

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

[Job Market Paper]

Grant Jacobsen

University of California-Santa Barbara

2120 North Hall

University of California

Santa Barbara, 93106-9210

jacobsen@econ.ucsb.edu

Phone: (717) 315-5503

Fax: (805) 893-8830

*I thank Matthew Kotchen, Robert Deacon, Olivier Deschenes, and Charles Kolstad for helpful comments. I also thank participants at a UCSB seminar, the Western Economics International Association's Conference, and the University of Colorado Environmental and Resource Economics Workshop.

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

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 renewed them again a year later. This research has implications for how information campaigns, which are commonly used by policy-makers to address market failures, affect the behavior of households.

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 welfare gains.

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 (Intergovernmental Panel on Climate Change, 2007), fewer than 50 percent of Americans believe there is “solid evidence that the Earth is warming due to climate change” (Pew Research Center for People & the Press, 2008). Recognizing this discrepancy, many governments and non-governmental organizations have supported or undertaken efforts to raise awareness. The publications of the reports by the International Panel on Climate Change (e.g. Intergovernmental Panel on Climate Change, 2007) and the Stern Review (Stern, 2007) 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” (United Nations Environment Programme, 2008); and the World Wildlife Fund’s “Earth Hour” 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 (World Wildlife Foundation, 2008).

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 presentations 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]” (The Norwegian Nobel Committee, 2007).

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 (e.g. Kraywinkel et al., 2007), or how many people were reached by an awareness campaign (e.g. Chockalingam, 2008), 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. Secondly, 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*” (Keegan, 2008).

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 donation 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 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 renew them again a year later.

This paper proceeds as follows. Section 2 discusses related literature. Section 3 provides background information on *An Inconvenient Truth* and voluntary carbon offsets. Section 4 describes the data. Section 5 discusses methods and results. Section 6 concludes the paper.

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 this paper is one of the first studies to use field data to establish a causal relationship between an awareness campaign and an increase in private provision of a public good. In a related paper, Reiss and White (2008) 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. This paper focuses on a different type of public appeal and a different behavior, and it also exploits spatial variation, in addition to temporal variation, which was the basis of the San Diego study. Spatial variation allows for the analysis to control for common time trends that were unrelated to the film.

Also related to this paper are studies that use contingent valuation methodology to examine the relationship between information and the private provision of a public good.

Ajzen et al. (1996) 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. (1990) show that willingness-to-pay for environmental commodities increases when more information is provided on the recreational services provided by environmental commodities. This paper provides field data that supports these earlier findings, which were based on hypothetical decisions.

Additionally, this paper contributes to the growing economics literature on the impact of mass media on behavior. Recent research has examined the relationship between media bias and voting (DellaVigna and Kaplan, 2007), violent films and violent behavior (Dahl and DellaVigna, in press), television and children's academic performance (Gentzkow and Shapiro, 2008), public exposure of corrupt officials and election outcomes (Ferraz and Finan, 2008), and cable television access and women's status in India (Jensen and Oster, in press). This paper examines the relationship of media and behavior in a new context: climate change.

3 Background

3.1 An Inconvenient Truth

An Inconvenient Truth (AIT) was the centerpiece of Al Gore's campaign to increase recognition of the problems associated with climate change. The film focuses on establishing the fact that climate change is occurring and then explaining its potential consequences. The conclusion of the film argues that climate change can be overcome by human action, but it does not explicitly call for individuals to purchase carbon offsets.

AIT opened nationally on June 2, 2006.¹ The film was shown in more than a 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.² The film

¹AIT's official theatrical release date was May 23rd, 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.

²Weekend box office information is publicly available at boxofficemojo.com.

became the fourth highest grossing documentary of all time and won the Academy Award for best documentary.

3.2 Voluntary Carbon Offsets

A voluntary carbon offset allows individuals or groups to mitigate climate change by making a financial donation to offset their own carbon emissions. In exchange for this financial donation, carbon offset providers invest in projects that reduce carbon emissions. Donations are commonly used to support renewable energy or reforestation programs. In 2007, projects supported by voluntary carbon offsets accounted for around 10.2 million metric tons of carbon.³ Offsets have received attention from academic researchers, the popular press, and government agencies.⁴

This paper uses data on carbon offsets from Carbonfund.org (henceforth Carbonfund). Carbonfund is a prominent offset provider and the first provider listed in Google’s search results for “carbon offset”. 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 donation of \$100. In exchange for this donation, 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.⁵ The annual term implies that the effect of the film, if it exists, may only be observable

³There is debate over the extent to which carbon offsets projects lead to actual emissions reduction (Government Accountability Office, 2008). 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.

⁴See Kotchen (in press) for a theory of voluntary environmental offsets. The New York Times published more than 30 articles or editorials on voluntary carbon offsets in the first ten months of 2008. The Federal Trade Commission hosted a workshop on voluntary carbon offsets as part of a regulatory review of its environmental marketing guidelines in January 2008 and the Government Accountability Office produced a report for Congress on the voluntary carbon offset market in August 2008 (Government Accountability Office, 2008).

⁵Carbonfund also currently offers event-specific offsets for flights and weddings. These options were not

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. Secondly, 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.

4 Data

This study uses two unique sources of data. The first data set 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 second set of data is a record of 12,902 carbon offsets that were made through Carbonfund during the period from March 2006 through March 2008. This data was provided by Carbonfund and represents Carbonfund’s entire history of providing offsets, with the exceptions that are described in the Data Appendix. Each record includes the date that the offset was made 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.⁶ 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.”⁷ 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 but

available at the time of the film’s release.

⁶See the Data Appendix for details on this process.

⁷An approximate conversion rate of great-circle miles to driving distance and travel time may be useful for the reader. Liss et al. (2005) 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.

Rhode Island contains a zip code with a distance of more than 25 great-circle miles.

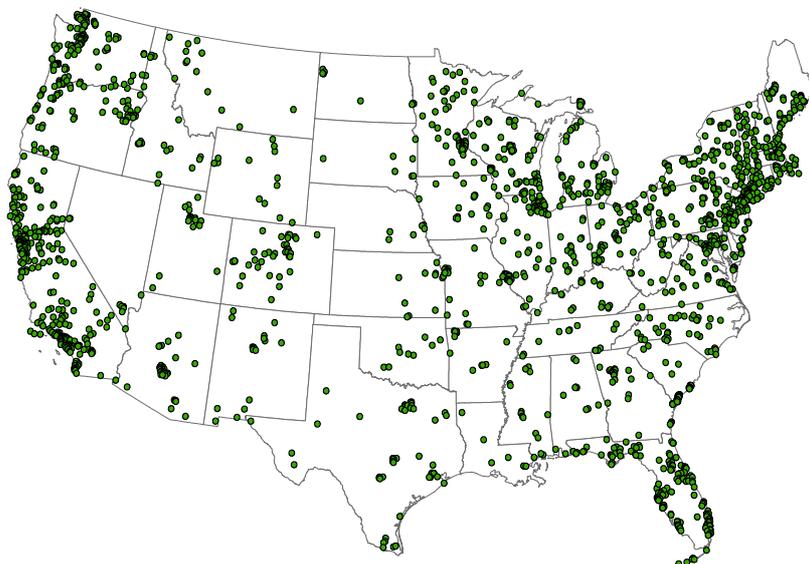


Figure 1: Location of theaters that showed AIT in the continental U.S.

I convert the carbon offset records into panel data that represents all of the 3,917 zip codes with at least one offset 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. 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. GIS data on each zip code’s distance to the film is then added to this panel, as well as ZCTA level demographic variables from the 2000 U.S. Census.⁸

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

⁸An alternative way to generate the data set 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 data set, which consists of 806,050 observations, produces very similar results in both magnitude and statistical significance.

of *Close* is that it designates a group that had relatively easier access to AIT; if the film led people to make 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, but then drop off with time. This drop off should occur because an individual who purchases an annual offset because of AIT is unlikely to make a second purchase within a year. 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. The effect of the film may have carried on longer, but at minimum it should be detectable in these two months.⁹ The primary estimations 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, and so June and July 2007 could be treatment months as well. For this reason, I examine the possibility of a second treatment period a year after the initial treatment period in the analysis as well.

The data is 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

⁹If 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.

code during any one month is 35. 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. However, this difference is largely due to population. 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.

It is important to note the film’s appearance was not randomly determined and that a zip code’s proximity to AIT is correlated with its demographics. Table 5 in the Appendix presents demographic variables, stratified by *Close*. The treatment zip codes are wealthier, more educated, and more densely populated. This issue will be discussed further in section 5.6.

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 comparing how the number of offsets in zip codes that were close to the film changed during the two months following AIT’s release relative to the change in the number of offsets in zip codes that were further away. 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 donations, 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 main estimations.¹⁰

¹⁰The similarity of the results is not surprising considering that average offset size is similar between treatment zip codes and control zip codes. After dropping 67 outliers that were greater than \$1000, the average offset size was \$92.07 and \$89.99 for treatment and control zip codes, respectively.

5.1 Graph

Figure 2 graphs the the natural log of the number of offsets made across time. The solid line represents all offsets that were made in treatment zip codes, or zip codes where *Close* equals 1, and the dashed line represents all offsets that were made within control zip codes, or zip codes where *Close* equals 0. The vertical line in the graph corresponds to the release of AIT in June 2006. Offsets were generally increasing over time, with large spikes in December months when offsets were likely purchased as holiday gifts. During most periods, there were about 6.5 times as many offsets made in treatment zip codes as control zip codes, or about 2 points on a log scale. Most importantly, for this study, is that the greatest difference between the two groups occurred in the two months immediately following AIT's release. To make the difference easier to detect visually, I subtract the mean difference between the two groups of zip codes during months outside of the treatment period, which is 2.003 log points, from the observations for the treatment zip codes and re-graph the data in Figure 3. After making this adjustment, the difference between the two lines indicates how the two groups of zip codes differed from each other at a point in time relative to how they differed from each other on average during the course of the sample period. The dotted line near the bottom of the graph Figure 3 is the difference between the control observations and the adjusted treatment observations. The graph shows that the two groups followed similar trends leading up to the film's release. 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, both groups returned to following similar trends. These differential trends at the time of the film's release provide initial evidence that AIT led to an increase in offsets.

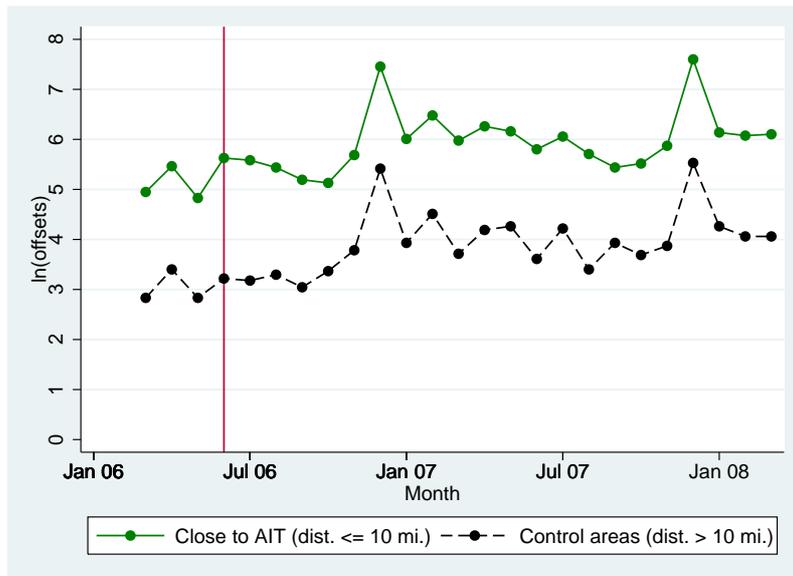


Figure 2: Natural log of the total number of offsets across time by proximity to AIT. The vertical line corresponds to AIT’s release into theaters.

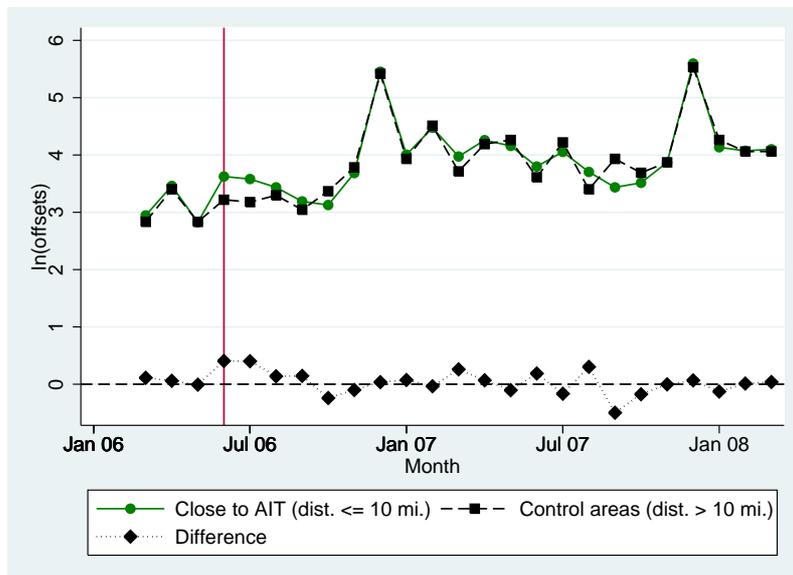


Figure 3: Natural log of offsets across time by proximity to AIT after adjustment for the mean difference between groups outside of the treatment period. The vertical line corresponds to AIT’s release into theaters.

5.2 Estimation

I first estimate the effect of AIT using a feasible generalized least squares 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

$$\ln(\text{offsets}_{it}) = \alpha + \gamma_1 \text{Close} + \gamma_2 \text{Close} \times \text{TreatmentPd}_{it} + \omega_t + \epsilon,$$

where ω_t is a vector of fixed effects for each month in the sample. To account for potential serial correlation in the residuals, I specify a first-order auto-regressive error structure. Results are similar regardless of whether or not the error structure is AR1 or iid. Estimates of the coefficient of interest, γ_2 , are unchanged whether the dependent variable is offsets or offsets per capita because population is effectively stable over the sample period.

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. (1984).¹¹ 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 (Cameron and Triverdi, 1998). I make the conventional assumption that λ_{it} has an exponential form, thus the mean of offsets_{it} is given by

$$\text{offsets}_{it} = \theta_i \exp(\beta \text{Close} \times \text{TreatmentPd}_{it} + \omega_t),$$

where ω_t is a vector of fixed effects for each month in the sample. The conditional joint density based on this specification conditions on the total number of offsets within each zip

¹¹I 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 briefly in footnote 14.

code over the sample period. Hausman et al. (1984) 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 (2002) 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.¹² 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. (2004) 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, 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.¹³

5.3 Estimation Results

The primary estimation results are reported in Table 1. I present all results in the form of exponentiated coefficients minus one. The results displayed in the table can be interpreted

¹²In a related paper, Guimaraes (2008) 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.

¹³Specifically, in the results reported in Table 1, which are discussed below, the bootstrap only increases the standard errors from .155 to .162 in the fixed-effects negative binomial and from .159 to .161 in the unconditional negative binomial.

as the percentage increase in offsets associated with a one unit increase the corresponding right-hand-side variable.

The FGLS results using the aggregated data, which are reported in column 1, indicate that the film led to a 51.7 percent increase in offsets in treatment zip codes during the first two months of its release. Results from the disaggregated data mirror 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 51.7 percent increase in offsets. The negative binomial estimates are reported in column 3 and indicate the film led to a 49.3 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.¹⁴ 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 Table 6, 7, and 8 in the Appendix.

In column 4 in Table 1, I present the results of an FGLS 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 56.9 percent increase in the total dollar amount of offsets, which is similar to the percentage increase in the number of transactions. I do not estimate specifications that use *amount* as the dependable for the disaggregated data because the number of zero values in *amount* when the data is in disaggregated form makes a log transformation problematic.

¹⁴There 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 (Cameron and Triverdi, 1998). 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.

Table 1: Estimates of the effect of AIT on offsets

<i>Dependent Var.:</i>	<u>ln(offsets)</u>	<u>offsets</u>	<u>offsets</u>	<u>ln(amount(\$))</u>
<i>Model:</i>	FGLS	NBFE	NB	FGLS
	(1)	(2)	(3)	(4)
Close x TreatmentPd	0.517***	0.514**	0.493**	0.569**
	5.680	2.564	2.572	2.466
Close	6.410***		1.179***	6.902***
	104.315		8.563	40.439
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 or z-stats are reported. 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. The R-squared for the equivalent OLS model in column 1 is .996. The R-squared in columns 2 and 3 are .33 and .07, respectively.

5.4 Examining the duration of the effect

The estimations in Table 1 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 duration of the film's effect more closely. First, because the assumption of a two-month treatment is somewhat arbitrary, I additionally examine whether or not the effect of the film is detectable three or four months after its initial release. Secondly, I examine the possibility that the film led individuals to make offsets sooner, but did not lead to additional offsets. To do so, I examine whether or not there was a relative *decrease* in offsets in treatment zip codes during the five remaining months in 2006 that followed the treatment period, as would be expected if the film moved the timing of offsets earlier, but did not lead to an overall increase in the number of offsets purchased. Lastly, based on the annual term of most offsets, I test for an effect one year after the film's release. An increase during this period would be evidence that individuals who purchased an annual offset because of AIT purchased a second offset after the term of their initial offset ended.

The results of these estimations are reported in Table 2. In column 1, I include two additional interaction variables. One variable is an interaction between *Close* and a dummy variable indicating August 2006, and one variable is an interaction between *Close* and a dummy variable indicating September 2006. These months were the third and fourth months after AIT's release. The coefficient on both of these interaction variables is insignificant, suggesting that individuals that were convinced by AIT to make an offset did so within two months of the film's release. In column 2, I include a variable that is an interaction between *Close* and *PostTreatmentPd*, which is a dummy variable that equals one for the months between August 2006 and December 2006. The coefficient on this variable is positive, small in magnitude, and statistically insignificant, all of which support the interpretation that the film led to the purchase of additional offsets, as opposed to a change in the timing of offset.¹⁵

Column 3 reports perhaps the most interesting result. In this specification, I include an interaction between *Close* and a dummy variable for June and July 2007, which were the months one year following AIT's release. If individuals who were convince by AIT to purchase an offsets because of AIT purchased them again after the original offsets expired one-year later, then we would expect the treatment and control groups too diverge again at this point. However, the coefficient on the interaction variable is both small in magnitude and statistically insignificant. This result suggests that individuals who initially purchased an offset due to the film did not do so again a year later.¹⁶

¹⁵This result is robust to longer definitions of the post-treatment period.

¹⁶The similarity in offset purchase patterns between treatment zip codes and control zip codes in June and July 2007 also 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.

Table 2: Examining the duration of the effect

	(1)	(2)	(3)
Close x TreatmentPd	0.523***	0.518**	0.534***
	2.611	2.521	2.589
Close x (Aug. 2006)	0.180		
	.722		
Close x (Sep. 2006)	0.202		
	1.067		
Close x (June & July 2007)		0.047	
		0.481	
Close x PostTreatmentPd			0.060
			0.745
Observations	97,925	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. All specifications include a fixed effect for each of the 25 months in the sample. *PostTreatmentPd* is a dummy variable equaling 1 for months from August 2006 through December 2006. Z-stats are reported 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 .37.

5.5 Examining Alternative Definitions of the Treatment Group

In this section, I examine whether the inverse relationship between distance to the film 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 group corresponding to a certain five-mile

range. 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 3. 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 3: Examining alternative definitions of the treatment group

	(1)	(2)
$(0 \leq \text{dist.} \leq 5) \times \text{TreatmentPd}$	0.579***	0.687*
	2.909	1.898
$(5 < \text{dist.} \leq 10) \times \text{TreatmentPd}$	0.280	0.367
	1.338	1.071
$(10 < \text{dist.} \leq 15) \times \text{TreatmentPd}$		0.264
		0.697
$(15 < \text{dist.} \leq 20) \times \text{TreatmentPd}$		-0.257
		-0.635
Observations	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. 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 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.

5.6 The identification assumption and robustness checks

A common trends condition is necessary for the consistency of a differences-in-differences estimator (Meyer, 1995). 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 and 3 indicate 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 relatively weak assumption that there was not a shock to treatment zip codes that occurred at the exact month of AIT's release and that ended at the same time that AIT was leaving theaters.

I estimate two robustness checks to conclude the analysis. First, I relax the assumption that the difference between control and treatment zip codes is fixed across time by estimating 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. Secondly, 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 4. In all specifications, the estimated impact of the film, which ranges from .44 to .65, is comparable to the base estimation, and remains statistically significant.

Table 4: Additional robustness checks

	(1)	(2)	(3)
Close x TreatmentPd	0.650***	0.444***	0.575**
	3.007	2.660	2.568
Close x month-year	0.008		0.008
	1.325		1.234
BA x TreatmentPd		0.007**	0.007**
		2.099	2.058
Income x TreatmentPd		-0.074***	-0.074***
		-2.610	-2.619
Pop. dens. x TreatmentPd		-0.001	-0.001
		-0.332	-0.330
Observations	97,925	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. 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 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.

A final issue that may be of concern 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.

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 any awareness campaign has been effective at changing behavior.

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 of offset provision outside of the time when the film was in theaters and estimates are robust to a variety of specifications. I also find little evidence that the effect persisted for more than a 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, it is worth noting that the data used come from only one organization and that offsets are not common 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 entire carbon offset sector. During the treatment period, Carbonfund sold 272 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 1, indicate that the film led to 91 additional offsets, or \$16,150 in additional donations. Based on Carbonfund’s pricing of \$5.5 per ton of carbon, this translates to approximately 2,900 tons of carbon. 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 (Trexler Climate + Energy Services, 2006) and the assumption that the increase that was experienced by Carbonfund was experience by all retailers in the industry, suggests that the film led to 3,640 additional offsets overall, corresponding to \$646,000, or 117,000 tons of carbon. To put this number in context, 117,000 tons of carbon is roughly equivalent to the amount of carbon emissions produced by running 20,000 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 Pew Research Center for People & the Press (2008), 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.¹⁷ 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, and, perhaps most importantly, 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 campaign increase the willingness of individuals to purchase carbon offsets, which is a relatively fringe behavior. Future research is needed to examine the effect of awareness campaigns on more mainstream behaviors. If climate change awareness campaigns do have an effect on other behaviors, it would be par-

¹⁷The 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.

ticularly interesting to examine whether or not the effect is short-lived, as appears to be the case with offsets.

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 ArcMAPTM Version 9.2, a component of ESRI's ArcGIS[©] 9. The distance measure was originally computed in decimal degrees and then was converted to miles using the formula: $\text{Earth's radius} \times \pi \times \text{distance in decimal degrees} / (180 \times 1600) = \text{distance in miles}$.

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 5: Demographic comparisons

Variable	<u>Close</u>		<u>Not Close</u>	
	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
Population density	19.986	40.618	1.895	3.381

Notes: Close equals one for 3,029 zip codes and zero for 880 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.

Table 6: Negative binomial results - Examining the duration of the effect

	(1)	(2)	(3)
Close x TreatmentPd	0.503**	0.490**	0.496**
	2.398	2.409	2.673
Close x (Aug. 2006)	0.153		
	0.637		
Close x (Sep. 2006)	0.160		
	0.680		
Close x (June & July 2007)		-0.028	
		-0.236	
Close x PostTreatmentPd			0.007
			0.081
Close	1.166***	1.184***	1.176***
	8.072	8.512	8.042
Observations	97,925	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. All specifications include a fixed effect for each of the 25 months in the sample. *PostTreatmentPd* is a dummy variable equaling 1 for months from August 2006 through December 2006. Z-stats are reported 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 .37.

Table 7: Negative binomial results - Examining alternative treatment group definitions

	(1)	(2)
(0 ≤ dist. ≤ 5) x TreatmentPd	0.543***	0.610
	2.623	1.340
(5 < dist. ≤ 10) x TreatmentPd	0.294	0.351
	1.512	0.969
(10 < dist. ≤ 15) x TreatmentPd		0.220
		0.542
(15 < dist. ≤ 20) x TreatmentPd		-0.287
		-0.627
(0 ≤ dist. ≤ 5)	1.555***	1.774***
	8.209	9.115
(5 < dist. ≤ 10)	0.371***	0.488***
	7.126	4.459
(10 < dist. ≤ 15)		0.196*
		1.811
(15 < dist. ≤ 20)		0.008
		0.108
Observations	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. 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 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 .07.

Table 8: Negative binomial results - Additional robustness checks

	(1)	(2)	(3)
Close x TreatmentPd	0.440**	0.486**	0.421**
	2.014	2.433	2.422
Close x month-year	-0.003		-0.004
	-0.579		-0.724
BA x TreatmentPd		0.005*	0.005*
		1.751	1.853
Income x TreatmentPd		-0.058**	-0.058**
		-2.017	-2.572
Pop. dens. x TreatmentPd		-0.001	-0.001
		-0.647	-0.758
Close	1.286***	0.332***	0.412***
	6.175	4.201	2.666
BA		0.033***	0.033***
		14.935	14.756
Income		-0.156***	-0.156***
		-5.518	-6.204
Pop dens.		0.005	0.005
		1.367	1.446
Observations	97,925	97,925	97,925

Notes: The dependent variable is offsets. The unit of observation is a zip code and month. 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 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 .07.

References

- Ajzen, I., T. C. Brown, and L. H. Rosenthal (1996). Information bias in contingent valuation: Effects of personal relevance, quality of information, and motivational orientation. *Journal of Environmental Economics and Management* 30(1), 43–57.
- Allison, P. D. and R. P. Waterman (2002). Fixed-effects negative binomial regression models. *Sociological Methodology* 32, 247–265.
- Bergstrom, J. C., J. R. Stoll, and A. Randall (1990). The impact of information on environmental commodity valuation decisions. *American Journal of Agricultural Economics* 72(3), 614–621.
- Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119(1), 249–275.
- Cameron, A. and P. Triverdi (1998). *Regression Analysis of Count Data*. Econometric Society Monograph No.30. Cambridge: Cambridge University Press.
- Chockalingam, A. (2008). World Hypertension Day and global awareness. *Canadian Journal of Cardiology* 24(6), 441–444.
- Dahl, G. and S. DellaVigna (in press). Does movie violence increase violent crime? *Quarterly Journal of Economics*.
- DellaVigna, S. and E. Kaplan (2007). The Fox News effect: Media bias and voting. *Quarterly Journal of Economics* 122(3), 1187–1234.
- Ferraz, C. and F. Finan (2008). Exposing corrupt politicians: The effects of Brazil’s publicly released audits on electoral outcomes. *Quarterly Journal of Economics* 123(2), 703–745.
- Gentzkow, M. and J. M. Shapiro (2008). Preschool television viewing and adolescent test scores: Historical evidence from the coleman study. *Quarterly Journal of Economics* 123(1), 279–323.
- Government Accountability Office (2008). The U.S. Voluntary Market Is Growing, but Quality Assurance Poses Challenges for Market Participants. *U.S. Governmental Accountability Office: Report to Congressional Requesters GAO-08-1048*.
- Guimaraes, P. (2008). The fixed effects negative binomial model revisited. *Economics Letters* 99(1), 63–66.
- Hausman, J., B. H. Hall, and Z. Griliches (1984). Econometric-models for count data with an application to the patents R and D relationship. *Econometrica* 52(4), 909–938.
- Intergovernmental Panel on Climate Change (2007). *Climate Change 2007*. Cambridge: Cambridge University Press.

- Jensen, R. and E. Oster (in press). The Power of TV: Cable television and women's status in India. *Quarterly Journal of Economics*.
- Keegan, R. W. (2008). Can a film change the world. *TIME Magazine* 171(11).
- Kotchen, M. J. (in press). Voluntary provision of public goods for bads: A theory of environmental offsets. *Economic Journal*.
- Kraywinkel, K., J. Heidrich, P. U. Heuschmann, M. Wagner, and K. Berger (2007). Stroke risk perception among participants of a stroke awareness campaign. *Bmc Public Health* 7, 7.
- Liss, S., N. McGuckin, S. Moore, and T. Reuscher (2005). *Our Nation's Travel: Current Issues*. Publication no. FHWA PL 05 015. Washington, D.C.: Federal Highway Administration.
- Meyer, B. D. (1995). Natural and quasi-experiments in economics. *Journal of Business & Economic Statistics* 13(2), 151–161.
- Pew Research Center for People & the Press, T. (2008). A deeper partisan divide over global warming. Press release available at <http://people-press.org/reports/pdf/417.pdf>.
- Reiss, P. and M. White (2008). What changes energy consumption? Prices and public pressure. *Rand Journal of Economics* 39(3), 636–663.
- Stern, N. (2007). *The Economics of Climate Change: The Stern Review*. Cambridge, UK: Cambridge University Press.
- The Norwegian Nobel Committee (2007). The Nobel Peace Prize for 2007. Press release available at <http://nobelprize.org/nobel-prizes/peace/laureates/2007/press.html>.
- Trexler Climate + Energy Services (2006). A consumer's guide to retail carbon offset providers. Report commissioned by Clean Air-Cool Planet, Inc.
- United Nations Environment Programme (2008). Climate Change Outreach Programme. UNEP Climate Change. Retrieved 29 May 2008. <http://www.unep.org/themes/climatechange/About/index.asp>.
- World Wildlife Foundation (2008). 36 Million Americans Take Part in WWF's Earth Hour. Press Release available at <http://www.worldwildlife.org/who/media/press/2008/WWFPresitem8732.html>.