some scholars (Ansolabehere et al. 1994; Ansolabehere and Iyengar 1995; Ansolabehere, Iyengar, and Simon 1999; Broder 2002) have argued that campaign advertising in general (and negative advertising in particular) serves to corrupt and debase democratic discourse, to mislead and confuse citizens, to shrink and polarize the electorate, and to constrain elected representatives in their efforts to promote good public policy. In short, television advertising, in this view, serves to undermine the integrity of our political system and, some have suggested, should be restrained or even eliminated.[1]

Thus, the focus of Jonathan Krasno's and Donald Green's essay (this issue)—the debate over the effects of political advertising on voter turnout—is one in which political scientists have something important to say to the public and to policy makers about a real-world issue. Employing aggregate models at the media market level along with information about the volume of presidential television advertising in the 2000 elections, Krasno and Green conclude that "the volume of advertising purchased by the presidential campaigns during the final weeks of the 2000 election had negligible effects on voter turnout" (2008, #). Krasno and Green contrast their nonfindings with research showing that advertising can serve to increase turnout (Freedman and Goldstein 1999; Freedman, Franz, and Goldstein 2004; Goldstein and Freedman 2002; Hillygus 2005), using our survey-plus-ad tracking approach as a principal foil. However, there is a bigger story that should not be obscured by their focus on this contrast: contrary to the work of some scholars and much media punditry, Krasno and Green's article adds to the growing consensus in the field that campaign advertising in general, and negative advertising in particular, is *not* "killing our democracy" (Broder 2002), nor even causing voters to stay home.[2]

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[1]As a first step, the Bipartisan Campaign Finance Reform Act of 2002 required candidates to "stand by their ads" by including carefully regulated statements that they approve of their messages.

[2]See Brooks' (2006) reanalysis of the original Ansolabehere and Iyengar studies in which the link between negativity and lower turnout disappears. In our own research, using multiple sources of data over multiple election years, we have never found any evidence that television advertising decreases turnout.

In this respect, the substantive findings from Krasno and Green's study of one presidential election year are consistent with our general conclusions about the effect of political advertising on voter turnout. But our evidence has also shown that advertising can often (although not always) have a small positive impact on voter turnout. In general, our conclusion is that, "Advertising does not have the capacity to boost voter turnout to universal levels; to the extent that ads help citizens cast a ballot, it is on the margins and in small doses" (Franz et al. 2007, 110).

Therefore, while we agree with Krasno and Green's most significant finding from their examination of the 2000 election, in this response we consider whether their analysis significantly advances the advertising/turnout debate. We are not convinced that it does. Most significantly, we have concerns about how they measure the advertising environment and how they measure advertising tone. We also disagree with Krasno and Green in their advocacy for the superiority of a fixed-effects approach to modeling advertising effects, their preference for using aggregate data over survey data, and their claim that grass roots canvassing techniques are more efficient than television advertising for voter mobilization. Finally, we have questions about the generalizability of null findings from a single study in a single year. Nonfindings are important, but not finding an effect with one model specification in one election year does not demonstrate that such effects can never occur. We hope these points will guide future empirical work on the effects of television advertising and will help to move the debate about advertising and turnout—a debate with real-world implications—in new directions.

The Complete Advertisement Environment and the Measurement of Negativity

Krasno and Green understand that presidential ads are not the only sort of advertising being aired in presidential elections. They write, "There may be an incidental correlation between presidential advertising and advertising by candidates for other offices" (#). To account for what could be substantial advertising from other races, they insert a control in some of their models for all the other ads aired in the market. As we argued in the very paper that Krasno and Green critique (Freedman, Franz, and Goldstein 2004), it is crucial to control for other advertising in other races.

However, when it comes to estimating the effects of advertising on voter turnout, there is no clear theoretical rationale for separating advertising into presidential and nonpresidential ads or for reporting results for only presidential advertising. The central question we addressed was not how presidential advertising influences turnout in a contest with multiple offices on the ballot, but instead, how advertising as a whole influenced turnout in a presidential election year. A presidential race at the top of the ballot obviously drives turnout to a significant extent, but one ought not ignore the actual advertising environment through which citizens experience election campaigns. Many states that are not competitive in a presidential race may have very competitive races further down the ballot, and these races can generate a great deal of advertising that has the potential to drive turnout. For instance, while the presidential candidates largely ignored the state of Washington in 2004, Washingtonians saw substantial amounts of advertising in its U.S. Senate and gubernatorial races. Thus, turnout is likely to be influenced by different factors in different states depending on the competitiveness of all sorts of contests, not just the presidential election.

Using Krasno and Green's data, we reestimated their turnout models using total ads aired in the media market as the main explanatory variable, rather than only including presidential ads.[3] Our models were identical in all other respects (we replicated the models Krasno and Green report in their Tables 2 and 3). As our results demonstrate (shown in Table 1 in this response), the effect of the total advertising environment was positive and significant in three of the six replications from their Table 2 and in three of the six heteroskedastic specifications from their Table 3.[4] Most importantly, two of the significant coefficients are present when fixed effects are included. Of course, the advertising effect is attenuated as the model becomes more

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[3]We are grateful to Krasno and Green for providing us with their data set.

[4]Krasno and Green bootstrap their standard errors for the heteroskedastic models, and we adopt their convention for this replication. But the need to bootstrap reinforces some of our concerns with their analysis. Bootstrapping is necessary (especially in their fixed effects models) because with 128 cases and over 40 independent variables, we lose confidence that the statistical estimation is robust (the specifications are maximum likelihood models, whose properties are asymptotic). The question then arises: why, with so much data at so many different units of analysis, do Krasno and Green insist on a specification that raises concerns about the reliability of the statistical relationships? We expand on this in the next section.

TABLE 1   **Replication of Krasno and Green Results Using Total Spots Aired**

			Table 2 replication				
Percentage-point gains	2.50*	1.30*	.38	.56*	N/A	−.31	.12
In Voter Turnout	(.53)	(.41)	(.32)	(.33)		(.34)	(.37)
per 10,000 ads	(.50)	(.42)	(.36)	(.34)		(.36)	(.40)
(Standard errors)							
(Robust standard errors)							
			Table 3 replication				
Percentage-point gains	2.10*	1.21*	.57	.80*	N/A	−.22	.44
In Voter Turnout	(.41)	(.68)	(.55)	(.28)		(.59)	(.33)
per 10,000 ads							
(Bootstrapped standard errors)							
Controls	Midterm turnout	Midterm turnout Fixed effects	Midterm turnout Fixed effects	Midterm turnout		Midterm turnout Fixed effects	Midterm turnout
			Past presidential turnout	Past presidential turnout		Past presidential turnout Campaign contacts and candidate visits	Past presidential turnout Campaign contacts and candidate visits

*p<.05 (one-tailed)
^Replications in Column 5 (N/A) are not necessary because by estimating the models with all ads combined, controlling for other types of ads is irrelevant.
^^Replications in Columns 6 and 7 do not include the control for all other types of ads (these are subsumed in the total ads variable

complex, particularly when both campaign contacts and candidate visits are included in the model, but across the six significant relationships, the mobilizing effect of 10,000 additional ads in the media zone ranges between six-tenths of a percentage point and 2.5%.[5]

In sum, when one reestimates the models using total ads instead of only presidential ads—which we believe is a more theoretically sound approach—the effect of advertising on turnout becomes positive and statistically significant across several different model specifications. That the effect is not uniformly consistent and large, however, does not conflict with our claims. Indeed, we interpret these findings to reinforce what we see as the most important substantive point of this exchange: across a wide range of reasonable models, there is evidence that political advertising can serve to mobilize voting, but there is no evidence that political advertising demobilizes.[6]

In our most recent work (Franz et al. 2007), we conducted aggregate county-level analyses of the relationship between advertising and voter turnout (in addition to making extensive use of surveys from the 2000 and 2004 elections). Our approach was slightly different than that of Krasno and Green. In estimating the rate of turnout in each county in 2000

[5]One challenge in reestimating the models is whether to include all ads from 2000 or all dates aired after some arbitrary cut-off (i.e., post-June 30). It should be noted that the correlation between total spots for the year and total spots in the media zone from July 1 to Election Day is 0.97. Additionally, Krasno and Green are partial to total gross ratings points in the media zone over total spots (although they report similar effects under both specifications). We reestimated all of the models from our Table 1 under three different "total ads" specifications—total points for the year, total spots aired post-June 30, total points post-June 30. We also estimated the models using Krasno and Green's time frame—total points in the media zone in the last three weeks of the election. All of these additional models are available in the online appendix to this response at http://journalofpolitics.org/articles.html.

[6]While we do not find significant effects in the fully saturated fixed-effects models (with controls for candidate visits and contacts), this is likely due to the fact that there is such a high correlation (.51) between advertising volume and candidate visits. Furthermore, it is candidate visits (not contacts) that are highly significant in most of their models. This suggests to us that the inclusion of these two highly correlated measures stresses the fixed-effects models, which suffer from low degrees of freedom.

and 2004, we included four predictors: the volume of advertising in that county (from all races), the rate of turnout in the previous presidential election, and indicators of whether there was a competitive U.S. Senate race and presidential election in the state. In the 2000 analysis, we found that the mobilizing effects of television advertising were "significant, but relatively modest" (109). Specifically, an increase of 10,000 ads in a given media market increased voter turnout in the county by only three-tenths of a percentage point (Franz et al. 2007).

We therefore concur with Krasno and Green that, at least in aggregate models, the mobilizing effect of campaign advertising may be more modest than what we have previously found at the individual level. In this light, we welcome Krasno and Green's study as a something of a corrective for unjustified enthusiasm—perhaps even irrational exuberance—about the potential of campaign advertising. However, to conclude that ads have no mobilizing impact whatsoever is to ignore the weight of the evidence.

Krasno and Green's specification of ad volume also raises issues about the relationship between *ad tone* and voter turnout. By not including information on the tone of advertising in all races, they run the risk of mischaracterizing the overall tone of the advertising encountered by citizens in many media markets. For example, because the 2000 presidential election was uncompetitive in Virginia, there were only a handful of presidential ads broadcast in the Commonwealth, and none at all during the last three weeks of the campaign. There was, however, a competitive and particularly heated contest for the Senate between Democratic incumbent Chuck Robb and Republican challenger George Allen. In the last three weeks of the campaign in the four media markets where the Wisconsin Advertising Project tracked ads in 2000 (Richmond, Roanoke, Norfolk, and Washington, D.C.), 11,450 ads were broadcast, 36% of them "pure" negative in tone and 48% of them contrast ads (those including both negative and positive messages). Krasno and Green's approach would have missed this important feature of the advertising environment in Virginia.

Coding the tone of all advertising in 2000—presidential, Senate, and others—we reestimated Krasno and Green's analysis of advertising tone. (They do not report the results of this analysis in a table, simply noting in the text that negativity never boosts turnout significantly.) When reestimating the analysis for overall tone, we found no instances in which "pure" negativity alone boosts turnout, but five instances (of 12 model specifications) in which the volume of

contrast advertising was positively (and significantly) related to turnout.[7] (All our model results for this analysis are available in the online appendix.) Again, this effect is consistent with what we have found in other analyses—no evidence that negative ads demobilize and some evidence that ads with negative content can stimulate turnout.[8]

Media Zones, Fixed Effects, and Surveys

Krasno and Green argue that any analysis of advertising effects must control for other factors correlated with turnout and account for differences across states. Their prose suggests that the best way—and perhaps the only way—to do this is with fixed-effects models containing a dummy variable for each state. It is, of course, the case that researchers must control for other factors in exploring the effect of campaign messages on turnout. To this end, scholars have employed different controls for competitiveness and different measures of exposure to advertising, but all the important work on advertising and turnout, whether using aggregate or survey data, accounts for other factors (see, among other examples, Ansolabehere and Iyengar 1999; Finkel and Geer 1998; Hillygus 2005; Kahn and Kenney 1999; Lau and Pomper 2004).

As for Krasno and Green's fixed-effects model, the sheer number of additional parameters that must be added to the statistical model potentially strains the analysis. Krasno and Green include 36 additional dummy variables to a model with only 128

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[7]In their examination of presidential ad tone, Krasno and Green also find the coefficient on contrast ads (though statistically insignificant) to be much larger than the effect for negative and positive ads ($\# <$). AU3

[8]In general, we have argued that it is most fruitful to combine "pure negative" and "contrast" ads into a single category. Krasno and Green reference the work of other scholars who assume either that contrast ads are unique or that they represent some midpoint between positive and negative ads (#). We think combining pure negative and contrast ads into one category makes particular sense when one is making causal inferences about the impact of an ad, as viewers are more likely to think about an ad in more or less global terms, rather than in terms of its constituent parts. Consistent with this argument, when coders at the Wisconsin Advertising Project were not given the "middle" choice of contrast in a randomly selected selection of 100 spots from the 2000 and 2004 elections, in 88% of the cases they coded the spot as negative. In other words, contrast ads contain a mix of positive and negative statements, but were almost universally deemed by coders to be negative. We reestimated the results with a combined negative/contrast category and provide the estimates in the web appendix.

observations, leaving them roughly three observations for each estimated parameter. As a result, collinearity among the independent variables is quite high.[9]

Because multicollinearity can lead to inflated standard errors, there is the risk that one will incorrectly reject the hypothesis that a true relationship exists (i.e., accept a false null). In Krasno and Green's case, fortunately, the standard error on the advertising variable in their OLS models does not increase across specifications.[10] On the other hand, the standard errors on the advertising measures are very sensitive in the heteroskedastic fixed-effects models. When bootstrapping our model results (following the approach of Krasno and Green), the standard errors on the advertising measures nearly double in size. Thus, we lose one significant effect (column 3 in the Table 3 replication).[11] Another approach would be to shift to a unit of analysis with more observations, such as the county (where variation in turnout, both as an outcome and a control variable, are higher than in the media zone). Indeed, running a county-level model with fixed effects for states would inspire far more confidence in the results. Better yet, one might supplement an aggregate-level approach with an individual-level analysis. Given that scholars now have so much

data to leverage against this question, it makes sense to look for relationships at multiple levels.

Krasno and Green, however, advocate the superiority of aggregate data over individual-level surveys (#). **AU5** Still, they also freely employ survey-based measures. First, they use data from the National Annenberg Election Survey aggregated to markets to get media zone estimates of voter contact.[12] This becomes an important control variable in their models of turnout.[13] Second, they also use the Annenberg National Election Survey to validate the use of the Gross Ratings Point (GRP) measure. Third, the very use of GRPs employs a survey-based measure; GRPs are calculated from voluntary reports of Nielsen households.[14]

We believe in multiple approaches to exploring the relationship between advertising and voter turnout. There may be some areas where aggregate approaches are superior, but there are also several advantages of surveys over aggregate-level data. Most notably, surveys allow one to make distinctions across individuals in their levels of ad exposure, rather than assuming that all individuals in a given market or "zone" are essentially interchangeable. This enables the researcher to speak about how the effectiveness of a particular ad varies with the characteristics of individuals, including their partisanship, levels of knowledge, interest in the campaign, and gender, just to name a few.

Ultimately, although there is nothing wrong with including fixed effects when degrees of freedom allow, that does not mean that this is the most appropriate way to measure the relationship between ad volume and turnout. Krasno and Green assert that their empirical results stand in contrast to the substantive conclusions we draw in our 2004 article. But as we demonstrate in Table 1 in this response, even with a fixed-effects approach, when one accounts for the total advertising environment in a media zone, one does indeed observe a mobilizing effect from political advertising. As such, their substantive revision of the mobilization evidence appears not to be the consequence of the fixed-effects specification but their undermeasurement of the total ad environment. Indeed, their fixed-effects approach is only one of many potentially valid approaches to investigating how advertising influences turnout, and we encourage researchers to explore this relationship at other units of analysis (which will generally provide a larger sample size) and to think theoretically about other, politically relevant variables that might be

[9]In Krasno and Green's model that includes advertising, turnout in the last three presidential elections, turnout in the last Senate midterm election, and fixed effects, the average variance inflation factor (VIF statistic) is 4.52, meaning essentially that the average estimated variance is over four times as large as what it would have been were the correlations among all the predictors 0.

[10]As noted earlier, however, there is a collinearity issue when both candidate visits and advertising volume are included in the same model—a problem that appears to be exacerbated when fixed effects are used.

[11]This is also true for our tone analysis. The coefficient for contrast ads is consistently large across model specifications, but as the number of independent variables increases, and after bootstrapping, the effects are weakened.

[12]We also question why the authors use only last three weeks of advertising, but nine weeks of voter contact (reported contacts after September 1). The time frame should be identical.

[13]But as Gerber and Green have noted: "An important drawback [to survey-based studies of voter contact and turnout] is that political contact may not be an exogenous predictor of turnout. If parties direct their appeals disproportionately to committed partisans, those most likely to vote will also be most likely to receive contact, and the apparent link between contact and turnout may be spurious" (2000, 653). So long as reported turnout and aggregate turnout are correlated (despite the problem of overreporting in surveys), it seems that Gerber and Green's criticism of survey-based measures of voter contact would apply here as well. It is important to note also that Krasno and Green find no evidence of endogeneity when it comes to the geographic targeting of advertising and expectations about turnout.

[14]See Calame (2007) for an argument by the *New York Times'* public editor that Nielsen ratings are a survey and that the *Times* has been mistaken in how it reports Nielsen ratings by not including a margin of error.

included in models beyond the state-level dummy variable.

Bang for Buck

In their review of previous work, Krasno and Green outline the basic rationale for why television advertising has the potential to encourage voter turnout. They also outline a number of reasons why television advertising might not increase turnout relative to other campaign investments. They note that the primary goal of advertising is not to mobilize and that empirically there are few actual exhortations to participate with television advertisements.

We agree that the goal of television advertising is not to mobilize and have always argued that any effects of advertising on turnout (or interest or efficacy) are essentially by-products of efforts to win elections. Moreover, we have never disputed that in-person mobilization contacts can have a more pronounced effect on voter turnout than advertising exposure. As we put it in our most recent work:

> In our models, we consistently showed positive effects for mobilization contacts, usually in the two to four percentage point range. These results are consistent with many previous observational and experimental studies (Gerber and Green 2000; Rosenstone and Hansen 1993; Verba, Schlozman and Brady 1995) and are greater and more consistent than the effects we see from television ads. This is not surprising, since increasing voter turnout is the express objective of such efforts. As we have argued, it is largely a spillover effect from campaign advertising. (Franz et al. 2007, 140)

Despite this substantive agreement, we question the relevance of arguments about the relative efficiency of investments in mobilization contacts versus television advertising. First, campaigns care little about efficiency and care intensely about winning elections. They are willing to spend as much as they can—and waste a great deal of it—in their quest for electoral victory. Nobody's goal in competitive politics is to raise turnout, in general, merely because high turnout is normatively good. Nor is there a prize for the campaign that was most efficient.

Second, we are not persuaded that the actual cost per voter mobilized through television advertising is greater than the per-voter cost for individual contacts. As Krasno and Green show, massive amounts of money are spent on political advertisements. Nevertheless, the manner in which ground efforts are targeted makes it likely that the net impact of advertising will be higher. Why? There are several

advantages to field experiments—including their higher external validity than lab experiments and their ability to assign at random who is contacted and who is not. Still, they may be ill suited to measuring the true magnitude of the effects of a real campaign. In the real world, campaigns do not randomly assign people to be mobilized or not. Instead, the parties focus their mobilization efforts on getting those who are most likely to vote to the polls (Goldstein and Ridout 2002). In 2004, for instance, the Republican Party launched a comprehensive new program to mobilize core Republican voters (Bai 2004). These voters, targeted because they were gun owners or sent their children to private schools or were members of particular church groups, were frequently people who would have voted even without the efforts of the party. As a result, real-world targeting rarely yields effects of the magnitude found by field experiments.

Certainly, it made sense for the Republicans to focus their mobilization activities on people who were likely to respond, party loyalists with a history of showing up at the polls and voting for the party. In the process, casual, infrequent and only moderately informed voters were likely not touched by the party's mobilization efforts. Instead, these people—those most in need of additional information to spur voting—were more likely to be reached by political advertising, which is much more indiscriminate in whom it touches. So while field experiments will be able to provide valid estimates of the *potential* impact of mobilization activities, they will likely exaggerate their net impact because real-world campaigns work to ensure turnout (and increased information) among their most reliable supporters.

Final Thoughts

Substantively, based on not only this exchange, but on the work of others as well, we believe there is one clear conclusion to be drawn about political advertising and turnout: Advertising can and sometimes does have a positive effect on voter turnout, but by no means is that effect large, universal, or consistent across election years. On the other hand, there is very little evidence that advertising, whatever its other effects, has any negative effect on voter participation in America.

Although this point has been made before, this fundamental finding is an important one. Scholarly findings and arguments about the effect of advertising in general and negative advertising in particular have had unusual penetration into the public sphere and have

serious normative consequences. As we conclude this essay in the summer of 2007, the opening salvos of the 2008 television ad war have already been fired, and there is sure to be public debate about the role of advertising. Political science has something to say about the likely effect of all this advertising on voter turnout.

And yet, while we can confidently predict that American democracy will survive and may even be enriched because of the barrage of political advertising that is sure to come in 2008, there are many other questions that are only now starting to be answered: Do different types of voters respond differently to political advertising? Do different types of ads (distinguished perhaps by their message, not just their tone) have different impacts on the likelihood that a citizen will participate? How might the timing of advertising influence levels of turnout? And how might the broader campaign context (whether the race features an open seat or an incumbent, for instance) interact with features of advertising to influence turnout?[15]

We are grateful for the opportunity to respond to the Krasno and Green essay on the effect of advertising on turnout. We strongly believe greater scholarly progress can occur when different scholars make their arguments side by side. We hope these articles taken together provide evidence for those concerned about the normative effects of political advertising on American democracy and provide guidance for others doing empirical work in the area of political advertising.

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[15]See Brooks and Geer (2007) for an examination of how the civility of ads influences political engagement. See Stevens et al. (2007) for an analysis of the differential effects of negative advertising depending on the direction and intensity of one's partisanship.