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Response to Franz, Freedman, Goldstein, and Ridout

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I. Introduction

During presidential elections, most voters in contested states face a barrage of campaign ads. While these commercials are meant to persuade and not necessarily mobilize voters, there are ample theoretical grounds to suspect they stimulate turnout by informing viewers and activating their sense of civic duty (Freedman et al. 2004) or by signaling a competitive election (Cox and Munger 1989). The behavioral consequences of exposure to campaign ads is interesting in its own right, but important political and policy issues hang in the balance as well. Candidates and their consultants have reason to wonder the extent to which their advertising spurs turnout as they allocate resources. Proposals to regulate campaign financing or campaigns themselves inevitably meet the objection that any reduction in the volume of campaigns will lead to a less informed, interested, and active electorate (Ansolabehere and Snyder 2000).

Our position is that presidential campaign ads have a minimal impact on voter turnout. In their response, Franz, Freedman, Goldstein and Ridout (FFGR) seem equivocal on this point. Their analysis of individual level survey data (Freedman et al. 2004) produced very large effects. We have nothing against survey data per se, but we are particularly cautious about using the non-validated vote item as a dependent variable given respondents' propensity to misremember or exaggerate. That is not necessarily a fatal error: a recent paper by Ashworth and Clinton (2006)

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that controls for potential endogeneity of ad exposure and turnout concludes that presidential campaign ads in 2000 had little effect on survey respondents' professed intention of voting. Indeed, FFGR's aggregate analyses generate an array of much smaller effects. They point out that their forthcoming book reports that exposure to 10,000 campaign ads increases turnout by a meager 0.3 percentage-points (FFGR 2008, p. #), leading them to back away from their survey results, describing them as the product of "irrational exuberance" (FFGR 2008, p. #). If that is their position, then both sides now agree: presidential advertising does not stimulate turnout.

However, FFGR caution against inferring that advertising's effect is "universal or consistent across election years." Perhaps, but their essay provides no evidence about any election other than the 2000 election. We look forward to applying our natural experiments framework to other elections when the data gathered by FFGR become publicly available. We expect to continue to find that presidential ads have a small impact on turnout, as they do not engage voters personally and rarely encourage viewers to vote.

II. Natural and designed experiments

Much of our remaining disagreement with FFGR, then, boils down to our contention that fixed effects approach we employ is the best way, as opposed to just a way, to answer this question. Why fixed effects? Because there are good reasons to think of states as political units in presidential elections. Presidential campaigns compete to win states; they care more about states than about the popular vote. Thus, all New Jerseyans are in the same political boat, but because of the boundaries of media markets, they receive widely varying amounts of advertising. The fixed effects model we use controls for these within-state similarities, allowing us to analyze the

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data as a series of within-state natural experiments. This is, of course, not the only way to analyze the data, but it is the most parsimonious way to control for the variety of factors that affect turnout rates across the states.

This approach necessarily limits our inquiry to presidential ads. Only the presidential race is national. If a viewer in New Jersey sees an ad for Gore emanating from Pennsylvania, she still has same opportunity to go to the polls for Gore that any Pennsylvanian has. The quasi-random variation in exposure to presidential ads *within* states lays the groundwork for our natural experiment.

FFGR accept the logic of this natural experiment, yet they seem to overlook the connection between this logic and the regression analysis derived from it. Suppose, for the sake of argument, that presidential ads had been randomly assigned to media zones: it would be a gross error to combine these experimentally-assigned ads with non-presidential ads, which do not necessarily obey the logic of an experimental design. Ads for House candidates, for example, tend to coincide with contested House campaigns; within state variation in House ads is far from random. If these districts have elevated turnout, lumping presidential ads together with House ads could introduce an upward bias in the estimated effect of presidential ads. Lumping together presidential and Senate ads may also introduce bias, because ads for a Senator in Pennsylvania are unlikely to stimulate turnout in neighboring Maryland.

There is a fundamental difference between *controlling* for alternative sorts of ads and redefining the measure of presidential ads so as to include other types of ads as well. Within an experimental context, non-presidential ads serve the innocuous function of soaking up disturbance variance, which potentially increases the precision with which the effects of

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presidential ads are estimated. FFGR, on the other hand, run the risk of contaminating the experiment by combining exogenous presidential ads with potentially endogenous non-presidential ads.

This move is one of the methodological missteps in their response. Four others warrant brief mention.

1. The use of fixed effects does not have any adverse implications for the unbiasedness or precision with which the effects of presidential ads are estimated. FFGR express general concerns about collinearity and a drop in the number of degrees of freedom, but both of these factors are explicitly accounted for in the standard errors we report. As FFGR concede, the use of fixed effects in this application *reduces* the standard errors.
2. The decision to include state-level fixed effects is designed to lighten the burden of the data analyst, who must otherwise include measures that account for all turnout-related dimensions of interstate that might be correlated with presidential advertising. FFGR seem to believe that their measures of interstate variation are sufficient to eliminate all threats of bias, a view that is both untestable and arguably dubious.
3. FFGR seem hesitant to accept media zones as the proper unit of analysis, suggesting instead that the analysis should drop down to the county level in order to increase the N. This suggestion ignores the fact that ads are assigned to media markets; subdividing media markets into smaller units increases N but may not decrease the standard errors because the subunits are assigned *as clusters* to varying levels of presidential advertising (Murray 1998).

4. FFGR ignore some important statistical nuances when assessing the statistical significance of media effects. The fact that some specifications turn out to generate statistically significant media effects was precisely our point: we argued that the specifications that show the largest effects are the ones that do the least to address problems of unobserved heterogeneity. As for the regressions that purport to show how negatively-toned ads increase turnout, one has to make allowance for the fact that the model contains more than one measure of advertising volume. A post hoc test of significance must correct the size of the test, using a Bonferoni-type correction. When this correction is made, the results are no longer significant.

Finally, FFGR question the relevance of our comparison of the efficiency of TV ads versus grassroots contacts, noting that winning – not boosting turnout – is campaigns’ aim. We agree with this characterization, but turning out one’s own supporters is often the key to victory, as both sides concluded in 2004, making the question of efficiency vitally important. The finding that TV ads have a minimal effect on turnout tells only part of the story. The rest comes from our comparison showing that other techniques, notably canvassing, work much better and are a much cheaper way to get out the vote. In other words, campaigns have good reason to proceed on parallel tracks to persuade undecided voters and to mobilize their supporters. TV ads cannot be counted on to do double duty. This may seem like an obvious point, but there is abundant evidence that it was not obvious to the presidential campaigns as recently as 2000, when expenditures on TV vastly outstripped spending on canvassing.

Part of the dispute at this point comes from FFGR’s concerns about the design of the experiments that generate the efficiency estimates for canvassing. Their doubts are unfounded.

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Most important, they are wrong to assert that experiments take place away from the real-world politics practiced by campaigns. In fact, there have been a series of experiments that create control groups by randomly subtracting households from a campaign's targeting list (e.g. Green and Gerber 2005). Voters' presence on this list in the first place, of course, is far from random; campaign consultants (usually) identified these residents as likely supporters but iffy participants. Even within this rarified category, the exact segment of the population where campaigns try hardest to mobilize, citizens who are canvassed turn out at a higher rate than the control group.

III. Conclusion

In contrast to Freedman et al. (2004) we conclude that TV advertising has minimal impact on actual turnout rates. FFGR emphasize that both of us agree in what they regard as one crucial matter: no matter how small television's effect, we also find that it is positive, thereby rebutting some of the more sweeping claims about the influence of political ads (e.g. Broder 2002). We take those claims about the demobilizing potential of ads less seriously, especially in presidential elections with the huge amount of free news coverage that the candidates receive, than they do. Even if we did not, we are more impressed with the tiny magnitude of ads' impact, than their sign. At least in the 2000 presidential election, and we suspect in other presidential contests, television ads had practically no influence on turnout rates in either direction.

This conclusion has a variety of practical and scholarly consequences. In the real world of political campaigns, it means that candidates and their supporters cannot count on their ads to spur their supporters to go to the polls. What they must do if they wish to mobilize voters is

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devote substantial resources to the grassroots techniques shown to work and work much more efficiently than do TV ads. Again, this may seem obvious, but the 2004 campaign stands out as an exception to the disproportionate emphasis on paid TV ads in previous presidential contests, not to mention races for other offices. The finding that commercials have such a minimal impact on voting rates also undercuts the argument made by opponents of campaign finance reform that limits on limits on soft money advertising pose a severe risk to political participation. Ads may be criticized, or even praised, on various grounds, but not for their impact on turnout, at least not in presidential elections.

Finally, we believe that our results help clarify matters for scholars interested in turnout and political campaigns. They suggest that TV ads had little to do with the decline in presidential voting following 1960, except insofar as ads affected overall campaign budgets, or for the fluctuations in turnout in recent years. Moreover, our results point students of campaigns in a different direction as they consider the effect of the hundreds of millions of dollars spent on presidential advertising. Whatever these ads accomplish, they do not spur voting.

References

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