ABSTRACT

Title of dissertation: THREE QUASI-EXPERIMENTAL AND EXPERIMENTAL PAPERS IN ENVIRONMENTAL ECONOMICS

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This dissertation utilizes quasi-experimental and experimental techniques to contribute to the literature on environmental and resource economics in three distinct ways but with the overarching goal of demonstrating the usefulness of experimental techniques to explore topics related to the environment. First, chapter one demonstrates the feasibility and draw-backs of using a quasi-experimental regression discontinuity approach to analyze the ozone regulation contained in the 1990 Clean Air Act Amendments. Chapter two explores the impacts how economic and social-psychology factors affect the adoption of an environmental technology, namely compact fluorescent light bulbs. In order to consider these factors, chapter two utilizes a large-scale field experiment informed by a theoretical model of adoption. Finally, chapter three utilizes a field experiment designed for a large apartment management company, to advance the literature on presumed (opt-out) and explicit (opt-in) consent procedures by exploring a willingness to pay to forgo the decision to opt-out of the installation of an environmental technology.

THREE QUASI-EXPERIMENTAL AND EXPERIMENTAL PAPERS IN EXPERIMENTAL ECONOMICS

by

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Dedication

To my Parents.

Contents

List of Tables v

List of Figures vi

1 Results Concerning the 1990 Clean Air Act Amendment 1
- 1.1 Introduction . . . 1
- 1.2 Non-Attainment categories of the 1990 Clean Air Act Amendments . 5
- 1.3 Previous Literature . . . 10
- 1.4 Research Design . . . 14
- 1.5 Data . . . 23
- 1.6 Estimation Procedure: Regression Discontinuity . . . 26
- 1.7 Estimation . . . 33
- 1.8 Results–Balance . . . 36
- 1.9 Results - Regression Discontinuity Estimates . . . 41
- 1.10 Results - Two Stage Least Squares . . . 44
- 1.11 Conclusion . . . 49

2 How Many Economists does it take to Change a Light Bulb? A Natural Field Experiment on Technology Adoption 51
- 2.1 Introduction . . . 51
- 2.2 Previous Literature . . . 55
- 2.3 Model . . . 60
  - 2.3.1 Theory: Discrete Choice Model of Adoption with Social Pressure and Attitude . . . 60
  - 2.3.2 Predictions . . . 72
    - 2.3.2.1 Predicted Probability of being home . . . 72
    - 2.3.2.2 Predicted Probability of purchasing, unconditional on answering door . . . 74
    - 2.3.2.3 Predicted Probability of purchasing, price and social pressure . . . 75
- 2.4 Experimental Design and Implementation . . . 75
- 2.5 Results . . . 82
  - 2.5.1 Selection: the impact of warning households . . . 83
  - 2.5.2 Comparison of Means: Economic and Social-Psychology Treatments . . . 85
  - 2.5.3 Hurdle Model: Economic and Social-Psychology Treatments . 89
- 2.6 Conclusion . . . 93

3 The Value of Choice: A WTA Estimate of an "Opt-Out" Option to Adopt Environmental Technology 96
- 3.1 Introduction . . . 96
- 3.2 Literature . . . 101
- 3.3 Experimental Design . . . 104

3.4 Empirical Methodology 111
3.5 Results 114
3.6 Conclusion 117

A Appendix: Results Concerning the 1990 Clean Air Act Amendment, Design Value Calculations 119
119
A.2 Theoretical discussion of impact of different levels of regulation on time taken to reduce emissions 121
A.3 Further discussion of Regression Discontinuity 122

B Appendix: How Many Economists does it take to Change a Light Bulb? A Natural Field Experiment on Technology Adoption 127

Bibliography 132

List of Tables

1.1 County or Partial County and Area Demographics by Attainment Status (1990 Census) . . . 27
1.2*a* County or Partial County Demographics within 0.01 ppm of Discontinuity (1990 Census) . . . 38
1.2*b* County or Partial County Demographics within 0.01 ppm of Discontinuity (1990 Census) . . . 39
1.3 Regression Discontinuity Results: Percentage change in DV from 1990 to 1996 and Time Adjusted . . . 42
1.4*a* Two-Stage Least Squares: Percentage change in DV from 1990 to 1996 45
1.4*b* Two-Stage Least Squares: Percentage change in DV from 1990 to 1996 46

2.1 Treatment Sample Size . . . 77
2.2 Survey Results on CFL Opinions . . . 77
2.3 Warning Treatment Results: Doors Answered and Purchase Decision 85
2.4 Price and Social Norm Treatment Results: Purchase Decision Conditional on Answering . . . 85
2.5 Price and Social Pressure Elasticities . . . 86
2.6*a* Two Stage Hurdle Model Estimates . . . 87
2.6*b* Two Stage Hurdle Model Estimates . . . 88
2.7 Door-Hanger Type Treatment Results: Doors Answered and Purchase Decision . . . 90

3.1 Installation and Apartment Information by Treatments . . . 108
3.2 Chi-square Tests of Difference in Installation Rates . . . 108
3.3 Responses and Turnbull Estimation Results: Willingness To Accept (WTA) . . . 109

List of Figures

1.1 Sacramento CA (Area) Ozone Attainment Classifications 12
1.2 Yearly Average Area DV Percentage Change by NA Category 18
1.3 Distribution of Annual Mean Monitor DVs 20
1.4 Distribution of Annual Mean Monitor One-Hour Averages 20
1.5 'First Stage' Monitor Level 28
1.6 'First Stage' County/Split Level 28
1.7 'First Stage' Area Level 29

2.1 Decision to Be Home (h) and Purchase (q) 69
2.2 Door-Hanger Examples 71
2.3 Treatment Cells 76

3.1 Notice for Apartment Residents (Italics Differentiate Language for Treatments) 98
3.2 First Webpage of Survey: Opt-Out Version 107

Chapter 1

Results Concerning the 1990 Clean Air Act Amendment[1]

1.1 Introduction

Ground-level ozone pollution is one of six "criteria pollutants" regulated by the Environmental Protection Agency (EPA) through the National Ambient Air Quality Standards (NAAQS), originally established in the1970 Clean Air Act (CAA or "the Act").[2] The NAAQS for ozone was altered in the 1990 Clean Air Act Amendments (CAAA) in order to more effectively decrease ozone pollution. Since the introduction of the 1990 CAAA through 2004, there has been a statistically significant decline in the second highest average annual one-hour ambient ozone concentration (ozone) measurement from pollution monitors throughout the United States, from 0.127 to 0.104 parts per million (ppm).[3] It is not clear if this decrease in ozone is attributable to the 1990 CAAA however, or if the decline is the result of an increased adoption of

[1]This is joint research with Assistant Professor Scott Lowe of Boise State University.

[2]The ozone was originally monitored under a standard for all photochemical oxidants but was changed specifically to ozone in 1979. The other criteria pollutants are: nitrogen oxides (NOx), sulfur dioxide (SO2), carbon monoxide (CO), lead (Pb) and total suspended particulates (TSPs - particulate matter (PM) later replaced TSPs). The 1970 CAA is actually an amendment to an earlier 1963 CAA which placed an entirely different agency, the Department of Health, Education and Welfare's National Air Pollution Control Administration, at the helm of national pollution control. The earlier act was little enforced however, and the establishment of a new agency focused solely on the environment, perhaps, provides the rationale for why the 1970 CAA is not typically referred to as an amendment. The EPA's website (www.epa.gov) and documents (especially: U.S EPA 1996, 2006) as well as Portney and Stavins (2000) provided historical information of the CAA and its amendments.

[3]The sample is restricted to ambient emissions measurements between 1990 and 2004 because the non-attainment regulation on Ozone changed in 2004. More information can be found on the EPA's website: http://www.epa.gov/ozonedesignations/.

low emissions technology unrelated to the CAAA, a nationwide shift in production focus towards non-polluting outputs, or some other cause unrelated to the CAAA.[4] Although a non-CAAA explanation for the decline in ozone pollution may seem unlikely, the results of earlier empirical work on the CAA contain mixed evidence concerning whether declines in pollution levels should be attributed to the CAA (Henderson (1996), Greenstone (2004), Auffhammer, Bento and Lowe (2005)).

The impact of increased pollution regulation is identified in this paper by comparing changes in ambient ozone pollution concentrations across five non-attainment categories, each with different regulations, established in the 1990 CAAA. Geographic *areas* across the United States were created by the EPA and assigned to non-attainment categories based on an assignment rule established in the 1990 CAAA and an area level measurement of ozone concentrations known as a "design value". In this paper, we reproduce the design values and use the regression discontinuity (RD) design created by the CAAA's assignment rule to estimate the impact of the difference in regulations. The RD design creates a quasi-experiment in which areas with different levels of regulation, but similar design values, act as counterfactuals. By comparing geographic areas with similar design values and using the exogenous difference in regulations caused by the assignment rule, a consistent estimate of the impact between the different regulation levels can be found.

The impact of regulation is analyzed over two different time periods, for two different types of ozone measurements. Different lengths of time are used to address

[4]The economic literature on the Environmental Kuznet's Curve offers at least one alternate suggestion to regulation that has motivated technology adoption and a shift in production: consumer demand. Goklany's Clearing the Air (1999) and Torras and Boyce (1998) both summarize this literature.

the two dimensions of the 1990 CAAA: regulatory variation exists in terms of the restrictiveness of regulation imposed as well as the time allotted for a geographic area to reduce its ambient concentration level below the NAAQS. In addition to two different time periods, two different types of ozone measurements are used to answer two different questions. First, the percentage change in design value addresses how different levels of regulation impact the EPA's targeted concentration levels. Second, we look at the percentage change in average ambient concentrations in order to reevaluate a result, originally found by Henderson (1996), of increasing average pollution concentrations in areas under more regulation. Finding an increase in average ambient concentrations is of interest because the NAAQS are continually being enforced at lower concentrations in order to reflect current scientific knowledge.[5]

Our research is the first that we know of to utilize the multiple regulatory categories created by the 1990 CAAA to evaluate the impact of the CAA on pollution.[6] The prior work by Henderson (1996) analyzed ozone levels between 1977 and 1987, while Greenstone (2004) focused on sulfur dioxide (SO_2), Auffhammer et al. (2005) on particulate matter of 10 micrometers or less (PM_{10}) and Chay and Greenstone (2003 and 2005) considered total suspended particulates (TSPs) in order to validate non-attainment status as an instrument for reducing ambient pollution levels.[7]

[5] The standard was most recently updated to 0.075 ppm over 8-hours on May 27th, 2008.

[6] Only one other paper has used the multiple non-attainment classifications of the 1990 CAAA but requests not to be cited. It uses descriptive statistics and discusses the demographics that influence assignment to focus on the impact of the economy on reducing emissions.

[7] There have been a number of other papers focusing on attainment designation and employment (Bartik (1988), McConnell and Schwab (1990), Duffy-Deno (1992), Becker and Henderson (2000), List and Kunce (2000), Berman and Bui (2001), Morgenstern, Pizer and Shih (2002), Greenstone (2002) and List, McHone and Millimet (2003)) but none are concerned with explaining the impact on pollution levels.

The existence of multiple regulation categories in the 1990 CAAA provides an extension of prior work which has only compared two levels of regulation stemming from designation as either Attainment or Non-Attainment. The multiple regulation levels provide additional evidence concerning the efficacy of the CAA by providing additional instances of regulatory variation.

This paper also contributes to the current literature by accounting for the EPA's area level assignment and area boundaries which do not always follow county boundaries as used in prior research (Henderson (1996), Kahn (2000), List, McHone and Millimet (2003), Chay and Greenstone (2005), Greenstone (2005)).[8] Inaccurately assigned boundaries may have led to the incorrect assignment of attainment status, and corresponding regulation, to a monitor due to its location within a county. We recreate area boundaries given by the Code of Federal Regulations in order to avoid any measurement error attributable to inaccurate area boundaries. An alternative solution was taken in Auffhammer et al. (2005), which used non-attainment status assigned at the city level to address a related regulation assignment issue concerning the targeting of regulation towards more heavily polluted locations within counties. In order to understand what impact the different levels of observation used in previous research have on the outcomes this paper briefly considers what questions are answered for three different levels of observation: monitor, county and area, and presents results for each. Finally, an RD design has not previously been used to analyze the CAA, although an assignment rule has been used

[8]This "boundary issue" is pointed out in Henderson (1996) and Greenstone (2005), and both mark counties as non-attainment if any portion of the county is in non-attainment.

by the EPA for some time. We provide evidence that an assumption of county level assignment has hindered use of an RD design in the past and that an RD design is reasonable once area level assignment is recognized although the results are hindered by the reduction in sample size.

Before discussing the previous literature further, it is helpful to review the 1990 CAAA to establish some terminology and become familiar with the Act's incentives to reduce emissions. Thus, this paper continues with a brief yet detailed review of the 1990 CAAA. This is followed by a review of the economic literature pertaining to the impact of the CAA on pollution. Our research design is then presented followed by the data used and an outline of the estimation procedure. Finally, the results are presented. The results concerning the impact of regulation on the percentage change in design values are mainly insignificant. The lack of significance is likely due to the constrained sample size when using the appropriate level of regulation: the area level. Although the results are mainly insignificant, they are suggestive that increased regulation has led to increased declines in pollution. A main finding of this paper is the extent that the results depend on the level of observation used. The variation in results suggests that care needs to be taken determining what level of observation is appropriate for the research question being posed.

1.2 Non-Attainment categories of the 1990 Clean Air Act Amendments

Prior to the 1990 CAAA, the NAAQS for ozone required that every monitor within an attainment area have no more than a single violation of the 0.12 ppm one-hour standard, per year.[9] An area failing to meet the NAAQS was labeled as "non-attainment", resulting in heightened restrictions on pollution sources. After the 1990 CAAA, the main 0.12 ppm attainment standard, originally set in 1979, remained, but the single non-attainment classification was replaced with five non-attainment categories with varying federally imposed restrictions. Each year beginning in 1990 and ending 2004, an area's *design value* has been used to designate areas into a non-attainment category according to the following rule (applicable range of design value): *Attainment* (less than 0.121), *Marginal* (0.121 - 0.137 ppm), *Moderate* (0.138 - 0.160 ppm), *Serious* (0.160 - 0.180 ppm), *Severe* (0.180 - 0.280 ppm) and *Extreme* (greater than 0.280 ppm).[10]

A monitor's design value for a given year is determined using one-hour average ambient ozone readings from the previous three years. The one hour averages from the past 3 years are ordered by size and, depending on how often the monitor was measuring ambient ozone, the 4th, 3rd, 2nd or 1st largest one hour average is used as the design value.[11] The EPA uses the highest design value reading from all the monitor design values within an area as the area's design value for that year. The EPA uses the highest monitor readings because scientific studies suggested short

[9]The NAAQS includes two standards, neither of which require nor expect economic costs to be considered: a primary standard to protect health and a secondary standard to protect welfare. Primary and secondary standards have always been equal for ozone.

[10]CAA Section 181. There are 9 categories in total: a "transitional" category was used for special cases of areas that were deemed temporarily in Moderate non-attainment, the Severe category is separated into Severe-15 and Severe-17 categories and a "non-attainment insufficient data" category exists in the CFR. The transitional and insufficient data categories were not used as it was not clear what regulation was imposed. The Severe categories were combined due to the already restricted sample size. Although, as written, two categories include the same reading (0.138 for both Marginal and Moderate for example) the language of the CAAA implies that the "better" category contains all readings less than the upper value while the "worse" category is greater than or equal to the lower value.

[11]See Appendix A for complete details on design value calculation.

periods of highly concentrated ozone exposure posed the most significant threat to public health and the environment.[12]

An area level design value was used as the assignment value for all areas except those assigned by a Metropolitan Statistical Area (MSA) "grouping-rule". The MSA grouping-rule was developed to assign areas within the same MSA or Consolidated MSA (CMSA) to the non-attainment status of the worst non-attainment area of that MSA or CMSA, if the worst area was in Serious, Severe or Extreme non-attainment.[13] However, the MSA rule could be avoided if sufficient evidence was provided by state agencies indicating that an area within the MSA would not contribute significantly to ozone. As an example, the areas with lower pollution within the Los Angeles-Riverside-Orange County CMSA were considered separately from the area within the CMSA containing the South Coast Air Quality Management District.[14]

The main restrictions for areas labeled as non-attainment, regardless of category, come through the submission of a state implementation plan (SIP) to the EPA. An SIP is a detailed plan of intent concerning how non-attainment areas will decrease pollution to meet and retain attainment status. The EPA may approve

[12] As detailed by the EPA, the NAAQS were established and updated based on a lengthy review process which included use of current scientific studies and assessments by interested parties. It is important to point out that some research indicates health impacts non-linear in exposure to ambient concentrations (Hazuchaa and Lefohnb (2007)).

[13] CAA Section 107.d.4.A.iv

[14] Although it is important to be aware of the MSA grouping-rule, it only appears to have been applied in two cases: 1) the cleaner area within the Los Angeles-Riverside-Orange County CMSA received a Severe non-attainment assignment although its calculated design value would have designated it as Serious, and 2) an area within the Boston-Worcester-Lawrence CMSA received a Severe non-attainment assignment rather than a Moderate.

an SIP, call for changes or, in a worst case scenario, mandate federal controls.[15] Further, an area faces penalties, such as the withholding of federal highway funds, for not abiding by an SIP.[16] SIPs are intended to provide flexibility by allowing each area to have a separate plan to achieve attainment, while also being restrictive given the requirement of federal approval dependent upon the SIP satisfying requirements outlined in the 1990 CAAA.

The set of requirements for an SIP become more restrictive as the non-attainment status becomes worse. For clarity, "worse" here refers to a greater failure of the attainment standard. For example, the Severe non-attainment designation requires more stringent controls and rules be enacted than Marginal non-attainment designation. As an example of the requirements for an SIP, the restrictions on mobile sources require mandatory vehicle inspections but under guidelines wihth some flexibility for Marginal areas, installation of gasoline vapor recovery systems for Moderate areas, specific guidelines for emissions testing during inspections for Serious areas, a plan to control work related vehicle miles traveled for Severe areas and a plan to control traffic during heavy traffic hours in Extreme areas. Further, the requirements are monotonically increasing: Worse non-attainment categories are subject to all of the requirements of the better non-attainment categories in addition to their own.

Separately from the SIP requirements, restrictions on pollution sources exist which also vary by non-attainment status. For example, new "major sources" of pol-

[15] Recently in 2003, a Federal Implementation Plan (FIP) was imposed on Laurel, Montana after the EPA partially disapproved the SIP for SO2 (Code of Federal Regulation - 40 CFR 52).

[16] An example of withholding of federal highway funds: Loftis, Randy Lee "EPA threat on smog is most Serious yet, Possibility of losing millions of dollars in funding could stir legislative action" The Dallas Morning News May 5, 1999

lution in non-attainment areas must install technology meeting the lowest achievable emissions rate (LAER), with little or no regard to economic feasibility, as well as offset the emissions generated. A major source is any new source with the potential to emit more than 100 tons of pollution per year for Moderate and Marginal areas, decreasing to 75, 50 and 25 tons per year for Serious, Severe and Extreme areas respectively.[17] Further, the emissions offsets used to satisfy the major source regulation begin at a ratio of 1.1 to 1 (offset to emitted) for Marginal areas and increase to 1.15, 1.2, 1.3 and 1.5 to 1 for Moderate, Serious, Severe and Extreme non-attainment areas, respectively. In comparison to non-attainment areas, the definition of a major source in an attainment area may be relaxed to include only pollution sources emitting more than 250 tons per year. Further, a major source in an attainment area must only install the best available control technology (BACT), which considers the economic aspects of the new pollution source.[18]

It is important to point to a second dimension of the non-attainment regulations that does not become more restrictive as the non-attainment categories become worse: the attainment status deadline. The deadline for achieving attainment with an ambient concentration of 0.12 ppm relaxes with the degree of attainment failure: three years for Marginal, six years for Moderate, nine years for Serious, fifteen years for Severe and twenty years for Extreme. The consequences of failing a deadline consist of non-attainment category reassignment and the requirement to submit a

[17]A major source may be an existing source which is expanding operations.

[18]It is not clear how the decision to relax the definition of a major source is made, the EPA only discloses that it is "depending upon the source" http://www.epa.gov/nsr/info.html.

new SIP.[19] The 1990 CAAA also included "milestones" in order to verify that areas under non-attainment categories with longer deadlines had effective SIPs.[20] The main milestone comes six years after the enactment of the Act, with additional milestones every three years thereafter. The six year milestone is a requirement for all non-attainment areas, other than Marginal, to reduce volatile organic compound (VOC) emissions which contribute directly to ozone formation, by 15% from the emissions level in 1990.[21]

1.3 Previous Literature

Henderson (1996) first evaluated the impact of the CAA on ozone pollution using a fixed effects model with monitor-level panel data from 1977, 1982, 1985 and 1987. The impact of a non-attainment status dummy variable was measured against the natural log of four dependent variables: the second highest daily maximum emission reading for the year, the mean annual reading, the median daily maximum in July and the mean July reading. Of the four pollution measures, ozone non-attainment only had a significant impact (5% confidence level) when median daily maximum readings in July were used as the outcome variable. The reduction in ozone attributed to non-attainment status, when significant, was 8%.

A fixed effect model identifies the impact of non-attainment status through areas that change non-attainment designation over the time period of the analysis. For Henderson (1996), 18% of his sample changed attainment status. A lack of

[19] CAA 181.b.2

[20] CAA 182.g

[21] CAA 182.b.1. Marginal areas were under a 3-year deadline to achieve attainment.

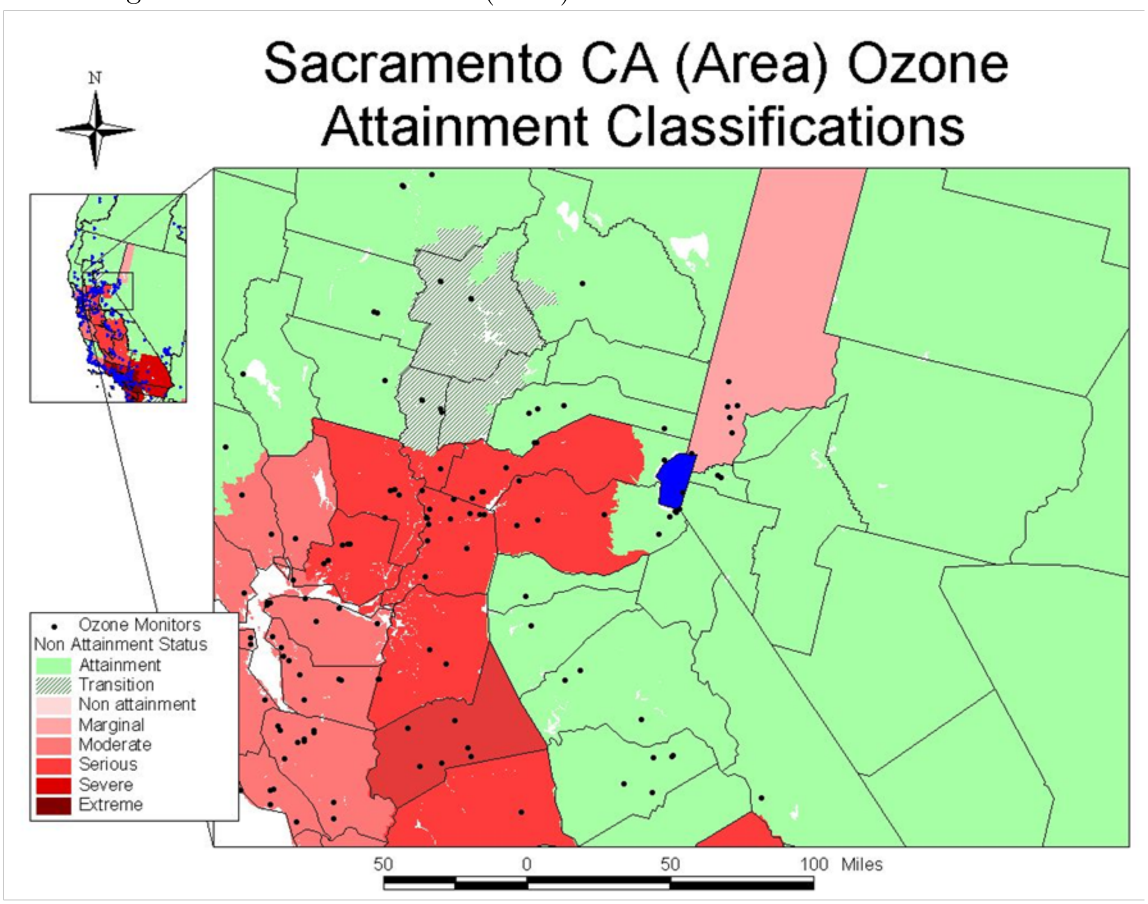
change in non-attainment status led Greenstone (2004) to use regressions with fixed effects only at the state-level to evaluate county-level annual mean SO2 measurements over three separate six-year periods: 1975 - 1980, 1981 - 1986 and 1987 - 1992.[22] Greenstone analyzed the impact that the fourth year's attainment status (1978, 1984 and 1990 respectively) had on one, two and three year changes in county-level SO_2 measurements, relative to the middle year of each period (1977, 1983 and 1989 respectively). Of Greenstone's results, only those from the final six-year period are significant and suggest non-attainment status caused a 7-11% decrease in SO_2.[23]

Both Henderson (1996) and Greenstone (2005) label counties with both non-attainment and attainment portions as being in non-attainment. This occurs because assignment level is at the area, not county, level and area boundaries may not follow county boundaries. It is not clear how severely this affects their estimates but it is clear that it may. County level assignment will affect estimates if two areas with different non-attainment statuses exist within a county, each with its own ozone monitor. For example, consider figure 1.1 which displays the San Francisco and Sacramento region of California with the multiple 1990 non-attainment classifications given by the legend, county boundaries delineated by lines and monitor locations indicated by black dots. The county labeled "A" is El Dorado, California and the different colors of the county show the two non-attainment designations which split the county in two areas, each containing monitors. We refer to these

[22]Greenstone made other determinations when analyzing the data: using the 2nd highest emissions level rather than DV although DV is not always second highest and reducing the sample to those only report 75% of the time or more when the DV is adjusted (still measured) for such areas.

[23]Insignificant results from other periods suggest that up to a 17% decrease may be attributable to non-attainment status..

Figure 1.1: Sacramento CA (Area) Ozone Attainment Classifications



counties as either "split" or "partial" counties and use them as a level of observation in place of counties. Only thirty-eight of the more than 3000 counties of the United States contain more than one area, but monitors are not located in every county. Restricting the sample of counties to the 338 counties with reported monitor readings in 1990 and 2004, 25 contain multiple areas and they are often heavy polluting counties such as Los Angeles. In order to avoid any possible bias, area boundaries given by the Code of Federal Regulations were carefully accounted for in our analysis.

An issue similar to incorrectly assigning attainment status according to county boundaries was brought up by Auffhammer, Bento and Lowe (2005). During conversations with air quality management district officials, Auffhammer et al. were told that cities within the same non-attainment area may face different regulatory restrictions due to targeting that is not explicitly part of the CAA. The air quality management district officials indicated that cities with higher pollution were targeted to bring about the largest health benefits and cause a pollution reduction in line with the NAAQS. Auffhammer et al. (2005) accounted for this in their analysis by comparing county level and sub-county level assignment and their results support an attenuation bias: an insignificant decrease in average PM_{10} emissions from 1990 to 2000 in California is attributable to non-attainment status at the county level, while a significant decrease is found when city level non-attainment status is used in the analysis. Although Auffhammer et al. provide a reasonable solution to determine the level of regulation within an area, it remains to be determined what observation level should be used to analyze the CAA.

The design of the 1990 CAAA implies that a decline in area level ozone, specifically area design values, was the primary target of the amendment regardless of what the larger intent of the EPA may have been. Because area design values were the primary target, using an area level observation would best determine the effectiveness of the 1990 CAAA in achieving its goal. However, from a research stand point, change in ambient ozone concentrations at the monitor level would be appropriate to gauge the impact of the CAA more broadly, so long as regulation is appropriately accounted for as in Auffhammer et al. (2005). For these reasons, results are presented here for both changes in area and monitor level emissions. We also report results at the county level in order to provide comparisons to previous research.[24]

1.4 Research Design

We use the variation in regulation between non-attainment categories in order to determine the impact of increased pollution restrictions by comparing changes in ozone pollution over time between "neighboring" categories. Neighboring categories refer to categories which share a cutoff point, such as Marginal and Moderate, and Serious and Severe. The analysis focuses on neighboring categories because of the RD estimation procedure used but regression results are also reported which do not limit the results to neighboring categories. Because the design value is the EPA's

[24]Because assignment occurs at the area level, counties cannot be used to determine the effectiveness of the CAA. Using counties as the level of observation would be valid to determine the general impact of the CAA if a county level attainment status was determined and regulation was assumed to have been uniformly applied to all monitors within a county. However, Auffhammer et al. (2005) suggests that targeting occurred within counties.

targeted ozone pollution measurement, focus is placed on the change in design values although the change in average ambient ozone is also considered. The RD estimation procedure ensures that observations that are similar (other than treatment) are compared, but percentage changes are used in order to remove any starting point bias that may come from areas with high initial levels of pollution. Pollution measurements for 1990 are used as the initial year for percentage calculations because 1990 was the last year before the 1990 CAAA would have had an impact. However, two different time periods are used in order to address the two dimensions of each category's regulation: restrictions and attainment deadline.

It is necessary to compare the non-attainment categories of the 1990 CAAA, rather than compare the 1990 CAAA with the CAA prior to 1990, because no areas remained under pre-1990 non-attainment regulation after 1990.[25] Without areas under pre-1990 regulation there are no control observations so that the overall impact of the 1990 CAAA cannot be directly assessed without making assumptions and simulating pollution declines.[26] Instead, the question addressed here, by comparing different categories, is what relative impact the different sets of regulations had on pollution. This question is notably differen from a direct determination of the effectiveness of the 1990 CAAA but does not preclude all discussion because the variation in regulation creates testable hypotheses which allow inference concerning the Act's effectiveness. The main hypotheses stems from the assumption which

[25]Attainment areas prior to 1990 remained under the same regulation post 1990 if they remained in attainment, however, it is a comparison of pre-1990 non-attainment regulations and post-1990 non-attainment category regulations that would be of interest.

[26]U.S. EPA (1997 and 1999) used assumptions and simulation to analyze the impact of the CAA.

follows from regulations in more restrictive areas containing all of the regulations in less restrictive areas (and thus being more restrictive with certainty):

H1: *if the 1990 CAAA were effective, areas under more restrictive regulations should have resulted in declines in ozone that were no smaller in magnitude than areas under less strict regulation, ceteris paribus.*

In order to test this main hypothesis, the two dimensions of the non-attainment regulation are addressed by using different outcome variables. Each outcome variable is a percentage change rather than an absolute change. Percentage changes are used in order to not bias the reduction due to an observations inital level of pollution. This bias may occur due to variation in the difficulty of reductions related to the existing pollution level. The decision to use percentage changes does not perfectly coincide with the main hypothesis above but we feel that correcting for the initial level of pollution is important.

The main outcome variable is the percentage change in design values over deadline specific time periods. This outcome is represented as: $y^D_{NA} = \frac{O_{T_{NA}} - O_{90}}{O_{90}}$, where NA denotes which non-attainment classification the observation falls under, T_{NA} is either the appropriate deadline year for non-attainment classification NA or the cutoff year of 2004, whichever is greater, and O_t is a general representation for an ozone measurement in year t (i.e. either a design value or average ambient ozone). As an example, the percentage change in the design value from 1990 through 1993 is used for observations in Marginal non-attainment, $y^D_{Marginal}$, while the change in the design value from 1990 through 1996 is used for observations in Moderate non-attainment $y^D_{Moderate}$. The different lengths of time are for a specific outcome measurement and results with outcomes using a uniform length of time are also discussed. Although this "deadline" analysis will provide an understanding of how the 1990 CAAA categories compared in reducing pollution, it will not separately identify the impact of regulatory restrictions from the different amounts of time provided under each category's deadline.

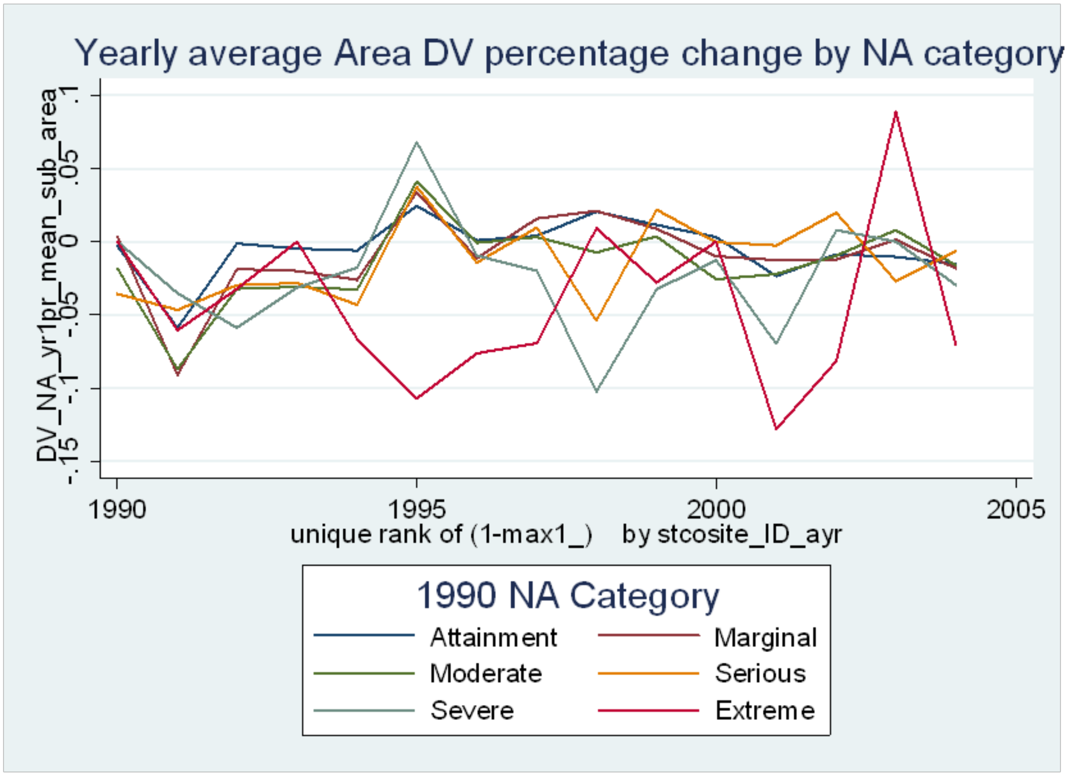
In order to determine the impact of each category's deadline separately from the increased SIP requirements and restrictions, we would need observations that vary in restrictions under the same attainment deadline as well as observations under the same restrictions but different attainment deadlines. The six year milestone mentioned previously most resembles the former scenario because each non-attainment category is under the same six year time deadline for a reduction in the ozone contributing VOC pollutant. Because a better opportunity does not exist to exploit varying restrictions under the same deadline and VOC levels have a direct impact on ozone pollution, we use the six year milestone to consider the impact of increased restrictions by analyzing the percentage change in design values from 1990 to 1996. This second outcome of interest is given by:

$$y_{NA}^{M} = \frac{O_{96} - O_{90}}{O_{90}}$$

. This "milestone" outcome variable provides the most direct test of whether areas under more restrictive regulation reduce pollution by no less than areas under less restrictive regulation.

It is possible that the different non-attainment deadlines impact an area's

Figure 1.2: Yearly Average Area DV Percentage Change by NA Category



pollution reduction timeline, and thus would influence an analysis using milestone outcomes by confounding the impact of the restrictions with the impact of the deadlines.[27] Unfortunately, observations varying by attainment deadlines but under the same restrictions do not exist. A brief look at how emissions reductions fluctuate over time addresses some of the concern that deadlines influence an area's pollution reduction over time. Figure 1.2 plots one year average percentage changes in area-level design values across time for the different non-attainment groups, with the percentage changes from year t to $t+1$ plotted in year t. The figure shows roughly similar declines over time with the exception of the Extreme category and the Severe

[27]Theoretically, it is not clear what impact different levels of regulation have on the time taken to reduce emissions. See Appendix A for a theoretical model.

category around 1998.[28] A regression of one year average changes on yearly fixed effects, non-attainment categories and interactions of year and category, supports these graphical inferences with the only significant difference being the Severe category in 1998 and a significant increase in the Serious category in 2002. Neither of these differences provides strong evidence to suggest that deadlines do have a substantial influence on when pollution is reduced and provides some evidence that deadlines do not substantially influence the interpretation of the "milestone" analysis. Again, there is unfortunately no way to fully separate the two dimensions of the non-attainment category regulations.

The focus of this analysis is on comparing area design values which are the ozone measurement of interest to the EPA. However, a percentage change in average ambient ozone levels could also be evaluated to consider Henderson's (1996) earlier results suggesting that ozone reductions follow the "letter of the law" rather than the intent of the law. However, this finding no longer holds as shown in figures 1.3 and 1.4 which are comparable to figures 1.1 and 1.2 given by Henderson (1996) but reflect our more recent data.[29] Figures 1.3 and 1.4 display shifts *downward* in both the distribution of the design value and of one-hour average annual means, indicated by the solid line for 1990 and the dashed line for 2004.[30] As in Henderson (1996), there

[28] A regression of one year average changes on yearly fixed effects, non-attainment categories and interactions of year and category, supports these graphical inferences with the only significant difference being the Severe category in 1998 and a significant increase in the Serious category in 2002.

[29] Only monitors, counties or areas with ozone measurements in both years were used to avoid biasing the results due to removal of existing and placement of new monitors (Greenstone(2004)).

[30] Figure 1.3 and 1.4 restrict the sample size to those monitors with design value measurements for 1990 and 2006 and annual mean measurements in 1990 and 2005. Each average is given by the vertical lines of the corresponding solid or dashed line in the figures. The vertical dotted line marks 0.12 ppm standard in figure 1.3.

Figure 1.3: Distribution of Annual Mean Monitor DVs

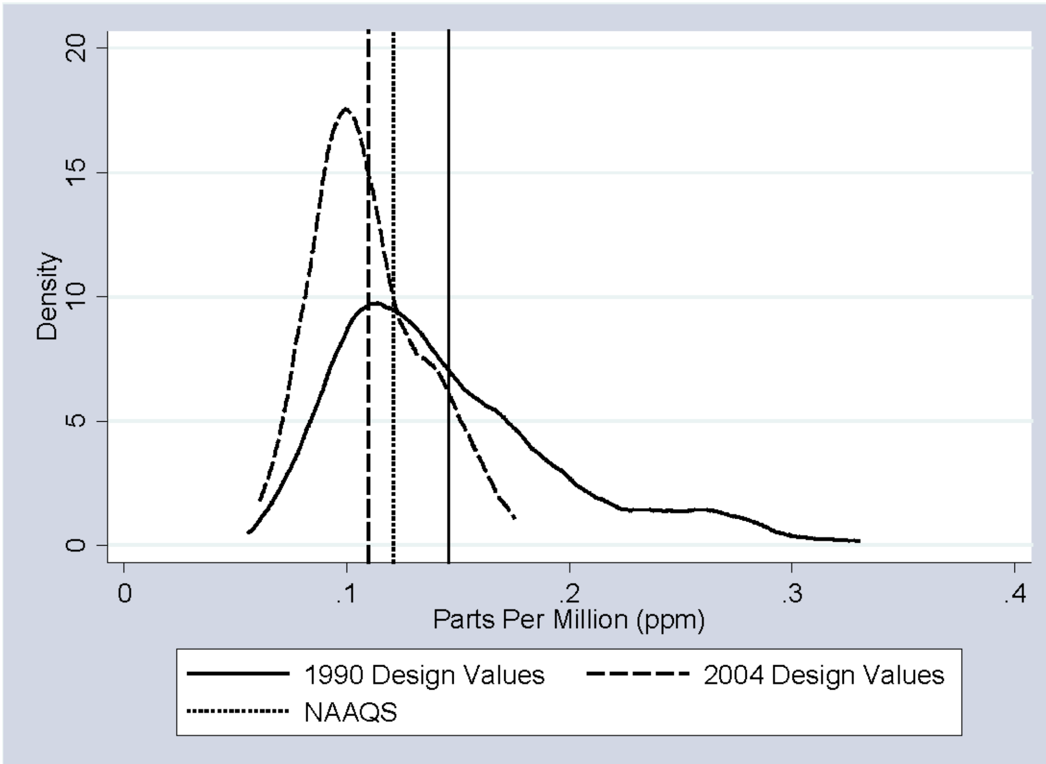
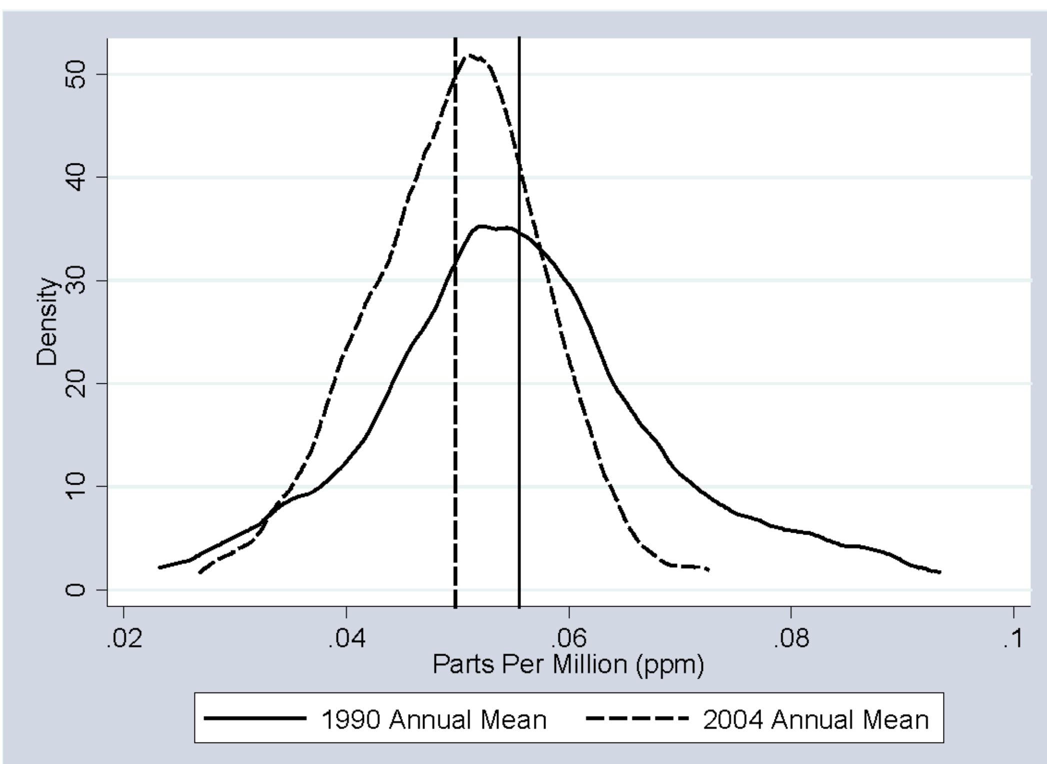


Figure 1.4: Distribution of Annual Mean Monitor One-Hour Averages



is a statistically significant decline in the ozone measurement of primary interest to the EPA, but unlike Henderson (1996) the more recent data also shows a decline in the annual hourly-average. Our analysis indicates that the average design value declines significantly from 0.146 ppm to 0.110 ppm, between 1990 and 2004. This is accompanied by statistically significant decline in the average annual mean from 0.056 to 0.050, between 1990 and 2004. These results suggest that areas are now meeting the intent of the CAA and not just the "letter of the law."

There are some concerns to this research design. First, using the 1990 design value as a control variable used to designate areas into a non-attainment category as well as part of one of the outcomes would typically cause endogeneity issues of simultaneity. However, this type of endogeneity is not an issue here due to the exogeneity of the 1990 CAAA assignment rule and the RD estimation technique used. The assignment rule would not have been exogenous had areas known of it well enough ahead of time to react and adjust emissions to achieve a certain non-attainment designation. It is unlikely that areas could affect their initial designation in this way considering the events leading up to the 1990 CAAA.[31]

[31] As early as the end of the previous deadline for meeting the ozone standard, December 31, 1987, there was talk of amending the CAA to include variation in deadlines dependent on the degree of failure. However, it appears no information concerning the actual non-attainment designations was known (M. Wald "E.P.A. Plans Flexible Deadlines on Air Pollution" New York Times November 11, 1987). Information concerning the designation of non-attainment status first became known during the year and a half long process, starting on June 12th 1989 when President Bush proposed the changes, of signing the amendments into law on November 15, 1990. However, over that time the cutoff points for the non-attainment categories varied and were not given as final until May of 1990 (Version two of the House bill: 101 H.R. 3030.). Further, the monitor readings used to form the first designation was typically taken from 1987 through 1989, making it unlikely that any area could attempt to alter its readings to fall in a desired non-attainment designation (Federal Register 40 CFR 81 Vol. 56 No. 215 November 6, 1991 pg. 56697). Thus, the group designations may be considered an exogenous event though the signing of the 1990 CAAA and the likelihood of multiple non-attainment designations was known ahead of time. More importantly, it is unlikely that areas would ever have been able to alter the monitor readings themselves.

Second, although it is reasonable to assume that areas could not select their initial non-attainment designation, areas may change non-attainment categories after the 1990 designation. Areas changed non-attainment categories after 1990 mainly in response to changes in ozone pollution, but the EPA did approve the Sacramento (California) Metro Area's proposal to be moved from a Serious to a Severe non-attainment classification in 1995.[32] This selection into a higher category is troubling but appears to have only occurred for Sacramento. Obviously, any change in attainment status alters the level of regulation an area is subject to. Because of the possibility of changes in non-attainment designation after 1990, the results presented here must be interpreted as the impact of the regulations from the *initial* non-attainment assignment in 1990.

Finally, the 0.121 ppm attainment level presents two issues. First, this attainment level existed prior to the 1990 CAAA and areas may have already initiated plans to decrease emissions. Any trend of decreasing concentrations that existed before 1990 may confound the analysis, however, given our analysis this would only be true if the trends were correlated with the multiple breaks in regulation of the new non-attainment groups. Second, because each non-attainment group has the same goal of attainment at 0.121 ppm, it is unlikely that there will be very large differences

[32]The 1994 California SIP states:"(Sacramento) proposes a bump-up in classification from Serious to Severe, which allows the region to design a strategy that attains by 2005. Although every effort was made to show attainment by the Serious area deadline of 1999, no feasible control strategy could be identified to meet that timeframe. The full benefits of new state and federal measures, which are dependent on vehicle turnover, will not be realized by 1999. Because of this, extraordinary local measures would have to be imposed in the Sacramento region to meet a 1999 deadline. These measures would cause Severe disruptions to the regional economy and were therefore deemed infeasible." Approval is recorded in the Federal Register, February 9, 1996 Vol. 61 No. 28 pp. 4887 - 4890.

in outcomes across non-attainment categories. That is, because the estimation procedure focuses on observations with similar starting points and the attainment goal is the same across non-attainment categories at 0.121 ppm, differences across non-attainment groups are expected to be small (especially for the deadline outcome, y^D).

1.5 Data

To the best of our knowledge, all previous studies of non-attainment status with the CAAAs and their impacts on improvements in ambient air quality have utilized county-level non-attainment measures. A closer inspection of the Code of Federal Regulations (CFR) (1991) reveals that some non-attainment designations do not adhere strictly to the county boundaries.[33] In these cases, the spatial characteristics of the non-attainment designations are not based on political boundaries or subdivisions, but rather on major roads, rivers, national forests, or air quality management areas (airsheds), to name but a few. Regardless of the divergence from the sociopolitical boundary, the inaccurate assignment of non-attainment boundaries from the CFR may have led to the incorrect assignment of non-attainment classifications to the monitors within those boundaries.

A major contribution of this paper is that the CAAA non-attainment area

[33]For example, El Dorado County in California is Serious non-attainment for ozone, however there are portions that are in attainment. Specifically: "All portions of the county except that portion of El Dorado County within the drainage area naturally tributary to Lake Tahoe including said Lake" (CFR 1991). In this specific case, the master list of monitors includes four monitors in the attainment "drainage" area of Lake Tahoe, and four monitors located in the Serious non-attainment portion of the County.

boundaries have been correctly accounted for in the data by using GIS tools and the exact area descriptions reported in the CFR.[34] Specifically, any areas that have attainment or non-attainment designations that deviate from the official county boundaries outlined by the United States Geological Survey in the National Atlas were hand-coded into the GIS.[35] Using the correct non-attainment boundaries and the specific geographic coordinates for each ambient air quality monitor, the attainment status of each area was used to correctly assign non-attainment status to the monitors within those boundaries.[36]

For each monitor, the four highest one-hour ozone levels for each year from 1987 through 2004 were obtained from the EPA. These values were used to calculate design values according to an EPA memorandum from June 18th, 1990 (details in Appendix A). A monitor's design value for a given year is determined using one-hour average ambient ozone readings from the previous three years. The one hour averages from the past 3 years are ordered by size and, depending on how often the monitor was measuring ambient ozone, the 4_{th}, 3_{rd}, 2_{nd} or 1_{st} largest one hour average is used as the design value.[37] The EPA uses the highest design value reading from all the monitor design values within an area as the area's design value for that

[34]As recorded for each state in Title 40 Part 81of the CFR.

[35]By county, we mean any "primary" or "first-order" subdivision of a State. Some States refer to their primary subdivisions by names other than "counties", including parishes, election districts, boroughs or independent cities. In these cases we treat the parish, election district, borough or independent city as we do the county in the remainder of our sample. For ozone non-attainment, the States that presented multiple attainment and/or non-attainment classifications within the same contiguous counties were Arizona, California, Colorado, Connecticut, Illinois, Kentucky, Maine, New Hampshire, New Mexico, New York, North Carolina, Oregon, Virginia and Washington.

[36]Only thirty-eight of the more than 3000 counties of the United States contain more than one area, but monitors are not located in every county. Restricting the sample of counties to the 338 counties with reported monitor readings in 1990 and 2004, 25 contain multiple areas and they are often heavy polluting counties such as Los Angeles.

[37]See Appendix A for complete details on design value calculation.

year. The highest monitor readings are used for the design value because scientific studies suggested that short periods of highly concentrated ozone exposure posed the most significant threat to public health and the environment.[38]

The area level design value was used as the assignment value for all areas except those assigned by a Metropolitan Statistical Area (MSA) "grouping-rule". The MSA grouping-rule was developed to assign areas within the same MSA or Consolidated MSA (CMSA) to the non-attainment status of the worst non-attainment area of that MSA or CMSA, if the worst area was in Serious, Severe or extreme non-attainment.[39] However, the MSA rule could be avoided if sufficient evidence was provided by state agencies indicating that an area within the MSA would not contribute significantly to ozone. As an example, the areas with lower pollution within the Los Angeles-Riverside-Orange County CMSA were considered separately from the area within the CMSA containing the South Coast Air Quality Management District.[40]

In addition to non-attainment classifications and ambient ozone concentrations, we also utilize data from the 1990 census. Census variables serve two related purposes: 1) a balance check of how the treatment and control samples compare and 2) controls for parametric regressions which allow for use of more observations. Tract level data from the 1990 census, broken down by non-attainment category and

[38]As detailed by the EPA, the NAAQS were established and updated based on a lengthy review process which included use of current scientific studies and assessments by interested parties. It is important to point out that some research indicates health impacts non-linear in exposure to ambient concentrations (Hazuchaa and Lefohnb (2007)).

[39]CAA Section 107.d.4.A.iv

[40]Although it is important to be aware of the MSA grouping-rule, it only appears to have been applied in two cases: 1) the cleaner area within the Los Angeles-Riverside-Orange County CMSA received a Severe non-attainment assignment although its calculated design value would have designated it as Serious, and 2) an area within the Boston-Worcester-Lawrence CMSA received a Severe non-attainment assignment rather than a Moderate.

aggregated to the partial county and area level, is displayed in table 1.1.

Census variables included in table 1.1 are those that are believed to in some way influence the production of ozone precursor pollutants, including nitrogen oxides (NO_x) and volatile organic compounds (VOCs) which produce Ozone (O_3) through a photochemical process. VOCs are found in a number of household products, such as paints, glues and printers, and are produced during the manufacturing of these products as well as many other chemicals. A majority of NO_x pollution comes from agricultural production and mobile sources.[41] Contributors to VOCs and NO_x thus include agents in the local economies, and other human pollution sources, while inhibitors include social-political variables that would decrease these pollutants indirectly.

1.6 Estimation Procedure: Regression Discontinuity

In addition to linear regression, Greenstone (2004) used propensity score matching to evaluate how non-attainment status impacted SO_2 concentrations. Greenstone used propensity score matching rather than regression discontinuity because he determined that non-attainment status was not "mechanically assigned". However, the inability to determine the assignment rule may have been a result of assigning non-attainment status at the county level. Although assignment is not exact, figures 1.5 , 1.6 and 1.7 demonstrate that the 1990 CAAA assignment rule for ozone is more closely followed when the appropriate area level is considered rather than the partial-county level.

[41] U.S Emissions Inventory 2005, EPA.

		County and Partial County						Area					
Attainment Status:		A	MA	MO	SR	SV	EX	A	MA	MO	SR	SV	EX
Number of Observations:		179	53	84	52	53	4	165	39	33	12	11	1
Race	Hispanic	4.87%	1.79%	5.25%	9.75%	8.94%	28.48%	4.69%	2.98%	5.26%	11.49%	9.15%	37.84%
	Black	10.91%	9.00%	9.59%	12.27%	12.03%	6.33%	11.27%	9.81%	11.52%	10.32%	10.37%	10.81%
	Amer. Ind.	1.43%	0.34%	0.47%	0.45%	0.30%	0.54%	1.48%	0.44%	0.87%	0.61%	0.57%	0.32%
	Asian	1.47%	1.01%	2.41%	2.82%	2.59%	7.06%	1.54%	1.34%	2.07%	2.08%	2.97%	10.63%
	Other	0.05%	0.06%	0.07%	0.19%	0.10%	0.19%	0.05%	0.06%	0.07%	0.08%	0.08%	0.24%
	White	81.28%	87.81%	82.22%	74.53%	76.04%	57.40%	80.97%	85.37%	80.21%	75.42%	76.85%	40.16%
Commute Time	<20mins	54.99%	53.15%	47.27%	47.00%	42.93%	40.74%	55.43%	53.58%	50.77%	47.67%	44.65%	36.71%
	20 to 60	37.82%	40.21%	45.56%	44.17%	45.45%	45.28%	37.44%	38.62%	41.05%	43.30%	44.61%	52.63%
	>60	3.92%	3.68%	4.38%	6.03%	9.13%	11.27%	3.92%	4.49%	4.82%	6.17%	7.58%	7.92%
Mode of Transportation	Private	90.52%	90.51%	90.39%	87.71%	83.37%	90.64%	90.38%	89.02%	87.58%	87.47%	86.09%	85.96%
	Public	1.05%	1.62%	2.73%	4.46%	8.89%	2.68%	1.13%	2.46%	3.99%	4.85%	5.32%	6.59%
	Other	5.16%	4.91%	4.09%	5.03%	5.26%	3.97%	5.28%	5.21%	5.07%	4.82%	5.42%	4.70%
	None	3.27%	2.96%	2.79%	2.80%	2.48%	2.72%	3.21%	3.31%	3.36%	2.86%	3.16%	2.75%
	Public_rail	0.02%	0.08%	0.36%	1.79%	4.76%	0.04%	0.06%	0.73%	1.40%	2.14%	2.40%	0.06%
Age of residents	1 to 6	10.36%	9.78%	10.20%	10.48%	10.36%	11.81%	10.40%	9.92%	10.01%	10.29%	10.31%	11.11%
	7 to 13	10.53%	9.70%	10.03%	9.85%	9.85%	10.40%	10.54%	9.85%	9.85%	10.23%	9.93%	9.64%
	14 to 18	7.11%	6.69%	6.75%	6.61%	6.64%	6.77%	7.12%	6.78%	6.72%	7.07%	6.75%	6.78%
	19 to 24	8.66%	8.75%	8.67%	9.53%	8.71%	9.69%	8.71%	8.77%	8.59%	8.77%	9.19%	10.54%
	25 to 39	24.88%	24.78%	25.90%	26.71%	26.16%	27.62%	24.81%	24.73%	25.17%	25.00%	25.77%	28.19%
	40 to 69	30.20%	31.06%	30.28%	29.33%	30.67%	27.09%	30.08%	30.80%	30.63%	30.17%	30.02%	27.29%
	70 and up	8.26%	9.25%	8.17%	7.50%	7.62%	6.63%	8.33%	9.14%	9.02%	8.40%	8.04%	6.45%
Household Income	< 30k	54.04%	52.36%	46.09%	44.25%	38.71%	39.81%	54.03%	53.77%	49.74%	50.01%	45.05%	43.05%
	30 to 75k	39.79%	40.76%	44.40%	44.35%	46.64%	45.35%	39.70%	39.22%	41.71%	40.25%	43.36%	41.55%
	75 to 140k	5.26%	5.84%	8.02%	9.84%	12.20%	12.39%	5.33%	5.86%	7.14%	8.19%	9.84%	12.30%
	> 150k	0.91%	1.05%	1.48%	1.56%	2.45%	2.44%	0.94%	1.15%	1.41%	1.55%	1.75%	3.10%
Housing values	< 50k	33.10%	28.23%	21.80%	11.15%	11.43%	1.54%	33.27%	31.74%	28.50%	27.46%	20.65%	1.51%
	50 - 100k	46.89%	44.67%	43.11%	27.75%	30.78%	13.69%	46.75%	40.61%	39.27%	34.07%	29.39%	6.07%
	100 - 250k	17.65%	24.46%	25.96%	50.93%	44.39%	56.07%	17.47%	23.60%	24.85%	30.62%	36.64%	49.48%
	250 - 500k	2.02%	2.29%	7.43%	8.83%	10.56%	23.04%	2.13%	3.41%	6.08%	6.61%	11.09%	30.92%
	> 500k	0.34%	0.34%	1.70%	1.34%	2.85%	5.66%	0.38%	0.63%	1.30%	1.24%	2.23%	12.01%
Selected Industries	Man. N. D.	7.84%	7.40%	7.06%	6.54%	7.52%	5.40%	7.87%	7.91%	7.54%	7.41%	6.07%	7.43%
	Man. D.	10.84%	11.70%	12.42%	8.21%	10.38%	12.73%	10.88%	11.32%	12.42%	10.74%	10.83%	13.05%
	Agricultural	3.70%	2.30%	2.25%	3.56%	1.70%	2.35%	3.59%	2.98%	3.05%	2.68%	3.08%	1.28%
	Mining	0.78%	0.48%	0.29%	0.38%	0.29%	0.17%	0.77%	0.67%	0.46%	0.78%	0.48%	0.16%

Each cell reports the demographics for census tracts within a county, parital county or area. "Partial" counties refer to those counties made partial by the boundary of the EPA areas. Attainments status are abbreviated as follows: Attainment (A), Marginal Non-Attainment (MA), Moderate Non-Attainment (MO), Serious Non-Attainment (SR), Severe Non-Attainment (SV) and Extremem Non-Attainment (EX). The sample restricted to counties and areas with monitors that report emissions in both 1990 and 1996.

Table 1.1: County or Partial County and Area Demographics by Attainment Status (1990 Census)

Figure 1.5: 'First Stage' Monitor Level

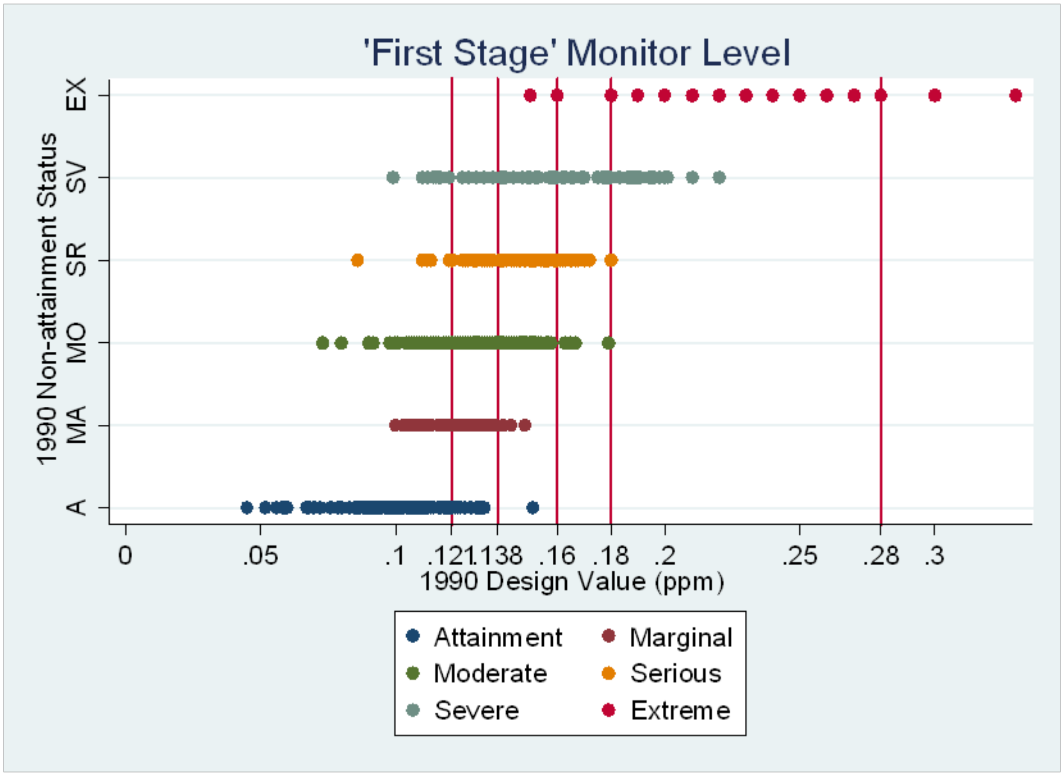


Figure 1.6: 'First Stage' County/Split Level

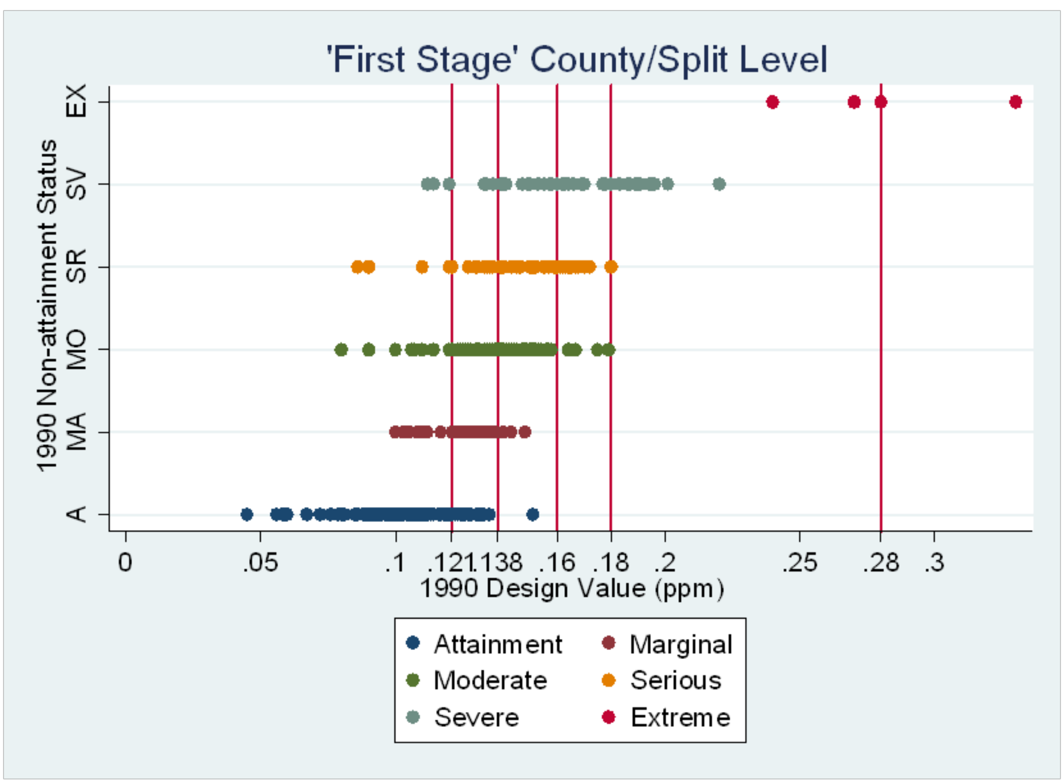
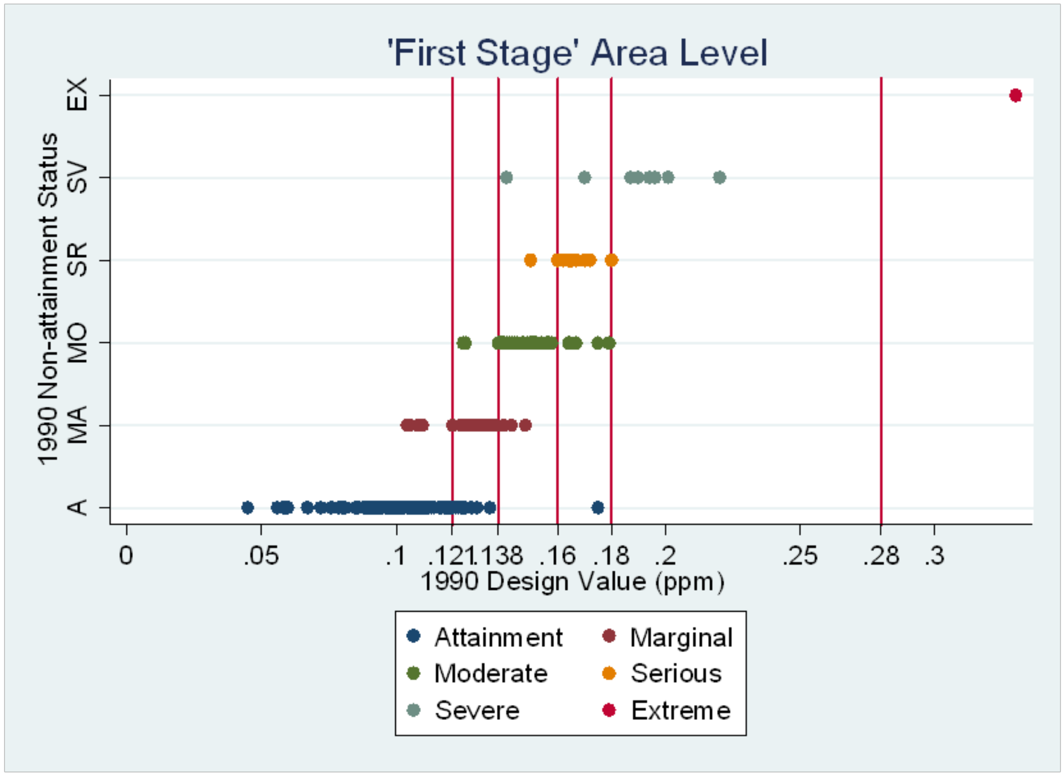


Figure 1.7: 'First Stage' Area Level



Figures 1.5 , 1.6 and 1.7 relate our calculated 1990 design values on the horizontal axis to 1990 non-attainment (NA) status on the vertical axis. The figures differ by the observation level used. Figures 1.5 is at the monitor level, figure 1.6 is at the county of partial county level and figure 1.7 is at the area level. Thus, each dot on the figures represents either a monitor (figure 1.5), a county (figure 1.6) or an area (figure 1.7).The sample for each graph is restricted to observations with design values in both 1990 and 1996.[42] As mentioned previously, non-attainment assingment is at the area level and area boundaries may not follow county bound-

[42]As pointed out previously by Greenstone (2004), monitors are placed in areas of high pollution and removed from areas of low pollution over time. The sample is restricted to avoid bias because monitors are more likely to be removed from areas achieving attainment. 1990 and 1996 are chosen here because that is the one of a difference in those years is one of the outcomes of interest, other combinations tell the same story.

aries. The monitor level is the most disaggregated as there may be multiple monitors within a country and multiple counties within an area. The number of observations in each figure typically decreases as the level of observation aggregates because partial county and area design values are calculated as the highest monitor design value within the county and area respectively.[43]

If ozone assignment followed the 1990 CAAA perfectly, each non-attainment category in figures 1.5 , 1.6 and 1.7 would only show observations between one set of vertical lines denoting a change in category, also known as the discontinuity points: 0.121, 0.138, 0.160, 0.180 and 0.280 ppm. For example, all design values falling between 0.138 and 0.160 would be given a Moderate non-attainment designation and those would be the only Moderate non-attainment designations. As can be seen in figure 1.6, there is overwhelming evidence that observations do not follow the assignment rule at the county level, which is the level of assignment used by Greenstone (2005). However, figure 1.7 shows that instances of observations not assigned according to the assignment rule drop dramatically when area level assignment is used. This suggests that the 1990 CAAA assignment rule is followed and RD is an appropriate method of analysis so long as the correct level of assignment is used.

Figure 1.7 does show that some areas do not appear to be assigned according to the 1990 CAAA assignment rule and our calculated design values. This "mis-assignment" may occur if the EPA occasionally assigned areas by unobservable or

[43]It is possible for the observation count to increase because some partial-counties and areas will have different monitors that start a period and end a period which will individually not be included in the monitor count because each monitor alone does not both start and end a period. Another caveat is that two counties split after the 1990 designation: Charles City County, VA and King County WA; they have been included in the analysis as non-partial counties because our analysis is as of the 1990 designation.

observable attributes other than or in addition to an area's design value. It is not possible to know for certain what may have led the EPA to this decision but one can imagine the EPA not designating an area into a more restrictive category if the area was subject to a severe weather event that impacted the photochemical production of ozone in the years leading up to 1990. Fortunately, selection by unobservable and observable variables is addressed by using a "fuzzy" RD design, which weights a strict RD estimate according to the "fuzziness", or the number and degree of mis-assigned observations around the discontinuity point.

Further, although propensity score matching accounts for selection on observables and appears to be a natural choice for dealing with a fuzzy RD design, Imbens and Lemiuex (2008) point to the ignorability assumption underlying propensity score matching as a reason to avoid using matching when an assignment rule exists. The essence of the argument is that a fundamental difference must exist between two observations with similar design values that were not placed in the same category by the assignment rule. If the difference that led to the placements is expected not to impact an outcome, than the ignorability assumption needed for propensity score matching is understandable. However, in the case of assignment to a regulation, it is hard to imagine the unobserved difference not affecting the reduciton of pollution as that is likely why the EPA chose to place the areas in different categories although they had similar design values.[44]

[44] Battistin and Rettore (2008) work through a divergent though related use of regression discontinuity. They suggest that the known discontinuity in an RD design can be used to validate the ignorability assumption away from the discontinuity point by testing for selection bias once covariates are controlled for around the discontinuity. Essentially, covariates are used to control for the unobserved differences that Imbens and Lemiuex (2008) are concerned with. It is important to point out that this argument is only made for instances where only one set of non-compliers exists:

Misassignment in figure 1.7 may also be attributable to miscalculated design values. The design values may be miscalculated for two reasons: 1) the number of days a monitor is operable is affected by unrecoverable information, directly impacting which one hour maximum is used (details in Appendix A) and 2) most 1990 design value were calculated from 1987 - 1989 emissions data but, without indication, some were calculated from data recorded over 1998 - 1990. Unfortunately, it is not possible to correct our design values as the EPA does not provide yearly design values. Both instances of miscalculation could result in our design values being either larger or smaller than the actual design values, thus it is not clear what the impact is on the estimates.

Because figure 1.7 implies that the 1990 CAAA assignment rule is followed, we use an RD design to estimate the effect of increased regulation on the change in pollution over time. The RD design is used around the five discrete changes in non-attainment status of the 1990 CAAA, also known as the discontinuity points: 0.121, 0.138, 0.16, 0.18 and 0.28. The five comparisons are between "bordering" categories: Attainment and Marginal, Marginal and Moderate, Moderate and Serious, Serious and Severe and Severe and Extreme. An RD design identifies what is known as the "average treatment effect" (ATE): E(y_1 - y_0), where y1 generically indicates the pollution change for an area that has received stricter regulation, or the "treatment", and y0 indicates the pollution change for a "control" area receiving the less strict regulation. However, one important caveat is that RD estimates

eligible observations that are not treated (looking at the case of treated ineligible observations results in covariates controlling for the opposite selection bias).

are only applicable around each point of discontinuity, unless the treatment effect is assumed homogenous with respect to the design value, because a restricted sample near each point of discontinuity is used. Thus, the estimates may not be used with any certainty to extrapolate on the ATE away from the discontinuity points and direct comparisons may only be made between non-attainment categories which share a point of discontinuity. Because of this limitation of using an RD design, RD estimates are referred to as local average treatment effect (LATE).[45]

1.7 Estimation

The general RD estimate (fuzzy or sharp) is most simply represented as:

$$\hat{\delta}^{RD} = \frac{\hat{Y}^{+} - \hat{Y}^{-}}{\hat{D}^{NA+}_{m,a} - \hat{D}^{NA-}_{m,a}} \tag{1.1}$$

where:

$$\begin{array}{ll} \hat{Y}^{+} = \frac{\sum_{m\in M} y_m \cdot w_m}{\sum_{m\in M} w_m} & \hat{Y}^{-} = \frac{\sum_{m\in M} y_m \cdot (1-w_m)}{\sum_{m\in M} (1-w_m)} \\ \hat{D}^{NA+}_{m,a} = \frac{\sum_{m\in M} D^{NA}_{m,a} \cdot w_m}{\sum_{m\in M} w_m} & \hat{D}^{NA-}_{m,a} = \frac{\sum_{m\in M} D^{NA}_{m,a} \cdot (1-w_m)}{\sum_{m\in M} (1-w_m)} \end{array} \tag{1.2}$$

and M denotes the subsample of observations satisfying $v *^{NA} -bw < v_m, a < v *^{NA} +bw$, where bw>0 is the chosen bandwidth, $v*^{NA}$ is the discontinuity point above which the area should have been assigned to the non-attainment category NA, and $D^{NA}_{m,a}$ is a dummy variable indicating the monitor m in area a was classified NA. Finally, wi is the weight defined by an indicator function distinguishing which side of the discontinuity each observation is on, within the subsample: $w_i \equiv 1(v*^{NA} <$

[45] See Appendix A for furhter discussion of Regression Discontinuity.

$v_{m,a} < v *^{NA} + bw$). If the assignment were strict, a sharp RD design would be used and the denominator of 1.1 would be equal to 1. Otherwise, the denominator is a measure of whether an observation is likely to be assigned to NA given that $v*^{NA} < v_{m,a}$.

Estimation by 1.1 places equal weight on observations within the subsample by using uniform kernel weights. However, using uniform kernel weights results in a larger bias for boundary point estimates than other kernel weights (Imbens and Lemieux (2008)). Thus, we also use other kernel weights in the following two local polynomial models (LPMs) to estimate δ^{RD}:

$$(\hat{\alpha}, \hat{\beta}^{\alpha}) = \underset{\alpha,\beta}{argmin} \sum_{i=1}^{n} (y_i - \beta_0^{\alpha} - w_i \sum_{i=1}^{p} \beta_j^{RHS,\alpha} (v_i - v*^{NA})^j$$

$$-(1 - w_i) \sum_{i=1}^{p} \beta_j^{LHS,\alpha} (v_i - v*^{NA})^j - \alpha w_i)^2 \cdot k_h(v_i - v*^{NA}) \tag{1.3}$$

$$(\hat{\alpha}, \hat{\beta}^{\gamma}) = \underset{\gamma,\beta}{argmin} \sum_{i=1}^{n} (D_i^{NA} - \beta_0^{\gamma} - w_i \sum_{i=1}^{p} \beta_j^{RHS,\gamma} (v_i - v*^{NA})^j$$

$$-(1 - w_i) \sum_{i=1}^{p} \beta_j^{LHS,\gamma} (v_i - v*^{NA})^j - \gamma w_i)^2 \cdot k_h(v_i - v*^{NA}) \tag{1.4}$$

where n is the total number of observations in the two non-attainment categories being compared, p is the order of polynomial ($p = 1$ for local linear regression), $k_h(v_i - v*^{NA}) = \frac{K(\frac{v_i - v*^{NA}}{h})}{h}$ is the kernel function with bandwidth h, and the remaining variables are as defined above.[46] The RD estimate is then estimated as a ratio of the estimates of α and γ: $\hat{\delta} = \frac{\hat{\alpha}}{\hat{\gamma}}$

[46] Estimation was done in Stata and coding was helped considerably by working from code made available by professor Doug Miller's at U.C. Davis. The same bandwidth was used for both 1.3 and 1.4.

The cross-validation measure suggested in Imbens and Lemieux (2008) is used to select the bandwidth, h, for the above semi-parametric estimation.[47] The bandwidths result with observations of between 5 and 127, for the various estimations due to the multiple non-attainment category groupings and outcome variables. In order to increase the number of observations, a two stage least squares estimate in which w is used as the instrument for D will also be used.

An RD estimate $\hat{\delta}^{RD}$ evaluated with a uniform kernel is numerically equivalent to a two stage estimate.[48] This equivalence is pointed out most recently by Imbens and Lemiuex (2008) who discuss how the bandwidth may be increased to include additional observations in order to strengthen the estimates as well as allow for the inclusion of covariates to reduce some bias. The priority in introducing covariates should be to reduce any existing bias and increase efficiency.[49] Secondary are the other benefits of increasing the number of observation by increasing the bandwidth. Increasing the bandwidth and including covariates but estimating under the framework of two stage least squares generates equivalent estimates to $\hat{\delta}^{RD}$ so long as covariates are relatively balanced close to the discontinuity points. Thus, conditional on the covariates being somewhat balanced, we introduce covariates in order to increase the number of observations by estimating $\hat{\delta}^{RD,TSLS}$ through two stage

[47] Each discontinuity point has a bandwidth estimate but only one bandwidth is used for both sides of each discontinuity as well as each estimate: α and γ.

[48] As first pointed out by Hahn et al. (2001), w does not have to be an exogenous instrument so long as the main assumption of RD holds (continuity of treatment) and the bandwidth is adjusted to create an exogenous situation. In other words, the bandwidth and w work jointly as an instrument to create a random event.

[49] An additional benefit from using two-stage least squares is avoiding the "curse of dimensionality" that arises quickly when using local-polynomial modeling. Essentially, the estimation would need to compare and weight over a number of "dimensions" i.e. covariates.

least squares and the following two equations:

$$y_i = \beta_0^{TSLS} + \delta^{RD,TSLS} D_i^{NA} + \beta^{RHS,TSLS} w_i (v_i - v{*}^{NA})$$

$$+\beta^{LHS,TSLS}(1 - w_i)(v_i - v{*}^{NA}) + \beta_x^{TSLS} x_i + \varepsilon_i \quad (1.5)$$

$$D_i^{NA} = \beta_0^{TSLS} + \beta_w^{TSLS} w_i + \beta^{RHS,TSLS} w_i (v_i - v{*}^{NA})$$

$$+\beta^{LHS,TSLS}(1 - w_i)(v_i - v{*}^{NA}) + \beta_x^{TSLS} x_i + \eta_i \quad (1.6)$$

where x_i is a vector of exogenous covariates thought to influence pollution levels, and ε_i and δ_i are normally distributed error terms and $E(\varepsilon_i \eta_i) = 0$.

1.8 Results–Balance

One measure of the strength of an RD estimate is how closely the RD design resembles an experiment in its construction. This is measured by the balance of the other variables across the treatment groups which may influence the outcome of interest, or any possible selection into the program. Table 1.1 displays 1990 census descriptive statistics by attainment category for areas and counties, or counties made partial due to EPA area boundaries not following county lines.[50] The differences in demographics between areas of better and worse non-attainment categories are as one may expect when comparing large urban areas to smaller urban and rural areas.

[50] The sample is restricted to areas and counties reporting design values in 1990 and 1996. County demographics do not match area demographics because not all counties within every area report design values in those years.

The Hispanic and Black race demographic increases as ozone quality deteriorates in table 1.1, changing quite a bit for the Extreme area (the South Coast Air Quality Management District). Longer commute times and use of public transpiration, including rail which is included separately due to its lower pollution, as well as a slight shift towards a younger age demographic and a larger incomes and housing values also reflect the similarities of urban demographics and those for areas of high ozone levels. Of the 4 industries included (non-durable and durable manufacturing, agriculture and mining), the one noticeable difference is that there is a proportionally much larger percentage of employment in mining in attainment areas, although this typifies a rural non-polluting area.

Looking at table 1.1, it is hard to argue that the average demographic statistics are well balanced across non-attainment categories. However, the RD design limits observations within each attainment category to a bandwidth so it is important to also evaluate balance within that bandwidth. For simplicity, a bandwidth of 0.01 parts per million (ppm) is used for the results presented in tables $1.2a$ and $1.2b$ which show the demographic comparisons at the county and partial county level. The county/partial county level of observation is used as it is more disaggregated and more likely to be unbalanced than the area level. The two tables consist of five comparison groups, each group with four columns. Four columns are necessary because the assignment process did not result in a strict regression discontinuity design, as made evident in figures 1.1, 1.2 and 1.3. For those observations that are on the incorrect side of the discontinuity, such as attainment counties above 0.121 ppm, the results are shaded grey in the two tables.

Discontinuity Point and Relation to Discontinuity:		Below 0.121		Above 0.121		Below 0.138		Above 0.138	
Attainment Status:		A	MA	A	MA	MA	MO	MA	MO
Number of Observations:		49	5	18	26	26	17	4	21
Race	Hispanic	4.39%	1.45%	4.21%	1.50%	1.75%	4.95%	1.67%	5.06%
	Black	11.63%	8.66%	12.36%	10.69%	8.48%	12.66%	17.65%	8.62%
	Amer. Ind.	0.61%	0.23%	0.59%	0.29%	0.25%	0.28%	0.21%	0.32%
	Asian	0.93%	1.08%	0.85%	0.67%	0.69%	1.36%	0.46%	1.74%
	Other	0.05%	0.10%	0.04%	0.05%	0.06%	0.07%	0.04%	0.07%
	White	82.39%	88.48%	81.96%	86.80%	88.78%	80.68%	79.97%	84.19%
Commute Time	<20mins	52.69%	53.62%	46.65%	54.01%	52.57%	46.72%	48.36%	48.47%
	20 to 60	40.65%	40.65%	43.39%	39.76%	41.04%	46.19%	41.50%	44.91%
	>60	4.01%	2.86%	6.93%	3.52%	3.83%	4.77%	5.72%	3.70%
Mode of Transportation	Private	91.85%	88.15%	91.39%	91.38%	91.59%	90.78%	87.28%	90.93%
	Public	1.12%	3.07%	0.81%	1.27%	1.31%	3.13%	1.40%	2.11%
	Other	4.38%	5.91%	4.77%	4.64%	4.54%	3.76%	6.90%	4.03%
	None	2.65%	2.87%	3.03%	2.72%	2.56%	2.32%	4.42%	2.92%
	Public_Rail	0.02%	0.06%	0.03%	0.06%	0.12%	0.51%	0.04%	0.25%
Age of residents	1 to 6	10.27%	9.90%	10.13%	9.87%	9.70%	10.31%	10.00%	10.46%
	7 to 13	10.29%	9.07%	10.18%	9.78%	9.79%	10.31%	10.08%	10.30%
	14 to 18	7.14%	6.36%	7.21%	6.70%	6.84%	7.01%	7.21%	6.95%
	19 to 24	8.89%	9.42%	9.15%	8.74%	8.59%	8.29%	10.27%	9.44%
	25 to 39	24.83%	25.04%	24.79%	24.79%	24.60%	24.86%	23.53%	25.84%
	40 to 69	30.44%	30.22%	30.82%	30.99%	31.37%	30.63%	30.27%	29.42%
	70 and up	8.14%	9.99%	7.71%	9.13%	9.10%	8.59%	8.64%	7.59%
Household Income	< 30k	54.10%	50.92%	50.46%	53.66%	51.68%	50.14%	60.53%	46.03%
	30 to 75k	39.93%	41.48%	42.02%	40.33%	41.60%	41.70%	33.91%	45.17%
	75 to 140k	5.12%	6.51%	6.49%	5.12%	5.78%	6.94%	4.44%	7.63%
	> 150k	0.85%	1.09%	1.03%	0.89%	0.94%	1.22%	1.12%	1.17%
Housing values	< 50k	32.66%	23.10%	32.17%	29.71%	30.20%	30.60%	41.46%	22.38%
	50 to 100k	48.75%	43.91%	41.40%	46.53%	44.15%	46.81%	39.58%	47.06%
	100 - 250k	16.65%	30.10%	23.50%	21.98%	23.65%	18.66%	15.78%	25.65%
	250 - 500k	1.73%	2.53%	2.67%	1.58%	1.78%	3.30%	2.79%	4.37%
	> 500k	0.21%	0.37%	0.26%	0.20%	0.21%	0.63%	0.39%	0.54%
Selected industies	Man. N. D.	8.45%	5.50%	8.43%	8.54%	8.93%	7.72%	7.74%	7.17%
	Man. D.	12.10%	9.30%	10.90%	10.84%	12.64%	12.18%	7.52%	13.12%
	Ag	3.05%	1.79%	3.10%	2.20%	2.02%	1.49%	5.27%	2.64%
	Mining	0.47%	0.08%	0.94%	0.58%	0.56%	0.50%	0.30%	0.24%

Each cell reports the demographics for census tracts within a county or parital county that has a design value within 0.01 ppm of the discountinuity point (above or below as indicated by the column headers. The disconinuity points were created by the attainment status cut off. "Partial" counties refer to those counties made partial by the boundary of the EPA areas. Attainments status are abbreviated as follows: Attainment (A), Marginal Non-Attainment (MA), Moderate Non-Attainment (MO), Serious Non-Attainment (SR), Severe Non-Attainment (SV) and Extremem Non-Attainment (EX). The sample restricted to counties and areas with monitors that report emissions in both 1990 and 1996.

Table 1.2*a*: County or Partial County Demographics within 0.01 ppm of Discontinuity (1990 Census)

Discontinuity Point and Relation to Discontinuity:		Below 0.160		Above 0.160		Below 0.180		Above 0.180		Below 0.280		Above 0.280	
Attainment Status:		MO	SR	MO	SR	SR	SV	SR	SV	SR	EX	SR	EX
Number of Observations:		13	12	2	9	4	6	1	7	0	1	0	1
Race	Hispanic	3.44%	6.99%	0.73%	3.57%	28.01%	13.42%	27.70%	7.39%		25.60%		27.37%
	Black	7.76%	13.20%	0.95%	23.76%	5.54%	16.50%	5.32%	11.47%		5.05%		7.86%
	Amer. Ind.	0.27%	0.47%	0.30%	0.21%	0.37%	0.39%	1.12%	0.31%		0.76%		0.68%
	Asian	2.16%	2.08%	0.93%	2.71%	2.88%	2.84%	2.68%	3.16%		3.40%		4.08%
	Other	0.05%	0.19%	0.02%	0.09%	0.12%	0.13%	0.20%	0.10%		0.18%		0.23%
	White	86.32%	77.09%	97.07%	69.67%	63.08%	66.72%	62.98%	77.57%		65.01%		59.79%
Commute Time	<20mins	47.31%	47.89%	64.80%	42.06%	51.74%	43.08%	61.26%	46.54%		43.81%		42.70%
	20 to 60	46.57%	44.74%	29.31%	49.80%	42.50%	43.60%	30.75%	45.29%		38.02%		40.67%
	>60	3.36%	4.78%	2.32%	5.68%	3.22%	11.07%	5.90%	5.68%		15.16%		14.21%
Mode of Transportation	Private	91.64%	91.41%	90.55%	83.70%	90.61%	85.39%	92.73%	86.15%		92.05%		93.58%
	Public	1.98%	2.15%	0.51%	8.24%	2.08%	7.95%	0.92%	6.21%		0.92%		0.70%
	Other	3.63%	3.84%	5.38%	5.60%	4.76%	4.41%	4.27%	5.15%		4.02%		3.30%
	None	2.75%	2.59%	3.56%	2.46%	2.55%	2.26%	2.09%	2.49%		3.01%		2.42%
	Public_Rail	0.06%	0.40%	0.02%	2.88%	0.19%	3.04%	0.03%	2.05%		0.02%		0.01%
Age of residents	1 to 6	10.23%	10.76%	9.24%	9.39%	11.11%	11.29%	13.53%	9.89%		12.31%		13.44%
	7 to 13	9.87%	10.41%	10.14%	8.71%	10.33%	10.46%	12.22%	9.04%		10.91%		12.08%
	14 to 18	6.60%	6.90%	7.03%	6.38%	7.18%	6.81%	7.16%	6.21%		6.61%		7.15%
	19 to 24	8.26%	9.26%	7.10%	10.94%	9.39%	8.51%	8.35%	9.30%		8.22%		9.23%
	25 to 39	26.73%	25.85%	23.19%	27.55%	25.67%	26.64%	26.21%	25.83%		26.05%		27.67%
	40 to 69	30.42%	29.52%	32.66%	29.91%	28.56%	29.08%	26.27%	30.74%		27.14%		25.03%
	70 and up	7.91%	7.30%	10.64%	7.11%	7.75%	7.21%	6.26%	8.99%		8.75%		5.41%
Household Income	< 30k	45.25%	45.12%	56.75%	38.56%	46.80%	39.68%	51.88%	40.20%		44.53%		42.51%
	30 to 75k	45.02%	44.79%	38.51%	46.37%	42.25%	46.80%	40.54%	45.66%		44.68%		46.88%
	75 to 140k	8.23%	8.70%	3.95%	12.90%	9.44%	11.47%	6.52%	11.79%		9.06%		9.40%
	> 150k	1.50%	1.39%	0.78%	2.17%	1.51%	2.05%	1.06%	2.35%		1.73%		1.21%
Housing values	< 50k	18.70%	14.03%	54.80%	5.15%	12.48%	8.90%	11.60%	9.77%		1.91%		2.04%
	50 to 100k	43.99%	33.37%	39.92%	24.55%	30.13%	26.38%	57.73%	29.55%		21.93%		25.14%
	100 - 250k	28.99%	45.48%	5.06%	57.91%	48.11%	48.57%	28.62%	45.92%		64.63%		62.59%
	250 - 500k	6.73%	6.29%	0.21%	10.49%	8.31%	13.84%	1.85%	12.35%		9.83%		9.35%
	> 500k	1.59%	0.83%	0.01%	1.90%	0.97%	2.32%	0.19%	2.41%		1.69%		0.88%
Selected industries	Man. N. D.	7.35%	7.77%	8.72%	4.87%	6.24%	6.51%	3.45%	8.35%		4.01%		4.49%
	Man. D.	13.35%	8.84%	21.93%	7.26%	10.76%	13.46%	3.85%	10.89%		10.82%		11.29%
	Ag	1.54%	2.91%	3.72%	1.06%	3.78%	1.76%	9.63%	1.11%		4.26%		2.11%
	Mining	0.21%	0.21%	0.92%	0.10%	0.19%	0.24%	5.13%	0.11%		0.20%		0.20%

Each cell reports the demographics for census tracts within a county or parital county that has a design value within 0.01 ppm of the discountinuity point (above or below as indicated by the column headers. The disconinuity points were created by the attainment status cut off. "Partial" counties refer to those counties made partial by the boundary of the EPA areas. Attainments status are abbreviated as follows: Attainment (A), Marginal Non-Attainment (MA), Moderate Non-Attainment (MO), Serious Non-Attainment (SR), Severe Non-Attainment (SV) and Extremem Non-Attainment (EX). The sample restricted to counties and areas with monitors that report emissions in both 1990 and 1996.

Table 1.2*b*: County or Partial County Demographics within 0.01 ppm of Discontinuity (1990 Census)

Similarly to the results of table 1.1, tables 1.2a and 1.2b do not provide strong evidence that the RD design balances the descriptive statistics as well as a random experiment. Some of the larger discrepancies are highlighted in tables 1.2a and 1.2b. The two tables do provide some insights into which demographics may have a relationship to pollution. For instance, four statistics on the incorrect side of the discontinuity have been outlined to point out the difference in demographic make-up from those counties in the same non-attainment area on correct side of the discontinuity. Picking one in particular, the 9 Serious (SR) non-attainment counties above the 0.160 ppm discontinuity point are 24% black on average while the 12 SR non-attainment counties below .160 ppm are only 13% black. This result provides a somewhat interesting anecdote that areas incorrectly marked as Serious have a smaller black population than those with an actual ozone level that would result in Serious. This suggests that segregated migration away from more polluted areas or pollution targeting of blacks may exist. Further, comparing these percentages with the 0.95% black population in the Moderate counties *above* the 0.160 cutoff, lends some support to the suggestion that assignment was based on the percentage make up of black residents, suggesting that a control for race may be necessary.

Tables 1.1, 1.2a and 1.2b also display the diminishing sample size as failure of attainment worsens. The sample size also reduces as the level of observations expands, moving from monitor to county to area. This affects the estimates presented below, especially the lack of estimates for the Extreme discontinuity point due to no Serious observations within the appropriate bandwidth.

1.9 Results - Regression Discontinuity Estimates

Estimates are presented in this section for the percentage change in design value for three levels of observation: area, county or partial county, and monitor, across two different time periods: 1990 through the first milestone in 1996, y^M_{NA}, and 1990 through each non-attainment category's attainment deadline, y^D_{NA}. As can be seen from table 1.3, although there is a lot of variation across the estimates, all but six of the results are insignificantly different from zero at a reasonable confidence level. The variation in estimates that exists across the three different levels of observation does suggest that the level of observation needs to be chosen carefully, but the insignificance and overall noise in the estimates implies that the situation may not have been ideal for the chosen RD estimation procedure.

The main columns of table 1.3 are broken out by the two time periods, with the 6 year milestone time period consisting of the left six columns and the time adjusted results in the right six columns. The time periods are then broken down into columns of results which use a uniform and Epanechnikov kernels, and those, in-turn, consist of columns for results by observation level: area, county and then monitor.[51]The rows are broken out by the non-attainment comparison groups, Attainment and Marginal through Serious and Severe moving downwards. Unfortunately, identification involving the Extreme category is not possible due to the limited number of observations.

The RD estimates in table 1.3 are interpreted similarly to a difference-in-

[51]A uniform kernel assigns equal weight to each observation while a Epanechnikov kernel assigns a weight of: $K(x) = \frac{3}{4}(1 - x^2)$ where K(x) is defined as above in (10).

Observation level:	Percentage Change in DV from 1990 to 1996						Percentage Change in Time Adjusted DV					
	Uniform Kernel			Epanechnikov Kernel			Uniform Kernel			Epanechnikov Kernel		
	Area	County	Monitor	Area	County	Monitor	Area	County	Monitor	Area	County	Monitor
	Attainment and Marginal											
RD Estimate	**-1.6991**	**-0.2695** [c]	**-0.0157**	**-3.4611**	**-0.2920** [c]	**-0.0286**						
Estimated SE	*4.2519*	*0.1511*	*0.2438*	*16.1622*	*0.1596*	*0.1616*						
Top Estimate	-0.2459	-0.1918	-0.0024	-0.2643	-0.2071	-0.0067						
Bottom Estimate	0.1447	0.7118	0.1517	0.0764	0.7093	0.2328						
Observations	14	23	64	14	23	74						
Bandwidth	0.01	0.015	0.007	0.01	0.005	0.008						
	Marginal and Moderate											
RD Estimate	**-0.3201**	**1.0034**	**1.3957**	**-0.4677**	**0.6465**	**1.1195**	**0.7383**	**0.8182**	**2.4608**	**0.7383**	**0.6814**	**1.6235**
Estimated SE	*0.5941*	*5.3295*	*7.0220*	*1.0555*	*2.1992*	*4.0790*	*0.8749*	*3.2678*	*15.5831*	*0.7734*	*1.9671*	*7.1058*
Top Estimate	0.1248	0.1087	0.0556	0.1438	0.1022	0.0628	0.3691	0.1162	0.0788	0.3691	0.1165	0.0757
Bottom Estimate	-0.3898	0.1083	0.0398	-0.3075	0.1581	0.0561	0.5000	0.1421	0.0320	0.5000	0.1710	0.0466
Observations	7	10	68	7	10	68	6	9	69	6	9	69
Bandwidth	0.01	0.006	0.007	0.01	0.007	0.008	0.01	0.007	0.007	0.007	0.007	0.008
	Moderate and Serious											
RD Estimate	**40304.2**	**2.9293**	**4.0786**	**40304.2**	**-0.3118**	**-5.7967**	**0.0658**	**0.0454**	**-0.7639**	**0.0658**	**0.1248**	**-0.8017**
Estimated SE	*1.34E+10*	*29.7460*	*43.1778*	*1.66E+10*	*1.3534*	*128.2191*	*0.1339*	*0.1679*	*1.8537*	*0.0515*	*0.1531*	*2.4454*
Top Estimate	-0.0721	-0.1169	-0.0967	-0.0721	-0.0472	-0.0710	0.0658	0.0215	-0.0973	0.0658	0.0591	-0.0826
Bottom Estimate	0.0000	-0.0399	-0.0237	0.0000	0.1514	0.0122	1	0.4745	0.1274	1	0.4738	0.1030
Observations	4	20	53	4	20	53	2	13	48	2	13	48
Bandwidth	0.01	0.014	0.012	0.01	0.014	0.014	0.01	0.014	0.012	0.006	0.014	0.014
	Serious and Severe											
RD Estimate	**-0.0555** [a]	**-0.8850**	**-0.0465**	**-0.0555** [b]	**-0.4174**	**0.0097**	**-0.8952** [a]	**-0.2081**	**0.6019**	**-0.8952** [a]	**-0.2333**	**0.6019**
Estimated SE	*0.0202*	*2.0575*	*0.6042*	*0.0221*	*0.2858*	*0.5983*	*0.2007*	*0.1643*	*2.1352*	*0.1959*	*0.1661*	*1.8674*
Top Estimate	-0.1666	0.1698	0.0116	-0.1666	0.2406	-0.0024	-0.8952	-0.2081	-0.2006	-0.8952	-0.2333	-0.2006
Bottom Estimate	3.0000	-0.1919	-0.2500	3.0000	-0.5764	-0.2500	1	1	-0.3333	1	1	-0.3333
Observations	5	13	10	5	13	10	3	5	4	3	5	4
Bandwidth	0.02	0.011	0.007	0.02	0.002	0.009	0.02	0.011	0.007	0.014	0.02	0.005

Results from regressions discontinuity estimation for two outcomes: (1) percentage change in design value (DV) from 1990 to 1996 and (2) percentage change in DV adjusted for moving deadline in regulation (time adjusted), and three observation levels: (1) Area, (2) County or Partial County and (3) Monitor. The numerator ("Top") and denominator ("Bottom") estimates are reported separately from the final regression discontinuity estimate ("RD Estimate") to provide a better understand of the final estimate. A bandwidth was calculated for each estimate using the cross-validation measure suggested in Imbens and Lemieux (2008). Results from two kernel weightings are reported separately: Uniform and Epanechnikov. To report significance: *a* indicates significance at the 1% level, *b* indicates significance at the 5% level, and *c* indicates significance at the 10% level (two tailed).

Table 1.3: Regression Discontinuity Results: Percentage change in DV from 1990 to 1996 and Time Adjusted

differences estimate. Each estimate is the percentage difference (in decimals) of the differences in emissions over the 1990 and 1996 period, or over the non-attainment deadline adjusted time-period, between the relevant non-attainment categories. Under the assumptions for the RD design, it is assumed that any other impacts on the change in emissions other than the regulation of the non-attainment categories of each group are differenced out. For example, although insignificant, the Attainment and Marginal RD estimate for the county level change over the 1990 to 1996 period implies that the regulation of the Marginal non-attainment category, relative to the regulation of the Attainment category, may have resulted in an additional 26% drop in the design value measurement using a uniform kernel versus a 29% drop in emissions using an Epanechnikov kernel.[52] This estimate of the change in the design values comes from a restricted subsample of Attainment category observations within a bandwidth of 0.015 ppm and 0.005 ppm of 0.121 ppm, respectively.

There are some unexpected estimates in table 1.3, such as those that are well above a 100% change. The source of this issue appears to be the denominator estimate, which, from (9) above, represents the probability of being in the treatment group conditional on being above the discontinuity point. In addition to the denominator estimate at times being very small, inflating the RD estimate greatly, it is in two instances well above 1 and occasionally surprisingly negative, causing a reversal in the impact given by the numerator (for instance, suggesting a relative increase rather than a relative decline). These issues are due to the non-strict assignment, the use of a linear probability model and, most likely, the small number of

[52]Standard errors were calculated according to Porter (2003).

observations. The difference in the number of observations across estimates is due to the difference in bandwidths which are also given in the tables. The minimum of the numerator and denominator bandwidths was used each estimate and each bandwidth was calculated according to the cross-validation measure suggested by Imbens and Lemieux (2008), as mentioned above.

The numerator estimates in tables 1.3 provide an understanding of how the estimates vary according to which observation level is used, although this is under the caveat that they are not corrected for treatment selection. Across the monitor, county and area level, the estimates differ in magnitude and sign within each non-attainment group and time period. This implies that careful consideration of the level of observation needs to be taken into account when deciding what question is being asked as the answers may be quite different. However, considering the issues mentioned in the preceding paragraph, it is difficult to draw much inference from the estimates in table 1.3. As discussed above, in order to address the concern that sample size may be an issue we introduce covariates in order to increase the number of observations by estimating $\hat{\delta}^{RD,TSLS}$ through two stage least squares.

1.10 Results - Two Stage Least Squares

The results from estimating two-stage least squares regressions, equations 1.5 and 1.6, are presented in tables 1.4a and 1.4b. Table 1.4a presents the results for the differences in emissions over the 1990 and 1996 period, while table 1.4b is for the non-attainment deadline adjusted time-period. There are ten pairs of columns

	Monitor										County and Parital County									
	Marginal NA		*Moderate NA*		*Serious NA*		*Severe NA*		*Extreme NA*		*Marginal NA*		*Moderate NA*		*Serious NA*		*Severe NA*		*Extreme NA*	
Non-Attainment (NA)	0.043	0.033	-0.249	-0.164	-0.212	-0.243	-0.315	-0.305	-0.006	-1.186	0.117	0.15	-0.23	-0.116	-0.085	-0.073	44.186	-0.381	0.001	-1.046
	(0.09)	(0.08)	(0.19)	(0.10)	(0.12)	(0.119)*	(0.122)**	(0.128)*	(0.02)	(0.97)	(0.12)	(0.17)	(0.081)**	(0.055)*	(0.06)	(0.09)	(4,931.59)	(0.21)	(0.03)	(1.09)
DV - DV(NA): above	-3.012	-4.093	5.479	3.174	1.519	1.31	0.58	1.71	0.764	-0.334	-2.225	-3.119	16.61	15.823	6.026	6.031	185.183	1.535	1.366	2.683
	(1.245)*	(1.187)**	(2.88)	(1.76)	(1.53)	(1.63)	(2.67)	(2.15)	(0.284)**	(1.00)	(0.582)**	(0.710)**	(8.130)*	(5.215)**	(1.621)**	(1.780)**	(20,114.65)	(4.19)	(0.391)**	(2.57)
DV - DV(NA): below	-6.303	-6.259	-5.828	-5.218	-3.956	-0.612	2.189	-1.502	-1.159	1.264	-12.211	-14.117	-3.258	-7.299	-9.104	-10.487	-1978.575	5.224	-1.563	-6.906
	(3.72)	(3.40)	(3.85)	(3.23)	(7.24)	(6.30)	(3.26)	(2.71)	(0.294)**	(1.56)	(8.71)	(10.72)	(1.71)	(1.856)**	(3.517)**	(3.301)**	(219,616.30)	(5.91)	(0.000)**	(8.27)
% Commuting in Vehicle		0.093		0.024		-0.024		-0.348		0.102		0.075		0.935		1.008		-1.175		-0.491
		(0.07)		(0.19)		(0.23)		(0.21)		(0.25)		(0.29)		(0.93)		(0.72)		(1.18)		(1.39)
% Commuting by Public Transportation		-0.483		-0.404		-0.308		-0.291		-2.202		-1.041		0.305		1.464		-2.033		-2.435
		(0.45)		(0.31)		(0.32)		(0.25)		(1.40)		(1.44)		(1.30)		(1.02)		(1.80)		(2.12)
% with commute of 20 to 60 minutes		0.087		0.243		0.169		-0.063		-0.205		0.185		0.345		-0.189		0.273		0.471
		(0.05)		(0.072)**		(0.10)		(0.16)		(0.47)		(0.10)		(0.20)		(0.21)		(0.36)		(0.98)
% with commute greater than 60 minutes		0.008		-0.019		0.213		0.562		1.725		0.114		0.076		-0.172		3.045		3.54
		(0.15)		(0.31)		(0.26)		(0.268)*		(1.63)		(0.27)		(0.56)		(0.47)		(1.71)		(2.84)
% employed in non-durable manufacturing		-0.005		-0.122		0.093		0.095		-2.856		0.248		-0.314		0.012		0.939		-1.909
		(0.11)		(0.17)		(0.28)		(0.49)		(2.23)		(0.15)		(0.44)		(0.57)		(0.82)		(1.57)
% employed in durable manufacturing		-0.095		-0.027		-0.407		-0.014		0.958		-0.129		-0.511		0.042		0.108		0.487
		(0.09)		(0.15)		(0.22)		(0.23)		(1.38)		(0.12)		(0.220)*		(0.24)		(0.50)		(1.31)
% employed in agriculture		-0.694		-0.326		0.333		-0.364		-0.616		-0.454		4.538		0.676		-1.992		-2.091
		(0.158)**		(0.41)		(0.31)		(0.31)		(0.84)		(0.26)		(0.765)**		(0.67)		(1.27)		(3.56)
% employed in mining		0.337		-2.51		-4.256		2.328		0.617		-0.506		-3.905		-0.283		-5.643		24.178
		(0.63)		(0.913)**		(2.41)		(1.102)*		(2.07)		(0.78)		(2.93)		(1.77)		(2.802)*		(41.53)
% Hispanic		0.016		0.176		0.028		0.062		1.212		0.202		-1.301		-0.003		0.837		3.277
		(0.06)		(0.16)		(0.11)		(0.11)		(0.88)		(0.12)		(0.263)**		(0.35)		(0.63)		(2.88)
% Black		0.102		0.014		-0.006		-0.177		0.838		0.029		0.036		-0.107		-0.206		-0.962
		(0.042)*		(0.05)		(0.08)		(0.13)		(0.59)		(0.06)		(0.14)		(0.17)		(0.21)		(1.21)
% Other		3.989		-0.602		2.822		-3.809		-8.489		-4.817		-24.92		-5.402		-21.37		74.442
		(2.77)		(3.67)		(2.19)		(3.08)		(9.56)		(16.85)		(29.50)		(4.59)		(7.489)**		(192.49)
% Asian		0.084		0.337		0.103		-0.415		3.196		-0.095		1.377		0.245		-1.832		2.658
		(0.28)		(0.119)**		(0.18)		(0.27)		(3.02)		(0.21)		(0.363)**		(0.40)		(1.08)		(3.48)
% Native American		-0.134		-0.151		-0.311		-0.589		6.888		0.069		-2.022		1.301		5.139		-34.188
		(0.063)*		(0.35)		(0.34)		(2.56)		(9.72)		(0.21)		(1.010)*		(0.87)		(8.73)		(30.45)
% housing value less than $50,000		0.125		-0.059		0.067		0.056		0.192		-0.254		-0.182		0.084		-0.264		-0.117
		(0.07)		(0.08)		(0.10)		(0.09)		(0.28)		(0.28)		(0.13)		(0.19)		(0.31)		(0.35)
% housing value bewteen $50k and $100k		0.062		-0.1		0.019		-0.026		0.735		-0.318		-0.225		0		0.234		0.311
		(0.07)		(0.09)		(0.08)		(0.07)		(0.58)		(0.26)		(0.103)*		(0.09)		(0.17)		(0.24)
% housing value bewteen $100k and $250k		0.131		-0.026		0.179		-0.173		1.102		-0.364		-0.286		0.137		-0.19		-0.139
		(0.08)		(0.09)		(0.089)*		(0.14)		(0.96)		(0.36)		(0.123)*		(0.23)		(0.22)		(0.45)
Constant	-0.129	-0.328	0.08	-0.035	-0.045	-0.135	0.016	0.421	-0.177	-1.002	-0.103	0.054	0.075	-0.754	-0.037	-0.987	-16.641	0.885	-0.165	-0.163
	(0.036)**	(0.108)**	(0.13)	(0.15)	(0.03)	(0.19)	(0.06)	(0.26)	(0.013)**	(0.69)	(0.015)**	(0.40)	(0.07)	(0.81)	(0.03)	(0.66)	(1,844.60)	(0.98)	(0.025)**	(1.49)
Observations	276	265	238	230	244	234	179	176	134	133	188	188	63	63	72	72	56	56	29	29

Non-Attainment (NA) is possibly endogenously assigned. An assigned NA from a calculated design value (DV) is used as the IV. Robust standard errors in parentheses. * significant at 5%, ** significant at 1%. Some monitors are located in census blocks with no census information, resulting in declines in observations when census data is included.

Table 1.4*a*: Two-Stage Least Squares: Percentage change in DV from 1990 to 1996

	Monitor									County and Parital County								
	Marginal NA	*Moderate NA*		*Serious NA*		*Severe NA*		*Extreme NA*		*Marginal NA*	*Moderate NA*		*Serious NA*		*Severe NA*		*Extreme NA*	
Non-Attainment (NA)		-0.21	-0.106	-0.2	-0.231	-0.38	-0.444	-0.176	-24.565		-0.197	-0.092	-0.126	-0.131	1.483	-0.481	-0.207	-0.998
		(0.18)	(0.09)	(0.097)*	(0.098)*	(0.091)**	(0.207)*	(0.042)**	(38.52)		(0.070)**	(0.05)	(0.057)*	(0.09)	(7.84)	(0.32)	(0.016)**	(1.85)
DV - DV(NA): above		4.614	2.177	1.911	0.928	1.196	-2.318	0.19	-3.149		11.666	9.567	5.6	4.96	12.188	0.122	1.452	2.907
		(2.69)	(1.56)	(1.35)	(1.44)	(3.20)	(4.19)	(0.50)	(2.83)		(6.77)	(3.562)**	(1.643)**	(1.670)**	(42.48)	(5.13)	(0.237)**	(4.83)
DV - DV(NA): below		-5.507	-6.11	-2.31	0.458	11.699	10.275	-1.808	1.984		-3.653	-6.438	-5.405	-6.168	-75.932	4.861	-0.777	-6.378
		(3.90)	(3.055)*	(6.80)	(5.38)	(3.490)**	(6.70)	(0.333)**	(2.78)		(1.514)*	(1.792)**	(5.06)	(5.02)	(337.91)	(9.42)	(0.000)**	(13.90)
% Commuting in Vehicle			-0.078		0.075		-0.91		396.786			1.202		1.099		-1.309		-1.333
			(0.18)		(0.20)		(0.455)*		(625.16)			(0.66)		(0.76)		(1.47)		(2.12)
% Commuting by Public Transportation			-0.385		-0.389		-1.334		1980.96			0.88		1.232		-2.114		-3.803
			(0.25)		(0.30)		(0.617)*		(3,125.45)			(0.88)		(1.02)		(2.29)		(5.62)
% with commute of 20 to 60 minutes			0.21		0.136		-0.124		31.469			0.145		-0.173		0.388		0.758
			(0.063)**		(0.09)		(0.21)		(55.47)			(0.17)		(0.25)		(0.51)		(1.65)
% with commute greater than 60 minutes			0.013		0.257		0.384		303.791			0.864		0.408		3.771		3.436
			(0.26)		(0.25)		(0.46)		(481.37)			(0.46)		(0.56)		(2.55)		(4.97)
% employed in non-durable manufacturing			-0.015		0.059		-0.577		-485.251			-0.519		-0.259		1.413		0.909
			(0.16)		(0.26)		(0.75)		(766.20)			(0.39)		(0.57)		(0.96)		(1.66)
% employed in durable manufacturing			0.041		-0.023		0.052		111.745			-0.134		-0.008		0.69		0.258
			(0.13)		(0.16)		(0.29)		(176.51)			(0.16)		(0.26)		(0.80)		(1.71)
% employed in agriculture			-0.271		0.371		-0.427		-1525.19			3.965		0.568		-2.167		-3.827
			(0.39)		(0.29)		(0.39)		(2,396.34)			(0.755)**		(0.69)		(1.63)		(3.38)
% employed in mining			-1.96		-5.957		3.675		1893.66			-3.127		-0.76		-4.538		-2.664
			(0.784)*		(2.436)*		(2.08)		(2,972.33)			(2.55)		(1.56)		(2.47)		(31.69)
% Hispanic			0.109		0.009		-0.68		-80.663			-1.003		0.051		0.495		2.211
			(0.16)		(0.11)		(0.299)*		(128.40)			(0.252)**		(0.37)		(0.67)		(4.17)
% Black			-0.015		0.041		-0.317		-149.303			0.091		-0.102		-0.141		-0.825
			(0.05)		(0.08)		(0.135)*		(236.84)			(0.11)		(0.17)		(0.26)		(2.04)
% Other			3.128		-0.115		-2.491		-18.52			-21.05		-8.526		-22.74		95.669
			(2.85)		(2.66)		(2.40)		(239.99)			(22.84)		(7.67)		(12.43)		(393.21)
% Asian			0.25		0.183		0.143		0			0.748		0.178		-1.459		2.349
			(0.097)*		(0.18)		(0.40)		-			(0.283)**		(0.47)		(1.27)		(6.83)
% Native American			-0.592		-0.777		0.599		0			-2.602		0.062		12.454		-12.152
			(0.247)*		(0.284)**		(4.62)		-			(0.782)**		(0.90)		(9.12)		(47.32)
% housing value less than $50,000			-0.054		-0.037		0.106		0			-0.326		0.078		-0.484		-0.185
			(0.09)		(0.09)		(0.19)		-			(0.107)**		(0.19)		(0.37)		(0.45)
% housing value bewteen $50k and			-0.09		-0.094		0.081		0			-0.173		0.002		0.188		0.08
			(0.09)		(0.08)		(0.14)		-			(0.077)*		(0.11)		(0.26)		(0.38)
% housing value bewteen $100k and			-0.018		0.053		-0.352		0			-0.381		0.152		-0.432		-0.483
			(0.09)		(0.07)		(0.25)		-			(0.099)**		(0.22)		(0.31)		(0.71)
Constant		0.043	0.006	-0.062	-0.162	-0.089	1.058	-0.262	-385.63		0.04	-0.973	-0.044	-1.074	-0.681	0.97	-0.196	0.604
		(0.13)	(0.13)	(0.025)*	(0.16)	(0.026)**	(0.58)	(0.037)**	(606.68)		(0.06)	(0.56)	(0.03)	(0.68)	(2.84)	(1.22)	(0.016)**	(1.79)
Observations		232	225	224	215	92	89	53	52		63	63	71	71	53	53	27	27

Non-Attainment (NA) is possibly endogenously assigned. An assigned NA from a calculated design value (DV) is used as the IV. Robust standard errors in parentheses. * significant at 5%, ** significant at 1%. Some monitors are located in census blocks with no census information, resulting in declines in observations when census data is included.

Table 1.4*b*: Two-Stage Least Squares: Percentage change in DV from 1990 to 1996 in each table though two pairs in table 1.4b are not estimated. Each pair reports estimates for the impact of a different non-attainment category's discontinuity point, with the left column of each pair reporting a base regression without covariates, and the right column including covariates from the 1990 census. For example, left columns under Marginal NA, measure the impact of the marginal designation by looking at the 0.121 ppm cutoff. The first five pairs are estimates at the monitor level and the second five pairs ares estimates at the county and partial county level. Unfortunately, it was not possible to estimate the area level do to sample size issues.

The main coefficient of interest is "Non-Attainment (NA)" which is equivalent to an RD estimate from a uniform kernel RD design without a specified bandwidth. With the exception of a very peculiar result for the Severe Non-Attainment category, one Extreme category result that is due to collinearity from missing census data, and very large (100%) declines for the Extreme category, the impact of the non-attainment categories are reasonable though mainly insignificant with percentage changes in design values between 1990 and 1996 ranging from an increase of 4% for the Marginal non-attainment category to a decrease of 38% for the Severe category, and percentage declines in design values for the relevant deadlines between 9% for the Moderate category and a 48% for the Severe category. With the exception of the impact of the Marginal category, the estimates suggest that the non-attainment categories have had a substantial impact on design values which appears to increase with increased regulation. As expected, the results do change by the level of analysis and outcome variable.

There is no clear indication of whether the impacts increase or decrease when the level of analysis changes from the percentage change between 1990 and 1996 to the timeline adjusted measure. The estimates suggest that the Moderate category results in a lower decline when the design values at the deadlines are considered. This result is surprising as the deadline for the Moderate and Marginal categories are six and three years respectively. Thus, the result implies that the impact of Marginal category was slightly larger at the time of the Moderate category's deadline. However, for the other categories such as the Severe category, the impact is larger for the deadline adjusted outcome measure which is somewhat intuitive as the timeline measure compares monitors and counties with more time to reduce emissions to those with less time. Because the specification estimated is designed to compare design values around the same starting point, with the same goal of 0.121 ppm but with different deadlines, this result implies that monitors and counties with design values on the "high-end" of their category range were not making their deadlines. For the Serious category, the change depends on whether the monitor or county level observation is used, pointing again to the importance of the observation level.

The difference in estimates based on the level of observation is once again found in the two-stage least squares analysis. Although the difference in estimates are often not as large as that found in the local-polynomial RD estimates, the impact of the Serious category is attenuated to a large degree when using the County level analysis rather than the Monitor level. The estimates of the impact of the Serious category on the percentage change from 1990 to 1996 drop from a 21-24% decline for the Monitor level analysis to a 7-13% decline for the County level. The differences provide more evidence that the observation level needs to be carefully considered when discussing outcomes relative to the CAA.

The additional covariates used in the regression come from the 1990 Census and are measured at the tract level for the monitor or the appropriate county or area level for the respective observation level. The measures included are: the percentage of households commuting in a private vehicle, commuting by public transportation, with a commute time of 20 to 60 minutes, with a commute time greater than 60 minutes, employed in the non-durable manufacturing industry, employed in the durable manufacturing industry, employed in the agricultural industry, employed in the mining industry, percentage of races: Hispanic, Black, Asian and American Indian and housing values below \$50,000, between \$50,000-\$100,000 and between \$100,000 and \$250,000. The employment covariates and the covariates of increased travel time are included as observation specific attributes which cause higher ozone levels prior to assignment and thus could be targeted by regulations. Additionally, covariates such as race were included based on the balance results reported in tables $1.2a$ and $1.2b$. There is no uniform pattern in the coefficients which result and the covariates do not appear to have a great impact on the RD estimate with the exception of producing a much larger impact on the 1996 deadline outcome in the Extreme category.

1.11 Conclusion

The severity of the ozone problem has been characterized by the EPA as: "the most widespread and persistent urban pollution problem," emphasizing the importance of determining a way to reduce emissions.[53] The EPA amended the CAA in 1990 in an attempt to bring about a reduction in ozone after many failed prior deadlines. This paper used the assignment rule from the change in regulations to consider the impact of the different levels of regulations as well as the different deadlines imposed through the amendment. Overall, the results were mainly insignificant, although the estimates do suggest that differences in regulation have had an impact on the decline in ozone. The insignificance of the results may be due to the non-strict assignment of the non-attainment status which is addressed to some degree by including control variables and additional observations through larger bandwidths using a two-stage least squares RD approach.

This paper demonstrates that the CAA may be analyzed using RD methodology under the correct observation level. By analyzing the data at different observation levels the extent that the results depend on the level of observation used becomes clear. Prior papers have used monitor and county levels for analysis when neither may have been correct. Although using the area level of assignment greatly limits results by severely limiting sample size, without accounting for the correct level of regulation assignment, it would be possible to choose the level according to the results to suit the questions. This result implies that future work should be careful to detail the correct level of regulation assumed in any analysis.

[53]Overview of the 1990 Clean Air Act Amendments (CAAA)

Chapter 2

How Many Economists does it take to Change a Light Bulb? A Natural Field Experiment on Technology Adoption[1]

2.1 Introduction

The slow adoption and diffusion of energy-efficient and thus, cost-saving, technologies, has been referred to in previous literature as an "energy paradox" (e.g. Jaffe and Stavins (1994)). While much of this previous research has focused on positive analysis, the importance of exploring the "energy paradox" goes beyond efforts to uncover explanations for the slow diffusion of such technologies. There is also need for normative studies that set forth to identify actions that motivate adoption and achieve the economic and social benefits of reduced energy consumption. This paper focuses on this latter consideration by placing its focus on the factors which impact residential adoption of environmental technologies. This is done by utilizing a theoretical model to inform the design of a large scale door-to-door natural field experiment on technology adoption. The field experiment took place in the suburbs of Chicago, IL, and consisted of directly approaching households to offer compact fluorescent light bulbs (CFLs) for purchase under varying prices, frames, and "warning" notices placed at households the day prior to soliciting. To date, 8,815

[1]This is joint research with Professor John List of the University of Chicago and Assistant Professor Michael Price of The University of Tennessee

households were approached, and 32% of those households answered their doors.

This paper is motivated by the potential for reducing energy consumption through technology adoption in residential houses. We consider CFLs in this paper because the rate of CFL adoption and diffusion throughout the United States satisfies the two primary conditions for a technology suffering from the energy paradox: the technology has not been fully adopted and suffers a slow rate of diffusion, and CFLs are a cost-saving technology that is rationale to purchase from a price stand-point.[2] This lack of diffusion is costing the environment and the economy through pollution and energy costs because CFLs are roughly 75% more efficient in energy use than traditional incandescent light bulbs. A common considered calculation is that replacing one incandescent in every household in the U.S. with a CFL would prevent the equivalent annual greenhouse gas emissions of 420,000 cars and reduce energy expenditure by $806 million. Although CFLs are the focus of this paper, the results from the field experiment could be used to inform adoption of other environmental technologies, such as low flow shower heads or technology involved with energy "smart grids". There are a number of other technologies which can be considered in the residential sector, creating a great opportunity to approach President Obama's goal of reducing greenhouse gas pollution by 28% by 2020 because the housing sector accounts for 21% of gas emissions (U.S. Energy Information Administration).

The theoretical model presented in this paper is adapted from theory developed

[2]The Department of Energy's "CFL Market Profile" released in March, 2009, found that only 11% of the light sockets feasible for CFLs in U.S. households were being used by CFLs, although 70% of U.S. households did have at least one CFL.

for door-to-door charitable solicitations in Della Vigna, List, and Malmendier (2009). This theory extends naturally to a consumer's decision to purchase and adopt an environmental technology from a door-to-door solicitor. The theory informs the design of the field experiment so that the underlying social pressure and attitudes towards CFLs can be discussed. This is done by including treatments which allow households to select out the salesperson interaction. Selection occurs due to door-hanger warning notices placed on the household's door a day prior to a salesperson's visit. In addition to a control group that does not receive a warning, some households are warned and some are warned with a notice that allows them to check a box signifying that they do not want to be disturbed. The results from these warning treatments are suggestive that social pressure exists, with a 10% and 28% decline in answer rate when the households are warned and warned with option of not being disturbed, respectively.

For the households that do answer the door, our natural field experiment includes treatments aimed at assessing factors stemming from economic theory as well as theories found in social-psychology. The impact of economic costs on technology adoption is considered by including treatments which vary the price of the environmental technology offered for purchase. The influence of social-psychology factors are assessed through treatments which vary a social norm statement in the experimental script, varying the level of social pressure imposed on the household. We focus on the impact of social norms due to recent research which suggests that social norms have a strong impact on behavior related to environmental conservation while research on other psychological influences, such as the "Foot-In-The-Door" technique, is less conclusive (e.g. Goldstein, Cialdini and Griskevicius (2008) and Cialdini and Goldstein (2004)).

By including treatments motivated by both economic and social-psychology disciplines in the same field experiment, we create as close to an apples-to-apples comparison of what the disciplines would suggest motivate adoption as possible. The results indicate that both price and social pressure through a social norm statement have significant impacts on the decision to purchase environmental technology. However, price and social pressure motivate purchase on two different margins. Whereas social pressure has greater influence along the extensive margin by encouraging households to purchase one package of CFLs, reductions in price work along the intensive margin by encouraging the purchase of more than one package. These results are intuitive and informative when considering methods of adoption encouragement in the future. Further, by incorporating treatments with both price and social norms we are able to estimate a monetary value for using social norms to encourage adoption. This "price" for social norms is substantial, with the demand adjustments due to the social norm statements equivalent to between a 30% and 70% price reduction.

A secondary interest of this research is to use surveys conducted during the field experiment to identify issues remaining as barriers to the adoption of CFLs. Previous research separates the barriers to adoption into three main areas: the initial high cost of CFLs, consumer preferences for the lighting abilities of incandescent light bulbs, and their incompatibility with existing lighting infrastructure (Lefevre, T'Serclaes, and Waide (2006) and Reynolds, DeSisto, Murray, and Kolodinksy (2007)). The field experiment we conducted was ideally situated to inform this discussion. Beyond simply providing an indication of what improvements need to be made to increase the amount of CFLs adopted, combining survey responses on the decision to purchase or not to purchase CFLs with the price variation in the treatments allows some indication of what may be causing the presumed energy paradox. In brief, the survey results suggest that quality and the ability to dispose are of greatest concern.

The next section of this paper places our research in the existing literature. The paper continues by developing the theoretical model and empirical approach. This is followed by a discussion of the experimental design, its implementation and the results (again, this is an early draft so more results to follow). As a preview, our results suggest that price and social norms have a substantial impact on the decision to adopt but that they work through different channels. Finally, we conclude with a brief statement.

2.2 Previous Literature

The discussion surrounding Griliches' 1957 economic analysis of hybrid corn provides an early demonstration of how economists have traditionally considered profit as the motivation for adopting new technologies while other academic fields have focused elsewhere (see Skinner and Stiger (2005) for a recent addition to the discussion). Although price remains a consideration, economists have broadened their focus to understand aspects of technology adoption beyond profit motives as discussed in Hall (2004). Recent papers have considered how information affects adoption (Duflo, Kremer, and Robinson (2004)) and how that information flows through societies or social networks in various settings (Bandiera and Rasul (2006), Conley and Udry (2010), and Oster and Thornton (2010)). This paper continues to broaden the considerations of technology adoption by economists by directly comparing prices and social norms in the same field experiment.

Recent work on residential energy conservation has used non-pecuniary techniques adopted from research by social-psychologists such as the various forms of personal persuasion reviewed in Cialdini (1993). This work has generally focused on changing energy usage behavior rather than specifically encouraging adoption of energy efficient products, such as in Schultz et al. (2007), Allcott (2009) and Ferraro and Price (2010). These papers utilize social norms by reporting a household's energy use relative to the average energy use of households on its energy bill. This technique has led to average declines in monthly energy use that range from 2 - 3% but have been as large as 5.28%, 6%, and 8.3% for high energy consuming households in Ferraro and Price (2010), Allcott (2009), and Schultz et al. (2007) respectively.[3] This paper is directly related to this recent work but focuses on changing behavior to motivate adoption of a specific technology in order to avoid the potential "boomerang" effect or other mean reversions after the experiment.[4]

[3]One benefit of focusing on encouraging adoption rather than a behavior change is the removal of a return to the normative behavior or a "boomerang" effect (e.g. Shultz et al. (2007) and Allcott (2009)). However, this is not certain as the effect may still exist through using more electricity (such as to compensate for the poor quality of light).

[4]It is important to note that we do not have data on energy usage at the household level so cannot be certain that mean reversion or some behavior response similar to the boomerang effect is not occurring. However, we did conduct follow up surveys with a number of households that purchased CFLs from our sales-persons. The follow up surveys consisrted of first offering the households $5 or an additional package of 4 CFLs if they could show us a CFL purchased from us that is currently being used. We were able to contact 141 households in the follow-up and of those 141, only 3 were not using CFLs while 134 wanted the free pack of CFLs. Thus, the follow-up

The impact of social norms on conservation behavior was further demonstrated in Goldstein, Cialdini, and Griskevicius (2008), which varied the level of social pressure of hotel guests by stating that previous guests in the "hotel" *or* "hotel room" had reused bathroom towels over the course of their stay. This small change in social pressure from announcing the results relative to guests in the hotel to results relative to guests in the hotel *room*, resulted in an increase of towel reuse of 49.3%. In line with the recent behavioral economic approaches towards energy conservation and the findings involving social norms, the field experiment implemented for this paper incorporated social norms in order to consider a social-psychology approach to technology adoption.[5] As discussed below, varying the social norms due to proximity were easily adjusted for the technology adoption case considered here.

For energy efficient or environmental technologies, previous economic research has found that the upfront cost of technology has a larger impact on adopting energy saving technologies than the marginal cost of energy. The larger impact of adoption costs is often shown for corporations, such as in Anderson and Newell (2004) which discussed the large impact on the adoption of energy efficient technology at manufacturing plants by using data from responses to energy audits offered by the Department of Energy's Industrial Assessment Centers program (see also Soderholm and Klaasen (2007) and Otto and Reilly (2008)). However, the upfront cost has also been found to be very burdensome for residential households, for ex-

results are highly suggestive that many households are using the CFLs.

[5]Other commitment and consistency techniques were considered, such as the "foot-in-the-door" technique, but inconsistent results reviewed in Burger (1999), Guadagno, Asher, Demaine, & Cialdini, (2001) and Cialdini and Goldstein (2004), as well as a discussion with Noah Goldstein affirmed our focus on social norms.

ample Jaffe and Stavins (1995) contains a theoretical model of technology adoption which, after estimating the model for residential homes using survey data from the National Association of Home Builders, found that technology costs have an impact on adoption that is three times larger than that for energy prices.

This previous research suggests that subsidizing the upfront cost of technology adoption may be the strongest tool in the economic toolbox to encourage adoption. Additionally, subsidies provide a reasonably approachable market-based technique to administer in a natural field experiment. Although market-based environmental policies have been on the minds of economists since Pigou's seminal work on welfare in 1912, subsidizing new environmental technologies to encourage adoption stands as a somewhat controversial approach under standard economic assumptions of informed rational agents and perfect capital markets. This paper does not take a stand on market imperfections or what explains subsidizing new technology as economically rational.[6] Rather, we consider the impact that it may have on adoption rates and thus consider the impact of subsidizing a new technology.[7] Thus, we included price treatments as an economic approach to adoption.

There have been a number of other research papers discussing efforts to promote energy efficient behavior and the adoption CFLs more specifically. However, these efforts have not been designed as treatments to uncover the impact of various methods of encouragement but instead have typically considered CFL adoption poli-

[6]One justification for considering motivation of technology adoption is that pollution continues to not be fully priced in to the market. Therefore, society is still facing a cost that markets have not born and a subsidy could provide a way to lessen this externality.

[7]For a discussion of subsidizing new technologies and a broad overview of technology adoption see Jaffe, Newell, and Stavins (2002).

cies and programs that had already occurred (Martinot and Borg (1998); Lefevre, et al. (2006)). This lack of experimental implementation resulted in these studies primarily discussing barriers to adoption for CFLs through survey results. The previous research has found that various barriers to CFL adoption exist, such as cost and quality as well as a lack of information and desire to change behavior, but has left many interesting questions unanswered (e.g. Lefevre, et al. (2006) and Reynolds, DeSisto, Murray, and Kolodinksy (2007)). Further, a broad review of the existing literature on energy conservation efforts, Abrahamse, Steg, Vlek, and Rothengatter (2005), points out that these programs predominately promote adjusting behavior rather than adopting new technologies.

The research discussed in this paper utilizes a carefully designed natural field experiment on technology adoption (see Harrison and List (2004) for a general discussion of field experiments). Thus, our paper differs from previous work on CFL adoption in two ways: 1) a clearly designed experiment and 2) a focus on technologies, rather than behaviors. Our focus on technologies rather than behaviors is also a step away from recent research on energy conservation more broadly (Schultz et al. (2007), Allcott (2009) and Ferraro and Price (2010)). This focus is intentional as these reductions are permanent as opposed to alterations in behavior, which have been shown to go away without incentives in the case of changes in smoking and weight loss behavior (e.g. Jeffrey and Wing (1995), Donatelle, et al. (2004), Volpp, et al. (2006) and Volpp, et al. (2008)). The design of the natural field experiment and the incorporation of the variation in prices and statements of social norms are informed by a theoretical model which is discussed next.

2.3 Model

2.3.1 Theory: Discrete Choice Model of Adoption with Social Pressure and Attitude

The theoretical model derived in this section is based on Della Vigna, List, and Malmendier (2009) (DLM). The theoretical model developed in DLM motivates a natural field experiment on door-to-door charitable solicitation. This theory extends naturally to the consumer's decision to purchase an environmental technology from a door-to-door solicitor and informs experimental treatments discussed more fully in the next section. The theory developed here for technology adoption differs from DLM in two fundamental aspects: first, social pressure in the model presented below is based on a dichotomous decision of whether to purchase or not, rather than a function of how much to donate, and second, price is included for the consumable good of interest. These differences allow for the inclusion of experimental treatments that vary in price as well as social pressure, which provide greater understanding of adoption by allowing comparison of both monetary and non-monetary motivations.

The consumer's purchase decision is modeled in two stages: the first stage is the decision of whether or not to be home and the second stage is the decision to purchase from a door-to-door salesperson selling CFLs, conditional on being home *and* answering the door. We begin discussing the model with the second stage. When deciding whether to purchase CFLs, labeled here q, households face the following utility function:

$$U(q) = u(W - pq) + av(q, Q_{-i}) - s(q) \tag{2.1}$$

where $s(q) = S(\rho) \cdot 1_{q=0}$ (a social pressure function)

and

$W \equiv$ wealth

$a \equiv$ attitude toward private and public benefits of purchase

$q \equiv$ quantity of CFLs purchased at price p

$Q_{-i} \equiv$ quantity of CFLs purchased by all other households

$\rho \equiv$ proximity of adoption rate

$S(\rho) \equiv$ measure of the level of social pressure.

The model of the purchase decision has three components: a direct impact through the expenditures affect on overall wealth, an indirect impact that is a combination of the private and public benefits of the purchase, and a social impact due to making the decision to purchase in front of someone else.

The first and last components of the purchase model are similar to the additively separable version of the model discussed in Levitt and List (2007), in which both wealth and moral factors are considered in the utility function. In the model discussed here, the decision to purchase affects overall wealth but has some moral implications due to social pressure. The second component of the model reflects the consumer's attitude towards CFLs. This attitude is formed from both the private consumption of the good and from the good's contribution to the public good through its environmental attributes. This combination of private and public characteristics in a good for private consumption has recently been more completely discussed and explored in Kotchen (2006). We do not attempt to parse the private and public attributes in this model or the experiment discussed below.

The first component of the model is the direct utility for the consumer from overall wealth, $u(W - pq)$, and is affected by the decision to purchase the new technology at price p through a reduction of wealth to be spent on other goods, represented here by W. The direct utility is assumed to be a "standard" strictly increasing concave utility function and thus: $u'() > 0$ and $u''() \leq 0$. The purchase also affects the second component of the model, indirect utility, $av(q, Q_{-i})$, through the private and public attributes of CFLs. The parameter a, captures the consumer attitude toward the private and public attributes and the attitude may be negative. For example, the quality issues reported in earlier research indicate that many consumers may have strong negative attitudes towards private consumption. It is possible that these could well outweigh any possible positive attributes through altruistic interests (both pure and impure) resulting from the green technology. This public good is the same as discussed in the charitable literature but is focused here on the environment so that the total contributions to the public good in our model is the summation of the CFL purchases of all individuals: $Q = Q_{-i} + q$.[8]

Consumers with a positive a are assumed to exhibit overall positive attitudes towards the private and public attributes of CFLs, and the reverse for consumers

[8] This ignores monetary donations to the public good, inclusion of which would consider how the non-1-to-1 conversion for CFL to public good versus other forms of contributions affects purchase decisions (see Kotchen 2006 for a theoretical discussion). However, since contributing to a public good through donations is also not 1-to-1, our current approach fits into the majority of the existing literature.

with a negative a. The direct utility function is assumed to be either concave or convex depending on a. If $a > 0$ the indirect utility function is assumed to be concave, $v_q(q, Q_{-i}) > 0$ and $v_{qq}(q, Q_{-i}) \leq 0$, while if $a < 0$ it is assumed to be convex, $v_q(q, Q_{-i}) > 0$ and $v_{qq}(q, Q_{-i}) \geq 0$. This change in concavity follows the intuition of how the purchase impacts utility. If there is a distaste for CFLs then $a < 0$ and the convexity assumption assures that there is a diminishing marginal impact of further purchases as there is when $a > 0$ and $v(q, Q_{-i})$ is concave.

The final term of the utility function, $s(q)$, captures the impact of social pressure that the consumer experiences by being approached in person by a door-to-door salesperson. For the field experiment discussed next, the level of social pressure, $S(\rho)$, is a function of the level of the social norm stated by the salesperson in terms of the proximity of the adopters to the consumer,ρ. The proximity of the social norm was varied in the field experiment by using or omitting the following statements: "70% of the households that were surveyed *in the U.S.* owned at least one CFL" and "70% of the households that we surveyed *in this area* owned at least one CFL."[9] The social pressure function also includes an indicator function that is equal to one when a consumer purchases a package of CFLs, $1_{q=1}$. This is the correct specification in terms of the stated social norm, but it is important to note that this specification carries with it the underlying assumption that consumers are only affected by social pressure if they do not purchase any of the new technology. That is, the social pressure results in our theory and experiment are suggestive of the

[9]The adoption rate was generated by surveying households in a two week pilot study. We were extremely fortunate to have the same adoption rate as the rest of the nation from this pilot study as the adoption rate from our later surveys is indeed higher. In our experiment, ρ varies by treatment between "national" and "area".

decision of whether or not to purchase, not how much to purchase.

The solution for the level of quantity purchased in equation 2.1, $q*$, is a function of attitude a, price p and social pressure $S(\rho)$, but can be characterized by attitude "thresholds." These can be compared to the attitude thresholds in DLM and, as shown in that paper, these thresholds lead to an intuitive discussion of the results. The thresholds for attitude are:

$\underline{a}(p, S(\rho)) \equiv \frac{u(W)-u(W-p)-S(\rho)}{v(1,Q_{-i})-v(0,Q_{-i})}$ (indifferent between buying 0 and 1)

$\bar{a}(p, S(\rho)) \equiv \frac{u(W-p)-u(W-2p)}{v(2,Q_{-i})-v(1,Q_{-i})}$ (indifferent between buying 1 and 2)

The attitude threshold for purchasing increases with the amount purchased, or $\underline{a} < \bar{a}$, and the threshold is inversely related to the gain in marginal indirect utility. Intuitively, a larger marginal gain from increasing consumption reduces the threshold at which consumption occurs but thresholds do remain in order with respect to the amount purchased. Then, using these thresholds, for fixed $p = p_1$ and $S(\rho) = S_1$ and any attitude type a, there is a unique optimal purchase amount $q^*(a, p_1, S_1)$ which is weakly increasing in a:[10]

(i) $q^*(a, p_1, S_1) = 0$ for $a \leq \underline{a}(p_1, S_1)$

(ii) $q^*(a, p_1, S_1)) = 1$ for $\underline{a}(p_1, S_1) < a < \bar{a}(p_1, S_1)$

(iii) $q^*(a, p_1, S_1) \geq 2$ for $\bar{a}(p_1, S_1) \leq a$.

As expected from the intuition of the thresholds, for a given level of social pressure and price, consumers of higher attitude type decide to purchase or purchase more

[10]Simplifying assumptions were made when the consumer was indifferent.

CFLs. Intuitively, consumers receive positive indirect utility which interacts directly with attitude type, so with everything else held constant, increases in attitude type lead to increases in purchases.

Both price and social pressure are taken as constant when establishing the attitude thresholds, but both price and social pressure have obviously important impacts on the purchase decision and the design of our field experiment. A change in social norms only affects the lower attitude threshold which influences the decision to purchase, not how much to purchase. The change in the lower attitude threshold with respect to social pressure is proportional to the additional indirect utility from purchasing the first CFL: $\frac{\partial \underline{a}(\cdot)}{\partial S(\cdot)} = \frac{-1}{v(1,Q_{-i})-v(0,Q_{-i})}$. A similar impact results from a change in the proximity measure, ρ: $\frac{\partial \underline{a}(\cdot)}{\partial \rho} = \frac{-\partial S(\cdot)/\partial \rho}{v(1,Q_{-i})-v(0,Q_{-i})}$. Intuitively, an increase in social pressure lowers the attitude threshold because consumers with lower levels of attitude are induced into purchasing due to social pressure. Further, the larger the impact of purchasing given by the difference in indirect utility, the less social pressure is a factor in the purchase decision.

Unlike a change in social pressure, a change in price impacts both the attitude threshold indicating the decision to purchase and the threshold to purchase more than one package of CFLs. Looking at the lower threshold first, a change in price results in: $\frac{\partial \underline{a}(\cdot)}{\partial p} \equiv \frac{u'(W-p)}{v(1,Q_{-i})-v(0,Q_{-i})}$. Once again the denominator is positive by assumption but now the numerator depends on the slope of the direct utility which is assumed positive everywhere. Thus, an increase in price intuitively leads to a higher attitude threshold and similarly a decrease in price leads to a lower threshold. The impact of price on the decision to purchase more than one CFL package has similar intuition, that relationship is represented by: $\frac{\partial \bar{a}(\cdot)}{\partial p} = \frac{-u'(W-p)+2u'(W-2p)}{v(2,Q_{-i})-v(1,Q_{-i})}$. The numerator is again positive under the assumption of an increasing concave direct utility function, resulting in a positive relationship between price and the attitude threshold indicating a purchase of more than one package. Essentially, when price increases, the indirect benefits from adopting have to increase as well in order for a purchase to occur.

As mentioned, the decision to purchase the new technology is modeled in two stages, with a first stage decision concerning a household's presence at home and answering the door. The likelihood of someone from the household being home and answering the door in the first stage is represented here by h, and the household's baseline probability of being home and answering the door is represented by h_0. It is assumed to be costly to adjust from the baseline probability, h_0, and that cost is assumed to be a convex cost function $c(h)$. In the field experiment discussed next, we provide the households an ability to adjust the likelihood of being home by placing door-hangers on their doors which announce the hour that a salesperson will be knocking on doors, the day before the salesperson arrives (the language on the door-hangers is discussed in the experimental design section but can be seen in figure 2.2). This ensures that the households that we warn have an ability to adjust the likelihood of being home because, with some probability, members of the household see the warning and know that they will be encountering a door-to-door salesperson.

The first stage decision is then represented as a household solving the following maximization decision for whether someone will be home and will answer the door, which incorporates the optimal purchase decision, q^*, from stage two:

$$\max_{h\in[0,1]} h[u(W-pq^*)+av(q^*,Q_{-i})-s(q^*)]+(1-h)[u(W)-av(0,Q_{-i})]-c(h) \quad (2.2)$$

The decision is based on a tradeoff between the utility resulting from the optimal decision in the second stage, the utility obtained from not being home, $u(W)$, and the cost of adjusting the probability of being home, $c(h)$. The optimal solution is characterized as follows: For any type a and fixed $p = p_1$ and $S(\rho) = S_1$, there is a unique optimal probability of being at home $h^*(a, p, S(\rho))$ that is non-decreasing in a.

(i) For $S(\rho) > 0$, there exists an $a_0 \geq \underline{a}$ such that

- (a) $h^*(a, p, S(\rho)) = h_0$ for $a = a_0$,
- (b) $h^*(a, p, S(\rho)) < h_0$ for $a < a_0$ and
- (c) $h^*(a, p, S(\rho)) > h_0$ for $a > a_0$.

(ii) For $S(\rho) = 0$ (no social pressure),

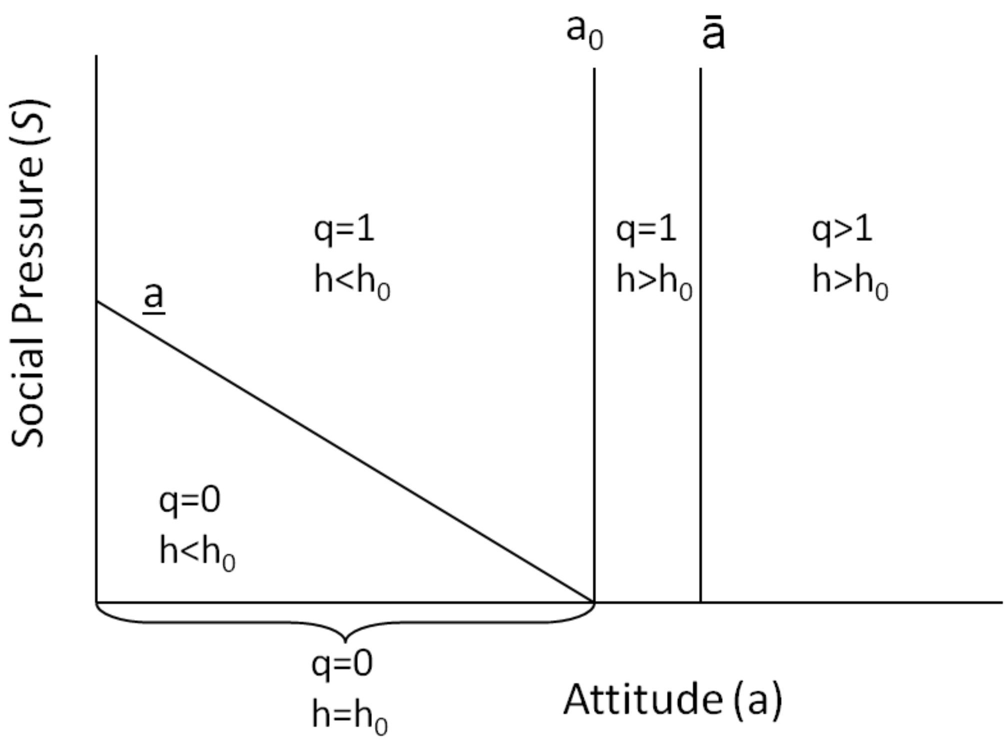
- (a) $h^*(a, p, 0) = h_0$ for $a \leq \underline{a}$ and
- (b) $h^*(a, p, 0) > h_0$ for $a > \underline{a}$.

From these solutions, the decision to stay home can be understood as a function of attitude, social pressure and price. However, it is important to note that price and social pressure, which are experimentally varied in this research, are not known when the household makes its decision to be home so we assume households have an expected level of social pressure in the absence of what is induced by our treatments. If there is no expected social pressure, households with a sufficiently low taste for CFLs ($a \leq \underline{a}$) are not induced to alter their behavior ($h = h_0$) while households with a taste above the lower threshold ($a > \underline{a}$) adjust their probability of being home in order to take advantage of the opportunity to purchase the new technology. When social pressure exists (in expectation by the household) households with low tastes for the private and public benefits of CFLs take measures to avoid the door-to-door salesperson. This decision to lower the probability of being home changes at a_0. Households with a above $\underline{a}$ but below a_0, exert effort to not be home but will purchase if found at home in order to avoid the cost of social pressure.

Figure 2.1 displays visually the decision to be home and purchase in the attitude and social pressure space. The attitude thresholds are represented as discussed above. Note that for a given positive level of social pressure, if attitude increased for a given consumer, that consumer would at first decrease her probability of being home for very low levels of attitude. However, above the minimum attitude threshold, $\underline{a}$, she would decide to purchase if at home and above a_0 would take steps to increase the probability of being home to take advantage of the opportunity to purchase the good. This theory indicates that some consumers alter their "at-home" decision because the optimal purchase decision for them is not to purchase. Thus, when confronted with social pressure, consumers may adopt even though they are making an otherwise utility reducing decision.

The household decision on being home or not has so far been based on the assumption that adjusting h is costly and is characterized by the cost function $c(h)$.

Figure 2.1: Decision to Be Home (h) and Purchase (q)

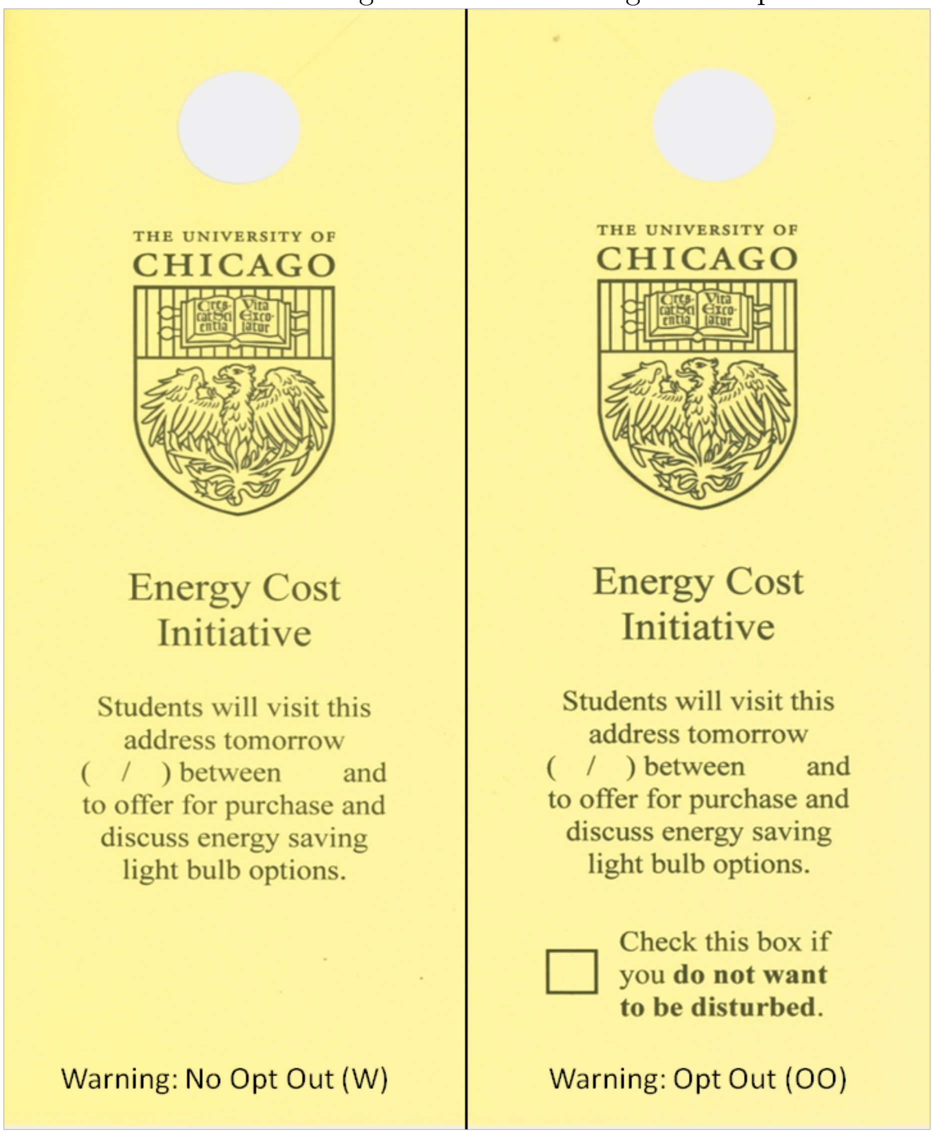


Attitude thresholds, $\underline{a}$, a_0 and $\bar{a}$ depicted in graph.

In our experimental design, we include a treatment which allows for the household to "opt out" without cost by marking a box on the door-hanger. Figure 2.2 shows examples of the door-hangers used in the experiment. Two warning treatments, "Warning" (W) and "Warning with Opt Out" (OO), and one control group, "No Warning" (NW), were included in oufigurer experimental design. Households in the W treatment group receive notices that state: "Students will visit this address tomorrow (date of visit) between (one hour time block) to offer for purchase and discuss energy saving light bulb options". Households in the OO treatment receive the same message but with an additional box located at the bottom of the notice next to the following statement: "check this box if you do not want to be disturbed." Finally, the NW treatment group of households does not receive a notice on their door. The option of opting-out of the solicitation allows the household to lower their probability of being approached to zero, or in the case of our model lower the effective probability of being home to zero ($h = 0$), at no cost ($c(0) = 0$).[11] This additional treatment provides indications of whether social pressure exists as for the case of no social pressures, $S(\rho, \kappa) = 0$, households should never opts out for any a while if social pressure exists, $S > 0$, opting out should occur for sufficiently low attitude types, $a < a_0$.

[11] There may be some social cost not accounted for here to publicly indicating that you do not wish to be disturbed as the door-hangers must remain on the door with this indication marked until the following day when one of the peddlers/solicitors visited the house.

Figure 2.2: Door-Hanger Examples



Warning: No Opt-Out on left, Warning Opt-Out on right

2.3.2 Predictions

Further assumptions are necessary to generate predictions which can be empirically tested. First, we assume that consumers are heterogeneous in attitude with a distributed with c.d.f. F. Second, following DLM, an intuitive framework for discussing the model's predictions is through two scenarios: (1) the existence of attitude that would encourage adoption but no social pressure, characterized by $F(\underline{a}) < 1$and $S = 0$, and (2) very limited attitude to encourage adoption and positive social pressure, characterized by: $F(\underline{a}) = 1$and $S > 0$. The assumptions underlying these two scenarios are used to generate predictions for being home and purchasing under the three treatments involving door-hangers, and these predictions can then be compared theoretically and empirically.

2.3.2.1 Predicted Probability of being home

The probabilities that the adopter is at home, $p(H)$, in the NW, W and OO treatments are given by:

$p_{NW}(H) = h_0$

$p_W(H) = (1 - r)h_0 + r \cdot \int_{-\infty}^{\infty} h^*(a, p, S(\rho))dF$

$p_{OO}(H) = (1 - r)h_0 + r \cdot \int_{a_{OO}}^{\infty} h^*(a, p, S(\rho))dF$

where r is the probability that the household sees the door-hanger and $a_{OO} = \begin{cases} -\infty & \text{for } S = 0 \\ a_0 & \text{for } S > 0 \end{cases}$ because all adopters with attitude lower than a_0 that would otherwise choose to avoid being home $(h*(\cdot) < h_0)$ can opt-out without cost. Under scenario 1 with attitude but no social pressure, the probability of being at home is higher in both cases with door-hangers, W and OO, than it is in the case without door-hangers, because households that want to purchase select into being home and households that don't want to purchase do not avoid answering the door as there is no social pressure from saying no. However, under scenario 2 with very limited attitude and positive social pressure, the probability of being at home is highest in the non-warned case, NW, lower for W, and lowest for OO. Or:

$F(\underline{a}) < 1$ and $S = 0 \rightarrow p_W(H) = p_{OO}(H) > p_{NW}(H)$

$F(\underline{a}) = 1$ and $S > 0 \rightarrow p_{NW}(H) > p_W(H) > p_{OO}(H)$.

The proof for prediction this first prediction is included in the appendix; the intuition is straightforward. When no social pressure is present, there is no negative no-monetary ramification from declining to purchase and, thus, no reason for households to avoid answering the door in the treatments with warning (whether W or OO). Therefore, only the sub-sample of the population that chooses to increase the probability of being home is affected, increasing the overall probability of encountering someone. However, when there is social pressure but limited attitude types of a sufficient level to encourage adoption, households that wish to not purchase will choose to avoid answering the door if at all possible, leading to households that have not been warned (NW) having higher probabilities of being found home. Further, the households that wish to not purchase and that can opt-out without cost do so, resulting in a lower probability for the OO treatment group.

2.3.2.2 Predicted Probability of purchasing, unconditional on answering door

The probabilities that the household adopts unconditional of whether door is answered, $p(A)$, in the NW, W and OO treatments are given by:

$p_{NW}(A) = h_0 \cdot (1 - F(\underline{a}))$

$p_W(A) = (1 - r)h_0 \cdot (1 - F(\underline{a})) + r \cdot \int_{\underline{a}}^{\infty} h^*(a, p, S(\rho))dF$

$p_{OO}(A) = (1 - r)h_0 \cdot (1 - F(\underline{a})) + r \cdot \int_{a_0}^{\infty} h^*(a, p, S(\rho))dF.$

Under scenario 1 with attitude but no social pressure, the probability of being at home is higher in both cases with door-hangers, W and OO, than it is in the case without door-hangers, and the probability is the same in the case of door-hangers. This increase in the probability of being home results in an increase in the probability of purchasing as well. However, under scenario 2 with very limited attitude and positive social pressure, the sample of the population that will answer the door is highest in the non-warned case, NW, lower for W. and lowest for OO. Or:

$F(\underline{a}) < 1$ and $S = 0 \rightarrow p_W(A) = p_{OO}(A) > p_{NW}(A)$

$F(\underline{a}) = 1$ and $S > 0 \rightarrow p_{NW}(A) > p_W(A) > p_{OO}(A)$

The proof is once again in the appendix, and the intuition is similar to the first prediction of being home.

2.3.2.3 Predicted Probability of purchasing, price and social pressure

Predictions also follow from the variation of price and social pressure in experimental treatments. There is no way for the household to know the price or the variation in social pressure prior to answering the door so both factors only influence the decision to purchase once the door has been answered. Two sets of predictions result, one for price:

$$p_j(A|H, p_L) < p_j(A|H, p_H) \text{ for } j\epsilon\{NW, W, OO\}$$

and one for social pressure:

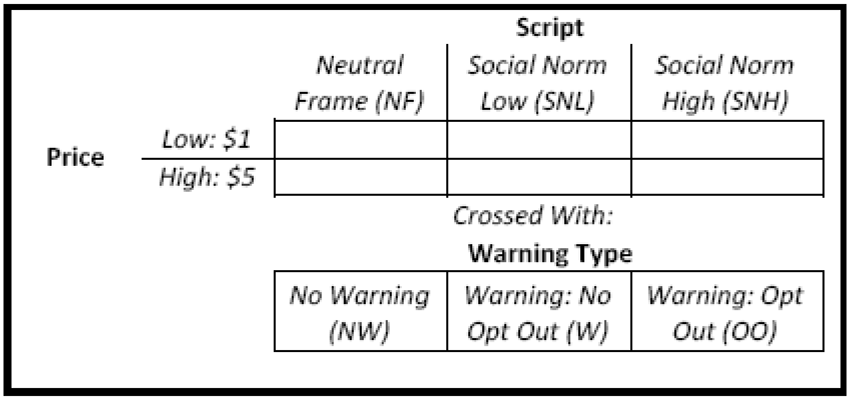
$$p_j(A|H, \rho_0) < p_j(A|H, \rho_L) < p_j(A|H, \rho_M) \text{ for } j\epsilon\{NW, W, OO\}.$$

In these two sets of predictions, $p_L$and $p_H$are the \$1 and \$5 treatments, respectively, while ρ_0, ρ_L, and ρ_H indicate the treatments "neutral frame", "social norm low" and "social norm high". These predictions follow directly from comparative static results for the attitude threshold discussed above.

2.4 Experimental Design and Implementation

The theoretical model above directly informs the design of our natural field experiment, which could be utilized for any energy efficient technology but was used for CFLs in this research. Beginning with the second stage, script and price treatments provides variation to identify the impact of social pressure and price on the decision to purchase. However, the decision to purchase could be due to underlying social pressure present in all treatments or the private and public attributes of the purchase. The model's first stage suggests a treatment to parse the impacts of atti-

Figure 2.3: Treatment Cells



tude and social pressure through including a treatment allowing consumers to select into or out of the purchase situation. In order to provide households with the opportunity to select out of the purchase situation, door-hangers were placed on the doors of households the day before the households were visited to offer CFLs for purchase. These door-hangers, shown in figure 2.2, informed the household that it would be visited the following day by someone with an "offer for purchase and discuss energy saving light bulb options," creating the opportunity for the household members to vary their likelihood of being home. In total, there are 18 treatment cells in the experimental design. These cells are displayed in figure 2.3 . Table 2.1 gives the sample size by treatment cell. The experimental implementation and treatments will be discussed more in depth below. The experiment was implemented by offering households up to 2 packages of CFLs to purchase at their doorstep (each package contained 4 CFLs). Approaching consumers at their doors may not appear to be the most applicable method for technology adoption. However, energy conservation programs often employ door-to-door approaches to encourage adoption of technologies such as CFLs. Some examples of recent programs are the Clean Development

Social Pressure and Price Treatments		Door-Hanger Treatments *No Warning*	*Warning: No Opt*	*Warning: Opt Out*
Neutral Frame	*$1*	480	474	473
	$5	435	546	501
Social Norm Low	*$1*	447	508	535
	$5	493	544	491
Social Norm High	*$1*	454	469	481
	$5	431	511	542
Each cell gives the number of households approached for the treatment group.				

Table 2.1: Treatment Sample Size

Survey Question: Why do you own or plan to purchase CFLs?		Survey Question 7a-No: Would you mind telling me why you don't own or plan to purchase any CFLs?	
Energy Efficiency	55%	Disposal Concerns	18%
Cost Savings	28%	Quality Concerns	20%
Environment	18%	Too Expensive	11%
Longer Lasting	14%	Limitations	10%
Don't Know	8%	Enough bulbs already	39%
Quality	3%	Don't like them/Care	11%
N=478		*N=61*	
Households were permitted to give multiple responses.			

Table 2.2: Survey Results on CFL Opinions

Mechanism projects in the South Urban Lahore district of Pakistan and Visakhaptanam India, and a door-to-door project in Boulder, Colorado in the summer of 2010. Although there is a natural interest in working within stores, that setting would provide a select sample of consumers looking to replace their light bulbs. On one hand such a sample is of interest, but the benefits gained by installing CFLs is immediate and thus an ideal sample to approach is all consumers that use traditional incandescent light bulbs. Further, approaching households directly allows for greater ease of warning a household ahead of time as well as direct control of the stated social norm, which are necessary for uncovering the impact of social pressure.

Households were approached during the day on weekends by University of Chicago students employed through the Becker Center at the University of Chicago. The students were hired after responding to job advertisements placed around the campus of the University of Chicago and on the University's main electronic help wanted web site (marketplace.uchicago.edu). After responding to the advertisement, the students were briefly interviewed. The students were then hired and given a time to come back for a training session. The training sessions lasted approximately 30 minutes and were conducted with multiple students so that they could practice scripts with each other.

Students were driven out to the suburbs on Saturdays and Sundays to approach households over 4 one-hour blocks of time each day: 10 a.m. to 11 a.m., 11 a.m. to noon, 1 p.m. to 2 p.m. and 2 p.m. to 3 p.m. The households were grouped into these hour-long blocks for the students, with each block containing roughly 25 houses and each one-hour block of houses randomly assigned to a treatment so that a given student typically had a different treatment each hour. The CFL sales took place on weekends between June 2009 and June 2010 with a break in the winter months and on weekend days when it was either too cold or raining.

Households in the following locations around Chicago were approached: Arlington Heights, Elmwood Park, Evanston, Lemont, Libertyville, Oak Park, River Forest, Roselle, Skokie, and Wheaton. These suburbs range in median household income from $47,315 to $89,284 as reported in the 2000 U.S. Census ($1999). This is in comparison to the United States national and Illinois median household incomes of $41,994 and $46,590 respectively. Clearly the sample approached in this study had higher median incomes then is typical in the U.S. One concern with lower income areas is that it is not possible to go door-to-door within apartment complexes, making it necessary to use locations with free-standing houses which are most often found in higher income areas. Regardless, the range of median incomes from the areas visited is an important caveat to our results and limits the generalizability of the results.

In addition to the main treatments which will be discussed next, surveys were conducted after the purchase decision was made. The surveys were intended to provide some indications of a consumer's interest in and preferences for or against CFLs. The survey rate is considerably lower than the door answer rate at 7.2% versus 32%. Table 2 reports results from two survey questions regarding the decision to own or plan on purchasing CFLs. The results indicate that disposal and quality concerns are the main factors keeping consumers from utilizing CFLs while energy efficiency is the most often reported reason for owning.

The day prior to the students visiting households, a team of researchers and interns placed door-hangers on households in W and OO treatments. While placing the door-hangers on doors, the researchers and interns only interacted with members of the households if they were approached directly (i.e. household members were outside in the yard and witnessed the placement of the door-hanger). Although the student sales-persons were aware of the different scripts and prices for each treatment, they were not aware that only some houses had been warned of the visit the day prior via the door-hangers.

The main treatments were delivered through scripts given to student sales-persons for each one-hour treatment block of households. The scripts varied in two ways: stated social norm and sale price of CFL package (see appendix for scripts). The CFLs sold door-to-door cost between \$3.85 and \$7.15 for a package of 4 CFLs, before tax, in stores throughout the Chicago area. The average price tended to be around \$5.00 when tax was included so we set our baseline price for a package of 4 CFLs at \$5.00. We included one other price point, a low price of \$1.00.[12] The low price of \$1.00 was chosen for a number of reasons. The main reason is that, depending on quality and type, a package of 4 incandescent light bulbs can range from \$0.33 to \$5.00 but is typically around \$1.00. Thus, pricing the package of 4 CFLs at \$1.00 made it reasonably equivalent to purchasing a new package of incandescent light bulbs. A secondary reason was that logistically, selling goods for \$5.00 and \$1.00 simplifies producing change from purchases.

[12]Although demand is likely non-linear, only two prices were chosen due to sample size constraints. In the future, we hope to include a third price point to address the possibility of non-linear demand.

Social pressure can be applied in a number of ways to varying degrees of efficacy. For example, psychologists have written extensively on approaches specific to door-to-door sales such as the 'Foot-in-the-Door" approach and the "Door-in-the-Face" approach. As discussed in the literature review above, recent research has found social norms to be an effective social-psychological pathway to impact a person's decisions. This led us to incorporate social norms into our experimental design as a test for the impact that social-psychology can have on adoption. In order to incorporate social norms, a script without a statement of social norms, referred to as the "Neutral Frame" (NF) treatment, and two treatments involving a statement of social norms, "Social Norm Low" (SNL) and "Social Norm High" (SNH), were run. Both social norm treatments augment the general NF script by one statement prior to stating the price. SNL uses the phrase: "For instance, did you know that 70% of U.S. households own at least one CFL?" while the SNH treatment uses the phrase: "For instance, did you know that 70% of the households that we surveyed *in this area* own at least one CFL?" The change in the proximity with which the social norm is stated follows in line with Goldstein et al. (2008).

Since the treatments come directly from theory they act as a natural test of the model's predictions. In addition, including variation in social pressure and price allows for a pricing of social pressure as will be discussed in the results. Further, analyzing not only the decision to purchase but considering also how many packages of CFLs were purchased permits some indication of whether consumers appear to be "buying out" of social pressure or "buying in" to a new technology due to information contained in the social norm.

2.5 Results

The results in this section consider the probability of being home, the probability of purchasing, and the probability of purchasing zero, one or two packages of CFLs. The results support an overall story that social pressure, in addition to price, has a significant impact on the decision to purchase. A considerable amount of this story is explained simply by considering the raw means. However, a two step hurdle model was run in order to control for additional factors and explore the decision to purchase one or two packages.

The two step hurdle model that was run is conventional in nature. First, a probit model with fixed effects for solicitor s, time t, day d and city c was estimated with an outcome of the decision to purchase or not. The probit model is specified as follows:

$$y*_{i,s,t,d,c} = \beta_0 + \beta X_{i,s,t,d,c} + \delta T_{i,s,t,d,c} + \gamma_s + \nu_t + \omega_d + \eta_c + \varepsilon_{i,s,t,d,c}$$

where $y*_{i,s,t,d,c}$ is a latent variable that is not observed but instead is represented by a discrete variable indicating whether the household decided to purchase any packages of CFLs as follows: $y*_{i,s,t,d,c} = \begin{cases} 1 & if\ y* > 0 \\ 0 & if\ y* \leq 0 \end{cases}$. The independent variable $X_{i,s,t,d,c}$, is the one recorded variable from each household: "Do you own CFLs?" and $T_{i,s,t,d,c}$ are treatment variables (social pressure variables are dummy variables while price is included as a continuous measure). Finally, γ_s, ν_t, ω_d, η_c and $\varepsilon_{i,s,t,d,c}$ are conventional error terms. In some specifications, treatment dummy variables were interacted with other treatment dummies and with $X_{i,s,t,d,c}$ but for simplicity the model can be represented as above. The second step of the hurdle model was estimating the number of packages purchased for the sub-sample that chose to purchase using a Truncated Poisson model utilizing the same variables as the linear probability model with the dependent outcome variable as the number of packages purchased, either 1 or 2.

2.5.1 Selection: the impact of warning households

The consumer's decision on whether to be home is a main factor in the theoretical model developed above. This selection decision helps parse the impact of social pressure from the private and public attributes of CFLs. Intuitively, a household that avoids answering its door places the impact of social pressure above the benefits that could be gained from answering. As mentioned above, the experimental design utilizes door-hangers, shown in figure 2.2, placed on a household's door the day before the student solicitors arrived to allow for a household to select into or out of interacting with student salespersons. Using this experimental approach, DLM finds evidence that social pressure has a larger impact than the desire to donate to charities and our results are consonant with DLM's results: households avoid student salespersons when warned.

Table 2.3 reports the results for the doors answered and the purchase decision. The top half of table 2.3 reports the raw data while the bottom half includes the answer, purchase and conditional purhcase rates as well as results from a chi-square test of proportion. As shown in table 2.3, houses that were not warned answered the door 36.72% of the time, while households that were warned answered the door 33.22% and 27.39% of the time for the W and OO treatments respectively. Given the predictions in section 3.2.1, these results point towards the existence of social pressure. However, results from table 2.3 provide mixed support for the predictions in section 3.2.2 that assume the existence of social pressure. Unconditional on answering the door, houses that were not warned purchased 3.21% of the time, while households in the W and OO treatments purchased CFL's 3.83% and 2.81% of the time respectively. While the difference in purchase rates between the NW and OO treatments is consistent with the predictions under the extreme scenarios of our model discussed above, the difference across the W and NW treatments is not. Overall, the results point to the existence of social pressure but not to one of the extreme cases discussed in the previous section, rather that social pressure exists and that there are attitude types that are interested in purchasing CFLs.

An intuitive prediction from the selection permitted by the door-hangers is that, conditional on answering the door in the warned treatments, we should expect to see more purchases. This is indeed the case as shown in table 2.3. Conditional on answering the door, households in the W and OO treatments are purchasing 11.54% and 10.27% of the time compared to 8.75% of the time for households that are not warned. The significant difference between purchasing conditional on answering between the NW and W treatments suggests that warned households are selecting into answering their door knowing that they will be able to purchase the new technology. Evidence of increased purchases by households in OO and W treatments is also found in the fully specified regression results in table 2.6*b* discussed below. In short,

Warning Treatment	Number of Doors Knocked On	Number of Doors Answered	Number of Purchases
No Warning (NW)	2740	1006	88
Warning: No Opt Out (W)	3052	1014	117
Warning: Opt Out (OO)	3023	828	85

Percentages and Chi-Square Test of Proportions

Warning Treatment	Null for χ^2 test	*Percent of Doors Answered*		*Percent of Purchases*		*Percent of Purchases | Answering*	
NW	*NW - W = 0*	36.72%	*0.005*	3.21%	*0.201*	8.75%	*0.038*
W	*NW - OO = 0*	33.22%	*0.000*	3.83%	*0.374*	11.54%	*0.268*
OO	*OO - W = 0*	27.39%	*0.000*	2.81%	*0.026*	10.27%	*0.385*

*P-values for χ^2 test of proportions given in italics. Households that chose to opt-out of treatment OO are included as doors knocked on but not answered. There were 352 households of the 3023 in the opt out treatment that chose to opt out.

Table 2.3: Warning Treatment Results: Doors Answered and Purchase Decision

Table 4: Price and Social Norm Treatment Results: Purchase Decision Conditional on Answering

Price Treatment	Number of Doors Answered	Number of Purchases	Percentage Purchased	Null Hypothesis	P-value
Low Price ($1)	1426	207	14.52%	*$1 - $5 = 0*	*0.000*
High Price ($5)	1422	83	5.84%		
Social Norm Treatment					
Neutral Frame (NF)	995	79	7.94%	*NF - SNL = 0*	*0.016*
Social Norm Low (SNL)	865	97	11.21%	*NF - SNH = 0*	*0.007*
Social Norm High (SNH)	988	114	11.54%	*SNH - SNL = 0*	*0.826*

P-values for χ^2 test of proportions given in italics.

Table 2.4: Price and Social Norm Treatment Results: Purchase Decision Conditional on Answering

it appears that while both treatments positively affect the likelihood of purhcase, only the W treatment is significant and only the OO treatment has a significant impact on the number of packages purchased.

2.5.2 Comparison of Means: Economic and Social-Psychology Treatments

The results for mean purchase rates across the price and social norm treatments are shown in table 2.4. As can be seen from the table, price has a large and significant impact on demand, with our price reduction from $5 to $1 resulting in almost a

Mid-point Price Elasticity of Demand			
Social Pressure:	**Neutral Frame (NF)**	**Social Norm Low (SNL)**	**Social Norm High (SNH)**
Elasticity:	-0.845	-0.853	-0.643

Equivalent Price Change for NF Elasticity		
	Change in Demand	**Equivalent Percentage Change in Price**
Social Norm Statement Change from NF to SNL		
High Price ($5)	23.73%	**-28.10%**
Low Price ($1)	25.35%	**-30.02%**
Social Norm Statement Change from NF to SNH		
High Price ($5)	59.46%	**-70.40%**
Low Price ($1)	25.35%	**-30.02%**

Given the percentage change in demand due to the addition of the social norm statement, the equivalent percentage change in price for each price level was calculated as the the change that would result in a price elasticity equal to the mid-point elasticity under the neutral frame: -.845.

Table 2.5: Price and Social Pressure Elasticities

250% increase in the decision to purchase. Incorporating the amount of packages purchased into these raw results leads to a mid-point price elasticity of demand of -.844 under the NF treatment as shown in table 2.5. With the caveat that the variation in price is quite large, this elasticity measurement can be used with the results of the social-psychology treatments to provide a price estimate on the value of the social norms used here. Before discussing that implied elasticity, first consider results from using the statement of social norms reported in table 2.4.

The results in table 2.4 suggest that stating social norms does impact the decision to adopt new technology with both the SNL and SNH treatments having a significantly higher rate of purchase than NF. As reported in table 2.4, 7.94% of the households that answer their door purchase in the NF treatment, while 11.21% and 11.54% of households decided to purchase in the SNL and SNH treatments respec-

	Probit	Truncated Poisson	Probit	Truncated Poisson	Probit	Truncated Poisson	Probit	Truncated Poisson
Low Price	0.524***	0.667***	0.568***	0.681***	0.579***	1.141**	0.581***	1.981***
	(0.0818)	*(0.1820)*	*(0.0838)*	*(0.1790)*	*(0.1410)*	*(0.4510)*	*(0.1730)*	*(0.6480)*
Social Norm Low (SNL)	0.212**	0.123	0.0377	0.102	-0.0254	0.495	0.00215	0.702
	(0.0954)	*(0.1220)*	*(0.0999)*	*(0.1200)*	*(0.1630)*	*(0.5180)*	*(0.2200)*	*(0.5580)*
Social Norm High (SNH)	0.233**	-0.0566	0.258***	-0.051	0.328**	0.543	0.339*	0.880*
	(0.0976)	*(0.1390)*	*(0.0996)*	*(0.1360)*	*(0.1520)*	*(0.5100)*	*(0.1860)*	*(0.5190)*
Opt Out	0.123	0.281*	0.126	0.275*	0.124	0.263*	0.124	0.269*
	(0.1270)	*(0.1640)*	*(0.1330)*	*(0.1590)*	*(0.1320)*	*(0.1580)*	*(0.1320)*	*(0.1540)*
Warning	0.125	0.0933	0.124	0.0992	0.126	0.108	0.127	0.116
	(0.1080)	*(0.1740)*	*(0.1150)*	*(0.1720)*	*(0.1150)*	*(0.1700)*	*(0.1150)*	*(0.1720)*
Warning-Env	0.102	0.228	0.113	0.213	0.111	0.193	0.11	0.17
	(0.1230)	*(0.1590)*	*(0.1230)*	*(0.1590)*	*(0.1240)*	*(0.1560)*	*(0.1250)*	*(0.1540)*
Opt Out-Env	-0.0812	-0.173	-0.046	-0.169	-0.0457	-0.167	-0.0459	-0.192
	(0.1460)	*(0.1940)*	*(0.1480)*	*(0.1860)*	*(0.1480)*	*(0.1810)*	*(0.1480)*	*(0.1780)*
Own CFLs			-0.195	-0.0241	-0.194	-0.00498	-0.177	0.965*
			(0.1440)	*(0.1460)*	*(0.1430)*	*(0.1440)*	*(0.2080)*	*(0.5570)*
Own CFLs Missing			-1.237***	-0.286	-1.236***	-0.279	-1.236***	-0.295
			(0.1560)	*(0.1880)*	*(0.1560)*	*(0.1900)*	*(0.1560)*	*(0.1860)*
SNL*Low Price					0.0991	-0.45	0.093	-0.482
					(0.2070)	*(0.5270)*	*(0.2090)*	*(0.5200)*
SNH*Low Price					-0.112	-0.693	-0.116	-0.869*
					(0.1900)	*(0.5290)*	*(0.1930)*	*(0.5030)*
SNL*Own CFLs							-0.0387	-0.237
							(0.1990)	*(0.2350)*
SNH*Own CFLs							-0.0177	-0.247
							(0.1770)	*(0.2210)*
Own CFLS * Low Price							0.00406	-0.908*
							(0.1630)	*(0.5500)*
Constant	-1.818***	-0.806***	-1.122***	-0.734***	-1.130***	-1.149**	-1.141***	-2.020***
	(0.1020)	*(0.2210)*	*(0.1710)*	*(0.2560)*	*(0.1860)*	*(0.4870)*	*(0.2130)*	*(0.6760)*
City Effects	*None*	*None*	*None*	*None*	*None*	*None*	*None*	*None*
N	2848	290	2848	290	2848	290	2848	290
pseudo R-sq	0.041	0.126	0.164	0.129	0.165	0.132	0.165	0.136

Standard errors are clustered at the treatment block level and are given in parentheses (* p<0.10, ** p<0.05, *** p<0.01). Note: 76 households are droped when Surveyor Fixed Effects are inluded because 4 surveyors did not sell any packages.

Table 2.6a: Two Stage Hurdle Model Estimates

tively. This 40% increase in the purchase rate is somewhat surprising considering the small amount of additional information provided in these treatments. Although table 2.4 suggests that the SNL treatment has a large effect, this effect declines and loses significance when control variables are included in a probit regression as reported in tables 2.6a and 2.6b. On the other hand, tables 2.6a and 2.6b

show that the low price and the SNH treatment, which uses a statement from

	Probit	Truncated Poisson	Probit	Truncated Poisson	Probit	Truncated Poisson	Probit	Marginal Effects	Truncated Poisson	Marginal Effects
Low Price	0.599***	1.982***	0.602***	2.038***	0.599***	2.011***	0.537***	**0.0746**	1.985***	**1.7305**
	(0.1760)	*(0.6390)*	*(0.1800)*	*(0.6280)*	*(0.1760)*	*(0.6410)*	*(0.1780)*	***(0.0252)***	*(0.6120)*	***(0.5290)***
SNL	0.119	0.464	0.12	0.529	0.119	0.481	0.0115	**0.0016**	0.624	**0.5443**
	(0.2300)	*(0.5730)*	*(0.2310)*	*(0.5590)*	*(0.2300)*	*(0.5780)*	*(0.2250)*	***(0.0313)***	*(0.5440)*	***(0.4740)***
SNH	0.357*	0.854*	0.354*	0.842*	0.357*	0.870*	0.188	**0.0261**	0.935*	**0.8148**
	(0.1850)	*(0.5080)*	*(0.1840)*	*(0.4950)*	*(0.1860)*	*(0.5120)*	*(0.1840)*	***(0.0257)***	*(0.5200)*	***(0.4549)***
Opt Out	0.184	0.311*	0.185	0.268*	0.183	0.309*	0.196	**0.0272**	0.396**	**0.3450**
	(0.1380)	*(0.1650)*	*(0.1370)*	*(0.1570)*	*(0.1380)*	*(0.1640)*	*(0.1280)*	***(0.0178)***	*(0.1770)*	***(0.1546)***
Warning	0.224*	0.152	0.232*	0.121	0.223*	0.16	0.270**	**0.0375**	0.0245	**0.0214**
	(0.1210)	*(0.1790)*	*(0.1210)*	*(0.1720)*	*(0.1230)*	*(0.1790)*	*(0.1190)*	***(0.0166)***	*(0.1800)*	***(0.1567)***
Warning-Env	0.0847	0.182	0.0908	0.282*	0.0854	0.172	0.046	**0.0064**	0.225	**0.1961**
	(0.1270)	*(0.1630)*	*(0.1290)*	*(0.1610)*	*(0.1270)*	*(0.1620)*	*(0.1300)*	***(0.0181)***	*(0.1780)*	***(0.1540)***
Opt Out-Env	-0.0254	-0.2	-0.0241	-0.135	-0.0258	-0.188	-0.0952	**-0.0132**	-0.252	**-0.2198**
	(0.1470)	*(0.1810)*	*(0.1470)*	*(0.1850)*	*(0.1470)*	*(0.1820)*	*(0.1460)*	***(0.0202)***	*(0.2160)*	***(0.1882)***
Own CFLs	-0.189	1.038*	-0.187	1.085**	-0.188	1.051*	-0.172	**-0.0239**	1.071**	**0.9340**
	(0.2130)	*(0.5470)*	*(0.2130)*	*(0.5460)*	*(0.2140)*	*(0.5430)*	*(0.2210)*	***(0.0306)***	*(0.5220)*	***(0.4553)***
Own CFLs Missing	-1.258***	-0.232	-1.258***	-0.223	-1.257***	-0.23	-1.216***	**-0.1689**	-0.168	**-0.1468**
	(0.1580)	*(0.1850)*	*(0.1570)*	*(0.1850)*	*(0.1590)*	*(0.1820)*	*(0.1700)*	***(0.0233)***	*(0.2110)*	***(0.1836)***
SNL*Low Price	0.0504	-0.406	0.0529	-0.44	0.0505	-0.432	0.0319	**0.0044**	-0.498	**-0.4339**
	(0.2130)	*(0.5130)*	*(0.2180)*	*(0.4930)*	*(0.2130)*	*(0.5190)*	*(0.2110)*	***(0.0293)***	*(0.4590)*	***(0.3995)***
SNH*Low Price	-0.126	-0.847*	-0.116	-0.832*	-0.126	-0.875*	0.0975	**0.0135**	-0.863*	**-0.7520**
	(0.1960)	*(0.4850)*	*(0.1980)*	*(0.4700)*	*(0.1960)*	*(0.4910)*	*(0.1960)*	***(0.0272)***	*(0.4770)*	***(0.4164)***
SNL*Own CFLs	-0.103	-0.186	-0.103	-0.229	-0.104	-0.188	-0.0264	**-0.0037**	-0.459	**-0.4003**
	(0.2070)	*(0.2380)*	*(0.2070)*	*(0.2550)*	*(0.2070)*	*(0.2380)*	*(0.2120)*	***(0.0295)***	*(0.3090)*	***(0.2704)***
SNH*Own CFLs	-0.00828	-0.301	-0.00357	-0.273	-0.00813	-0.315	0.0355	**0.0049**	-0.41	**-0.3570**
	(0.1810)	*(0.2230)*	*(0.1800)*	*(0.2240)*	*(0.1810)*	*(0.2210)*	*(0.1830)*	***(0.0254)***	*(0.2570)*	***(0.2249)***
Own CFLS * Low Price	0.0179	-0.984*	0.0156	-1.012*	0.0181	-0.995*	0.0528	**0.0073**	-0.82	**-0.7145**
	(0.1640)	*(0.5420)*	*(0.1650)*	*(0.5380)*	*(0.1640)*	*(0.5380)*	*(0.1710)*	***(0.0237)***	*(0.5030)*	***(0.4379)***
Constant	-1.328***	-1.984***	-1.306***	-1.867***	-1.338***	-1.911***	-1.185***		-1.946***	
	(0.2480)	*(0.6830)*	*(0.2480)*	*(0.6700)*	*(0.2670)*	*(0.6890)*	*(0.2920)*		*(0.6890)*	
City Effects	*City Fixed Effects*	*City Fixed Effects*	*City and Time Fixed Effects*	*City and Time Fixed Effects*	*City and Day Fixed Effects*	*City and Day Fixed Effects*	*City, Time, Day and Surveyor Fixed Effects*		*City, Time, Day and Surveyor Fixed Effects*	
N	2848	290	2848	290	2848	290	2772		290	
pseudo R-sq	0.174	0.147	0.175	0.154	0.174	0.147	0.24		0.2	

Standard errors are clustered at the treatment block level and are given in parentheses (* p<0.10, **p<0.05, *** p<0.01). Note: 76 households are droped when Surveyor Fixed Effects are invluded because 4 surveyors did not sell any packages.

Table 2.6*b*: Two Stage Hurdle Model Estimates households in increased proximity to the approached household, continue to have an impact with the inclusion of control variables.

As mentioned, an important result from our initial study is the ability to "price" the impact of the social norm used. The mid-point price elasticity of demand under the NF treatment is -0.845. As shown in table 2.5, based on this elasticity, the reduction in price that would result in the same quantity change as that of introducing a statement similar to our SNH treatment at \$5.00 is 70%. That is, stating the "high" social norm when the CFL packages cost \$5.00 results in the same increase in demand as reducing the package price to roughly \$1.50! Even SNL has a surprising impact on purchase rates: the equivalent of a 30% price reduction, or a reduction in price to roughly \$3.50.

2.5.3 Hurdle Model: Economic and Social-Psychology Treatments

Tables 2.6a and 2.6$b$report the results for a number of specifications of a two stage hurdle model of CFL adoption in a series of 8, 2-column pairs for each specification run. The left column of each pair reports the Probit model while the right column reports estimates from the Truncated Poisson model. The specification for each model increases in the amount of control variables moving from left to right across each table and from table 2.6a to 2.6b. Table 2.6a does not include any fixed effects, and begins with the first two columns of the table as the baseline model which includes dichotomous control variables for the two social norm treatments: Social Norm Low (SNL) and Social Norm High (SNH), the two warning treatments:

	Number of Doors Knocked On		Number of Doors Answered		Number of Purchases	
Door Hanger Type:	*Energy (E)*	*Environmental (ENV)*	*E*	*ENV*	*E*	*ENV*
Warning: No Opt Out (W)	1483	1569	511	503	55	62
Warning: Opt Out (OO)	1538	1485	416	412	46	39
Total (T)	3021	3054	927	915	101	101

Percentages and Chi-Square Test of Proportions

Warning Treatment	*Null for χ^2 test*	Percent of Doors Answered			Percent of Purchases			Percent of Purchases | Answering		
	Door Hanger Type:	*E*	*ENV*		*E*	*ENV*		*E*	*ENV*	
W	*W: E - ENV = 0*	34.46%	32.06%	*0.16*	3.71%	3.95%	*0.727*	10.76%	12.33%	*0.436*
OO	*OO: E - ENV = 0*	27.05%	27.74%	*0.668*	2.99%	2.63%	*0.544*	11.06%	9.47%	*0.451*
T	*T: E - ENV = 0*	30.69%	29.96%	*0.539*	3.34%	3.31%	*0.937*	10.90%	11.04%	*0.922*

P-values for χ^2 test of proportions given in italics. The difference in door-hangers was a title of "Energy Cost Initiative" for the *Energy* type and "Environmental Initiative" for the *Environmental* type, otherwise, the door-hangers were identical.

Table 2.7: Door-Hanger Type Treatment Results: Doors Answered and Purchase Decision

OO and W, and a dummy variable, "Low Price," indicating the $1 treatment. Additionally, dichotomous indicators are included for houses that received a door-hanger stating "Environment Initiative" ("Warning-Env" and "Opt Out-Env") rather than "Energy Cost Initiative"; there was no significant difference in either doors answered or purchase rates so the results have been pooled.[13] The last 2 columns of table 2.6a include the coefficient estimates for the fully specified model without fixed effects. In addition to the variables in the baseline regression, the fully specified model includes seven additional controls. The "Own CFLs" variable indicates whether the household currently owns CFLs. This variable is the result of the solicitors asking each household if they currently have or are familiar with CFLs. However, because households did not always respond to the inquiry a second variable is included in the analysis, "Own CFLs Missing," which indicates if the household did not respond. These missing values in "Own CFLs" are recorded as zeros. The coefficient on "Own

[13]The results of the comparison are found in table 2.7 for the number of doors answered and the purchases conditional on answering.

CFLs Missing" is negative and significant in the probit regressions for all specifications, which is in line with a presumption that missing values indicate a shorter conversation (an immediate "No") at the household.

Table 2.6b adds four sets of fixed effects to the more fully specified model in the last two columns of table 2.6a: "City" indicating the City, Village or Suburb the household is in, "Time" denoting one of the four possible hours (10am, 11am, 1pm and 2pm), "Day" for the day of the weekend (Saturday or Sunday), and "Surveyor" for one of the 47 surveyors. The last four columns represent the eighth and final hurdle model which includes all controls and fixed effects. Marginal effects are only included for this fully specified model to conserve space as it is the focus of the interpretation of the results.

The impact of "Own CFLs" on the decision to purchase is negative and insignificant for all specifications. This inconclusively implies that new consumers may be more likely to adopt the technology. On the other hand, conditional on purchasing, "Own CFLs" has a significant positive effect on the amount purchased under the more fully specified models. This suggests that consumers that do own CFLs are taking advantage of having the CFLs offered to them at their door by purchasing an additional package in comparison to consumers that do not own CFLs but, after accounting for what appear to be immediate "No" responses through "Own CFLs Missing," are consumers that do not own CFLs are just as likely to purchase as those that do. Rows 12 to 14 of table 2.6a and 2.6b show that the price and social norm statements have no significant impact on consumers that own CFLs as one would expect. In fact, the effect is insignificantly negative for the purchase amount, which may indicate that consumers who own CFLs are reacting negatively to what they see as a sales "pitch."

The coefficients on the main treatments point towards an intuitive story of social pressure and price. Price has a consistent significant impact across the specifications and, in the full specification, suggests that the low price increases the likelihood of purchasing by 7.5%. On the other hand, the SNH treatment has a positive but barely significant impact (10% confidence) on the decision to purchase with the exception of the fully specified model. Although SNH does not have a significant positive impact on purchasing, it does significantly affect the number of packages purchased (10% confidence). However, the impact of SNH on the amount purchased is half the size of the low price, suggesting that social pressure motivates the household to purchase one package while price leads to purchasing two packages, the maximum allowed. One interpretation of this result is that households are purchasing one package to remove the social pressure imposed in the SNH treatment, while price motivates purchasing as much as possible.

Somewhat surprisingly, the interaction between SNH and low price does not increase the amount purchased to a large degree. The interaction term for SNH and low price is the one significant interaction term (at the 10% level) and it is negative. Although it is negative, it is smaller than the coefficient on SNH, leading to an overall positive impact that is slightly larger than the impact from low price alone. This small positive result implies that when it is less expensive to buy out of the social pressure from social norms, slightly more households do so. The lack of large impact from the interaction term may be due to the constraint on purchasing only up to 2 packages of CFLs however it is not possible to tell.[14]

As mentioned briefly in section 5.2, contrary to the evidence from the raw means on the decision to purchase, there is a large difference in the impact of the two social norm treatments when control variables are included. Looking across the specifications, the impact of SNL is initially positive and significant in the specification with the least amount of covariates but loses significance when controls are introduced and also reduces in magnitude. This result provides support for the importance of how targeted a social norm is and is in line with previous research (Goldstein et al. (2008)).

2.6 Conclusion

Environmental concerns have grown in recent years and increasing energy efficiency through household technology adoption could alleviate a portion of these concerns. Unfortunately, adoption of environmental technologies such as CFLs historically has been slow. Our research offers the first direct comparison across multiple academic disciplines of theories on how to spur technology adoption. This direction provides greater understanding of the motives surrounding consumer adoption while also potentially providing the keys to alleviate some of society's concerns over energy usage and environmental quality.

Non-pecuniary techniques to motivate residential energy conservation used in research by social psychologists and behavioral economists have generally focused

[14] Anecdotally, households did occasionally request to receive more lightbulbs, even enticing our student salespersons to come back with "as many as you have". However, allowing for an unlimited number of packages was not realistic in terms of budgeting for this field experiment.

on changing energy usage behavior rather than focusing on encouraging adoption of energy efficient products. This is evident in broad reviews of the existing literature such as Abrahamse, Steg, Vlek, and Rothengatter (2005), as well as more recent research such as Schultz et al. (2007) and Allcott (2009). However, we chose to focus on motivating technology adoption as a specific behavioral change in order to avoid the issues that cause the beneficial energy conservation results to lessen over time, such as the "boomerang" effect and mean reversion. It is possible that adjustments in lighting usage caused our study to not result in the full energy reduction potential. We attempt to address this by conducting follow-up interviews but this should be considered more fully in future research provided that energy usage data can be obtained.

The results from our initial study suggest that in addition to price reductions, social norms do play a role in the decision to adopt new technology. As reported above, 7.94% of the households that answer their door purchase in the neutral frame treatment, while 11.21% and 11.54% of households decided to purchase in the SNH and SNL treatments. However, only the SNH treatment has a significantly positive impact across most of the specifications and a significant effect at the 10% level on the number of packages being purchased.

Although price and social norms both have an effect, the effect on the amount purchased through SNH was found to be half the size of the effect from price. This results implies that price and social norms affect consumers on two different margins. Price appears to affect the intensive margin by motivating consumers to purchase as much as possible, while social norms affects the extensive margin by getting consumers to purchase one package.

Price and social norms working on different margins has important implications when considering how to motivate consumers to utilize new environmental technologies. The results suggest that the use of price reductions through subsidies should be considered when there is a desire to encourage further diffusion of the technology, while social-psychology motivations should be considered when attempting to encourage adoption. This intriguing result is intuitive and could be easily explored further in future research.

Chapter 3

The Value of Choice: A WTA Estimate of an "Opt-Out" Option to Adopt Environmental Technology

3.1 Introduction

The decision to consent to an action seems simple enough, either you approve of the action or you do not. However, empirical evidence points to starkly higher rates of consent if consent is presumed from the outset using what is known as an “opt-out” consent procedure rather than an “opt-in” procedure which requires explicit consent. Sunstein and Thaler (2003) discusses the possible social and even personal benefits resulting from this “libertarian paternalistic” opt-out procedure, but also points out concerns expressed by many individuals for how it affects an agent’s ability to choose. The concern is that an agent’s choice is influenced by consent procedures, such as opt-out, which do not require her to take action when making a decision. This debate implicitly suggests an agent places value on making the choice herself. However, agents’ values for the ability to take action in these procedures appear to have not been rigorously investigated. This paper first contributes additional evidence to the discrepancy between opt-out and opt-in procedures and then provides a first step towards understanding how much value agent’s place on taking action in opt-out procedures. This is done by estimating an apartment tenant’s willingness to accept

(WTA) to forgo an opt-out option to adopt water and energy saving aerator faucets such as low-flow shower heads in apartment units in Denver, CO.

Data for the empirical evaluation of this research is generated through a natural field experiment run at 13 apartment buildings, managed by Equity Residential (ER), in the Denver area. Both opt-in and opt-out procedures were conducted by having building managers place notices throughout the apartment buildings and in tenant mailboxes in order to announce an opportunity to install new water and energy saving aerator faucets. The notice can be seen in figure 3.1 with adjustments for treatment groups denoted in italics within parentheses. The notices differed in two ways based on treatment: 1) opt-in or opt-out and 2) inclusion or exclusion of a social norm statement.[1] If a tenant accepted the offer, the aerators were provided for free and installed by the apartment building maintenance staff at no cost to the tenant. In the opt-out treatments, if a tenant chose to opt-out, she was subsequently offered $0, $5, $10 or $20 in gift cards, depending on treatment, in exchange for overturning her opt-out decision and having the technology installed (thus forgoing her opt-out option).

A tenant's decision regarding the financial offer to forgo use of an opt-out option reveals whether the offer was adequate compensation to forgo her ability to take action and opt-out. This information is used to estimate her WTA to forgo the option. Due to the design of the experiment, the outcome evaluated is not a comparison of opt-out and opt-in procedures but rather a comparison of opt-out and

[1]The statement that was included is: "Based on a recent survey, nine out of ten people who live in our Denver properties tell us that living an environmentally friendly lifestyle is important to them." This can be seen in figure 3.1.

Figure 3.1: Notice for Apartment Residents (Italics Differentiate Language for Treatments)

Equity Residential

DENVER WATER

Attention Residents

Based on a recent survey, nine out of ten people who live in our Denver properties tell us that living an environmentally friendly lifestyle is important to them. We share that thinking and are working on reducing our impact on the planet. (*We believe that promoting an environmentally friendly lifestyle is important and are working on reducing our impact on the planet.*) For example, in the last year we added recycling to all our Denver locations – including your property.

As you may already be aware, Denver Water is raising water prices in 2010. On average, Denver residential customers will pay $40 to $50 more per year in 2010

In cooperation with Denver Water, we will install new plumbing fixtures in your apartment at no cost to you. Denver Water has offered to replace the faucet aerators (that's the very end of the faucet where the water comes out) and shower heads with new more efficient ones.

We've tested these products and think you will be happy with the performance in your home and the water savings on your bill. A typical apartment, with two residents who take 10 minute showers each day, will save over 5,000 gallons a year with these new fixtures.

Installations will occur during the week of February 22nd
and will require less than 10 minutes.

We want you to be very satisfied with your experience in your home,
IF YOU (*DO NOT*) WANT these new products installed,
please let us know through this website:

www.tinyurl.com/DenverWatersurvey

If you can't get to this address or if you have any concerns, please contact the Management Office

Thank you,
The Greenwood Plaza Team

what has been referred to as "mandatory consent." Mandatory consent is perhaps poorly phrased as consent is not requested at all but rather, the action is mandated. However, agents can takes steps after wards to reverse the action.[2] Mandatory consent is a truly paternalistic act and thus, valuing a tenant's option to opt-out should be seen against this comparison procedure rather than a more libertarian method. This valuation does result in a WTA measure that represents an agent's willingness to forgo her ability to take action and opt-out. However, because it is impossible to receive money to forgo an opt-out option without also deciding to opt back in, an alternative interpretation for the WTA estimate arises: the WTA to opt back in to adoption. This is a completely reasonable alternative interpretation and cannot be separated in this case from the decision to forgo an opt-out option because the WTA estimate is generated for tenants that have chosen to opt-out. [3]

The sample used for this study is a seemingly often overlooked sample for studies on environmental technology adoption: the approximately 32% of rented occupied housing in the United States (2007).[4] This sample was a natural choice to use to investigate a question on adopting environmental technology because renters often either respond to new technology being offered through promotion programs,

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[2]For example, in the case of an organ donor registry, mandatory consent would result in all agents being signed up for the registry but providing an ability for the agent to remove herself from the registry later. Mandatory consent is not far from current policy as, for example, the U.S. Congress passed legislation in 2007 that bans incandescent light bulbs in 2014.

[3]An alternative field experiment that could identify a value for just the opt-out option, would be to offer money to those that decide to forgo the the opt-out option. However, this alternative would be much more expensive to conduct or result in severely limited ranges of values offered to subjects. Further, this estimate would suffer from the alternative interpretation of WTA to adopt. Alternatively, a WTP for the opt-out option could be estimated through offering tenants the option of paying to not adopt. That was not an option in this circumstance and suffers from similar alternative interpretations.

[4]American Housing Survey for the United States: 2007 http://www.census.gov/hhes/www/housing/ahs/ahsfaq.html

or adopt new technology because landlords force them to.[5] This sample generated a key discrepancy found in other research on such things as organ donation and retirement savings programs: utilizing an opt-in procedure results in a lower rate of adoption, 24.46%, than an opt-out procedure, 91.19% (e.g. Abadie and Gay (2004), Beshears, Choi, Laibson and Madrian (2009)). Unfortunately, only one apartment building was placed in the opt-in treatment because the low rate of installation was insufficient for apartment management company, ER, to justify continuing the treatment. Thus, although this limited sample of data is interesting and in line with the stark differences to earlier results it is far from conclusive.

The results for the WTA to forgo the opt-out option are suggestive that tenants do place value on the ability to opt-out: only 7 of 153 tenants that opted out accepted monetary offers. This level of non-response to the monetary offers resulted in a mean lower bound WTA estimate of $18.88 that was constrained to a great deal by the highest value offered in the experiment: $20. Thus, the actual WTA could be much larger and this result should be truly be taken as a lower bound estimate. For instance, taken at face value this result implies that offering $18.88 to tenants should on average induce them to forgo their opt-out decision or that forcing tenants to adopt the aerators only causes those that would opt-out $18.88 of disutility. However, realistically the true WTA could be much larger for these tenants and ER, the principal in this case, would need to consider how this disutility would cause the tenants to respond.[6] Future research should consider increasing this

[5] Apartment management will typically use mandatory consent through announcing installation of a new technology to be adopted in all apartment units.

[6] If the principal were a social planner, it would instead be a trade off between the energy and

constraint (keeping research budgets in mind!) in order to better explore the WTA estimate.

This appears to be the first investigation of its kind. The information gained from this project will inform how to encourage adoption of new environmental technologies and further understand the welfare implications of consent procedures. The next section places this research in the existing literature. Section 3 covers the experimental design. Two econometric approaches used are briefly discussed in section 4. Section 5 contains the results while section 6 provides a short discussion.

3.2 Literature

The economic literature on opt-in and opt-out programs is not extensive in the field of environmental technology adoption. In fact, this paper appears to be the first in the area and the first to estimate a value for having an opt-out option. However, looking more broadly for literature on presumed and explicit consent, previous research often finds a substantial difference between the two procedures. An often cited result when discussing opt-in and opt-out procedures is the difference in organ donation rates between the two procedures. Abadie and Gay (2004) looked at organ donation rates in 22 different countries over a 10-year time period and found that countries using presumed consent had between 25-30% higher donation rates than those with explicit consent, after controlling for other determinants of donation rates. Similarly, Johnson and Goldstein (2003) compared raw consent rates for 11

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cost savings of the technology in comparison to the disutility of the individuals that did not want to adopt.

countries and found an almost 60% difference between opt-out and opt-in consent procedures. The study went further to conduct an online survey with a hypothetical question of whether the respondent would donate their organs. The 161 respondents were randomly placed into opt-in, opt-out and neutral or no default treatments and the results showed that the opt-out treatment led to donation rates almost twice as high as the opt-in treatment (42% vs. 82%) and that the no default treatment led to rates comparable to opt-out (79%).

Presumed versus explicit consent has also been investigated in the context of investment decisions. Madrian and Shea (2001) first demonstrated the difference using data from before and after a change in consent procedures for enrolling in a 401(k) plan at a large company. The change was from an opt-in procedure to an opt-out procedure and the results showed that, when the appropriate sub-sample was compared (short tenure or recently employed employees), the opt-out consent procedure resulted in a 50% increase in the participation rate accounting for various covariates such as rage, age and gender. More recently, large differences between consent procedure were found in Beshears, Choi, Laibson and Madrian (2009), which discusses the impact of opt-out options on savings broadly and points to raw data from one company that shows a 35% difference in enrollment in savings plan depending on procedure, and Benartzi, Peleg and Thaler (2007) which focuses the discussion on the Pension Protection Act of 2006 but points to raw data on retirement investment plan participation rates that have increased 68.5% due to switching from opt-in to opt-out procedures. The natural field experiment discussed in this paper adds to this previous literature by providing another disparity in consent rates dependent on procedure under another context: environmental technology adoption. This paper also extends the literature by being the first of its kind to provide an empirical estimate for how much agents are affected by consent procedures.

While the opt-out method has been found to increase participation rates, it comes with ethical reservations. For example, medical ethics committees have been assessing the research recruitment, testing and vaccination procedures with a concern that opt-out procedures may affect patients in an unethical way by influencing their decisions (Halpern, Ubel and Asch (2007) and Junghans et al. (2005)). Separately, there is currently a debate over whether and how to restrict information gathering procedures on the Internet, with the choice of either opt-in or opt-out procedures as a main question (Johnson, Bellman and Lohse 2002). While there are several ethical considerations to be made, it is clear that the opt-in treatment has had an impact when applied (see for further discussion on ethical use of default options). The ethical reservations give some indication of the issues that some have with presumed consent that are discussed in Sunstein and Thaler (2003). Although these ethical concerns seem well formed and are discussed broadly, there is little empirical evidence of the welfare impacts of the procedures (Choi et al. 2001 and Choi et al. 2003 consider optimal default options and behavior for savings plans). This paper attempts to fill this gap in the literature and inform the level of ethical concern by estimating a WTA for removing an option to opt-out.

3.3 Experimental Design

The natural field experiment was run at apartment buildings located throughout the city of Denver, Colorado. Thirteen apartment buildings with an average of 319 occupied apartment units per building were randomly assigned into treatment groups. The apartment buildings are owned and operated by Equity Residential (ER), a company that owns and operates apartment buildings throughout the United States. ER has a desire to improve the environmental efficiency of their buildings without negatively affecting the opinions of tenants. With these goals in mind, I worked with ER to determine a feasible treatment design to determine the value that tenants place on their ability to decide on installing new environmental technology in their apartments. The new technology that ER wanted tenants to consider installing was low-flow aerators such as shower heads and other faucet heads. There are a variety of types of low-flow aerators with some saving up to 50% of water consumption and 50% of the energy cost from water heating. Neither ER nor the tenants faced any monetary costs for the aerators or for installation because Denver Water covered both the technology's cost and installation.

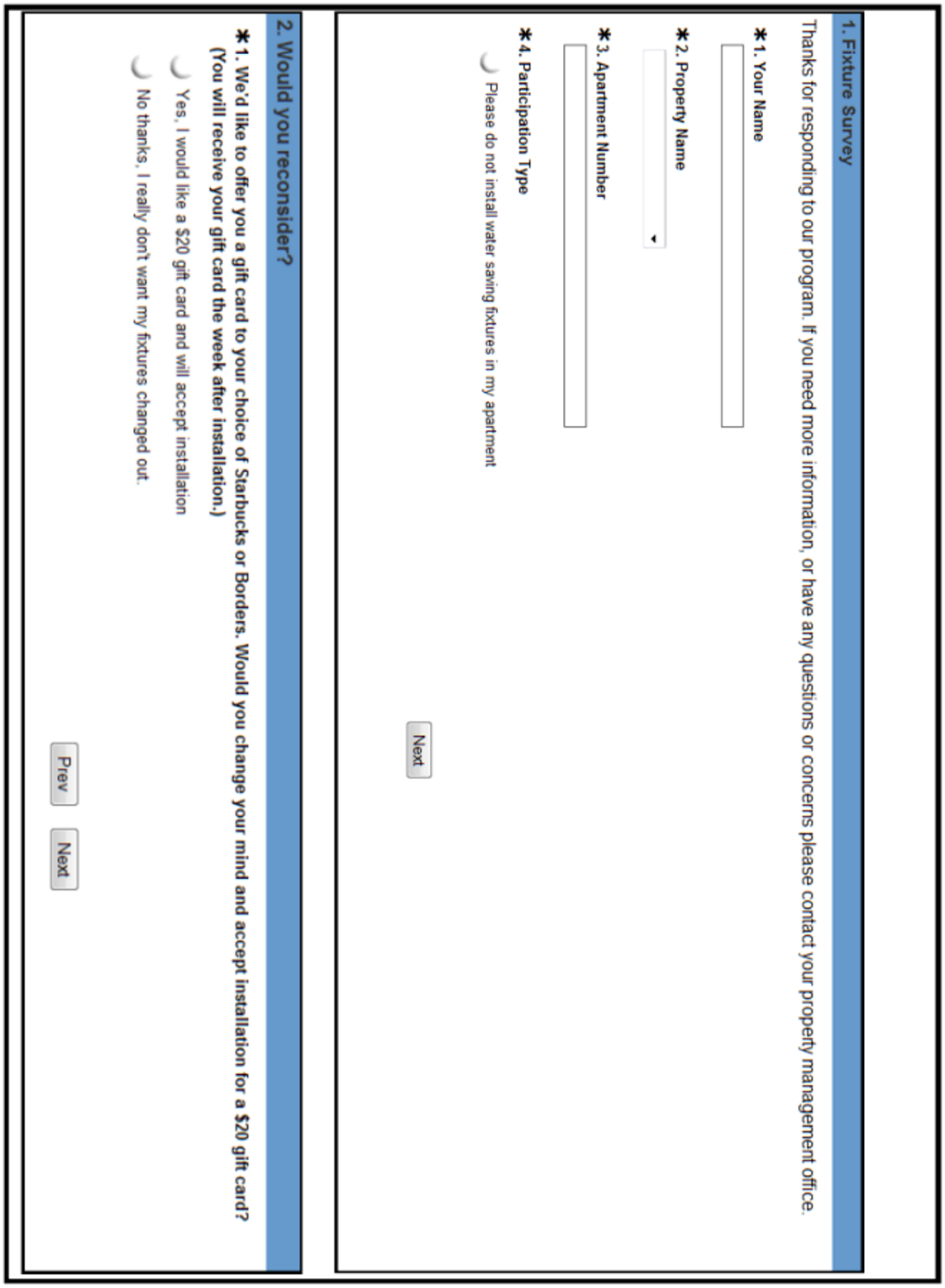
The initial goals of the experiment were to explore the difference in opt-in and opt-out consent procedures for environmental technology, generate an estimate for the value that tenants place on the procedures, and assess the impact that a social norm statement has on these procedures. To achieve these goals, the sample was initially split into apartment buildings that would receive opt-in procedures and those that would receive opt-out procedures, and both sub-samples had treatments involving the social norm statement and monetary incentives. However, upon receiving the first round of opt-in results which consisted of two apartment buildings (one each of opt-in and opt-out), ER decided that it could not continue running the opt-in treatments in additional buildings. Thus, only one apartment building received the opt-in treatment. Although this decision by ER altered the original experimental design, it occurred early enough in the experiment to adjust the design and expand the treatment cells aimed at determining the WTA to forgo an opt-out option.

Building managers in each of the apartment buildings placed notices throughout the apartment buildings and in tenant mailboxes to announce the opportunity to install new aerators. The notices can be seen in figure 3.1 with the adjustments for treatment groups denoted in italics within parentheses for the two differences based on treatment: 1) opt-in or opt-out and 2) inclusion or exclusion of the following social norm statement: "Based on a recent survey, nine out of ten people who live in our Denver properties tell us that living an environmentally friendly lifestyle is important to them." Such notices are common for apartment buildings managed by ER, as is the method of response that was used for this experiment: the web based survey platform SurveyMonkey™. The statistic used in the social norm statement was generated from a previous survey conducted by ER. The intent of including the statement was to determine if social norms could be utilized to encourage fewer tenants to opt-out because social norms have been found to be effective promoting energy conservation in previous research (Herberich, List and Price (2010), Ferraro and Price (2010), Allcott (2009) and Goldstein, Cialdini and Griskevicius (2008)).

Tenants responded to the notice by accessing the website provided on the notice. Naturally, this imposes a cost on the tenants that would like to opt-out. Although ER states that their tenants have access to the Internet and, further, that a majority of the tenants communicate with ER managers, establish their rental contract and pay their monthly rent on the Internet, this caveat concerning transaction costs needs to be acknowledged. Unfortunately, no clean test of how much of a hindrance this transaction cost was is available. Those tenants that accessed the website provided on the notice were directed to an Internet based survey that was differentiated based on treatment. The surveys were accessed via slightly differentiated web names, for example: "http://tinyurl.com/DenverWaterForm" and "http://tinyurl.com /SurveyDenverWater". An opt-out version of the first page of the survey is displayed in the upper half of figure 3.2 . The opt-in version only differs by option four: "Please do not install water saving fixtures in my apartment," being replaced with: "Please install water saving fixtures in my apartment." Tenants that chose to opt-out or opt-in in the treatments that did not receive financial offers were directed to a second page that thanked them for their time.

Tenants that chose to opt-out and were in treatments that received financial offers were directed to an additional page offering compensation to alter their decision. There were a total of 3 different payment treatments which differed in the amount of compensation offered to the renter: i) \$5, ii) \$10 and iii) \$20. The amounts were chosen in part due to financial constraints given such a large number of potential subject payments but with the expectation that these values would provide a suitable range of responses. Unfortunately, as the data below indicates,

Figure 3.2: First Webpage of Survey: Opt-Out Version



Treatment	Apartment Buildings	Average Price*	Number of Apartments	1st Round Installation	Total Installation
Opt In	1	$ 847.11	184		24.46%
Control	2	$ 998.85	694		85.30%
Without Survey	2	$1,093.17	896		89.96%
Opt Out $5	3	$1,098.03	928	*93.64%*	93.97%
Opt Out $10	2	$1,110.48	569	*91.92%*	91.92%
Opt Out $20	3	$ 913.17	878	*94.53%*	94.99%
Total Opt Out	*12*	$1,040.42	3965	*91.30%*	91.48%
Total	13	$1,031.85	4149	*88.33%*	88.50%

*Average price is for rooms that were being rented at time of study. The vacancy rate was 3.8%

Table 3.1: Installation and Apartment Information by Treatments

	Opt in			**Survey**
Total Opt Out	66.84%		**Without Survey**	-1.97%
	0			*0.3491*
	Control	**Opt Out $5**	**Opt Out $10**	**Opt Out $20**
Without Survey	4.65%	-4.01%	-1.96%	-5.03%
	0.2339	*0.0996*	*0.1612*	*0.0094*
Control	x	-8.66%	-6.61%	-9.69%
		0.0501	*0.1037*	*0.0152*
Opt Out $5	x	x	2.05%	-1.02%
			0.2965	*0.5094*
Opt Out $10	x	x	x	-3.07%
				0.0353

*Results clustered at the level of treatment: the apartment building. Adjustment results in F-statistic

The difference in each cell is the row variable less the column variable (p-value given in parentheses

Table 3.2: Chi-square Tests of Difference in Installation Rates

these offers were not very effective in inducing tenants to reverse their opt-out decision. An example of a $20 offer is shown in figure 3.2 below the initial survey page. The tenants rejecting this offer were directed to a final page that thanked them for their time, while those that accepted were offered the choice of gift cards to either Starbucks or Borders.

In total, the experiment consisted of 6 treatment groups which can be seen in column 1 of table 3.1. The 6 treatments are based on 3 main variants: i)

WTA Responses				
	Offer	Total	Accept	Rate
	$5	59	3	5.08%
	$10	46	*0*	*0%*
	$20	48	4	8.33%

WTA Estimates using Turnbull Estimator			
Offer Range	p(WTA≤offer)	F(WTA≤offer)	E(WTA)
$0 to $5	2.86%	0.0286	$0.00
		0.0163	
$5 to $10	Pooled*	Pooled*	
$10 to $20	5.48%	0.0833	$0.55
		0.0399	
$20 to $60	91.67%	1	$18.33
		E(LBwta)	**$18.88**
		V(Lbwta)	**0.186**

Standard errors are given in parenthesis. *The $5 and $10 offer are pooled because the proportion of yeses to the $10 offer is lower than the proprotion for $5.

Table 3.3: Responses and Turnbull Estimation Results: Willingness To Accept (WTA)

opt-in or opt-out, ii) money offered and iii) social norm statement. The "Out-In" treatment included the social norm statement and was the only treatment involving opt-in procedures; three of the treatments involved offering financial compensation and included the social norm statement: "opt-out $5", "opt-out $10" and "opt-out $20;" and there were two treatments using the opt-out procedure that didn't offer financial compensation: "Opt-Out Control" and "OO Without Survey". The Opt-Out Control treatment is identical to the treatments offering compensation except it did not include offering compensation, while the OO Without Survey treatment removes the social norm statement from the notice and does not offer compensation.[7]

The first four columns of table 3.1 list the treatments and provide information on the sample. The first column has the different treatments in each row, the

[7] To be clear: All treatments other than OO Without Survey included the social norm statement discussed next.

second column gives the number of apartment buildings in each treatment group and the fourth column gives the number of apartment units in the treatment groups. As table 3.1 indicates, treatment was assigned at the apartment level. This was done to avoid within building discussions concerning treatments. Although this portion of the experimental design was influenced by ER's desire to avoid tenant disputes concerning fairness, assigning treatment at the building level also reduced the likelihood that renters in each apartment would become aware that they were part of an experiment.

Column 3 of table 3.1 provides the average apartment price for each treatment group. As seen in table 3.1 the sample is not perfectly balanced in terms of size or apartment price. Some information was available prior to determining the samples and an attempt was made to randomly draw the sample balanced on these measures. However, due to the small number of apartment buildings at which treatment was assigned, balancing the sample was severely hindered.

The experiment was conducted between February and May of 2010. Different apartment buildings received the announcements at different times in order to stagger the installation time of the aerators. Tenants were given 2 weeks to respond to the notices and installation occurred between 1 and 2 weeks after the survey was closed. Tenants that chose to receive compensation for forgoing their opt-out option received compensation in the form of a gift card for either Starbucks or Borders. Gifts cards simplified the payment process which consisted of mailing the cards to tenants from ER's corporate Office in Chicago. No cash offers were made so it is not possible to know how the tenants valued the gifts cards in comparison to cash.[8]

3.4 Empirical Methodology

There are two outcomes of interest in this research. First, the overall difference between opt-in and opt-out consent procedures. These results will be evaluated using Chi-square tests for differences in proportions. In order to account for clustering at the apartment building level, an adjusted Chi-square is used that was first suggested for survey data in Rao and Scott (1984). Second, the monetary treatments permit an estimate of the apartment renter's WTA, which is calculated with a Turnbull estimator. The Turnbull estimator was first introduced in Turnbull (1976). Haab and McConnell (1997) thoroughly covers the positive properties of this non-parametric estimator. The Turnbull estimator is often used for calculating WTP and is derived in regard to this so an alternate derivation to the standard empirical theory is provided in this section.

The Turnbull estimator follows from the intuitive notion of building a cumulative density function (CDF) based on the proportion of "yes" or "no" responses to WTP or WTA survey questions. At its essence, the estimator uses the responses to construct probability density weights to provide the appropriate weighting for each monetary offer, $\$v_j$, and then builds a WTA estimate from the weighted values. That is, consider a response to the WTA question: "**Would you change your mind and accept installation for a $\$v_j$ gift card?**" If respondent "i"

[8]Given the popularity of these stores, I do not feel that offering these options for gift cards dramatically effects the estimation from that which would result from offering cash.

says "yes" to the previous question, it is known that $WTA_i \leq \$v_j$, where WTA_i represents individual i's willingness to accept. By assuming respondents WTA has some distribution F, the probability of a randomly chosen individual having a WTA less than $\$v_j$and thus saying "yes" to the question above, can be represented as: $Pr(WTA_i \leq \$v_j) = F(v_j) = F_j$.[9] The probability that a respondent's WTA is in the interval between $v_{j-1}$and v_j can then be represented by $p_j$where $p_j = Pr(v_{j-1} < WTA \leq v_j) = F_j - F_{j-1}$for $j = 1, 2, 3...M + 1$.[10] Each probability can then be used to weight the offers and construct the WTA estimate.

For purposes of discussing the estimator, let M represent the number of different monetary amounts offered to a random sample of respondents drawn from the overall population. The random sample can then be broken down into a set of sub-samples $V = \{V_1,\ V_2...V_M\}$where V_j is the number of agents that received offer v_j, and the yes and number responses can be organized into similarly noted sets of $Y = \{Y_1,\ Y_2...Y_M\}$ and $N = \{N_1,\ N_2...N_M\}$, Y_j and N_j are the number of agents that said "yes" and "no" to the offer v_j, respectively. As a matter of construction, both the sum of all V_j and the sum of all $Y_j$and N_j result in the total number of agents. With this notation, the Turnbull estimator is derived formally from the likelihood function for either F_j or p_j: $L(F|N,Y) = \sum\limits_{j=1}^{M}[Y_j ln(F_j) + N_j ln(1 - F_j)]$ or $L(p|N,Y) = \sum\limits_{j=1}^{M}[Y_j ln(\sum\limits_{i=1}^{j} p_i) + N_j ln(1 - \sum\limits_{i=1}^{j} p_j)]$.

Haab and McConnell (1997) contains a thorough discussion of the Turnbull estimator for estimating WTP. The one deviation for WTA from the WTP derivation

[9]The notation follows closely to Haab and McConnell (1997). However, here I assume that the distribution F is continuous so that the strictness of the inequality is irrelevant.

[10]The value for $v_{M+1}$is generally taken to be ∞ so that $F_{M+1} = 1$.

in Haab and McConnell (1997) is that Y_j and N_j are exactly opposite in the likelihood function. Making this slight adjustment results in the following formulas for calculating the expected lower bound and its variance, $E(LB_{WTA})$ and $V(LB_{WTA})$:

$E(LB_{WTA}) = \sum_{j=1}^{M+1} v_{j-1}p_j$ and

$V\left(\sum_{j=1}^{M+1} v_{j-1}p_j\right) = \sum_{j=1}^{M+1} v_{j-1}^2 \left(V(F_j) + V(F_{j-1})\right) - 2\sum_{j=1}^{M+1} v_j v_{j-1} V(F_j)$,

where $p_j = \frac{Y_j}{Y_j+N_j}$ and $V(F_j) = \frac{F_j(1-F_j)}{Y_j+N_j}$. This calculation works for all j so long as the probability of individuals responding positively to the WTA questions is increasing monotonically. If this constraint fails, the following correction of pooling responses from multiple offer levels is utilized until the response rate increases monotonically as a CDF: $p_j = \frac{Y_j^*}{Y_j^*+N_j^*} - \sum_{k=1}^{j-2} p_k$ where $Z_j^* = Z_j + Z_{j-1}$.

It is important to clarify two caveats concerning the interpretation of the estimate in this paper. First, the WTA estimate discussed here is for tenants that have *chosen to opt-out*. That is, the tenants must first choose to opt-out before being offered money. This small clarification is due to the large portion of the sample that did not choose to opt-out. It is by assumption that no additional information can be gained from tenants that decided not to opt-out. An alternative assumption could be that these tenants have a WTA equal to zero. However, from a practical standpoint, the WTA is only of interest for tenants who choose to opt-out as only those individuals would need to be compensated to change their decision. Thus, although this caveat arises, the estimate of interest is generated by considering only the population that chooses to use the opt-out option. Under this assumption, the probability of a tenant's WTA being less than or equal to zero is zero: $F(WTA \leq$

$v_0 = 0) = F_0 = 0$. The second caveat was first discussed in the introduction and is the alternative interpretation for the WTA estimate: the WTA to opt back in to adoption. This arises because it is impossible to receive money to forgo an opt-out option without also deciding to opt back, and is a completely reasonable alternative interpretation.

3.5 Results

The first outcome of interest is the installation rate across treatments. The installation rate across all opt-out treatments prior to financial offers is found at the bottom of column 5 in table 3.1: 91.30%. This can be directly compared to a much lower installation rate of 24.46% for the apartment building in the opt-in treatment found at the top of column 5 in table 3.1. Not surprisingly, a chi-square test of this difference of 66.84%, is highly significant as shown in the top left of table 3.2, which reports the differences of pairs of treatments with the p-values below accounting for intra-cluster correlation at the apartment building level.

There are some concerns with interpreting this initial result as the true opt-out or opt-in level. First, there is a transaction cost to responding to the notice because it requires using the Internet, which may discourage some tenants from responding. Second, it's possible that tenants do not see the notice or that they do not read the entire not ice in order to respond. Both concerns would result in a decrease in the probability of response. Through conversations with ER, Internet access appears to not be a large concern, which leaves the possibility of not seeing the notice or some other reason that led a tenant to not realize that there was an action to take. However, for this to be a concern, approximately 65% of the sample would need to not see the flyers. Although there is no verification, it seems unlikely that such issues are of great concern.

Table 3.2 also reports results across the opt-out treatment groups. The first difference of note is the difference with and without the social norm statement (with and without survey): "Based on a recent survey, nine out of ten people who live in our Denver properties tell us that living an environmentally friendly lifestyle is important to them." There is a 1.97% decline in the decision to adopt when the statement is used but it is not significant. The direction of this result runs counter to recent research that has shown that social norms positively affect pro-social energy conservation behavior (Goldstein, Cialdini, and Griskevicius (2008), Herberich, List and Price (2010)). This result may be due to the weakness of the social norm that was used as the earlier research points out that that references to society with closer proximity to the agent have had larger impacts on energy saving behaviors than those of farther proximity. Future research should consider testing stronger social norm messages such as increasing the proximity of the responses, "people who live *in this apartment building*", or perhaps increasing the strength of the statement relative to the technology being adopted, "tell us that they enjoy helping the environment by using *these aerators*."

One of the interests in including a control group that was not offered compensation to forgo their opt-out option was to see if tenants heard about the post-opt-out offer prior to making their decision. If this was the case tenants would not otherwise opt-out would be more likely to opt-out in order to receive the compensation. However, the opposite is found: the percentage of respondents opting out is lower for apartments in the offer treatments 8.66%, 6.61% and 9.69% for the \$5, \$10 and \$20 offers respectively. Further, the differences from control for the \$5 and \$10 treatments are significant. It is possible that tenants heard about the offer and miss interpreted as receiving the offer if they did not opt-out. However, no such questions were reported to the apartment management. It is always possible that this result comes from the natural heterogeneity in the underlying data, but as these results are clustered at the apartment building level the significant results are puzzling.

The second and main outcome of interest is a tenant's decision to accept compensation for the opt-out option. This outcome leads to the estimation of a lower bound for the WTA to forgo an opt-out option for aerators, or the alternative interpretation of the WTA to opt back in. Table 3.3 presents the results for the Turnbull estimator described in the previous section. Because no one accepted the \$10 offer, the \$5 and \$10 offers are pooled. The resulting estimate is \$18.88 which is highly affected by the maximum offer due to the low acceptance rate. The range of offers used in this research may simple have been too low. However, it is also possible that tenants place an extremely high value on the opt-out option. Further research could provide evidence in this direction by increasing the maximum offer amount or by asking a hypothetical question to those tenants not accepting offers.

One interesting result in table 3.3 is the zero acceptance rates for \$10 but the positive rate for both \$5 and \$20. This results is likely due to random noise across treatment groups, evidenced by the significant difference in opt-out rates between the \$10 and \$20 treatments as reported in table 3.2.[11] However, one possible explanation for this is a crowding out of pro-social behavior (environmentalism etc.). It is possible that tenants are either willing to accept low offers as just "any offer" or "indication" to encourage them to adopt so long as the offer is low enough so that they still benefit from feeling like they took a pro-social action. However, when that offer raises it could become to large for a tenant to feel like the action is truly for society's benefit and declines until the offer becomes substantial enough for it to be worth the offer alone without any of the warm-glow benefit. Although this notion is intriguing, the current data is not able to fully explore such an idea.

3.6 Conclusion

Countries around the world are considering and enacting legislation to force consumers to use more environmentally efficient technology. For instance, the U.S. Congress passed legislation in 2007 that bans incandescent light bulbs in 2014. On one hand, increased adoption of environmental technology could have a profound affect on the environment. In the case of more efficient aerators considered in this research, energy and water consumption could be reduced by 50%. However, on the other hand, a question remains of whether public opinion should result in mandated adoption or whether the agents place a high value on the ability to opt-out. This paper finds that the welfare impacts are potentially severe for those individuals that

[11]It is possible that there is some underlying difference in tenants that lead to less acceptances: the lower opt-out rate in the \$10 offer treatment could imply that only those with very high distaste for the new aerators opted out, and thus would need to be compensated greatly to post-adopt. However, this is not clear as there is no significant difference between the \$5 and \$10 offer treatments.

choose to not have the new technology installed.

An extension to this research could utilize water usage rates before and after adoption to determine if there are any adjustments in faucet usage which would reduce the efficiency gains. This is of interest because there is concern that water usage changes after adoption, whether it is from renters reinstalling old shower heads or taking longer showers. Further, it is possible that agents that opt-out may alter their utilization in comparison to agents that opt-in. Unfortunately, at this time it has not been possible to obtain water usage rates.

Appendix A

Appendix: Results Concerning the 1990 Clean Air Act Amendment, Design Value Calculations

A.1 Design Value Calculations[1]

The design value for ozone is intended to verify that a monitor has not exceeded a NAAQS more than 3 times in any three year period. Since 1981, the design value for each monitor has been calculated as the 4^{th} highest monitor reading over the past three "complete" years of data. A year of data for a monitor is considered complete if valid maximum monitor readings are recorded for 75% of an "ozone season".[2] If three years of complete data are not available, the number of exceedences decreases but the data from the prior three year period is still used. For a monitor with two years of complete data in the previous three years, the 3rd highest measurement is used as the design value. Similarly, the 2nd highest measurement is used as the design value for a monitor with one year of complete data. Again, the number of years of data used is always three years, regardless of whether some of the years are incomplete. Design values were calculated in accordance with the EPA's assignment rule for monitors with more than one year of complete data available. First, the

[1] Details were taken from a June, 18 1990 memorandum from Director William G. Laxton, EPA's Green Book (http://www.epa.gov/air/oaqps/greenbk/laxton.html)

[2] Ozone seasons are determined on a state-by-state basis by the Aerometric Information Retrieval System and can be found here: http://www.epa.gov/oar/oaqps/greenbk/o3season.html.

number of complete years of data was determined. Then, one-hour daily maximum ozone emissions for each monitor were ranked and the appropriately ranked measurement was used as the design value. The calculation of a design value is slightly more complicated if none of the previous three years are complete. For monitors with less than 90 days of monitor readings during the prior three years, the design value is determined on a case by case basis. It is not clear what was used to determine the design value at these monitors so they were excluded from the analysis. For monitors with more than 90 days of readings during the prior three years, but no complete year, a two step process given by the EPA was followed to determine the rank: 1) the number of valid daily maximums during the 3-year period was divided by the required number of monitoring days per year calculated as 75% of the ozone season. 2) the resulting quotient was increased by 1 and the integer portion of this summation was used as the rank for choosing the design value. The completeness of a year of data is unfortunately determined by two non-recoverable adjustments. First, days that are assumed to be less than the standard of the NAAQS may be included in the count of valid days when calculating the percentage of the ozone season, but it is not clear who decides which days may be included. Second, the year for new sites may begin with the first day of monitoring, so long as the data is complete for 75% of the days between June and August. Both adjustments increase the likelihood of a year of data being complete; the first by including extra days and the second by shortening the season. Because it was not possible to recover these adjustments, the rank used to select the design value may result in a larger design value than that actually determined by the EPA.

A.2 Theoretical discussion of impact of different levels of regulation on time taken to reduce emissions

Consider a social planner deciding the duration of time taken, t, to reduce emissions to some level, e_T, by deadline year T under regulation, R. The social planer would need to consider the present value of the necessary administrative and regulatory costs in order to follow the CAAA's policies incurred over the t years and: $C(t|e_T,R)$. Where the costs of reducing emissions decreases convexly $(C'(t|\ e_T=k,\ R=r)<0,\ C''(t|\ e_T=k,\ R=r)>0)$ because more time adds flexibility as well as diffusion of current pollution. A social planner would also consider the welfare and quasi-rent adjustments when deciding the timeline for reducing emissions to e_T. To simplify the social planner's decision, assume the adjustments are only realized after e_T is achieved and there is no change otherwise. Allow the monetized value of achieving e_T to be represented as $b(e_T)$ so that the present value of achieving e_T in year t is: $B(t|e_T) = b(e_T)(\frac{1}{1+r})^{t+1}$, where r is an appropriate discount rate. The basic problem the social planner faces is thus: $\max_t B(t|e_T) - C(t|e_T, R)$ and the standard solution of equating the marginal cost with the marginal adjustment to welfare and quasi-rent results: $B'(t|e_T) = C'(t|e_T, R) \implies b(e_T)(\frac{1}{1+r})^{t+1}ln(\frac{1}{1+r}) = C'(t|e_T, R)$. An optimal time, t^*, exists so long as $B''(t|e_T) \leq C''(t|e_T, R)$ so that the second order condition is satisfied for a maximum, for simplicity, assume that a penalty, $P(e_T)$, is imposed if $t>T$, and is sufficiently large so that no solution larger than T is worthwhile. Using the implicit function theorem to evaluate the impact of regulation on time taken to reduce emissions results with: $\frac{\partial t^*}{\partial R} = \left\{ \begin{array}{ccc} + & if & C_{tR} < 0 \\ 0 & if & C_{tR} = 0 \\ - & if & C_{tR} > 0 \end{array} \right\}$

This comparative statistic does not provide a theoretical solution for how regulation impacts t. Further, within this setup, different deadlines T should not impact t other than moving along the corner solution of t=T, so long as the penalty $P(eT)$ is severe enough. Considering the comparative statistic, it is not possible to theoretically determine that areas under more restrictive regulation should reduce pollution more or less than areas under less restrictive regulations by the first milestone of 6 years.

A.3 Further discussion of Regression Discontinuity

If non-attainment categories were randomly assigned, a direct comparison of mean outcomes, E(y_1) and E(y_0), could be used rather than an RD design because random assignment would remove any statistical dependence between the treatment and the outcome and result in a consistent estimate of the LATE. However, the non-attainment categories are assigned according to the 1990 CAAA assignment rule and whether a direct comparison of expected outcomes can be made depends on what the design value, v, is correlated with. To see this, consider a linear regression which in effect compares the mean outcomes of the two non-attainment categories:

$$y_a = \gamma + \delta T_a + \varepsilon_a \tag{A.1}$$

where a indicates an area, T is a dummy for being in the stricter non-attainment category and ε is a conventional normally distributed error term. A consistent estimate of the ATE is given by δ so long as the identification assumption, E(ε | *1,T*)=0, holds (or the weaker assumptions: E(ε)=0 and cov(ε,T)=0, both hold). One way the identification assumption is violated is if treatment is correlated with an omitted variable, q, believed to also influence the outcome (cov(T,q)≠0 and E(y|*T,q*) is the conditional expectation of interest). Assuming q has an additive effect on y, including q in 1.1 results in:

$$y_a = \gamma + \delta T_a + \beta q_a + \varepsilon_a \tag{A.2}$$

If q is not included in 1.1, because it is either unobserved or simply ignored, a new error term $u = \beta q + \varepsilon$, would result:

$$y_a = \gamma + \delta T_a + u_a \tag{A.3}$$

and δ would be an inconsistent estimate of the ATE because E(u/*1,T*)≠0 due to the correlation between q and T. The correlation of concern is actually between the assignment value, in this case v, and q, because T is determined by v: *T=T(v).*

In the case of a pollution policy such as the 1990 CAAA, one possible q comes to mind that is similar to the unobserved individual specific skill or ability discussed in papers concerned with returns to education. In the case of pollution, the unobserved "skill" is an area's ability for attracting people and employers. This skill will be correlated with high initial levels of pollution due to a larger initial population and number of jobs, and will also be correlated with lower decreases in pollution so long as one is willing to assume that the relative differences in skill is permanent across areas over the timeframe of the analysis. Rather than considering an unobservable skill, one can imagine that the data will never be complete enough to capture every area specific attribute that attracts employers and residents. For instance, some private company information, beyond the observable level of initial employment, may be necessary to fully account for the variation in employment growth throughout the United States. Also, while the housing decisions of individuals may be influenced by some undocumented area appeal in addition to the observable geography and demographics.

It is possible to use the assignment rule and design value to break the correlation between T and u without using an RD design, but it requires strong functional form assumptions. The design value is useful due to its deterministic relationship with treatment. This relationship results with all correlation between the error term and the treatment necessarily being captured in: $E[u|T, v] = E[u|v]$, or more specifically: $E[q|T, v] = E[q|v]$. By including what is referred to as a "control function" of v, *k(v)*, in equation A.1, a consistent estimate of δ is produced so long as *k(v)* is correctly specified: $E[y_a|1, T_a, v_a] = \gamma + \delta T_a + \beta E[q_a|1, T_a, v_a] + E[\xi_a|1, T_a, v_a]$ $= \gamma + \delta T_a + \beta E[q_a|v_a]$ (assuming $E[\xi_a|1, T_a, v_a] = 0$ and $E[q|T, v] = E[q|v] =$ $\gamma + \delta T_a + k(v_a))$.

The specification of *k(v)* is intended to proxy $\beta E[q|1, T, v]$, and thus remove the dependence of q and T. A consistent estimate of δ then results through estimating:

$$y_a = \gamma + \delta T_a + k(v_a) + \varepsilon_a \tag{A.4}$$

assuming $E[\varepsilon|1, T, v] = 0$ holds.

Unfortunately, the correct specification of *k(v)* presents a significant problem when estimating equation A.4 because the specification must be exact or the estimate of δ will remain inconsistent. In the "simplest" case, a linear specification is assumed and the LATE estimate then measures the difference between the intercepts of two straight lines - one for each group. For specifications other than linearity, strong parametric assumptions need to be made (as though linearity were not strong enough). Fortunately, an RD design removes the necessity of specifying *k(v)* by restricting estimation to those areas with similar assignment values but different treatments (see Campbell (1969) for an early introduction to RD, Hahn, Todd and Van der Klaauw (2001) more recently).

The RD estimate of the ATE, δ^{RD}, results from the following comparison of expected outcomes:

$$\delta^{RD} = \underset{v \downarrow v*}{Lim} E[y|v] - \underset{v \uparrow v*}{Lim} E[y|v] \tag{A.5}$$

where v^* is the discontinuity or cutoff point for treatment so that areas with $v{>}v^*$ receive the stricter regulation "treatment". The consistency of the RD estimate can be seen by substituting y from equation A.3 into equation A.5:

$$\underset{v \downarrow v*}{Lim} E[y|v] - \underset{v \uparrow v*}{Lim} E[y|v] = \gamma - \gamma + \delta(1-0) + (\underset{v \downarrow v*}{Lim} E[u|v] - \underset{v \uparrow v*}{Lim} E[u|v]) \quad \text{(A.6)}$$

From 1.6 it can be seen that for δ^{RD} to estimate the true δ the last term in 1.6, which compares the expected conditional error term for areas close to the cutoff point, needs to be equal to zero. The last term in 1.6 is equal so long as sample is restricted enough so that an underlying RD assumption, that observations around the discontinuity points are similar other than non-attainment category, holds. Intuitively, an RD design restricts the sample enough so that observations are similar and assignment mimics random assignment.

Appendix B

Appendix: How Many Economists does it take to Change a Light Bulb? A Natural Field Experiment on Technology Adoption

Proofs

Parameters

$W \equiv$ wealth

$a \equiv$ attitude toward private and public benefit of purchase

$q \equiv$ quantity of CFLs purchased at price p

$Q_{-i} \equiv$ quantity of CFLs purchased by all other households

$\rho \equiv$ percent adopted

$\kappa \equiv$ proximity of adoption rate

$S(\rho, \kappa) \equiv$ measure of the lelvel of social pressure

$h \equiv$ chosen probability of being at home

$h_0 \equiv$ default probability of being at home

Proof of solution for $q^*(a, p_1, S_1)$:

Although it is discontinuous at 0, the function $U(q)$ defined in equation 2.1 is concave over the relevant space for this proof. Therefore, there will be a unique solution to the maximization problem.

(i) If $a \leq \underline{a}$, then $U(1) \leq U(0)$ and thus $U(q)$ is decreasing on the interval $[1, \infty)$ because of concavity.[1] Therefore, $U(q)$ is maximized at $q^* = 0$.

(ii) If $\underline{a} < a < \bar{a}$, then $U(1) > U(0) \implies q^* > 0$, and further $U(1) > U(2)$. Therefore: $q^* = 1$.[2]

(iii) If $\bar{a} \leq a$, then $U(2) \geq U(1)$ and thus $q^* \geq 2$.

To show that $q^*$is weakly increasing in a, notice that in case (iii) the implicit function theorem implies that:

$$\frac{\partial q}{\partial a} = \frac{-\frac{\partial v}{\partial q}}{p^2 u''(W - pq^*) + a\frac{\partial^2 v}{\partial q^2}} > 0$$

Proof of solution for $h^*(a, p, S(\rho))$:

Solving equation 2.2 for the optimal probability of being at home, h^*, results in:

$$c'(h^*) = [u(W - pq^*) - u(W) + av(q^*, Q_{-i}) - av(0, Q_{-i})] - s(q^*) \tag{B.1}$$

Because $c'(h)$ is strictly increasing, it is possible to invert $c'(h^*)$ and determine a unique solution, denoted here as $H * (a, p, S(\rho, \kappa))$. Because probability is bounded

[1]Note that this is only known for $[1, \infty)$ because of the discontinuity at 0.

[2]Note that it is possible that $U'(1){<}0$ but that $q^* = 1$.

below at 0 and above at 1, the solution $h^*(a, p, S(\rho, \kappa))$ then equals $h^*(a, p, S(\rho, \kappa)) = \max[\min[H * (a, p, S(\rho, \kappa)), 1], 0]$.

First, for attitude levels $a \leq \underline{a}$: By the proof of solution for $q^*(a, p_1, S_1)$, $q^*(a) = 0$ for $a \leq \underline{a}$. It follows that $c'(h^*) = -S(\rho, \kappa)$. Then, if $S(\rho, \kappa) = 0$, $h^*(a, p, 0) = h_0$ for $a \leq \underline{a}$, and if $S(\rho, \kappa) > 0$, then the right hand side of equation B.1 is strictly negative and thus $h^*(a, p, s(\rho, \kappa)) < h_0$ for $a \leq \underline{a}$.

Second, considering consumers with $a > \underline{a}$: from the proof of solution for $q^*(a, p_1, S_1)$, $U(q') > U(0) \; \forall \; q' > 0$. By definition $U(0) = u(W) + av(0, Q_{-i}) - S(\rho, \kappa)$ and $U(q') = u(W - pq') + av(q', Q_{-i})$. If $S(\rho, \kappa) = 0$ the term in brackets of equation B.1 is equal to $U(q') - U(0)$, and thus $c'(h^*) > 0 \implies h^* > h_0$ for $a > \underline{a}$. For $S(\rho, \kappa) > 0$, once again $q^* = q' > 0$ for $a > \underline{a}$ so that $s(q^*) = 0$ and the term in brackets of equation B.1 is equal to $U(q') - u(W) - av(0, Q_{-i}) = U(q') - U(0) - S(\rho, \kappa)$. Let $a_0(p, S(\rho, \kappa)) \geq \underline{a}$ be defined as the level of attitude for which the household is indifferent between purchasing and not purchasing but not facing social pressure $(U(0) - S(\rho, \kappa))$ so that the following equality holds: $u(W - pq^*) + a_0(\cdot)v(q^*, Q_{-i}) = u(W) + a_0(\cdot)v(0, Q_{-i})$. Then, if $S(\rho, \kappa) > 0$, for $a = a_0$, $c'(h^*) = 0 \implies h^* = h_0$, for $a > a_0$, $c'(h^*) > 0 \implies h^* > h_0$ and for $a < a_0$, $c'(h^*) < 0 \implies h^* < h$.

Alternatively, the positive relationship between $h^*$and a can be shown through the Inverse Function Theorem: Let $R(a)$ denote the right-hand side of equation B.1. Using the Inverse Function theorem, $\frac{\partial h^*()}{\partial a} = \frac{\partial R/\partial a}{c''(h^*)} > 0$ because $R(a)$ is continuous and, by the Envelope Theorem, strictly increasing for all $a > \underline{a}$. (Note: $h^*(a)$ is continuous by the continuity of $c'(h)$ and $R(a)$ over the space $a > \underline{a}$.)

Proof of Prediction 1

For scenario 1 ($S = 0$ and $F(\underline{a}) < 1$): For $S = 0$, $h * (\cdot) \geq h_0$ and $h * (\cdot) > h_0 \forall a > a_0$. Since $F(\underline{a}) < 1$, $\exists\ a > a_0$, as $\underline{a} = a_0$ when $S = 0$. Thus, both $p_W(H)$ and $p_{WOO}(H)$ are greater than $p_{NW}(H)$, as both include at least some $h * (\cdot) > h_0$ and no $h * (\cdot) < h_0$. Further, $p_W(H) = p_{OO}(H)$ is found trivially as a$a_{OO} = -\infty$for $S = 0$.

For scenario 2 ($S > 0$ and $F(\underline{a}) = 1$): For $S > 0$ and $a \leq \underline{a}$, $h * (\cdot) < h_0$by the proof of solution for $h^*(a, p, S(\rho))$. Thus, $p_{NW}(H) > p_W(H)$ and $p_{NW}(H) > p_{OO}(H)$ as $a \leq \underline{a}$ by assumption of very low attitude. Further, because $a_{OO} = a_0 > \underline{a}$, $h * (\cdot) = 0$ for all observations in OO (opt out option is chosen due to social pressure and limited positive attitude) but not in W, and thus $p_W(H) > p_{OO}(H)$.

Proof of Predication 2

For scenario 1 ($S = 0$ and $F(\underline{a}) < 1$): As in prediction 1, for $S = 0$, $h * (\cdot) \geq h_0$ and $h * (\cdot) > h_0 \forall a > a_0$. Since $F(\underline{a}) < 1$, $\exists\ a > a_0$, as $\underline{a} = a_0$ when $S = 0$. Thus, both $p_W(A)$ and $p_{OO}(A)$ are greater than $p_{NW}(A)$, as both include at least some $h * (\cdot) > h_0$ and no $h * (\cdot) < h_0$ and all $h * (\cdot) > h_0$purchase. Further, $p_W(A) = p_{OO}(A)$ is found trivially as $\underline{a} = a_0$ when $S = 0$.

For scenario 2 ($S > 0$ and $F(\underline{a}) = 1$): For $S > 0$ and $a \leq \underline{a}$, $h * (\cdot) < h_0$by the proof of solution for $h^*(a, p, S(\rho))$. Thus, $p_{NW}(A) > p_W(A)$ and $p_{NW}(A) > p_{OO}(A)$ as $a \leq \underline{a}$ by assumption of very low levels attitude. Further, because $a_0 > \underline{a}$, $h * (\cdot) = 0$ for all observations in OO (opt out option is chosen due to social pressure and limited attitude) but not in W, and thus $p_W(A) > p_{OO}(A)$.

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