# Dollars on the Sidewalk: Should U.S. Presidential Candidates Advertise in Uncontested States?*

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**Abstract**

Presidential candidates in the United States do not intentionally advertise in states without rigorous competition for electoral votes. However, in some areas of non-competitive states, media markets overlap with battleground states, exposing these regions to political ads. These spillover advertisements allow us to examine the relationship between advertisements and individual campaign contributions, with data from the Wisconsin Advertising Project and the Federal Elections Commission. Using propensity score matching within uncontested states, we find that 2008 aggregate giving in zip codes exposed to political ads was approximately $6,800 (31.3% of mean contributions) more than in similar zip codes without advertisements.

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# 1 Motivation

Between 2004 and 2008, U.S. presidential candidates and political parties spent over $750 million on televised campaign advertising in an effort to influence the outcome of the general election. However, because of the structure of the American electoral system, none of these advertisements were aired in five of the ten largest media markets - markets covering approximately 43 million voting-age individuals.[1] The goal of presidential candidates is not to maximize their share of the popular vote, but instead to win the majority of Electoral College votes, making it unnecessary, or even illogical, to advertise and campaign in states that are not competitive in the general election. For example, in 2004, Senator Kerry (D) won New York by 18 percentage points and President Bush (R) carried Texas by 23 points. In 2008, Senator Obama (D) carried New York by 27 points, while Senator McCain (R) won Texas by 15 points. Because these states were not competitive, no campaigning occurred. Despite the fact that New York, Dallas, and Houston are three of the largest media markets in the country (in terms of population), no candidates aired campaign advertisements in these areas. The winner-take-all system by state makes campaigning logical only in "battleground," or competitive states.[2]

However, if ads provide candidates with more than just votes, politicians may be overlooking a potential fundraising opportunity. For instance, if exposure to campaign advertising leads to increased monetary contributions to political candidates, then by not advertising in these non-competitive states, presidential candidates may be leaving "dollars on the sidewalk" that could be used to help them win elections.[3]

Economists have long studied the role of televised advertising in the product market, asking what advertisements do. Ackerberg (2001; 2003) finds that ads play an informative role in consumers' decisions, making consumers more likely to buy products they have never tried before. He finds no prestige effect, where celebrities persuasively promote a product, on viewers' consumption choices. In addition, Hertzendorf (1993) finds that ads are a signal of quality, where when a product airs on television, consumers assume that the firm is of a certain, high-quality type and are thus more likely to purchase the good. This study continues to explore the role of

[1] New York, Los Angeles, Dallas, San Francisco, or Houston

[2] Stromberg (2008) creates a model of where politicians should campaign to satisfy the electoral college and maximize vote share, and his equilibrium predictions match politicians' actual campaign schedules.

[3] See Gerber (1998), Levitt (1994), and Stratmann (2009) for more on the effect of campaign spending on election outcomes.

televised advertisements but focuses instead on the political market.

In the realm of political advertising, previous scholars have concentrated on the effect of advertising on turnout (Franz et al. 2008; Krasno and Green 2008; Lau et al. 2007; Gordon and Hartmann 2010; Shacar and Nalebuff 1999; Coate and Conlin 2004) and vote choice (Johnston et al. 2004; Huber and Arceneaux 2007; Franz and Ridout 2010), as this is the way consumers "purchase" a politician.[4] In a working paper, Lovett and Peress (2010) take a marketing approach and model the optimal targeting behavior for political ads and compare this to what politicians actually do. They find that politicians' advertisements serve a persuasive role, and advertisements should (and do) appear on shows that attract median voters who are likely to turn out. However, Ackerberg's "informative" effect of advertising may solicit "consumption" of the politician's product long before election day. For instance, if an individual wants to contribute to the candidate's campaign, he can do this at any point in the election (up to the capped limit). Instead of focusing on individuals' decisions to turn out to the polls or vote for a specific candidate, this study explores the potential that advertisements garner support via campaign contributions. We are the first to examine the relationship between campaign advertising and monetary contributions to presidential candidates and national parties.

This paper uses a novel data source, combining Wisconsin Advertising Project data (WiscAds) on televised campaign advertisements and Federal Election Commissions (FEC) data on individual campaign contributions to determine the financial returns to television advertisements in uncontested states. Our data allow us to use variation in advertisements at the media-market level and contributions data at the zip-code level to determine how people in different zip codes differ in terms of donations.[5]

We focus our analysis on non-competitive states, as there are other campaign events, such as speeches by candidates, campaign offices, and rallies in battleground states that we do not have adequate information on and cannot control for. While there is no intentional advertising in non-competitive states, some areas of these states receive spillover advertisements from competitive states, as the boundaries of media markets do not align directly with state borders. Huber and Arceneaux (2007) argue that including battleground states in a study of campaign strategy

[4] On an even broader scale, DellaVigna and Kaplan (2007) and Gentzkow (2006) study the effects of media slant and television entry respectively on voter turnout.

[5] Prior to this paper, research on campaign effects was concentrated at the county (Franz et al. 2008), or even media-zone level (Krasno and Green 2008).

introduces endogeneity, but by focusing only zip codes within non-competitive states, we are able to eliminate this bias.[6] In addition, we recognize that all areas of uncontested states that receive spillover ads (the treatment zip codes) may have characteristics that are intrinsically different than those areas in uncontested states that did not (the control zip codes). To address this, first, we only account for differences within an uncontested state, as this holds an electoral environment (i.e., state partisanship, number of electoral votes, etc.) constant. Second, realizing that comparing zip codes in Chicago, IL (who receive spillover ads) to zip codes in Rockford, IL (that do not receive spillover ads) may be making an inappropriate comparison, we employ propensity score matching. This way, we match treated zip codes to observationally similar control zip codes in the same uncontested state, and remove all zip codes without a nearest match.

We also test that the effect is not specific to 2008, including data from the 2004 Presidential Election to determine if zip codes in uncontested states who received ads in one election year and not the other donated more in the year they saw ads. Finally, this paper provides sensitivity analysis to account for the timing of the election. We want to ensure that the timing of the election is not simultaneously explaining rises in ads and contributions. Thus, we create a monthly panel of zip codes that received ads at some point in the election to determine the effects of increases in ads, controlling for the month of the election and including zip code level fixed effects.

We find that aggregate giving in zip codes exposed to advertising was, on average, $6,800 (or a third of mean contributions) more than comparable zip codes that were not exposed to ads. In addition, a 10% increase in monthly spillover ads yields approximately a 0.4% increase in aggregate monthly zip code giving. These findings are not specific to 2008: the effect is similar in 2004 and robust to the inclusion of zip-code level fixed effects. While we expect that in the product market, firms are optimally advertising to maximize their profit levels, we ultimately conclude that overlooking uncontested states may not be an optimal advertising strategy for presidential candidates.[7]

[6] While Huber and Arceneaux (2007) consider the effects of television advertising in only uncontested states, they do so by relying on self-reported survey measures of the candidates respondents voted for. We rely solely on observational data in our study.

[7] This paper does not investigate the strategic interaction of party advertising, and thus only finds the partial equilibrium effect of advertising on campaign contributions.

In Section 2, we explain why ads may generate giving. Section 3 describes the identification strategy and Section 4 describes the data. Section 5 explains the main estimation procedure along with results, and Section 6 displays refinements to the main effect. Section 7 includes the strategic implications of the effect, and Section 8 concludes.

## 2 Individuals' Decisions to Contribute

Since 2000, over $1.13 billion has been raised through individual donations for presidential candidates in general elections (FEC 2010). 2008 marked the first year that a presidential candidate rejected federal matching funds and continued to raise money from individual contributions throughout the general election campaign. In doing so, throughout the course of the entire campaign, Senator Obama raised nearly $656 million in individual contributions, which comprised 88% of all funds raised from June through November (OpenSecrets.org 2010). Even though Senator McCain accepted federal matching funds, 54% of his contributions still came from individual donations, amounting to $199 million (OpenSecrets.org 2010). These dollar amounts and percentages indicate the importance of individual-level contributions and demonstrate that they are an integral part of presidential general election campaigns. While other papers (Snyder 1990; Ansolabehere et al. 2003) focus on contributions from Political Action Committees (PACs) and corporations, the literature involving individual-level contributions is sparse.

Consistent with Ansolabehere et al. (2003), we assume that donating to a campaign directly enters a giver's utility function. We posit that ads provide an "informative" mechanism, teaching viewers something about the politician.[8] Without having to seek additional information, someone watching television can quickly learn something about the candidates running for office and perceive himself to be more informed about the election. The idea that short political videos are informative for viewers is not new to this study. Benjamin and Shapiro (2009) find suggestive evidence in an experimental context that individuals previously unaware about political candidates can predict 20% of the variance in electoral outcomes, by simply watching 10 second silent clips of the candidates' debates. They posit that a large factor in indviduals' abilities to predict the winners lies in observing characteristics such as likeability and attractiveness.[9]

[8] See Ackerberg (2001; 2003).

[9] This study shows 10-second clips of gubernatorial debates to experimental audiences completely unaware about these candidates, and asked them to determine who won the election.

Thus, we suggest that the information individuals receive from advertisements is very broad and not necessarily directly related to the policy preferences of the candidate. Instead, information simply serves as a resource and individuals with more information are more likely to participate in elections (Brady et al. 1995).

In addition, Anand and Shachar (2011) find that advertisements serve as a matching device to provide consumers with the necessary information to choose the correct product for their tastes and preferences. We expect that political ads serve a similar matching purpose for politicians and contributors. In particular, information reduces uncertainty and according to Alvarez (1998), people tend not to participate in politics if they are uncertain about the candidates running. Thus, we expect that by reducing the fixed cost of obtaining information about candidates, ads increase an individual's probability of contributing to a political campaign.

Unlike the relationship between advertising and voter turnout, the relationship between seeing an advertisement and contributing to a campaign can be immediate. After an ad airs, an individual may be enthusiastic about a candidate and prepared to vote for him. Unfortunately, he still must wait until November to actually cast his vote. By then, some of this enthusiasm may dwindle. Contributing, however, can be thought of as a consumption good, where an individual can donate as soon as he experiences excitement for a candidate.

## 3 Identification

To estimate the effects of advertisements on contributions, we employ a unique identification strategy, focusing our analysis on states that were not competitive throughout the presidential general election. The analysis exploits the fact that politicians do not outwardly advertise in non-battleground states. Figure 1 shows where U.S. presidential candidates and political parties advertised during the 2008 general election campaign. States that were competitive throughout the general election campaign, like Florida and Ohio, were inundated with ads; every part of the state received advertising. In states that were not competitive, like New York and Texas, only very small portions of the state received advertising; this is because of the incongruence of media market and state boundaries. Since borders of media markets do not directly align with borders of states, some areas of non-competitive states receive spillover advertisements from competitive states. For example, areas of northeastern New York receive spillover ads from

Vermont and New Hampshire, while areas of northern and western Texas receive spillover ads from New Mexico. These "accidental" advertisements in non-competitive states provide us with the ability to isolate and estimate the effect of campaign advertising on campaign contributions.

While we do expect that a similar relationship between television advertising and campaign contributions also exists in battleground states we limit our empirical work to non-competitive states. In battleground states, we face a problem with omitted variable bias, as we cannot control for critical campaign activity, such as locations of speeches made by the candidates, extensive get-out-the-vote campaigns, rallies, and other unobserved activity that could also encourage contributions. This makes it difficult to isolate the effects of advertising in determining an individual's level of contributions in states that received a lot of campaign activity. By limiting our analyses to non-competitive states, we are able to isolate the effect of television advertising from that of other campaign activities.[10]

Figure 2 further highlights the identification strategy used throughout this paper, focusing on Illinois. The gray areas on the map indicate places that received at least some television advertising during the 2008 presidential general election campaign. Throughout the race, Illinois was never considered a battleground state. As it was Senator Obama's home state, the Democratic candidate was predicted to win (and ultimately did carry) the state by a large margin. While neither presidential candidate intentionally advertised in Illinois media markets, several of the surrounding states - Indiana, Iowa, and Missouri - were competitive and received television advertising from both candidates throughout the election. Again, since media market borders do not align with state borders, individuals living in parts of Illinois still saw a significant level of advertising. People living in southwestern Illinois saw ads aired in the St. Louis media market; people living in southeastern Illinois saw ads aired in the Terre Haute media market, and individuals living in northwest Illinois saw ads aired in the Davenport media market. Voters in Chicago saw television advertisements because the Chicago media market also covers northwest Indiana. However, individuals living in media markets completely contained in Illinois, such as Champaign, Peoria, and Rockford, saw zero advertisements. This is consistent with the incentives of candidates, as all three markets fully contained in Illinois received zero ads, while all markets that overlapped with competitive states (IA, IN, MO) saw television advertisements.

[10]Huber and Arceneaux (2007) make this point in their study of the persuasive effects of television advertising.

# 4 Data Description

For contributions data, we rely on the Federal Election Commission's (FEC) individual contributions file. This file includes all contributions greater than $200 that were raised during the entire two-year election cycle (for example, 2007-2008 for the 2008 presidential election).[11] As the FEC does not require the reporting of campaign contributions less than $200, one limitation of this study is that we are unable to account for all the money raised by the candidates and parties.

From the FEC database, we examined only the national party committee contributions and the contributions assigned to each of the two major-party candidates in the 2004 and 2008 presidential election years.[12] We include all contributions aimed at the general election campaign; for 2008, that was any contributions reported from June 1 to Election Day. For the 2004 campaign, we extended the time period and examined contributions reported between March 1 and Election Day because both the Republican and Democratic presidential nominating campaign wrapped up quickly in 2004.

Because the individual-level FEC data do not enable us to control for important covariates such as income and ethnicity, we aggregate the individual contribution data to the zip code level. At this unit of analysis, we obtain demographic variables from the 2000 Census, such as population, population density, age, race, education, and income.[13] We also obtain information on all populated zip codes in the US in order to determine who did not give. Using zip code as the unit of analysis is a significant improvement over previous work (studying advertisements and turnout) that use county or higher levels of aggregation. Zip codes within the same county can be expected to differ greatly in their campaign contributions. For example, within Los Angeles County, there are zip codes with extremely high per capita incomes (Beverly Hills) and zip codes with much lower per capita incomes (Compton). Since we do not have information on individuals that did not give (i.e. a list of all individuals, allowing us to have people give $0), the

[11] The maximum "hard money" contribution is now indexed to inflation. In 2004, after the passage of the Bipartisan Campaign Reform Act (BCRA), the contribution limit to candidates was raised to $2,000. In 2008, the amount increased again, to $2,300. Also, in 2004, individuals were permitted to contribute $25,000 to the national party committees and in 2008, that amount increased to $28,500 (FEC 2010).

[12] This is the same approach Mutz (1995) took in her study of the influence of horse-race media coverage on presidential contributions.

[13] We choose population instead of voting age population, since legally, people younger than 18 are permitted to contribute to a political campaign. However, voting-age population and population correlate at 0.996.

zip code is the smallest unit of analysis at which we can reliably study campaign contributions. In addition, because the Census provides us with a comprehensive list of all populated zip codes in the United States, when merged with the FEC data, we are able to determine in which zip codes no one contributed money to the candidates or national party committees. This way, when we perform our analysis, we are not restricted to models that look only at zip codes that contain "givers."

For campaign activities, we utilized the Wisconsin Advertising Project's (WiscAds) tracking of televised campaign advertisements. WiscAds has recorded all airings of candidate, party, and group advertisements during presidential campaign elections since 2000. We limit the time frame to the same period covered by the FEC data, meaning we are focusing specifically on the relationship between contributions and advertising during the general election cycle of the past two presidential elections. Throughout the paper, we use some measure of the number of advertisements aired by the presidential candidates and political parties.[14] Each year of the WiscAds data, however, has a different number of media markets included: WiscAds tracked the largest 100 media markets in 2004 (or 85% of the U.S. population), but all 210 U.S. media markets in 2008. We focus our analysis on 2008, using 2004 data to verify that 2008 was not an anomaly, and include one specification that aggregates 2004 and 2008 data, using only the largest 100 media markets.[15]

To determine whether or not a state was competitive over the course of the general election, we use both polling data and prediction markets. We first examine Intrade Prediction Market data to determine if each state's election was competitive throughout the 2008 Presidential cycle. A state is considered competitive if the predicted probability of the Democratic candidate winning was within ten percentage points of the predicted probability of the Republican candidate winning at any point during the general election campaign.[16] Prediction markets provide

[14]Other research uses gross ratings points (GRPs) as the measure of advertising. However, GRPs are a survey-based measure of how many people may have seen a particular program or advertisement (Calame (2007); see also Franz et al. (2008)). Airings are observed.

[15]We use the general election campaign for this paper because the incentives for advertising during the primary election are quite different than they are for the general election. In the primaries, the timing of contests and the rules pertaining to the allocation of delegates drive advertising. This means there are fewer accidental spillover markets that receive advertising.

[16]This definition is robust to different specifications, including marking only the closest 15 states as competitive and both decreasing and increasing the threshold by 5 percentage points. Ten percentage points, in the data, seemed to most clearly mark states that had no advertising from states with outward advertising, and hence we focus on this benchmark.

an average estimate of the probability that an event occurs, and these predicted probabilities are based on beliefs (Wolfers and Zitzewitz 2006; 2004). These give us a reliable measure of the competitiveness of a given state for each month of the election, giving a consistent measure for each state and each month of the probability that each candidate will win the electoral votes in a state.[17]

As a reliability check, we also look at polling data, as obtained from Pollster.com to determine whether the Democratic and Republican candidates were ever within 10 percentage points of one another in each state. Using polling data to label a state as competitive or not is logical because it mirrors the way that politicians determine whether or not they are competitive in a particular state. The measure from Pollster.com labels more states as competitive than the Intrade prediction markets do, and is somewhat more volatile. One cautionary note about the polling data is that the number of polls varies dramatically by state. For example, in 2008, there were over 100 polls conducted in Pennsylvania, approximately 75 of which were taken after the Pennsylvania primary was over. In contrast, there were just 11 state-wide surveys conducted in Utah, all of which were following the Utah primaries. The measures of competitiveness, however, are comparable, so for our purposes, we used the Intrade measure. Unless specified otherwise, our results are robust to a competitiveness measure based on polling data.[18] A list of all states that are considered non-competitive is included in Appendix A.

Table 1 displays summary statistics of candidate, party, and total contributions for 2008 in both competitive and non-competitive states. Although the caps on contributions to the national parties (RNC and DNC) are less restrictive than the caps on contributions to individual candidates, dollars contributed to candidates greatly exceed contributions to committees. It is important to point out that in cases where candidates accepted federal funding, such as Senator McCain in 2008, contributions could still be made to the Republican National Committee (RNC) in support of his campaign expenses, so these two "goods" - contributing to a candidate and contributing to a committee - can be thought of as substitutes.[19]

[17] Malhotra and Snowberg (2010) and Snowberg et al. (2007) also use the predicted probabilities in prediction markets to study political behavior. The former examines how primary and caucus results affect a candidates chances in the general election, and the latter investigates the increase in financial fluctuations preceding a presidential election based on beliefs of which candidate win a close presidential election.

[18] Our competitiveness measures are also consistent with the Cook Report, with four exceptions. However, when we drop those states from our analysis, our results do not change.

[19] We do, in fact, see an increase in contributions to the RNC post-September, as Senator McCain could no longer accept contributions in his name. We did not see this same substitution pattern in Senator Obama's

Table 2 shows the distribution of contributions in both competitive and non-competitive states by zip code. In all locations, the median contribution is significantly less than the mean, meaning that many zip codes contain no contributors, and over half of those that do, only give small amounts.[20] Even the 75th percentile of contributions is significantly lower than the mean contribution, so it is clear that the majority of dollars contributed are coming from zip codes that are in the top 95 percent of the contributions distribution. In the analysis that follows, we examine both zip codes with average contributing patterns as well as zip codes with a large amounts of individual giving.

# 5 Propensity Score Estimation and Results

Several concerns exist when estimating the effects of spillover advertisements on campaign contributions. Fundamentally, we want to ensure that the zip codes we compare are as similar as they can be, based on demographic characteristics like population density, per capita income, and race. For example, looking at Figure 2, we do not want to compare zip codes in Chicago to zip codes in Champaign, unless we have some reason to believe those zip codes are similar. Instead, we want our counterfactual to capture the level of campaign contributions from zip codes in the Chicago media market had those zip codes not been exposed to any televised advertisements.[21] Given this objective, we employ propensity score matching to pair like zip codes based on observable characteristics and estimate the effect of being "treated" with ads. Our treatment level is 1,000 ads, which is a low level of ads that are aired in competitive states.[22] The analysis takes each "treated" zip code, pairs it with the most similar zip codes in observables, and computes the average treatment effect on the treated zip codes.[23]

Within the matching framework, we use both kernel and nearest-neighbor specifications to pair treated and untreated zip codes based on density, median household income, percent of

contributions.

[20] Or, at the zip code level, this means that a fewer number of people may be contributing.

[21] As concern may arise that Chicago is intrinsically different than the rest of the sample, because it was the hometown of Senator Obama, we replicate all analysis omitting this media zone and find similar results.

[22] If we increase or decrease this treatment threshold by 20%, the results remain consistent.

[23] Mattei and Bia (2008) have developed a propensity score estimator for continuous treatments. However, we opt to use a binary treatment, in favor of a multi-level treatment since we restrict matches to be within a state. In some states, there is only one spillover media zone, and thus, only one intensity of treatment in that state. For instance, NJ only receives spillover ads via the Philadelphia media market, and thus have a level of treatment in this media zone only.

African Americans, percent of Hispanics, and percent of college graduates.[24,25] We calculate the propensity score based on the logit specification with state fixed effects shown in Equation 1.[26] We do our matching with replacement, allowing two "treated" zip codes to have the same zip code as the closest "control." We compute our standard errors based on bootstrapping methods.[27] Our empirical specification is in Equation 1 below.

$$Prob(Treatment_{z,s} = 1 | \boldsymbol{X_{z,s}}, \delta_s) = \Phi(\boldsymbol{X'_{z,s}}\beta + \delta_s) \tag{1}$$

$$\begin{aligned}
\boldsymbol{X}_{z,s} &\equiv \text{Median Household Income, Percent Hispanic, Percent African American,} \\
&\quad \text{Percent of College Graduates, Population Density} \\
Treatment_{z,s} &\equiv \text{Dummy for receiving ads} \\
\delta_s &\equiv \text{State fixed effects}
\end{aligned}$$

To summarize, we pair zip codes within states that are alike in observable characteristics. For the nearest-neighbor estimation, we take an average of the nearest (up to) 20 matches.[28] For the kernel specification, we take a weighted average from the distribution of propensity scores, using the normal distribution,[29] which gives a weight approaching zero to zip codes that are not very similar in observables to the "treated" zip code.[30] Matching within state compares zip codes in Davenport, IL to zip codes in Champaign, IL, allowing rural areas with lower population densities to be compared to other rural areas, and dropping the treated zip codes that have no matches in observable variables. This specification enables us to pair each zip code receiving spillover ads with its most similar "controls" in the state in order to conclude how much people

[24] See Rosenbaum and Rubin (1983), Rosenbaum and Rubin (1985) for more on matching.

[25] While we would ideally like to match on political variables, specifically voter turnout and levels of political interest, this information is not comprehensively available at the zip code level.

[26] Doing this results in dropping all non-competitive states that received no spillover advertisements. Results from the logit are presented in Appendix B.

[27] However, Abadie and Imbens (2008) find that bootstrapping gives incorrect standard errors with nearest-neighbor matching, so linear matching methods are used in Appendix B to confirm our results.

[28] We also set a caliper of 0.0001 standard deviations in our main nearest-neighbor specification, removing all zip codes that do not come within the caliper of matching from the estimation entirely. In Appendix B, we show results for nearest neighbor with different calipers.

[29] The results are robust to instead using an Epanechnikov kernel, as is shown in Appendix B.

[30] Since all matching occurs within states, balancing reports are done by state. However, an overall balance report is available in Appendix B. State-by-state balancing reports are available upon request.

in a given zip code would have contributed had they received no televised advertisements.[31]

There are several limitations to this empirical specification. First, we assume that the assignment of treatment is "strongly ignorable" given the observables from Equation 1.[32] This means that people are randomly assigned into treatment and control groups conditional on observables. For example, we assume Hispanics who select to live in urban areas close to the border are equivalent to those that choose to live in urban areas deep within the state. In addition, we assume that local radio and local news coverage of the national election is similar across treatment and control groups. In general, we think this assumption is valid since news on broadcast networks is more likely to cover local events, while cable news covers the national election and coverage is consistent across markets. However, the assumption will be violated if a Presidential candidate visits the competitive state, local news covers the event in a spillover market, people in the uncontested spillover area see this on a television program, and they choose to contribute based on this coverage.[33]

Table 3 shows results from the propensity score matching, using both the kernel and nearest neighbor estimates. We find that the average treatment effect on the treated (ATT) of receiving ads for people in zip codes in uncontested states, relative to not receiving ads in observationally similar zip codes in the same state is between $6,800 and $7,200 for the 2008 general election cycle. When we compare this to the mean contributions level in zip codes in non-competitive states, $21,816, this is approximately a third of mean contributions in both specifications.[34] In other words, zip codes in non-competitive states that received accidental spillover advertisements contributed, on average, $6,800 more than did zip codes in non-competitive states that did not receive any television advertisements.

A significant benefit of aggregating the data to the zip-code level is our ability to include all zip codes that did not give. However, this makes it important to look not only at the average effect, but at the distribution of effects overall. The bottom half of Table 3 displays propensity matching results, only it no longer looks at mean effects, focusing instead on the top 10 and 25

[31] If we create a placebo effect and match two zip codes within an uncontested state that both received no advertisements, we find the difference in contributions to be effectively zero.

[32] See Rosenbaum and Rubin (1983) for more on strong ignorability.

[33] We find that most visits from candidates do not occur at the same time as the maximum level of advertising, and we estimate the correlation between the level of advertisements and dollars contributed in the following section.

[34] In all comparisons to means that follow, we compute the sample mean in the cut of the data analyzed.

percent of the effects distribution. We see that the median effect is disproportionately smaller than the average effect, as expected since we include zip codes that do not give in this specification. While the 75th percentile is slightly smaller than the mean effect, the 90th percentile is significantly larger than the mean. Thus, we are interested in determining what factors distinguish the zip codes with the largest effect sizes. Table 4 characterizes the observable differences between treated zip codes with large effects (above the 90th percentile) and treated zip codes with smaller effects (below the 90th percentile.) Even though we matched on observables for our counterfactual, we still find that when comparing zip codes similar in income and population density, the dense markets with high median household incomes have the strongest effect sizes (i.e. markets like DC and Chicago) probably because they have the largest pool of "givers" who are responsive to political ads.[35]

# 6 Refinements

There are two additional concerns that we address in this section in order to ensure that the propensity score matching results are robust. First, we validate that the relationship we find is not specific to the 2008 election. Second, we investigate the timing of the election cycle, using advertisements as a continuous variable.[36]

## 6.1 Beyond 2008?

Since we previously focused on the effects of advertising on dollars contributed during the 2008 presidential general election, we want to ensure that these results are not unique to 2008. Specifically, one might be concerned that results are driven by Senator Obama's record-setting fundraising efforts (and rejection of federal matching money).

This specification addresses the difference in aggregate zip code giving in a year the zip code received ads, when compared to one when it did not. Including data from both the 2004 and 2008 presidential general elections, this specification allows us to identify the effect of being

[35] We are also careful to ensure that the top of the distribution is balanced in our propensity score estimation, and not just the mean values.

[36] All of our results are also robust to dropping all zip codes within counties reporting more than 10% of the population commuting to different states from the Census' ACS. This results in dropping 1,064 zip codes, primarily including those in the Philadelphia, St. Louis, and Reno media markets, where more than 10% of commuters travel from uncontested to contested states to work.

"treated" on a single zip code, as is shown in Equation 2. The identification of $\beta_1$ comes from zip codes that received spillover ads in one election cycle but not the other, based on receiving ads from a bordering state in one election and not the other.[37] For example, states like Indiana and Virginia were competitive in 2008 but not in 2004, which results in zip codes in Illinois and Maryland (non-competitive states in both 2004 and 2008) receiving spillover ads in one election cycle, but not the other. Arkansas and Washington were competitive in 2004 and not in 2008, resulting in spillover ads in Idaho and Tennessee respectively in 2004, but not in 2008.[38] This parameter is identified via 2,096 zip codes (4,192 observations) that flip from "treated" to "untreated" or vice versa over the two election cycles. Throughout, we are careful to cluster our standard errors at the media zone level, as errors may be correlated geographically. This is an important robustness check to ensure that the effect we find is not merely a case study of the 2008 election. The specific empirical model is shown in Equation 2.

$$Cont_{z,y} = \beta_0 + \beta_1 Treatment_{z,y} + \beta_2 Y08_y + \delta_z + \eta_{z,y} \tag{2}$$

$$\begin{aligned} Cont_{z,y} &\equiv \text{Contributions in zip code z in year y} \\ Treatment_{z,y} &\equiv \text{Zip code z in year y received ads} \\ Y08_y &\equiv \text{Dummy for 2008} \end{aligned}$$

We again find a significant, positive effect between television advertising and campaign contributions, as is shown in Table 5. When "treated" with ads, in either 2004 or 2008, aggregate zip code contributions were on average, $9,369 more than when they were not "treated." This result is slightly larger in magnitude than our previous results using the matching framework, and the dollar amount corresponds to 27% of mean contributions. Thus, even when using two years of data, we still find that being exposed to ads increases aggregate contributions.

These results presented in Table 5 also indicate that individuals contributed about $14,000 more in each zip code in 2008 as compared to 2004. We posit two reasons for the stronger relationship in 2008. First, as mentioned earlier, Senator Obama raised a record level of con-

[37] The representativeness of this sample to the entire set of uncontested states is included in Appendix A.

[38] A full list of states where we find cases of flipped "treatment" is included in Appendix A, along with figures depicting these markets geographically.

tributions, resulting in a greater level of dollars contributed in 2008; the larger dollar amount raised could contribute to a stronger effect. Second, in 2004, the Republican candidate (President George W. Bush) was an incumbent; it is likely that advertisements lead to diminished contributions as the candidate is a known entity. Again, we suspect that advertisements are, at least in part, an information shortcut about the candidate. If the electorate already knows a lot about the candidate, because he or she is an incumbent, television advertisements may only provide new information about the current status of the race. In other words, potential campaign donors were not learning anything new about President Bush in 2004 when they saw television ads, though they may still have been learning about Senator Kerry.

We now turn to one final specification, designed to test whether the effects of television advertising on campaign contributions persist when we consider the timing of the election cycle, as well as the level of advertising.

### 6.2 The Timing Issue: Monthly Panel of Zip Codes, 2008

While we argue that the demographics we employ are reasonable observables on which to match, we validate this with an additional specification designed to overcome the potential problem of "hidden bias" in assignment to the treatment or control group. For instance, we may be concerned that zip codes receiving spillover ads may differ in terms of past political participation, which we do not have information on at the zip code level. To ensure that such unobservables are not driving our results, we propose an additional model including monthly variation in ads and contributions, which includes zip code level fixed effects to control for any time-invariant unobservables at the zip code level.

In addition, this specification has the benefit of allowing us to examine the timing between advertisements and contributions. When testing the effects of advertising on variables like voter turnout and vote choice, the decision of whether to vote or for whom to vote necessarily comes after the advertising occurs. Campaign contributions are different. Individuals might see television ads and immediately decide to contribute to the campaign; they do not have to wait until Election Day to act. Using a panel of zip codes throughout the 2008 Presidential general election, we exploit the timing of ads and contributions, focusing on monthly variation in both variables.

We want to show that cumulative ads from time $t_0$ through time $t_1$ (or contemporaneous ads at time $t_1$ alone) explain contributions at time $t_1$ alone. To do this, we break the election into monthly periods from June through October.[39] Since border states change in competitiveness over the course of an election, each zip code receiving spillover ads will receive different amounts of ads each month. Thus, we expect monthly contributions to vary based on spikes in advertisements, and for each observation (a zip code, month pair) we look at the contributions (and logged contributions) by month, as in Equation 3.[40] This specification allows us to use a continuous measure of ads to determine if an increase in advertisements increases contributions in a zip code. Our empirical model is shown in Equation 3.

$$Cont_{z,m} = \beta_0 + \beta_1 log(Ads)_{z,m} + \gamma_m + \delta_z + \eta_{z,y} \tag{3}$$

$$\begin{aligned} Cont_{z,m} &\equiv \text{Dollars (or logged dollars) contributed from } z \text{ in month } m \\ log(Ads)_{z,m} &\equiv \text{Alternately, logged cumulative ads up to month } m \\ &\quad \text{or logged ads in month } m \text{ alone} \\ \gamma_m &\equiv \text{Month fixed effect} \\ \delta_z &\equiv \text{Zip code fixed effect} \end{aligned}$$

We alternate between two measures of our key independent variable, monthly advertisements and cumulative advertisements. We argue that each of these captures different aspects of the advertising environment and might affect contributions in slightly different ways.[41]

First, we use monthly ads to capture the dynamic nature of the general election campaign. Candidates advertise based on their perceived competitiveness of the contests and thus the number of advertisements aired can vary dramatically from month to month. For instance, Pennsylvania was very competitive in June 2008 and received many ads, which meant that

[39] For our purposes, the 4 days in November are included in October.

[40] We also cluster our standard errors at the media zone in this specification.

[41] We choose the number of ads and not spending on advertising as our independent variable of interest, since politicians are getting these ads "for free." Also, the level of ads is a measure of intensity, where 1,000 ads in Philadelphia should not be different than 1,000 ads in Chicago in terms of how they affect individuals' campaign contributions despite the price difference. While the cost of airing an ad is a proxy for market size, it is not a good predictor of zip code size and income. See Stratmann (2009) for more on the difference between spending and advertisements.

southern New Jersey received the same large number of spillover advertisements. However, as the 2008 election progressed, PA became less competitive, Senators Obama and McCain aired fewer ads there, and thus southern NJ received fewer spillover ads in September than June. The effect of monthly ads on monthly contributions measures the immediate effect of television ads on individual campaign contributions. Second, we may think that a higher dosage of total ads may increase the probability that any individual in a zip code may see an ad. Since we are measuring contributions at the zip code level, we also examine the possibility that cumulative ads up to a certain month can explain contributions in that specific month alone. Thus, the effect of cumulative ads on monthly contributions measures the likelihood that an individual has seen ads over the course of the campaign. This specification also uses zip-code-months as the unit of analysis, which allows us to account for unobservables at the zip code level as well as consider more explicitly the causal relationship between ads and contributions.

With the inclusion of monthly data and zip code level fixed effects, it is no longer feasible to include all zip codes that never gave in 2008, as we did in the propensity score matching. Instead, we are looking only at zip codes that contributed something (and received ads) during at least one of the months of the general election campaign. Table 6 shows us the role of the election cycle. Across all specifications, controlling for the volume of advertising, contributions in September and October are significantly higher than contributions in June (the excluded month), July, and August. Table 6 further illustrates the results of the monthly panel specification. In columns 1 and 2 we see that a 10% increase in monthly ads, i.e. 1,000 to 1,100 ads, yields approximately a 0.4% increase in aggregate monthly zip code giving. Columns 3 and 4 show a similar effect using cumulative ads as the key independent variable. There, we see that increasing ads by 10% leads to a 0.6 % increase in aggregate zip code giving for that single month.

These specifications are also consistent with the matching results. In the matching specification, going from zero to 1,000 ads (the treatment level) resulted in a 31% increase in contributions over the course of the general election cycle. In the monthly zip code panel specification, increasing from 0 to 200 ads per month yielded an approximately 25% increase in monthly contributions.[42]

Overall, each of our three empirical specifications - propensity score matching, a 2004-2008

[42]If instead, we look at ads as a binary treatment effect in this setup, we obtain also obtain parallel results to the matching specification.

zip code panel, and a monthly panel of zip codes in 2008 - indicate that television advertising has a statistically significant effect on campaign contributions at the zip-code level. However, up until this point, candidates have been receiving these increased campaign donations "for free." In other words, because of the incongruence between media market and state boundaries, candidates were unable to avoid airing spillover ads in non-competitive states if they wanted to cover competitive states with ads. The next section of this paper examines whether candidates could intentionally advertise in non-competitive states in an effort to increase their overall fundraising totals.

# 7 Strategic Implications

Since the top of the distribution has a large influence on the mean propensity score matching results, it seems that a politician may be able to receive additional resources by intentionally advertising in non-battleground states with very high income individuals. For instance, though New York state has not been competitive in a number of years, running ads in the New York media market could result in enough contributions to not only pay for those advertisements, but also to generate additional money to redistribute to other areas of the country that are more competitive.

To explore this further, we write down the politician's objective function for each market fully contained in an uncontested state, i.e. that did not receive spillover ads, to determine the optimal level of ads in each market. We begin with the simple profit maximization problem shown in Equation 4, where "profit" is equal to the increase in dollars contributed in a media market due to advertising, $C_M$, less the total cost of running a certain number of ads. Here total cost is equal to the marginal cost of an ad, $P_M$ times the number of ads aired in a month, $A_M$, assuming constant marginal cost.[43] The 2008 Governor, House, and Senate WiscAds data report estimated costs of ads in each market, so this allows us to identify the median cost in each market, or the price of an ad. The median ad cost for presidential ads should not be different from the median ad cost for these advertisements as networks are not permitted to price discriminate when selling advertising slots, and politicians all tend to advertise on similar programs and at similar times of day. A presidential candidate solves the following maximization

[43]We scale profit by 5, since there are 5 months in the general election and the estimates found the marginal effect of monthly ads on monthly giving. We also implicitly assume that prices are set exogenously in the market, as markets must charge the same price to politicians as firms.

problem in each market fully contained within an uncontested state.

$$\max_{A_M} \Pi_M \quad = \quad 5(C_M - P_M A_M) \text{ s.t } \Pi_M \geq 0 \tag{4}$$

Substituting the marginal relationship between ads and contributions from Equation 3 and multiplying this effect by the number of zip codes, $Z_M$, we obtain the following simplification, where $\beta_1 = 101$ from Column 1 of Table 6.

$$\Pi_M \quad = \quad 5(Z_M \beta_1 log(A_M) - P_M A_M)$$

Taking first order conditions, and assuming there is an interior solution in each market[44] yields the following, where $\hat{A}_M$ is the optimal amount of advertisements in market $M$.

$$\begin{aligned} \frac{\partial \Pi_M}{\partial A_M} \quad &= \quad \frac{Z_M \beta_1}{\hat{A}_M} - P_M = 0 \\ &\Rightarrow \quad \hat{A}_M = \frac{Z_M \beta_1}{P_M} \end{aligned}$$

We first take five media markets that contain some of the top contributing zip codes in non-competitive states, all of which do not receive ads: New York, Los Angeles, Dallas, San Francisco, and Houston. Table 7 illustrates the results of this hypothetical policy experiment, using the optimal ads calculation described above. The second column simply reports how many zip codes are in each of these five media markets, and the third displays the median cost, or price of one ad in each market. Next, in column 4, we report the calculated optimal level of monthly ads in each market. From this, we also calculate the net gain by market as described above, which we report in the final column. In all five media markets listed, we assert that candidates could make money by simply airing ads in media markets in these non-competitive states. From these five markets alone, presidential candidates could potentially raise approximately $891,000 in individual contributions if they advertised at each market's optimal level.[45] We expect that this estimate actually understates the effects in these markets, since we learned from the propensity

[44] If the profit at the optimal point is less than zero, this implies that the politician should not advertise in this market, and hence $\hat{A}_M = 0$.

[45] This figure is for both candidates together. Separating out specific parties is an avenue for future research.

score matching that higher income zip codes are those in which the effect is largest, so we consider this to be a lower bound. This "extra" money could then be redistributed to spending on a battleground state in order to increase a candidate's probability of winning that state's electoral votes.

Using this same methodology to predict expected gains, we find that if candidates were to air the "optimal level" of advertisements in each uncontested market, this would result in a $6.03 million gain in all markets in uncontested states, and only three markets would not generate a positive return.[46,47] Again, based on the positive relationship between television advertising and campaign contributions, we argue that presidential candidates are leaving money on the table that they could collect if they were to advertise in media markets contained solely within non-competitive states.

We understand that this policy recommendation makes out-of-sample predictions. However, we still assert that at a minimum, we find from our zip-code fixed-effects analysis that individuals gave more when their zip code was treated with ads than they did in the absence of ads. Thus, if an area was ever exposed to ads, it may be beneficial to a politician to continue advertising there, even in years where there is no overlap with a competitive state. For instance, even if New Hampshire were not competitive, advertising in Boston may still leave a politician with net gains.

# 8 Concluding Remarks

In 2000, George W. Bush spent $5.5 million on campaign advertisements in California - a state Governor Bush ultimately lost by twelve percentage points, and a state that has not been won by a Republican presidential candidate since 1988. The question of why governor Bush spent precious advertising money in California, when that money could possibly have been put to better use in more competitive states like Ohio, Pennsylvania, Wisconsin, and of course, Florida puzzles many scholars. According to a top Bush advisor, the expenditures were intended to fulfil the campaign's promise to be a force in California. "There was a commitment that we made to California early on, and that commitment was time and money," (Marks 2000).

[46]To put this into perspective, Obama and McCain together raised just over $855 million over their entire campaigns (OpenSecrets.org 2010).

[47]These three markets are Bend, OR, Laredo, TX, and Biloxi, MS.

Based on our examination of fundraising and advertising in the 2004 and 2008 campaigns, this strategy likely paid off. By running television advertisements in a non-competitive state, the Bush campaign may have raised more money from Californians than it spent in airing the ads and more than it would have raised had it not aired the advertisements at all.

However, in the two presidential elections since 2000, we have not seen any presidential candidates or political parties spend money to advertise in non-competitive states.[48] We argue that by not doing so, candidates and parties appear to be leaving "dollars on the sidewalk." In other words, because advertisements in spillover markets lead to increased campaign contributions, by not advertising in media markets with high-income zip codes, candidates may not be reaching their full fundraising potential. We ultimately conclude that targeting advertising in non-competitive areas with a large concentration of high income individuals may prove to be profitable for politicians.

[48]Interest groups, however have spent money advertising in non-competitive states. We do not consider these ads in our analysis because we are focused solely on candidate strategy and fundraising. Outside groups may have different incentives for advertising than influencing an election.

# 9 Figures

Figure 1: Presidential Candidate Advertising, 2008

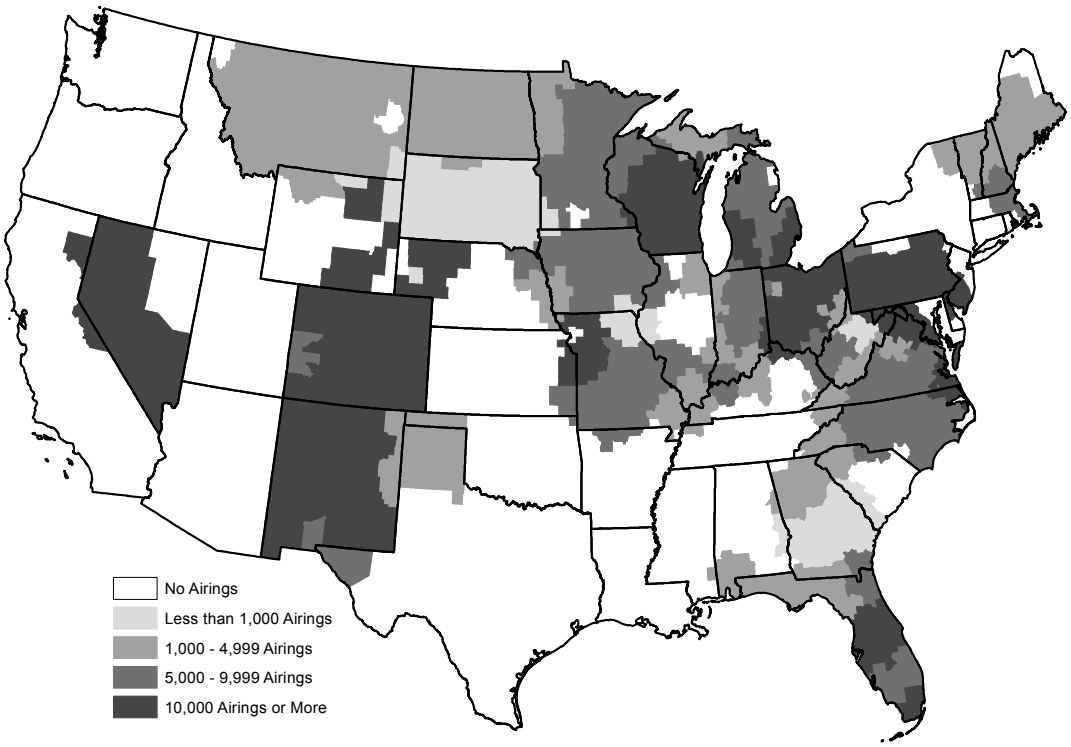
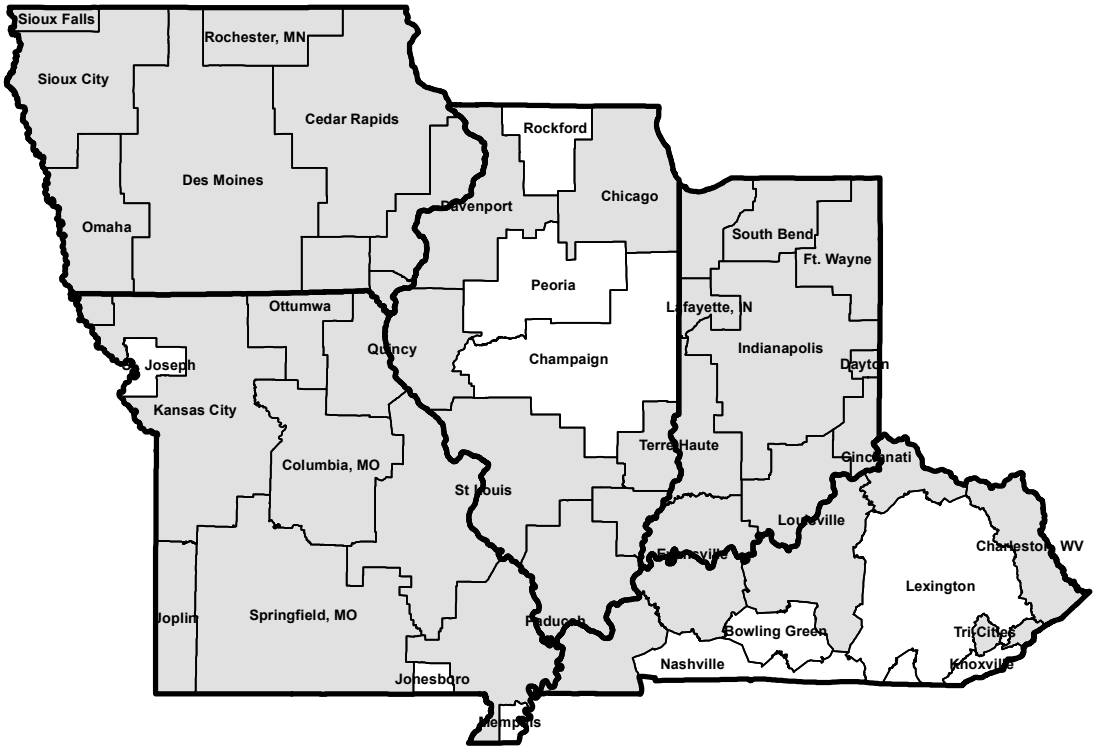


Figure 2: Illinois Airings, 2008 General Election



# 10 Tables

Table 1: Candidate versus Committee Contributions, 2008

| | All | Non-Competitive | Competitive |
|---|---|---|---|
| National Party | 1,956.2 | 2,101.58 | 1,671.14 |
| | (43.93) | (59.82) | (56.14) |
| Candidate | 17,494.08 | 19,714.19 | 13,140.72 |
| | (549.14) | (781.13) | (543.04) |
| Observations | 30,571 | 20,246 | 10,325 |

Mean dollars reported, standard errors in parentheses.

National party contributions made to the RNC or DNC.

Only contributions made during general election considered.

Table 2: Distribution of Contributions, 2008

| | Everywhere | Non-Competitive | Competitive |
|---|---|---|---|
| Mean | 19,450.28 | 21,815.76 | 14,811.86 |
| Median | 750 | 750 | 600 |
| 75 Percentile | 6,667 | 7,130 | 5,850 |
| 95 Percentile | 76,832 | 80,779 | 69,016 |
| Standard deviation | 102,316.7 | 118,245.3 | 59,563.88 |
| Observations | 30,571 | 20,246 | 10,325 |

Aggregate individual contributions (in dollars) at the zip code level are reported.

Table 3: Results: Propensity Score Matching 2008

| | (1) Contributions Kernel | (2) Contributions Nearest-Neighbor |
|---|---|---|
| ATT | 7.213*** | 6.822*** |
| | (1.436) | (1.445) |
| **Percent of Mean** | **33.10** | **31.31** |
| | | |
| **Top of Distribution of Treatment Effects** | | |
| 50th Percentile | 1.27 | 1.62 |
| | (8.65) | (2.39) |
| 75th Percentile | 6.28*** | 5.79*** |
| | (1.82) | (1.84) |
| 90th Percentile | 15.72*** | 19.44*** |
| | (3.94) | (3.93) |
| Number Treated | 6,397 | 6,343 |
| Total Sample | 20,215 | 20,150 |

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard Errors are in parentheses and computed using bootstrapping.

The Treatment is whether or not a zip code received spillover ads.

ATT is average treatment on the treated, reported in thousands of dollars.

The dependent variable is thousands of dollars contributed in a zip code.

Percent of mean calculated using the sample mean.

Table 4: Characterizing the Treated Zip Codes with the Largest Effects

| | Top 90% | Less than 90% | P-value |
|---|---|---|---|
| Total Ads | 6,181.5 | 6,470.4 | 0.22 |
| | (220.6) | (68.8) | |
| Income (in thousands) | 61.31 | 39.06 | 0.00 |
| | (0.977) | (0.18) | |
| Population (in thousands) | 25.51 | 5.90 | 0.00 |
| | (0.71) | (0.13) | |
| Percent of College Grads | 45.90 | 16.74 | 0.00 |
| | (0.72) | (0.14) | |
| Population Density (People per sq mile) | 4,233.16 | 592.99 | 0.00 |
| | (273.38) | (30.35) | |
| Treated Observations | 542 | 5,801 | |

Means reported. Standard errors in parentheses.
Only treated zip codes included.

Table 5: Results: 2004 and 2008, OLS with Zip Code Fixed Effects

| | Contributions |
|---|---|
| 2008 | 14.128*** |
| | (1.507) |
| Treatment | 9.369*** |
| | (1.507) |
| Constant | 22.552*** |
| | (1.442) |
| **Percent of Mean** | **27.31** |
| $N$ | 4,192 |
| Number of Groups | 2,096 |

Robust standard errors clustered at the media zone in parentheses
Effects reported in thousands of dollars.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Monthly Panel of Zip Codes in 2008

| | (1) Monthly Amount | (2) log(Monthly Amount) | (3) Monthly Amount | (4) log(Monthly Amount) |
|---|---|---|---|---|
| log(Monthly Ads) | 101.0* | 0.0399*** | | |
| | (58.54) | (0.0116) | | |
| log(Cumulative Ads) | | | 43.18 | 0.0629*** |
| | | | (114.8) | (0.0167) |
| July | 466.3 | 0.123*** | 499.7* | 0.0726 |
| | (343.8) | (0.0452) | (287.7) | (0.0438) |
| August | 560.2** | 0.638*** | 535.4 | 0.540*** |
| | (268.0) | (0.0620) | (373.5) | (0.0785) |
| September | 4055.0*** | 1.285*** | 4077.0** | 1.168*** |
| | (1460.1) | (0.0862) | (1686.6) | (0.106) |
| October | 2332.8*** | 1.314*** | 2432.1** | 1.146*** |
| | (599.0) | (0.0867) | (1005.6) | (0.115) |
| Constant | 2657.0*** | 2.262*** | 2839.8*** | 2.189*** |
| | (555.9) | (0.0501) | (452.3) | (0.0612) |
| $N$ | 41540 | 41540 | 41540 | 41540 |
| $R^2$ | 0.806 | 0.797 | 0.806 | 0.797 |

Robust standard errors clustered at the media zone in parentheses.

Excluded group is June. Monthly Amount in dollars.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: NYC, LA, San Francisco, Houston, and Dallas policy recommendations

| **Expected Gains to Advertising** | | | | |
|---|---|---|---|---|
| Market | Zip Codes | Market Price | Optimal Monthly Ads | Net Gains ($s) |
| NYC | 1,049 | 2,186 | 48 | 351,845 |
| Los Angeles | 473 | 946 | 50 | 166,270 |
| Dallas | 407 | 815 | 50 | 144,441 |
| San Francisco | 305 | 500 | 61 | 121,626 |
| Houston | 301 | 605 | 50 | 106,576 |

Market Price is the median cost based on WiscAds 2008 Gov, House, Senate Election advertisements.

# 11 Appendix A

Table 8: Geographic Areas Used to Identify the Diff-In-Diff Model

| Year | Market Receiving Ads | Battleground State | Spillover State(s) |
|---|---|---|---|
| 2008 | Atlanta | GA | AL |
| | Savannah | GA | SC |
| | Chicago | IN | IL |
| | Evansville | IN | KY, IL |
| | Louisville | IN | KY |
| | Tri-Cities | VA | TN |
| | Washington, DC | VA | DC, MD |
| 2004 | Memphis | AR | MS, TN |
| | Shreveport | AR | LA, OK, TX |
| | Spokane | WA | ID, MT |

Table 9: Competitiveness of States, 2004 and 2008

| | | 2008 | |
|---|---|---|---|
| | | Competitive | Non-Competitive |
| 2004 | Competitive | CO, FL, IA*, MI*, MO, NV, NH, NM, OH, PA*, WV | AZ, AR, KY, ME, MN, NJ, OR, WA, WI |
| | Non-Competitive | IN, NC, VA, GA | AL, CA, CT, DE, DC, HI, ID, IL, KS, LA, MD, MA, MS, MT, NE, NY, OK, SC, TN, TX, VT, WY |

Data for ND, RI, SD, and UT were unavailable for 2004, and are not included in this table.

Figure 3: Geographic Areas that Received Advertising in Either 2004 or 2008

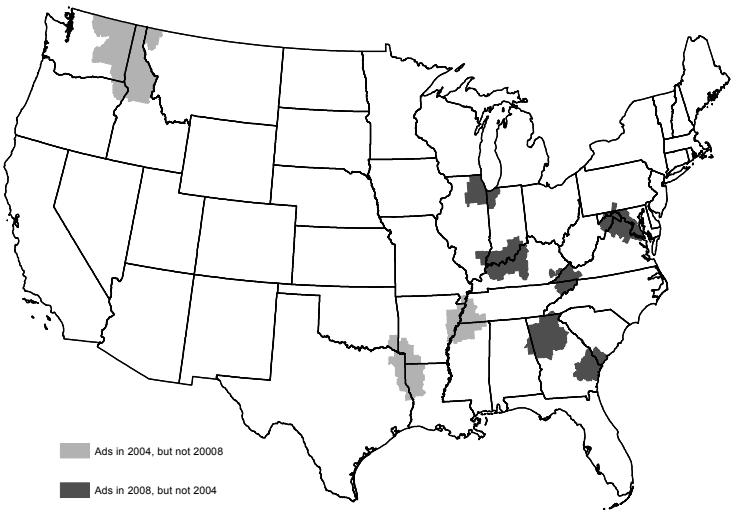
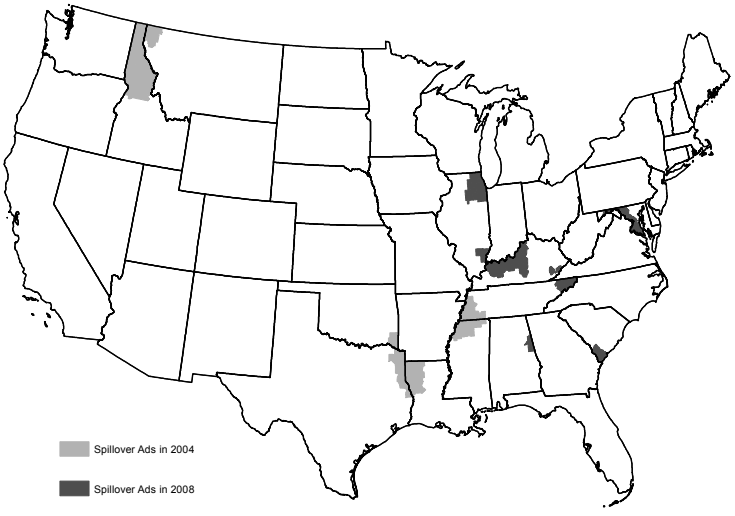



Figure 4: Spillover Zip Codes Used to Identify the 2004/2008 Panel



# 12 Appendix B

Table 10: Fixed Effects Logit Results used for Propensity Score Matching

| | (1) Treatment | (2) Treatment |
|---|---|---|
| Income | 0.00462** | 0.00411** |
| | (0.00207) | (0.00208) |
| Percent Hispanic | 1.090*** | 0.739*** |
| | (0.245) | (0.261) |
| Percent African American | -0.540*** | -0.707*** |
| | (0.157) | (0.163) |
| Density | 8.76E-6* | 1.38E-6 |
| | (5.25E-6) | (6.01E-6) |
| Percent of College Grads | 0.00563** | 0.00331 |
| | (0.00243) | (0.00249) |
| Population | | 0.0107*** |
| | | (0.00221) |
| *N* | 20,141 | 20,141 |

Standard errors in parentheses; Standardized beta coefficients

State fixed effects included

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Observations from states with no variation in treatment and control are dropped.

Table 11: Propensity Score Matching-Balance Report

| | Mean Treated | Mean Control | P-Value |
|---|---|---|---|
| Income (thousands) | 44.301 | 40.751 | 0.137 |
| Population (thousands) | 7.5715 | 7.7295 | 0.454 |
| Density (people per sq mile) | 699.47 | 875.42 | 0.42 |
| Percent Hispanic | 6.19 | 7.66 | 0.096 |
| Percent Black | 5.0 | 5.6 | 0.052 |

Only observations on the support are included.

This report is for the nearest neighbor specification.

Table 12: Robustness of Propensity Score Matching

| **Method** | **Effect Size** | **Standard Error** | **Off Support** |
|---|---|---|---|
| **Logit Specification 1** | | | |
| **Nearest Neighbor** | | | |
| N=20 | 7.324*** | (1.826) | 0 |
| N=10 | 7.280*** | (1.828) | 0 |
| N=1 | 6.509*** | ( 1.982) | 0 |
| **Radius Caliper** | | | |
| $\sigma = 0.01$ | 6.509*** | ( 1.982) | 0 |
| $\sigma = 0.001$ | 6.554** | (1.981) | 2 |
| $\sigma = 0.0001$ | 6.443*** | (1.874) | 54 |
| **Kernel** | | | |
| Epanechnikov | 7.380*** | (1.790) | 0 |
| Normal | 7.213*** | (1.803) | 0 |
| **Local Linear Regression** | | | |
| Epanechnikov | 6.117*** | (1.771) | 0 |
| **Logit Specification 2** | | | |
| **Nearest Neighbor** | | | |
| N=20 | 6.516*** | (1.844) | 0 |
| N=10 | 6.371*** | (1.814) | 0 |
| N=1 | 5.771*** | (2.217) | 0 |
| **Radius Caliper** | | | |
| $\sigma = 0.01$ | 5.771*** | (2.217) | 0 |
| $\sigma = 0.001$ | 6.038** | (2.166) | 4 |
| $\sigma = 0.0001$ | 4.455*** | (2.079) | 66 |
| **Kernel** | | | |
| Epanechnikov | 6.185*** | (1.810) | 0 |
| Normal | 6.484*** | (1.801) | 0 |
| **Local Linear Regression** | | | |
| Epanechnikov | 5.015*** | (1.793) | 0 |

Table 13: Representativeness of the 2004, 2008 Panel Sample

| | Observations | Mean | Std Err. | P-val |
|---|---|---|---|---|
| **Median HH Inc** | | | | |
| Full Sample | 18,128 | 39,493.1 | 124.95 | |
| Panel Sample | 2,094 | 45,132.53 | 437.59 | 0.00 |
| **Percent Over 65** | | | | |
| Full Sample | 18,128 | 0.142 | 0.00052 | |
| Panel Sample | 2,094 | 0.129 | 0.00156 | 0.00 |
| **Population** | | | | |
| Full Sample | 18,128 | 8655.783 | 96.486 | |
| Panel Sample | 2,094 | 13186.07 | 352.37 | 0.00 |
| **Percent White** | | | | |
| Full Sample | 18,128 | 0.845 | 0.0015 | |
| Panel Sample | 2,094 | 0.754 | 0.0057 | 0.00 |
| **Percent Black** | | | | |
| Full Sample | 18,128 | 0.070 | 0.0011 | |
| Panel Sample | 2,094 | 0.156 | 0.0051 | 0.00 |
| **Percent Hispanic** | | | | |
| Full Sample | 18,128 | 0.072 | 0.0011 | |
| Panel Sample | 2,094 | 0.055 | 0.0021 | 0.00 |
| **Density** | | | | |
| Full Sample | 18,128 | 1,213.99 | 37.83 | |
| Panel Sample | 2,094 | 1,682.391 | 77.104 | 0.00 |

Full Sample is based on all Non-competitive zips from 2008.

Panel Sample contains all the zip codes in the 2004, 2008 panel.

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