The Al Gore Effect: An Inconvenient Truth and Voluntary Carbon Offsets*

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Abstract

This paper examines the relationship between climate change awareness and household behavior by testing whether Al Gore's documentary *An Inconvenient Truth* caused an increase in the purchase of voluntary carbon offsets. I find that in the two months following the film's release, zip codes within a 10-mile radius of a zip code where the film was shown experienced a 50 percent relative increase in the purchase of voluntary carbon offsets. During other times, offset purchasing patterns for zip codes inside the 10-mile radius were similar to the patterns of zip codes outside the 10-mile radius. There is, however, little evidence that individuals who purchased an offset due to the film purchased them again a year later.

Keywords: climate change, voluntary carbon offsets, Al Gore, An Inconvenient Truth, awareness campaign

1 Introduction

Awareness campaigns that promote behavioral change exist across a wide spectrum of concerns, including health (e.g., National Breast Cancer Awareness Month events that encourage screening), political engagement (e.g., MTV's Rock the Vote program that facilitates voter registration and promotes voter turn-out), humanitarianism (e.g., the (RED) campaign that appeals to consumers to purchase products from companies that donate resources to HIV/AIDS programs in Africa), and environmental conservation (e.g., alert programs used by local municipalities to inform citizens of droughts and to ask them to voluntarily conserve water). These programs are appealing to governments and non-profit organizations because they have the potential to improve social welfare. Awareness campaigns generally provide information that is either aimed at helping individuals make better decisions of primarily private consequence, such as undergoing breast cancer screening, or aimed at persuading individuals to voluntarily limit consumption that creates negative social externalities, such as watering a lawn during a drought. If these programs are effective at changing behavior and the cost of the programs are sufficiently low, then awareness campaigns offer a potentially cost-effective way to achieve gains in social welfare.

Awareness campaigns are particularly relevant to the topic of global climate change. Despite the large body of evidence that shows anthropogenic climate change is occurring and is likely to be highly costly [16], fewer than 50 percent of Americans believe there is "solid evidence that the Earth is warming due to climate change" [27]. Recognizing this discrepancy, many governments and non-governmental organizations have supported or undertaken efforts to raise awareness. The publications of reports by the International Panel on Climate Change [16] and the Stern Review [30] were, in part, efforts to raise awareness; the United Nations' Climate Change Outreach Programme provided "governments additional tools for promoting climate change awareness at the national level" [33]; and the World Wildlife Fund's "Earth Hour", which occurs annually, involved more than 370 cities and 50 million individuals symbolically turning off non-essential lights and appliances for one hour at 8pm on April 1st, 2008 [34].

A unique awareness campaign related to global climate change has been spearheaded by former U.S. Vice President Al Gore. In addition to giving in-person speeches and pre-

sentations about the dangers of climate change, Gore starred in the 2006 documentary An Inconvenient Truth, which aimed to convince individuals to take action to reduce climate change. In 2007, Gore shared the Nobel Peace Prize with the IPCC for being "the single individual who has done the most to create greater worldwide understanding of the measures that need to be adopted [to counteract Climate Change]" [31].

Despite the widespread use of awareness campaigns, both with regard to climate change and other issues, it is unclear how often these campaigns are effective at changing behavior. While there have been a number of public health studies evaluating the effect of awareness campaigns, these studies tend to use survey data and to focus on how individual risk-perception changed [21], or how many people were reached by an awareness campaign [6], rather than measuring actual changes in behavior. There are two primary reasons why a behavioral change may not occur. First, the increased level of risk perception that is brought on by awareness campaigns may not by itself be sufficient to convince individuals to adopt new practices, especially in the case of risks related to public goods, where individuals have incentives to free ride. Second, awareness efforts may be most likely to reach those individuals who were already among the most informed about the issue. A recent Time Magazine article, "Can a Film Change the World?," asked this very question about An Inconvenient Truth, commenting that: "It has to be noted that the people who saw [An Inconvenient Truth] already cared enough to spend leisure time watching a lecture about melting polar ice caps. It's not clear minds were changed. The converted saw the film and worried more; the rest went to Pirates of the Caribbean: Dead Man's Chest" [19].

In this paper, I am able to test whether or not an awareness campaign created a change in behavior. I examine the impact of An Inconvenient Truth using a differences-in-differences research design that exploits spatial variation in the film's release to theaters. The measure of behavior change is the purchase of voluntary carbon offsets, a financial contribution that supports projects aimed at reducing carbon emissions. I find that in the two months following the release of the film, zip codes within a 10-mile radius of a zip code where the film was shown experienced a 50 percent relative increase in the purchase of voluntary carbon offsets. During times other than those two months, zip codes inside the 10-mile radius had similar offset purchasing patterns as zip codes outside the 10-mile radius. Econometric estimates

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are robust to a variety of specifications. I conclude that, at least in the short-term, An Inconvenient Truth caused individuals to purchase voluntary carbon offsets. In the longer-term, the available evidence suggests that individuals who purchased an offset due to the film did not purchase them again a year later.

2   Related Literature

There is a large body of research on the determinants of the private provision of public goods, and there have been large numbers of awareness campaigns seeking to increase such provision. Yet despite the prevalence of these campaigns, the literature on the relationship between awareness campaigns and public good provision is quite limited.[1] Two notable studies in the existing literature are Reiss and White [28] and Cutter and Neidell [7]. Reiss and White find that energy consumption dropped in San Diego during the 2001 California energy crisis after state agencies and utilities made public appeals for energy conservation. Cutter and Neidell show that traffic volume decreases in San Francisco on high-ozone days when the air quality management district issues "Spare the Air" announcements that urge residents to avoid driving. This paper provides additional evidence on the relationship between awareness campaigns and the private provision of public goods and provides some of the first evidence on awareness campaigns explicitly related to climate change.

Also related to this paper are studies that use contingent valuation methodology to examine the relationship between information and willingness to pay for a good that improves environmental quality. Ajzen et al. [1] find that willingness to pay for a public good increases with the quality of the arguments used to describe the good and Bergstrom et al. [3] show that willingness-to-pay for wetlands protection increases when more information is provided on the recreational services provided by wetlands, such as hunting and fishing. This paper provides field data that supports these earlier findings, which were based on hypothetical decisions.

Lastly, this paper contributes to the growing economics literature on the impact of mass media on behavior. For example, DellaVigna and Kaplan [9] show that the introduction of the Fox News Channel into a cable network increases the Republican vote share of the local area. Other research related to media and behavior has examined the relationship between

violent films and violent behavior [8], television and children's academic performance [12], public exposure of corrupt officials and election outcomes [10], and cable television access and women's status in India [18]. This paper examines the relationship of media and behavior in the context of climate change.[2]

3   Background on AIT and Voluntary Carbon Offsets

An Inconvenient Truth (AIT) was the centerpiece of Al Gore's campaign to increase recognition of the existence and consequences of climate change. AIT opened nationally on June 2, 2006.[3] The film was shown in more than one thousand theaters in the United States. AIT's domestic gross sales were approximately $12 million in June, $8 million in July, $1.5 million in August, and $1 million in September.[4] The film became the fourth highest grossing documentary of all time and won the Academy Award for best documentary of the year.

The majority of the film focuses on establishing the existence of climate change and explaining its potential consequences. The conclusion of the film is a general call to action. Al Gore encourages the audience to take action to mitigate climate change, asking the audience "Are we as Americans capable of doing great things even though they are difficult?" The only specific suggestions for actions were shown as the movie credits ran, and these suggestions included: change a light, drive less, recycle more, check your tires, use less hot water, avoid products with a lot of packaging, adjust your thermostat, plant a tree, turn off electronic appliances, spread the word. In general, the primary goal of AIT was to encourage individuals to adopt actions that will contribute to mitigating climate change.

This paper provides a test for whether AIT was effective at achieving this goal by examining one specific behavior that is consistent with the general call-to-action put forth by the film and for which a change in behavior is likely to be empirically detectable: the purchase of voluntary carbon offsets. An offset allows individuals or groups to mitigate climate change by making a financial contribution to offset their own carbon emissions. In exchange for this contribution, carbon offset suppliers invest in projects to reduce carbon emissions, such as renewable energy and reforestation projects. Carbon offsets are an increasingly popular consumer option. In 2007, projects supported by voluntary carbon offsets accounted for around 10.2 million metric tons of carbon.[5] Due to the growth of the voluntary carbon offset

market, offsets have recently received substantial attention from academic researchers, the popular press, and government agencies [11, 13, 20].

In particular, this paper examines the effect of the film on carbon offsets purchased through Carbonfund.org (henceforth Carbonfund), which is a retailer in the carbon offset market.[6] Two aspects of Carbonfund are notable. First, Carbonfund's offsets are typically for yearly amounts. For example, a customer can become a "DirectCarbon" individual for one year by making a contribution of $100. In exchange for this contribution, Carbonfund provides financial support toward projects that will offset carbon emissions by the estimated amount of emissions directly attributable to a representative customer during one year. Other options include offsets for one year of driving or one year of home energy use.[7] The annual term implies that the effect of the film, if it exists, may only be observable shortly after its release, and potentially one year later, because individuals who purchase an annual offset are unlikely to purchase another offset for at least a year. This issue is discussed further in the next section. Second, and importantly, Carbonfund is a non-profit organization and does not do any localized paid advertising, thus the analysis is unlikely to confound a demand driven response with a supply driven response from Carbonfund.[8]

Voluntary carbon offsets are just one example of the type of action encouraged by the documentary. Other possible behaviors that could have been influenced include the purchase of hybrid vehicles, CFL light bulbs, and energy efficient appliances, the use of public transportation, and the consumption of residential electricity. It would be ideal to examine whether or not the film had an effect on these behaviors as well. Unfortunately, there are two practical challenges with examining the possibility of an effect on these other behaviors. The first challenge is data availability. Because of the broad release of the film and its release during the middle of a calendar year, the empirical approach used in this paper requires that any dataset be available at both a fine spatial and fine temporal resolution. These types of datasets are not readily available. The second challenge is statistical power. The average effect of the film on many behaviors, if it exists, will be small because climate change awareness plays a small role in most consumer decisions and because only a subsample of the population went to see AIT in theaters. Consider residential electricity consumption. Because many factors affect residential electricity demand, it might be unrealistic to expect

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consumers to reduce consumption by more than 10 percent. If only 2 percent of individuals in a zip code saw the film,[9] then the average effect of the film in that zip code would be .2 percent. Because there is substantial noise in residential electricity estimation, an effect of this size is unlikely to be statistically significant and thus an empirical test is unlikely to lead to definitive conclusions.[10] For carbon offsets, on the other hand, one of the primary determinants of consumer willingness to pay is concern about climate change. As such, the film could plausibly have had a large and statistically identifiable effect on offset sales. This feature of carbon offsets makes them a desirable behavior in which to test for an effect of the film.

4 Data

This study uses two unique sources of data. The first dataset is a list of all 1,389 U.S. zip codes where AIT appeared in a theater. This information was provided by Paramount Vantage, the film's distributor. The second set of data is a record of 12,902 carbon offsets that were purchased from Carbonfund during the period from March 2006 through March 2008. These data were provided by Carbonfund and represent Carbonfund's entire history of providing offsets, with the exceptions that are described in the online Appendix.[11] Each record includes the date that the offset was purchased, the dollar amount of the offset, and the zip code of the individual who purchased it.

I use the list of zip codes, U.S. Census Zip Code Tabulation Areas (ZCTAs) shape files, and GIS software to compute the distance between each United States zip code and the nearest zip code that showed AIT.[12] The computed distance measure is the "great-circle distance" between each zip code's centroid and the centroid of the nearest zip code where AIT was shown. A great-circle mile is equivalent to a mile "as the crow flies."[13] A map of locations where AIT was shown is displayed in Figure 1. The distribution of the film was nationwide, but there is variation in distance to AIT within most regions; every state contains at least one zip code with a distance of more than 20 great-circle miles to a zip code where the film was shown.

[FIGURE 1 HERE]

I convert the carbon offset records into panel data that represents all of the 3,917 zip codes with at least one offset purchase on record. The dataset includes an observation for each zip code in each of the 25 months in the sample period and therefore consists of 97,925 total observations.[14] The variable *offsets* reports the number of carbon offsets that were purchased in a given zip code during a given month and the variable *amount* reports the total dollar amount of these offsets. I then add GIS-calculated data on each zip code's distance to a zip code where the film was shown to this panel, as well as ZCTA level demographic variables from the 2000 U.S. Census.

I generate two additional variables. The variable *Close* is a time-invariant binary variable indicating whether or not a zip code's distance to the film was less than 10 miles. Zip codes within 10 miles serve as the "treatment group" in the analysis. The important attribute of *Close* is that it designates a group that had relatively easier access to AIT; if the film led people to purchase carbon offsets, then an increase in offsets should be detectable in areas with easier access to the film. This differential impact can be used to test whether AIT led individuals to purchase carbon offsets. I examine other treatment group definitions in the analysis. In one definition, the zip codes outside of 10 miles remain the control zip codes, but the zip codes inside of 10 miles are split into two different 5-mile treatment categories. A second definition designates zip codes outside of 20 miles as control zip codes and splits zip codes inside of 20 miles into four different 5-mile treatment categories.

I define a second variable *TreatmentPd* that is a binary variable indicating whether or not the month is June or July 2006. This period covers the time when 82 percent of AIT's domestic gross theater sales were made. The effect of AIT on offset purchases, if it exists, should start when the movie enters theaters. To the extent the effect of the movie depreciates over time, one would expect to observe fewer offset purchases later. Implicit in the two-month definition of the treatment period is the assumption that any individual who was convinced by AIT to purchase an offset did so within two months of the film's release. While the effect of the film may have carried on longer, it should be most apparent in these two months.[15] The primary estimation results are based on the two-month definition of the treatment period, but I test for an impact three and four months after the film as well. If the effect of the film was long-lasting, then there is potential that individuals who purchased

an annual offset because of the film would purchase an offset again one year later. If so, then an effect would be observed in June and July 2007 as well. I examine this possibility in the analysis.

The data are most clearly presented in a graph, which is included in the next section, but some summary statistics are reported here: 77 percent of zip codes are *Close*, the mean number of offsets per month is .131 and the greatest number of offsets purchased in a zip code during any one month is 35.[16] Treatment zip codes purchase more offsets than control zip codes; the mean number of offsets in a month is .149 for treatment zip codes, as opposed to .067 for control zip codes. The difference, however, is much smaller in per capita terms. The mean number of offsets per month per 1000 people is 5.7 in treatment zip codes and 4.2 in control zip codes. The average dollar amount of an offset is $103.4, and the mean of *amount*, which equals zero in months when no offsets were made, is $13.62.

5   Methods and Results

I estimate the impact of AIT on offsets using a differences-in-differences identification strategy. This approach estimates the impact of AIT by examining whether zip codes that were close to where the film was shown experienced an increase in the purchase offsets in the two months after the film was released, relative to the change that occurred during the same two months in zip codes that were not close to where the film was shown. I first present a graph of the data and then present estimation results. Because the total dollar amount donated can be heavily influenced by idiosyncratic outlying large offsets, I primarily focus the analysis on the number of offsets (i.e. the number of offset transactions). In general, results that use *amount* as the dependent variable produce similar results, and I report these results for the primary analysis. The similarity of the results is not surprising considering that average offset size is similar between treatment zip codes and control zip codes. The average offset amount is $104.66 and $99.03 for treatment and control zip codes, respectively.[17]

5.1 Graph

Figure 2 plots the the natural log of the number of offsets made across time. The thin solid line represents all offsets that were made in treatment zip codes (zip codes where *Close* equals 1) the dashed line represents all offsets that were made within control zip codes (zip codes where *Close* equals 0) and the thick solid line with markers represents the difference between these two groups across time. Additionally, the vertical dotted line in the graph corresponds to the release of AIT in June 2006 and the horizontal dotted line indicates the mean difference between the two groups of zip codes during months outside of the treatment period, which was 2.003 log points. The thin solid line and the dashed line show that treatment zip codes made more offsets than control zip codes in all periods and the purchase of offsets in both groups was generally increasing over time, with large spikes in December months when offsets were likely purchased as holiday gifts or for tax reasons. Examining the difference between the two groups across time, the graph shows that leading up to the film's release the difference between the two groups was similar the level typically observed outside of the treatment period. Treatment zip codes then experienced an increase in offsets at the time of the film's release that was not experienced by the control zip codes. Three months after the film's release, the difference between the two groups returned to the level typically observed outside of the treatment period. The relative increase in offset purchases in treatment zip codes at the time of the film's release provides initial evidence that AIT led to an increase in offsets.[18]

[FIGURE 2 HERE]

5.2 Estimation

I first estimate the effect of AIT using a linear regression model and aggregated data. The aggregated data consist of two observations per month: one observation is an aggregation of all zip codes with a distance of less than 10 miles from an AIT theater and one is an aggregation of all zip codes further away. This analysis has the benefit of being highly transparent; each observation corresponds to a point in Figure 2. I employ the following

conventional differences-in-difference specification

$$ln(\text{offsets per capita}_{it}) = \alpha + \gamma_1 Close_i + \gamma_2 \text{Close x TreatmentPd}_{it} + \omega_t + \epsilon_{it},$$

where ω_t is a vector of fixed effects for each month in the sample and ϵ_{it} is a normally distributed error term.[19]

Next, I estimate the effect of AIT on offsets using the disaggregated data that includes an observation for each zip code in each month. I estimate a fixed effects negative binomial model as developed by Hausman et al. [15].[20] Specifically, I assume the number of offsets within a zip code and a month, offsets$_{it}$, follows a negative binomial distribution with parameters $\theta_i \lambda_{it}$ and ϕ_i, where θ_i is a zip-code-specific fixed effect and ϕ_i is a zip-code-specific overdispersion parameter [5]. I make the conventional assumption that λ_{it} has an exponential form, thus the mean of offsets$_{it}$ is given by

$$E(\text{offsets}_{it}) = \theta_i \lambda_{it} = \theta_i \exp(\beta \text{Close x TreatmentPd}_{it} + ln(\text{population}_i) + \omega_t),$$

where ω_t is a vector of fixed effects for each month in the sample. Population size acts as an exposure variable and the coefficient on $ln(\text{population}_i)$ is constrained to one. The conditional joint density based on this specification conditions on the total number of offsets within each zip code over the sample period. Hausman et al. [15] report that this conditional approach cancels out the zip code fixed effects, θ_i, allowing for estimation of the parameter of interest, β, without concern for "incidental parameters" bias.

Allison and Waterman [2] note that the fixed effects negative binomial model lacks some of the properties of a traditional fixed-effects model, in that the model is able to estimate a coefficient for stable covariates.[21] Based on simulation evidence, Allison and Waterman recommend a traditional unconditional negative binomial model with dummy variables included to represent fixed effects. To examine the possibility of econometric sources of bias, I estimate both a fixed effects and an unconditional negative binomial. Due to the computational difficulties of estimating an unconditional negative binomial with 3,917 dummy variables, I estimate a specification that includes a single binary variable that indicates whether or not the zip code was a treatment zip code. The coefficient on this variable, Close$_{it}$, represents

an average fixed effect for treatment zip codes. This issue is discussed further in the next subsection.

Bertrand et al. [4] show that differences-in-differences estimation can overstate the precision of the standard errors. In this case, there is potential for both serial and spatial correlation. To adjust for potential correlation in the error terms, I compute the standard errors in each estimation using a block bootstrap [17], where each state is a block. All tables report block bootstrapped standard errors. The bootstrap only increases the standard errors slightly relative to the Huber-White robust standard errors.[22]

5.3   Estimation results

The primary estimation results are reported in Table I. I present all results in the form of exponentiated coefficients minus one. The results displayed in the table can be interpreted as the percentage increase in offsets associated with a one unit increase the corresponding right-hand-side variable.

The OLS results using the aggregated data, which are reported in column 1, indicate that the film led to a 49.5 percent increase in offsets in treatment zip codes during the first two months of its release. Results from the disaggregated data are similar the results of the aggregated data; AIT led to about a 50 percent increase in offsets in treatment zip codes. The fixed effects negative binomial model estimates are reported in column 2 and indicate that AIT led to a statistically significant 45.0 percent increase in offsets. The negative binomial estimates are reported in column 3 and indicate the film led to a 62.8 percent increase in offsets. The similarity of the estimates to each other and to the results from the aggregated data suggest that potential econometric sources of bias are not problematic in this case.[23] The remainder of this paper discusses estimates from the fixed effects negative binomial, because those results are weakest in statistical significance, but the results from the unconditional negative binomial are similar and are reported in Figure 4 and Tables V and VI in the online Appendix.

In column 4 in Table I, I present the results of OLS estimation that uses the aggregated data and uses the total dollar amount of the offset(s) as the dependent variable, as opposed to the number of transactions. The results indicate that the film led to a 55.4 percent

increase in the total dollar amount of offsets, which is similar to the percentage increase in the number of transactions.

[TABLE I HERE]

5.4   Examining other time periods

The estimations in Table I only test for an effect of the film two months after the film's release and use other periods as control periods. In this section, I examine the difference between the groups during other periods. Specifically, I examine whether the initial effect of the film persisted for longer than two months, whether the film simply led individuals to purchase offsets sooner, rather than to purchase additional offsets, and whether individuals who purchased an offset due to the film purchased them again a year later. To examine these possibilities, I use a more flexible estimation strategy that replaces *Close* x *TreatmentPd* with a set of sixteen interaction terms, consisting of an interaction of *Close* and each of the sixteen months from April 2006 through July 2007. The coefficients on each interaction term indicate how the treatment and control groups differed during the corresponding month relative to how the two groups differed on average during March 2006 and August 2007 to March 2008, which can be considered the control period in this regression.

The estimates are displayed in Figure 3. The estimates indicate that the film led to an increase in the purchase of offsets in treatment zip codes in the first two months after the film's release. There is some evidence that the film had an effect, albeit a lesser one, three and four months after its release, but this evidence is weaker in terms of statistical significance. By October 2006, five months after the film's release, the effect of the film appears to have declined to close to zero. In sum, the figure suggests that the film caused individuals to purchase offsets, that most individuals who made offsets due to viewing the film in theaters did so within the first two months of its release, and that almost all individuals who made an offset due to viewing the film in theaters did so within four months of its release.

The figure also allows for an examination of the possibility that the film led individuals to purchase offsets sooner, but did not lead to additional offsets. If the film only led individuals to purchase their offsets sooner, then there should be a relative decrease in the purchase of offsets in treatment zip codes in the months following July 2006. However, the coefficients

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on the interaction terms from August 2006 through February 2007 are small in magnitude and statistically insignificant, and the average coefficient across these months is close to zero. The graph supports the interpretation that the film led to the purchase of additional offsets, as opposed to a change in the timing of offset.

Lastly, the figure allows for examination of whether individuals who purchased offsets due to the film did so again a year later. If individuals who were convinced by AIT to purchase an offset bought them again after the original offsets expired one year later, then we would expect the treatment groups to experience a relative increase again one year after the original effect was observed. However, the coefficient on the interaction terms for June and July 2007 are small in magnitude and statistically insignificant, and their average value is close to zero. This result suggests that individuals who initially purchased an offset due to the film did not do so again one year later.[24] It should be noted that while Carbonfund sends frequent emails to their customers, they do not send a specific reminder email that indicates it has been one-year since the purchase of their previous offset. While the lack of a reminder email does not change the fact that the impact of the film appears to have been short-lived, it does raise the question of whether a more persistent effect would have been observed had a reminder notice been sent. It would be interesting to examine in future research whether reminder emails are an effective way to increase the purchase of offsets and whether these emails increase the persistence of awareness campaigns.

[FIGURE 3 HERE]

5.5   Examining alternative distance stratifications

In this section, I examine whether the inverse relationship between distance to a zip code where the film was shown and the purchase of offsets is robust to alternative categorization of zip codes. First, I estimate a specification that splits the original group of treatment zip codes into two different groups; one group consists of all zip codes with a distance of less than five miles and the other group consists of zip codes with a distance between five and ten miles. In this specification, the estimated effect of film remains the effect relative to zip codes that were greater than ten miles away. In a second specification, I split the group of zip codes with a distance of less than twenty miles into four groups, with each

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group corresponding to a certain five-mile band. In this specification the estimated effect is relative to zip codes that were more than twenty miles away.

Results from the two estimations are shown in Table II. Most of the estimates are not statistically significantly different from each other. However, the pattern in the point estimates provides further evidence of an inverse relationship between distance to the film and the purchase of offsets. In the first specification, the estimated effect of the film is larger in zip codes with a distance of less than five miles than in zip codes between five and ten miles, and both zip codes experienced an increase in offsets relative to zip codes with a distance of more than ten miles. In the second specification, the estimated effect of the film monotonically decreases as distance from the film increases, up until distance exceeds fifteen miles. One interpretation of this result is that once distance exceeds approximately fifteen miles, which corresponds to about a one-hour round-trip car ride to the see the film, then distance becomes prohibitive for potential moviegoers and additional distance does not change the likelihood of viewing a film.

[TABLE II HERE]

5.6 The identification assumption and robustness checks

A common trends condition is necessary for the consistency of a differences-in-differences estimator [25]. In this study, consistency of the estimator requires that treatment zip codes and control zip codes would have experienced similar proportional changes in offsets during June and July 2006 absent the release of AIT. One way to investigate the validity of this identification assumption is to return to the graph of the data. The identification assumption would be questionable if treatment zip codes and control zip codes appeared to be following different time trends either before the film's release or substantially after the film left theaters. Figure 2 indicates this is not the case; treatment and control zip codes followed very similar trends both before AIT's release and several months after AIT's release. Given this evidence, identification requires the modest assumption that there were no shocks to treatment zip codes, other than AIT, that both occurred at the exact month of AIT's release and that ended at the same time that AIT was leaving theaters.

To further validate the causal relationship between AIT and the purchase of offsets, I estimate two additional robustness checks. First, I relax the assumption that the difference between control and treatment zip codes is fixed across time and instead estimate a specification that allows for the difference between the treatment and control zip codes to diverge or converge linearly across months. This is accomplished by including an interaction of *Close* with a continuous variable, *month-year*, that indicates the month and year of the observation. Second, I estimate a specification that includes interactions between demographic variables and the treatment period. This specification controls for demographically correlated changes in offset provision during the treatment period that could potentially bias the earlier estimate, given the correlation between proximity to AIT and demographics. I also estimate a specification that combines both approaches.

Results of the robustness checks are reported in Table III. In all specifications, the estimated impact of the film, which ranges from .396 to .426, is comparable to the base estimation, and remains statistically significant.

[TABLE III HERE]

5.7   Additional discussion

Three topics deserve additional discussion. The first topic is external validity. Even though AIT is associated with a 50 percent increase in offsets in areas where it was released, there is no guarantee that this same 50 percent increase would have occurred had the film been released in other areas. For example, suppose the film was targeted at liberal markets where individuals are more likely to be concerned about climate change and thus were more likely to see the film and become convinced to purchase an offset.[25] Under such a scenario, the current 50-percent estimate would be an overestimate of the effect of the film had it been released in more conservative markets. An alternative hypothesis is that conservative viewers were the ones most likely to have their views substantially changed, and thus most likely to be convinced by the film to purchase an offset. Under this scenario, the current estimate would be an underestimate of the effect of the film had it been released in more conservative markets.

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One way to address these possibilities is to examine whether or not the estimated effect of the film differed in areas with more conservative voters. To this end, I acquired county-level data on voting patterns from the 2004 U.S. Presidential election [22] and merged it with the primary dataset. These data show that the release of the film was, in fact, significantly correlated with liberal vote share.[26] To evaluate whether or not this selection into liberal markets had an impact on the effect of the film, I re-estimate the base regression with the inclusion of *Close* x *TreatmentPd* x *Liberal Vote Share* and *TreatmentPd* x *Liberal Vote Share* as additional explanatory variables. The point estimate on *Close* x *TreatmentPd* x *Liberal Vote Share* indicates the effect was actually larger in more conservative zip codes. A 5 percentage point decrease in liberal vote share is associated with a 3.5 percentage point increase in the effect of the movie on offset purchases. The coefficient on the interaction term of interest, *Close* x *TreatmentPd* x *Liberal Vote Share*, is however, statistically insignificant, with a z-stat of -1.27. In sum, there is little evidence that the effect of the film would have been substantially different had it been targeted at a set of theaters in localities with different political tendencies.

The second topic deserving additional discussion is measurement error. Depending on a zip code's geometric shape, the location of its population, and the location of theaters within zip codes, a centroid-to-centroid distance measure may be a noisy indicator of actual distance to a theater for a zip code's resident. As such, estimates may be downward biased due to attenuation bias. This measurement error would become a concern only if it became large enough to change the classification of zip codes from treatment to control. Regardless, the estimates should perhaps be considered as lower bound-estimates.

The final issue of potential concern is whether or not the results change if every zip code is included in the analysis, as opposed to just those with an offset on record. I have focused the analysis on the dataset that includes only zip codes with an offset purchase on record primarily because the preferred econometric model, the fixed effects negative binomial, conditions on the total number offsets made in a zip code over the course of the sample, and thus zip codes with no offsets on drop out of the estimation. Similarly, in the aggregated data, the zip codes with no offsets on record fall out in the aggregation process, and the results are identical regardless of which disaggregated dataset is collapsed. The only part

of the analysis that could produce different results when the sample of all zip codes is used are the estimates from the unconditional negative binomial model. Accordingly, I report estimates from the unconditional negative binomial model using both samples in Tables V and VI in the online Appendix. The estimates are very similar regardless of which sample is used.

6   Conclusion

As the threat of climate change has become more apparent, governments and non-governmental organizations have sought to build public support for measures to address climate change. This had led to a number of efforts to raise climate change awareness, including the release of Al Gore's documentary *An Inconvenient Truth*. To date, little research has tested whether or not such awareness efforts are effective at changing behavior related to climate change. More generally, few studies have cleanly tested whether awareness campaigns are effective at changing behavior related to private provision of a public good.

This paper examines the relationship between climate change awareness and household behavior by testing for a causal relationship between viewing An Inconvenient Truth and the purchase of voluntary carbon offsets. I exploit a natural experiment induced by the spatial variation in the film's release to theaters. I find that in the two months following the film's release, zip codes that were within a 10-mile radius of a zip code where the film appeared experienced a 50 percent relative increase in offsets relative to zip codes that were further away. A graph shows that the two groups had similar patterns in offset purchases outside of the time when the film was in theaters, and estimates are robust to a variety of specifications. However, I find little evidence that the effect persisted for more than one year. The two groups of zip codes did not have divergent offset purchasing patterns one year after the film's release as would be expected if individuals renewed the offsets that were purchased due to the film.

While the effect is large in percentage terms, it is worth nothing that the data used come from only one organization and that offsets are still infrequent purchases. Some calculations are useful both for converting the percentage increase into more meaningful units and for extrapolating the results into the type of increase that might have occurred across the en-

tire carbon offset sector. During the two months after AIT's release, Carbonfund sold 544 offsets and took in $44,500 in sales revenue in treatment zip codes. These numbers, and the percentage increases from the aggregated data results reported in Table I, indicate that the film led to 182 additional offsets, or $16,150 in additional offsets. Based on Carbonfund's pricing of $5.5 per ton of CO2, this translates to approximately 2,900 tons of CO2. A rough back-of-the envelope calculation, based on available data that indicates Carbonfund's share of the retail offset market was at most 2.5 percent at the time of the film's release [32] and the assumption that the increase that was experienced by Carbonfund was experienced by all retailers in the industry, suggests that the film led to 7,280 additional offsets purchases nationally, corresponding to $646,000, or 117,000 tons of CO2. To put this number in context, 117,000 tons of CO2 is roughly equivalent to the amount of carbon dioxide emissions produced by running 5,500 average U.S. households for one year. This effect alone seems quite small in light of the fact that there are over 100 million households in the United States.

Based on the above numbers, it is clear that in order for the film to have had an appreciable effect, the change in the offset market must have been indicative of an overall change in public opinion and behavior. At least some evidence suggests this is the case. According to the Pew Research Center for People & the Press [27], the number of Americans believing that the earth was warming due to human activity increased from 41 percent to 50 percent from June to July 2006, which was the period when the film was in theaters.[27] It seems plausible that at least some of this change was created by the film, and that this change in public opinion may have influenced other behaviors such as transportation decisions, household electricity consumption, or political support for climate change legislation. Unfortunately, it is difficult to rigorously test whether or not AIT had an effect on these behaviors because any effect of a reasonable size would be difficult to detect precisely given data constraints.

In this light, this paper should perhaps be viewed as a first step in understanding how awareness campaigns influence behavior related to climate change. The results in this paper strongly indicate that climate change awareness campaigns increase the willingness of individuals to purchase carbon offsets, which is not a mainstream behavior. Future research is needed to examine the effect of awareness campaigns on other behaviors. If climate change

awareness campaigns do have an effect on other behaviors, it would be particularly interesting to examine whether or not the effect is short-lived, as appears to be the case with offsets.

From an environmental policy perspective, awareness campaigns offer the potential to improve public welfare by inducing individuals to limit negative externalities associated with their consumption or inducing individuals to contribute directly to a public good. In certain situations, awareness campaigns may offer a way for policy-makers to increase the uptake of goods that reduce carbon emissions, such as fuel efficient cars or appliances, in a manner that is cheaper than implementing a subsidy or tax-credit program. This paper provides further evidence that awareness campaigns can influence household behavior. However, more research is still needed to examine the full potential of awareness campaigns on volunteer behavior, and to assess when awareness campaigns offer the most efficient tool by which to achieve a policy objective.

7 Appendix

7.1 Data Appendix

U.S. Census 2000 ZIP Code Tabulation Area (ZCTA) shape files from the U.S. Census website were used to compute the distance measures. The ZCTA shape files are the Census bureau's approximations of the areas falling under each U.S. Postal Service zip code. The GIS software used was ArcMAP$^{\text{TM}}$ Version 9.2, a component of ESRI's ArcGIS$^{\copyright}$ 9. The distance measure was originally computed in decimal degrees and then was converted to miles using the formula: Earth's radius $\times \pi \times$ distance in decimal degrees$/(180 \times 1600) =$ distance in miles.

The ZCTA shape files do not include areas for zip codes that served specific companies or organizations with high volumes of mail, for P.O. Boxes, for general delivery addresses primarily located in areas otherwise served by rural routes or city-style mail delivery, or for areas that were either inactive or insufficiently represented in the U.S. Census Bureau's Master Address File. Due to these omissions, some theater's zip codes do not appear in the ZCTA shape files. If a theater's zip code did not appear in the shape files then I replaced the theater's zip code with a neighboring zip code that did appear in the shape files. The replacement zip codes were identified by looking up other zip codes within the theater's town/city at the U.S. Postal Service web site. In total, the zip codes of 30 of 1,389 theaters were recoded. Similar to the theater data, some of the zip codes in the offset data were not represented in the ZCTA shape files. The zip codes for these offset were re-coded in the same manner as was employed for the theaters. In total, the zip codes of 182 of 12,902 offsets were re-coded.

Lastly, some offsets were not included in the analysis. While Carbonfund sold offsets starting in April 2005, I exclude month priors to March 2006 because Carbonfund had not sold more than 40 offsets in a month until that time, with the exception of the Christmas surge in December 2005. In March 2006, Carbonfund's offset sales increased to 158 offsets and Carbonfund's growth proceeded more steadily thereafter. The increase in Carbonfund's offsets in March 2006 is most likely the result of the general emergence of carbon offsets into mainstream culture. Google Trends does not show an appreciable search volume for

"carbon offset" until the second quarter of 2006, which corresponds to the time when Carbonfund's offset sales increased substantially. I also drop recurring offsets from individuals on automatic offset plans. Until November 2006, Carbonfund promoted some plans where payments were automatically deducted, generally on a monthly or quarterly basis; 7 percent of Carbonfund's customers were on these recurring plans. The data used in the analysis include only the first offset by individuals on automatic plans because these offsets most accurately represent changes in demand over time. Additionally, offsets that were made through Carbonfund's partnerships with Working Assets, Environmental Defense, National Wildlife Fund, Evangelical Environmental Network, or Calvert are not included. These offsets are logged by a separate database and are highly sensitive to the actions of the partner agencies. Since purchases from these links mostly occur as large one or two-day shocks following partner events, they are not representative of day-to-day demand. Lastly, the data does not include the large offsets that Carbonfund has made for major corporations, such as Volkswagen.

7.2   Appendix Tables

[TABLE IV HERE]

[FIGURE 4 HERE]

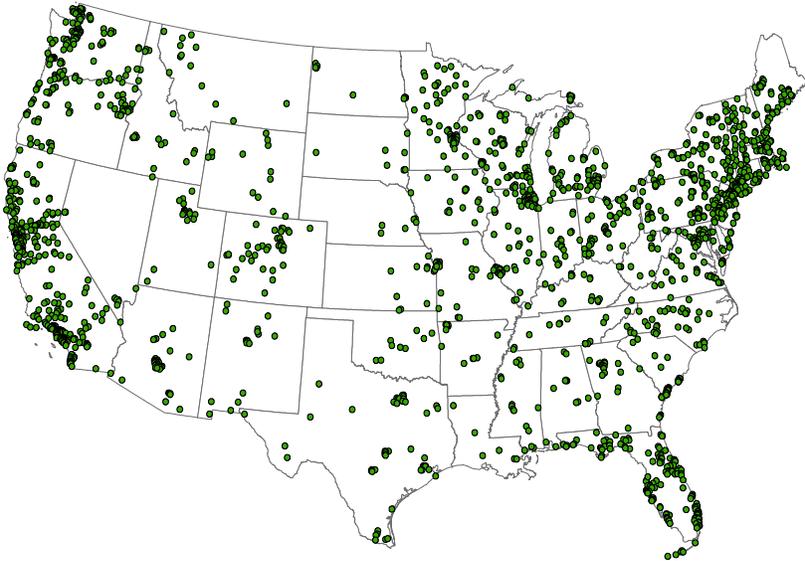[TABLE V HERE]

[TABLE VI HERE]

Figures and Tables

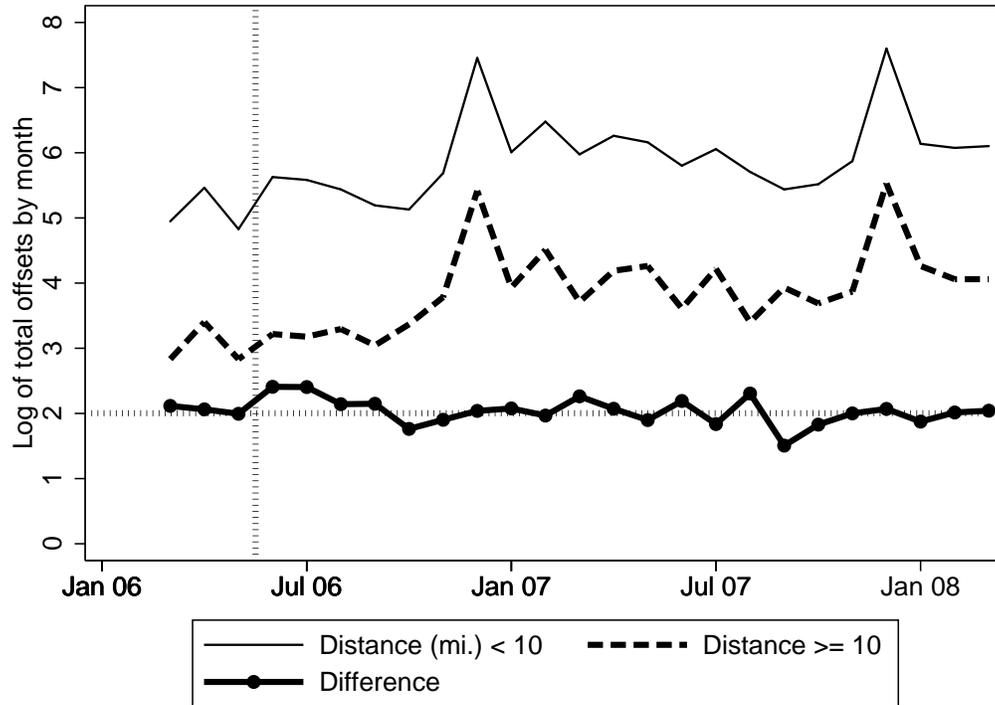Figure 1: Location of theaters that showed AIT in the continental U.S.

Figure 2: The thin solid line and the dashed line represents the natural log of the total number of offsets across time by proximity to AIT. The thick line with markers displays the difference between these two groups across time. The vertical dotted line corresponds to AIT's release into theaters in the beginning of June 2006. The horizontal dotted line corresponds to the average difference between treatment and control zip codes outside of the first two months when AIT was in theaters. Note that the greatest difference between the two groups occurs immediately after AIT's release into theaters.

Table I: Estimates of the effect of AIT on offset purchases

Dependent Var.:	ln(offsets)	offsets	offsets	ln(amount($))
Model:	OLS	NBFE	NB	OLS
	(1)	(2)	(3)	(4)
Close x TreatmentPd	0.495***	0.450**	0.628***	0.554***
	[10.826]	[2.346]	[2.835]	[4.647]
Close	0.300***		0.201*	0.385***
	[7.064]		[1.812]	[4.055]
Observations	50	97,925	97,925	50

Notes: Columns 1 and 4 use the aggregated data and columns 2 and 3 use the disaggregated data. All specifications include a fixed effect for each of the 25 months in the sample. T-stats are reported in brackets in columns 1 and 4 and z-stats are reported in brackets in columns 2 and 3. Z-stats are computed based on block bootstrap standard errors, where each state is a block. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance, respectively. R-Squared measures for columns 1 and 4 are .996 and .990, respectively. Pseudo R-Squared measures for columns 2 and 3 are .34 and .04, respectively.

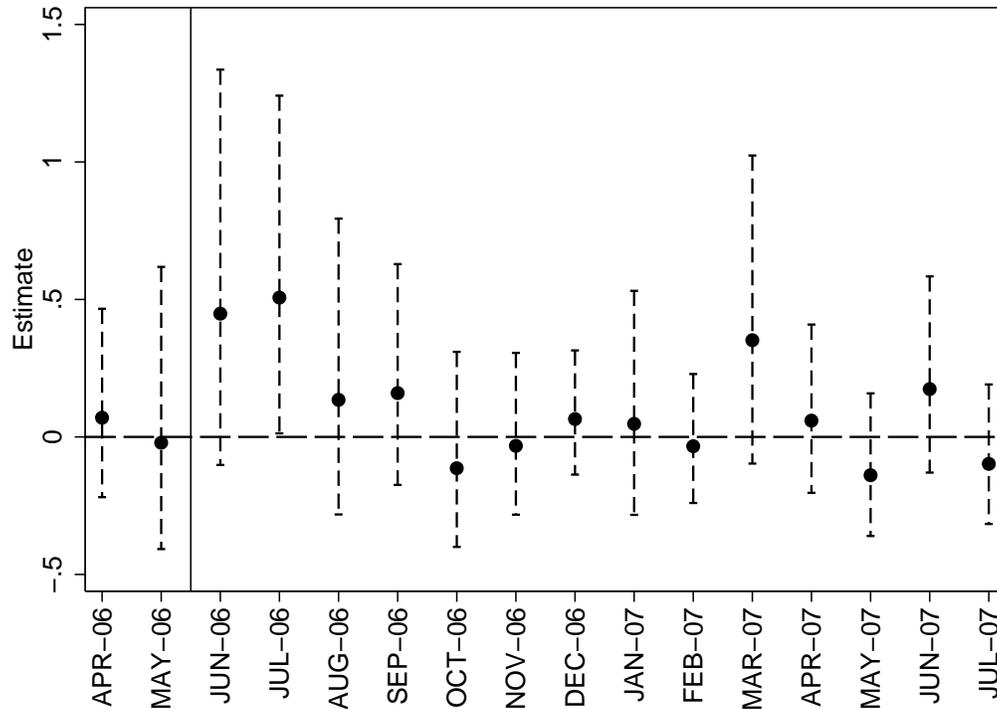Figure 3: This figure plots the estimates from a fixed effects negative binomial regression that includes 16 interaction terms, consisting of *Close* interacted with 16 months plotted above, and a vector of month-year dummies. The dashed bars display 95-percent confidence intervals. Confidence intervals are asymmetric because estimates are presented in the form of exponentiated coefficients minus one.

Table II: Examining alternative distance stratifications

	(1)	(2)
$(0 \leq \text{dist.} \leq 5)$ x TreatmentPd	0.501***	0.593
	[2.609]	[1.462]
$(5 < \text{dist.} \leq 10)$ x TreatmentPd	0.257	0.334
	[1.586]	[1.034]
$(10 < \text{dist.} \leq 15)$ x TreatmentPd		0.247
		[0.602]
$(15 < \text{dist.} \leq 20)$ x TreatmentPd		-0.262
		[-0.680]
Observations	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. The econometric model is a fixed effects negative binomial model. In column 1, the omitted group in the set of interaction variables is zip codes with a distance greater than 10 miles. In column 2, the omitted group in the set of interactions variables is zip codes with a distance greater than 20 miles. All specifications include a fixed effect for each of the 25 months in the sample. Z-stats are reported in brackets and are computed using block bootstrap standard errors, where each state is a block. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance, respectively. All pseudo R-squared measures are .34.

Table III: Additional robustness checks

	(1)	(2)	(3)
Close x TreatmentPd	0.429**	0.416***	0.396*
	[2.331]	[2.700]	[1.826]
Close x month-year	-0.001	0.006**	-0.001
	[-0.210]	[2.024]	[-0.237]
BA x TreatmentPd		-0.071**	0.006
		[-2.226]	[1.593]
Income x TreatmentPd		-0.001	-0.071**
		[-0.512]	[-2.161]
Pop. dens. x TreatmentPd			-0.001
			[-0.569]
Observations	97,925	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. The econometric model is a fixed effects negative binomial model. All specifications include a fixed effect for each of the 25 months in the sample. The variable month-year is a continuous variable indicating the month and year of the observation. BA reports the share of the age-25 or older population that has a bachelors degree, income reports a zip code's median income and is measured in units of $10,000, and population density is measured in units of population per 10,000 square meters. Z-stats are reported in brackets and are computed using block bootstrap standard errors, where each state is a block. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance, respectively. All pseudo R-squared measures are .34.

Table IV: Demographic comparisons

	Close		Not Close	
Variable	Mean	St. Dev.	Mean	St. Dev.
Median income (10000s)	3.661	1.588	3.189	1.060
Percent with BA	0.384	0.173	0.262	0.131
Pop. dens.	19.986	40.618	1.895	3.381

Notes: Close equals one for 3,029 zip codes and zero for 888 zip codes. The variable "Percent with BA" reports the share of the age-25 or older population that has a bachelor's degree. Population density is reported in units of population per 10,000 square meters.

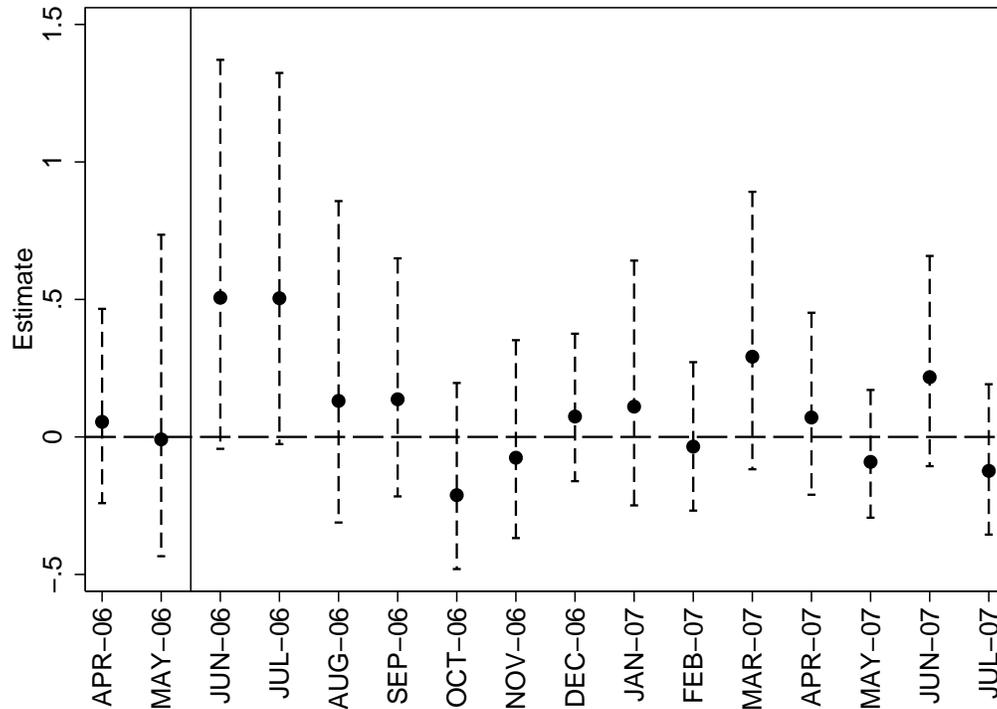Figure 4: This figure plots the estimates from a negative binomial regression that includes 16 interaction terms, consisting of *Close* interacted with 16 months plotted above, a vector of month-year dummies, and the variable *Close*. The dashed bars display 95-percent confidence intervals. Confidence intervals are asymmetric because estimates are presented in the form of exponentiated coefficients minus one.

Table V: Negative binomial results: Examining alternative distance stratifications

Sample:	Zips with an offset on record		All zips	
	(1)	(2)	(3)	(4)
(0 ≤ dist. ≤ 5) x TreatmentPd	0.692***	0.699	0.530**	0.569
	[3.010]	[1.395]	[2.572]	[1.288]
(5 < dist. ≤ 10) x TreatmentPd	0.370*	0.376	0.256	0.289
	[1.829]	[0.898]	[1.399]	[0.783]
(10 < dist. ≤ 15) x TreatmentPd		0.191		0.168
		[0.434]		[0.413]
(15 < dist. ≤ 20) x TreatmentPd		-0.374		-0.323
		[-0.920]		[-0.810]
(0 ≤ dist. ≤ 5)	0.352***	0.266*	5.153***	9.456***
	[2.760]	[1.892]	[13.597]	[14.552]
(5 < dist. ≤ 10)	-0.154**	-0.208**	1.360***	3.009***
	[-2.382]	[-2.029]	[9.455]	[10.663]
(10 < dist. ≤ 15)		-0.119		1.831***
		[-0.961]		[7.242]
(15 < dist. ≤ 20)		-0.025		1.131***
		[-0.191]		[5.309]
Observations	97,925	97,925	800,700	800,700

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. The econometric model is a negative binomial model. In column 1, the omitted group is zip codes with a distance greater than 10 miles. In column 2, the omitted group is zip codes with a distance greater than 20 miles. All specifications include a fixed effect for each of the 25 months in the sample. Z-stats are reported in brackets and are computed using block bootstrap standard errors, where each state is a block. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance, respectively. Pseudo R-Squared measures are .04 for columns 1 and 2 and .21 for columns 3 and 4.

Table VI: Negative binomial results: Additional robustness checks

Sample:	Zips with an offset on record			All zips			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Close x TreatmentPd	0.545**	0.591***	0.507**	0.478**	0.435**	0.537***	0.492**
	[2.563]	[2.960]	[2.510]	[2.411]	[2.238]	[2.753]	[2.449]
Close x month-year	-0.005		-0.005		-0.003		-0.003
	[-0.780]		[-0.829]		[-0.475]		[-0.488]
BA x TreatmentPd		0.006**	0.006**			0.003	0.003
		[2.194]	[2.194]			[1.043]	[1.044]
Income x TreatmentPd		-0.061***	-0.061***			-0.050**	-0.050**
		[-2.676]	[-2.675]			[-2.401]	[-2.401]
Pop. Dens. x TreatmentPd		-0.001	-0.001			-0.001	-0.001
		[-0.696]	[-0.696]			[-0.501]	[-0.501]
Close	0.287*	-0.253***	-0.198*	3.616***	3.803***	0.613***	0.678***
	[1.673]	[-3.771]	[-1.650]	[12.634]	[9.405]	[5.299]	[3.616]
BA		0.042***	0.042***			0.071***	0.071***
		[16.356]	[16.359]			[40.338]	[40.302]
Income		-0.153***	-0.153***			-0.199***	-0.199***
		[-8.009]	[-8.009]			[-7.712]	[-7.712]
Pop. Dens.		0.002**	0.002**			0.004**	0.004**
		[2.146]	[2.148]			[2.262]	[2.263]
Observations	97,925	97,925	97,925	800,700	800,700	800,700	800,700

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. The econometric model is a negative binomial model. All specifications include a fixed effect for each of the 25 months in the sample. The variable month-year is a continuous variable indicating the month and year of the observation. BA reports the share of the age-25 or older population that has a bachelors degree, income reports a zip code's median income and is measured in units of $10,000, and population density is measured in units of population per 10,000 square meters. Z-stats are reported in brackets and are computed using block bootstrap standard errors, where each state is a block. One, two, and three stars indicate 10 percent, 5 percent, and 1 percent significance, respectively. Pseudo R-squared measures are .07 for columns 1 through 3, .20 for columns 4 and 5, and .29 for columns 6 and 7.

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Notes

[1]A number of studies have looked at how awareness campaigns influence consumption of a *private* good. For example, Shimshack et al. [29] shows that consumers reduced fish consumption in response to FDA warnings about mercury content.

[2]Leiserowitz [23] also conducts a study related to the effect of media in the context of climate change. Based on survey data, Leiserowitz shows that people that saw the film "The Day After Tomorrow" were more likely to be concerned about the risks associated with climate change.

[3]AIT's official theatrical release date was May 23, 2006, but this release was limited to four theaters in Los Angeles and New York. AIT was released on DVD on November 21, 2006. The universal distribution of the DVD across the country prevents a clean test for an AIT DVD effect.

[4]Weekend box office information is publicly available at boxofficemojo.com.

[5]There is debate over the extent to which carbon offsets projects lead to actual emissions reduction [13]. The concern is that some projects supported by carbon offset providers may have occurred absent any support from the carbon offset provider. The fact that

offset purchasers at minimum believed that they were reducing emissions is sufficient to test whether AIT increased the willingness of individuals to take costly measures to reduce climate change. Additionally, the offsets under study in this paper were purchased through Carbonfund.org, an organization that has numerous quality assurance measures in place to ensure the legitimacy of their offsets.

[6]While it would be ideal to use market-wide data on carbon offsets, market-wide data do not exist at a fine enough spatial or temporal scale to test for an effect of the film.

[7]Carbonfund also currently offers event-specific offsets for flights and weddings. These options were not available at the time of the film's release.

[8]At the time of the film's release, Carbonfund's advertising consisted of Google "Ad-Words", which were not regionally targeted.

[9]AIT took in $24 million in domestic theater sales. Based on an average ticket price of $6.55 during 2006, the sales total implies 3.7 million viewers. The population of all treatment zip codes in aggregate was about 176 million, so 2 percent is an approximate viewing rate.

[10]Even though a .2 percent decrease is too small to test for statistically, it still might be considered an economically significant effect. The decrease would translate to an emissions reduction of about 10 Tg CO_2/year, or $200 million/year in avoided social costs (assuming a social cost of carbon of $20/ton).

[11]The Appendix is available at JEEM's online archive of supplementary material, which can be accessed at http://aere.org/journals/.

[12]The online Data Appendix contains details on this process.

[13]An approximate conversion rate of great-circle miles to driving distance and travel time may be useful for the reader. Liss et al. [24] report that in the samples collected for the 1995 and 2001 National Household Travel Surveys the average driving distance for an individual that went straight to work was 7.0 great-circle miles. For the same sample, the average driving distance was 12.6 miles and the average travel time was 23.1 minutes. These numbers suggest that one great-circle mile corresponds to about 1.8 miles of traversed road and 3.3 minutes of driving time. However, the conversion rate of a great-circle mile to driving time to theater may be a bit lower for trips to the theater than for trips to work because congestion is likely less of an issue on weekends and evenings when most films are viewed.

One great-circle mile likely translates to 2 to 3 minutes of driving for film goers.

[14]An alternative way to generate the dataset is to include an observation for every zip code in the United States, as opposed to every zip code with an offset on record. This dataset produces very similar results in both magnitude and statistical significance and is discussed further in Section 5.7.

[15]If the effect of the film persisted past two months, then estimates based on the two-month definition will *understate* the impact of the film during the two initial treatment months. This is because a partial-treatment month that is coded as a control month will result in an overestimate of the difference between treatment and control outside of the treatment period.

[16]It is important to note that the film's appearance was not randomly determined and that a zip code's proximity to AIT is correlated with its demographics. Table IV in the online Appendix presents demographic variables, stratified by *Close*. The treatment zip codes are wealthier, more educated, and more densely populated. This issue is discussed further in section 5.6.

[17]There is somewhat more variation in the dollar amount of offsets in treatment zip codes than in control zip codes. The standard deviation of offset amount is $261.36 in treatment zip codes and $181.09 in control zip codes.

[18]The other period in which the difference between the two groups was substantially different then it was on average during the control period is September 2007. In this period, control zip codes experienced a relatively greater increase in offsets than did treatment zip codes. Google Trends does not show an increased search volume for "An Inconvenient Truth", "Al Gore", or "carbon offset" in September 2007, so it appears unlikely that the atypical September purchasing patterns were caused by an event related to public awareness of either An Inconvenient Truth or carbon offsets. However, the empirical approach applied in this paper is unable to account for the precise cause of the September 2007 change.

[19]A convenient feature of the aggregated data is that after aggregation all observations report a positive amount of offsets. Because there are no observations with zero offsets reported, it is straightforward to implement a model that uses the log of offsets as the dependent variable.

[20]I use the fixed effects negative binomial as opposed to the fixed effects Poisson because of the overdispersion in *offsets*. The mean of offsets is .13 and the standard deviation is .52. Poisson results are very similar and are discussed in endnote 23.

[21]In a related paper, Guimaraes [14] shows that the fixed effects only fully cancel out in a fixed effects negative binomial model when there is a specific functional relationship between each fixed effect and the corresponding overdispersion parameter.

[22]Specifically, in the results reported in Table I, which are discussed below, the bootstrap only increases the standard errors from .155 to .158 in the fixed-effects negative binomial and from .163 to .172 in the unconditional negative binomial.

[23]There may be some concern that inserting one treatment dummy variable is not a valid substitution for inserting each individual zip code fixed effect in the unconditional negative binomial. One way to investigate the validity of the substitution is to check how the substitution changes the outcome in a Poisson regression. It is known that the fixed effects Poisson model and the unconditional Poisson with individual dummy variables produce identical results [5]. Therefore, in this case, it would be concerning if the fixed effects Poisson and unconditional Poisson with one treatment dummy variable produced substantially different results. I estimate both fixed effects Poisson model and an unconditional Poisson with a single treatment dummy variable and find that results are nearly identical; both models indicate the film led to a 48.4 percent increase in offsets and the z-statistics are 2.60 and 2.40 for the fixed effects and unconditional Poisson models, respectively.

[24]Additionally, the similarity in offset purchasing patterns between treatment zip codes and control zip codes in June and July 2007 seems to rule out the possibility that the effect that was detected during the initial treatment period was the result of treatment zip codes having different summer time purchasing patterns than control zip codes.

[25]A January 2007 survey conducted by Pew Research found that 71 percent of "liberal democrats" believed in anthropogenic climate change, whereas only 20 percent of "conservative republicans" believed in anthropogenic climate change [26].

[26]I define liberal vote share as the share of votes for Nader, Kerry or Bush that were cast for Nader or Kerry. The correlation between this variable and the probability that a zip code had access to the film is 0.33.

[27]The Pew Center did not collect information on public opinion on the this topic prior to June 2006. Surveys in August 2006 and Jan 2007 reported belief in anthropogenic global warming to be 47 percent.