ESSAYS ON THE ECONOMICS OF CARBON PRICING

by

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DISSERTATION ABSTRACT

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Title: Essays on the Economics of Carbon Pricing

This research examines the economics of carbon pricing programs with a focus on evaluating issues relating to distribution and equity concerning these policies.

The first substantive chapter, Chapter 2, empirically examines a policy question that emerged in evaluating California's carbon cap-and-trade bill. Many environmentalist and environmental justice groups felt that the carbon cap-and-trade program was causing non-carbon "copollutants" to be reduced by lesser amounts in low socioeconomic status neighborhoods. I find no evidence, at least in the electricity sector, that these concerns are borne out by the data.

The second and third substantive chapter describe a stated-preference survey of a large public university on the topic of internal carbon pricing programs. Internal carbon pricing programs voluntarily place a fee on an institutions carbon

emissions as a way of encourage emission reductions, raise funds for emission reduction projects and establish a reputation for sustainability.

Chapter 3 (With Trudy Ann Cameron and Steve Mital) describe the results of a structural choice model estimated using data from the survey. The willingness to pay for these programs are found to depend on not only the amount of carbon dioxide reductions achieved, but also depend on the distribution of the costs of the program, as well as the distribution of any revenue the program raises. Distributions for willingness to pay across the entire campus community are calculated for several models.

Chapter 4 presents a methodology that can be used to "transfer" the results and estimates from Chapter 3 to a different, unsurveyed university. Support for internal carbon pricing programs is found to heavily depend on the political and socioeconomic composition of the university population.

This dissertation includes previously unpublished co-authored material.

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CHAPTER I

INTRODUCTION

Human-caused greenhouse gas (GHG) emissions, and the change in climate they induce, are widely accepted by scientists to impose large future costs on society through sea-level rise, lower crop yields, and natural disasters that are more severe, more frequent, and broader in geographic scope. To limit this damage, GHG emissions (of which carbon is the most common) will need to be be drastically decreased. Economists widely concur that the most cost-effective way to reduce GHG emissions is to place a price on carbon, either in the form of a tax or by issuing limited quantities of tradeable permits (as in cap-and-trade).

This dissertation consists of three chapters, each of which explores some aspect of the economics of carbon pricing. I examine carbon-pricing enacted by the state, as well as non-state "internal carbon pricing programs." The common theme throughout the dissertation is an attention to issues of distribution and equity in the design of these programs, both in the retrospective evaluation of the outcomes of existing programs and in characterizing individual preferences over different prospective program design options.

Chapter II of this dissertation examines California's implementation of carbon pricing, in the form of a cap-and-trade system, on the spatial distribution of non-carbon co-pollutants. Concerns have been raised, in the public debate over California's carbon pricing program, that cap-and-trade is causing harmful co-pollutants, such as nitrous oxides (NO_X) and sulfur oxides (SO_x), to be reduced by lesser amounts in low-income and minority communities or, in extreme cases, even to increase. I empirically test whether this concern is borne out in the data using modern program evaluation tools: a semi-parametric matching estimator and a synthetic control approach. For both estimators, I find no statistical evidence of systematically lesser benefits or greater costs for low-income or minority communities with respect to changes in co-pollutants. The semi-parametric matching estimator suggests a general decrease in co-pollutant levels across the state. My results imply that changes to the cap-and-trade program to prevent inequitable abatement of co-pollutants (at the cost of efficiency) do not seem to be urgently needed, at least in the electricity sector.

Chapter III (with Trudy Ann Cameron and Steve Mital) reports on a stated-preference survey of a large public university designed to characterize individual preferences over alternative possible designs of an internal carbon pricing program. Internal carbon pricing programs involve an organization placing emission charges on its administrative subdivisions to encourage emission-reducing changes in behavior and/or to provide funds for projects that reduce carbon emissions. The survey consists of a discrete choice experiment, in the form an advisory referendum, where respondents choose between programs (including no program) with various amounts of carbon reductions, distributions of costs and revenues, and costs to the respondent. We find evidence of support for internal carbon pricing programs with estimated willingness-to-pay values above most standard estimates of the social cost of carbon. Distributional preferences prove to be important. Respondents prefer programs where a higher share of costs fall on polluters, where taxpayers share some of the costs, and where there is no revenue recycling.

Chapter IV (with Trudy Ann Cameron) describes a methodology in which a model similar to the one estimated from the survey in Chapter II can be used to construct estimates for other universities. This is an example of the so-called “benefits transfer” approach in environmental economics. The outlined method requires no additional survey to be fielded. To illustrate our method, we construct estimates of the distribution of willingness to pay for carbon pricing at a hypothetical university that draws its population from the same geographic areas as does the University of Kentucky. We assume this pseudo university has the same distributions of administrative data within its groups of students, faculty, and other employees (i.e. departmental affiliations etc.), but that the sociodemographic distribution for each person’s “permanent address” is (very) different from the population at the university used to estimate this model and representative, instead, of the University of Kentucky’s population. The sociodemographic characteristics at the zip code level are shown to be influential determinants of program preferences, and these characteristics differ considerably between the university where the study was conducted and our pseudo-University of Kentucky.

CHAPTER II

THE EFFECT OF CALIFORNIA'S CARBON CAP AND TRADE PROGRAM ON CO-POLLUTANTS AND ENVIRONMENTAL JUSTICE: EVIDENCE FROM THE ELECTRICITY SECTOR

Introduction

Cap-and-trade programs have become a popular tool for policy makers who wish to take steps to mitigate climate change by reducing carbon emissions. This popularity reflects the fact that cap-and-trade achieves a fixed level of abatement at minimum cost. Carbon cap-and-trade programs exist in the European Union, California, and Quebec, and are scheduled to begin in China.

Environmental Justice (EJ) groups in both California and other jurisdictions have expressed concerns that cap-and-trade programs may increase pollution levels in disadvantaged communities. In such a case, the aggregate net welfare effects from the policy may be positive, but some of the distributional consequences may be regressive.[1] Carbon dioxide, itself, is a uniformly mixing pollutant and therefore the spatial distribution of abatement actions, ultimately, has no relation to the distribution of benefits from the climate change mitigation objective of the program. However, pollutants that often co-occur with carbon dioxide, such as NO_x and SO_x, do not mix uniformly in the atmosphere. Any spatial redistribution of carbon emissions as a result of a cap-and-trade program may, in fact, have concurrent effects on NO_x and SO_x emissions, and thus have local effects on population exposures to these other pollutants. It is possible for carbon pricing

[1] See: Barboza and Megerian (2017), Guerin (2017), Geuss (2017), Kahn (2016), Mason et al. (2017), Climate Hawks Vote (2017)

to alter the spatial distribution of co-pollutant damages by changing the spatial distribution of economic activity.

There is a growing literature on the interaction between carbon pricing and co-polluants, includingMuller (2012), Agee et al. (2014), Fullerton and Muehlegger (2017) and Novan (2017). The theoretical relationship between co-pollutants and carbon dioxide is ambiguous. Technologically, NO_x and SO_x may be either complements or substitutes, relative to carbon dioxide, which means that carbon pricing may reduce or increase co-pollutant levels. Additionally, carbon pricing is frequently enacted in a setting where regulations on co-pollutants do not properly reflect the full damages of their emissions. This incomplete regulation presents an additional challenge in predicting the effects of carbon-pricing on co-pollutants and creates the possibility of adverse interactions between carbon pricing and co-pollutant emissions.

In the debate over the renewal of California's greenhouse-gas permit-trading program, EJ concerns have created opposition to the program from several environmental groups such as the Sierra Club and the California Environmental Justice Alliance. EJ concerns about the program are not limited solely to non-profit advocacy groups. Such concerns have also been expressed during the formal rulemaking process of the California Air Resource Board (CARB) by the board's official Environmental Justice Advisory Committee. In a meeting on February 15th, 2017, the advisory committee issued a statement criticizing cap-and-trade, stating that the program "does not reflect best practices in research or serve the interests of poor communities, communities of color, and indigenous communities in California and around the world" (California Air Resource Board (2017)).

EJ groups have argued that the ability of firms to reallocate carbon emissions from plant to plant (by transferring permits) will result in higher levels of pollution in minority and low-income communities. Such concerns are based on the perception among the EJ community that dirtier plants, which are disproportionately located in low-income and minority communities, will increase their emissions when allowed to buy permits, whereas cleaner plants will respond by lowering their emissions. These concerns tend to be voiced by non-economists, and thus often do not contain references to formal economic logic. Such a focus on (absolutely) dirtier plants ignores the important role of *marginal* abatement costs in determining changes in pollution levels after the introduction of an emissions trading program.[2]

Although the arguments by environmental justice groups are not grounded upon formal economic theory, regressive distributional effects could indeed occur if high-abatement-cost firms are located in disadvantaged communities. According to economic theory, it is the spatial distribution of *marginal* abatement costs among plants that will determine the changes in pollutants as a result of the program. Firm's with low marginal abatement costs will have an incentive to lower their emissions to sell permits to firms with higher marginal abatement costs. Thus, the core distributional concerns of the EJ groups could be valid if firms with relatively high marginal abatement costs are more likely to be located in disadvantaged communities.[3] It is note clear that this is necessarily the case.

Instead of market-based methods of regulation, EJ advocacy groups argue for command-and-control regulation, including per-facility carbon dioxide emission

[2]See Farber (2012) for a detailed discussion of the EJ arguments made against cap-and-trade.

[3]Section 2.2 discusses these arguments and the corresponding economic reasoning in more detail.

limits in addition to technology standards. Similar EJ concerns resulted in strong opposition from environmental groups when the state of Washington included a carbon-tax referendum in the 2016 election, and these concerns contributed to the failure of the referendum.

California's policy makers are required to consider the EJ impacts of both the current cap-and-trade program and any future programs. By law, all state government organizations must ensure that environmental regulation does not systematically harm individuals on the basis of race or income. U.S. federal regulations have also long required policy makers to consider the distributional impacts of environmental regulation. Executive Order 12898, issued in 1994, requires that the EJ impacts of all environmental regulations be considered when evaluating policy. An understanding of the distributional impacts of cap-and-trade programs is therefore important to policy-makers both in California, and for any future state or federal carbon pricing programs. Even though the economic basis for these advocacy group arguments is not always clear, and the implicit assumptions may not demonstrably hold, the question of the EJ impacts of cap-and-trade programs has become central to both the political economy of carbon pricing, and the legal obligations of policy makers.

There is extensive evidence that existing levels of pollution are often higher in low-income and minority communities than in other types of communities. However, previous work on emission markets has found little evidence of systematic differences in policy-induced abatement levels with respect to spatial variations in race or income. Most notably, Fowlie et al. (2012) study the effects of the Southern California NO_x emissions trading program (RECLAIM). They use a matched difference-in-differences estimator and fail to reject the null hypothesis of no systematic variation in program benefits according to either the racial composition or the income levels of the affected communities. However, the task of quantifying the change in the spatial distribution of co-pollutants that occur after the introduction of a *carbon* emission-permit market has received relatively little attention in the literature. While portions of my empirical approach will follow Fowlie et al. (2012), the setting for their paper differs in important ways from the setting for this chapter. Fowlie et al. (2012) consider a permit market program which directly regulates a harmful local pollutant, whereas this chapter addresses the effects of a permit market on pollutants that are not directly covered by the market in question but for which emission levels are nonetheless correlated with emissions of the regulated pollutant. There is no theoretical justification to believe that the pattern of abatement across areas with different demographics or income levels will the same for the carbon cap-and-trade program as the pattern for RECLAIM.

I utilize a dataset of hourly plant-level emissions for all power plants with more than 25 MWh of capacity across the United States. I observe emissions of CO_2, NO_x, and SO_x. I combine these emissions data with demographic data from the American Community Survey conducted by the U.S. Census, as well as EPA data on the characteristics of individual power plants. My dataset allows me to use non-California entities to help control for unobserved region-wide shocks that differentially affect communities based on their income levels or racial composition. Controlling for these region-wide shocks is not possible in studies that rely solely on administrative data from California's cap-and-trade program.

My empirical strategy first utilizes a semi-parametric matched difference-in-differences estimator to construct a control group for each regulated plant in

California. There are many possible strategies for matching. My initial approach is analogous to that of Fowlie et al. (2012). I match each treated unit to the closest M controls based on their distance in covariate space as defined by the Mahalanobis norm. This and similar estimators are discussed in Heckman et al. (1997), Heckman et al. (1998), Abadie and Imbens (2006), Abadie and Imbens (2011) and Haninger et al. (2017). This matching method allows for more flexibility in constructing counterfactual values than parametric methods, and limits the influence of non-similar control plants. This method also allows me to construct heterogeneous treatment effects which vary systematically with the demographics of nearby communities, permitting a direct test for any adverse environmental justice outcomes of California's cap-and-trade program.

As a second approach, I make use of the synthetic control method developed by Abadie et al. (2010). This method constructs the counterfactual outcomes for California emissions from the linear combination of control-state emissions that best tracks California's pre-treatment emissions. While I cannot directly compute heterogeneous treatment effects for each individual plant, I can compare estimates where the sample is restricted to low-income or high-minority-share communities to the results estimated on the full sample.

My results suggest that, on average, California electricity plants saw a reduction in co-pollutant emissions due to the carbon cap-and-trade program. The sign and magnitude of the key coefficient is negative regardless of the specification of the control group and for both methods, but it is not statistically significant for all possible choices of matched control groups or for the synthetic control results. Importantly, I find no robust evidence that this effect varies with either the income or the racial composition of the communities surrounding the plant. Thus, there is no compelling evidence in these data for adverse environmental justice impacts for co-pollutants in low-income or high-minority-share communities in California.

Two previous papers have made attempts to characterize the distribution of gains across demographics for California's carbon cap-and-trade program. Cushing et al. (2016) present statistics from Californian administrative data showing that regulated facilities are more likely to be located in low-income or minority communities, and that several industries have experienced increases in both their carbon emissions and their co-pollutant emissions. However, the Cushing et al. paper consists mainly of summary statistics and includes no formal statistical analysis. No attempt is made to control for unobserved heterogenity, or to estimate a causal effect for the program.

Meng (2017) uses CARB administrative data to estimate a difference-in-differences model to assess the levels of carbon abatement across advantaged and disadvantaged communities. These administrative data consist of carbon emissions reported to the state of California to document compliance with the cap-and-trade program. Meng finds no evidence that carbon abatement varies systemically with race or income in the local community. Meng also finds suggestive evidence of perhaps *more* abatement in low-income and minority communities, but the estimates are not statistically significant at conventional levels.

My approach differs from Meng (2017) in several ways. Meng's data allow him to see the full universe of entities regulated under California's cap-and-trade program, whereas my data are limited to electricity-generating firms. However, the data used in this chapter have two distinct advantages relative to Meng's. First, I can directly observe co-pollutant emissions, whereas Meng can observe only carbon emissions and has no data on co-pollutant emissions. Second, all the firms in Meng's dataset are located only in California. Thus, his identification strategy cannot control for national or regional trends unrelated to cap-and-trade that differentially affected emissions in advantaged and disadvantaged communities. The data used in this chapter are for the entire U.S. so I can use patterns of emissions for various sets of matched non-California firms as controls.

The chapter is organized as follows. Section 2.2 outlines the institutional background of the California cap-and-trade program. Section 2.3 explains the data and methodology. Section 2.4 presents the results. Section 2.5 discusses some limitations of the data and the analysis, and proposes some additional research that may be appropriate, as more data accumulate in the coming years and as firms reoptimize their capital stocks over the longer run. Section 2.6 concludes.

Program Background

In 2006, the California legislature passed Assembly Bill 32 (AB32). The law mandated a reduction in carbon dioxide emissions to 1990 levels by 2020. To meet these goals, California chose to establish a cap-and-trade program. California also adopted a low-carbon fuel standard, implemented energy efficiency regulations, and required electrical utilities to obtain more of their electricity from renewable sources.

In a cap-and-trade program, each firm must surrender a permit for each ton of carbon dioxide that it emits. The total quantity of permits is capped and firms are allowed to buy and sell permits for cash payments. A cap-and-trade program achieves a particular level of overall abatement at least cost because it allows firms with higher marginal abatement costs to "bargain" with other firms with lower marginal abatement costs to reduce their emissions instead. Thus the equilibrium spatial pattern of emissions is determined by the distribution of marginal abatement costs across firms. Firms with the lowest marginal abatement costs will typically do the most abatement, freeing up permits for sale to other firms that have marginal abatement costs higher than the market price of a permit.

California's cap-and-trade program began in 2013. The cap was initially set at two percent below 2012 emissions, declined two percent in 2014 and was scheduled to decline by three percent in each subsequent year (until 2020). State regulators estimate that the required decline in emissions represents a 15 percent reduction from the counterfactual "no program" trend in emissions. The AB32 program covers carbon dioxide as well as several other greenhouse gases.[4]

All electricity producers in California are covered by the program, as well as all large industrial sources emitting more than 25,000 megatons of CO_2-equivalent emissions per year. Fuel suppliers were brought under the cap in 2015. Around 450 entities, in total, were covered by the program as of 2015. CARB estimates that eighty percent of all Californian carbon emissions are subject to the cap.[5] Permit allocations to large industrial emitters were initially distributed at no cost to firms, based on the firm's historical emissions and the firm's energy efficiency, but an increasing proportion of permits will be auctioned as time goes on.[6] Electricity generators received free permits on the condition that all profits from the permits must benefit utility rate-payers. Permits may be banked, and firms may meet part

[4] The full list includes carbon dioxide (CO_2), methane (CH_4), nitrous oxide (NO_2), hydrofluorocarbons (HFCs), perfluorocarbons (PFCs), sulfur hexafluoride (SF_6), nitrogen trifloride (NF_3).

[5] Sources of greenhouse emissions that remain outside the cap include agriculture and emissions from residential and commercial sources.

[6] Increasing auction shares reflect the transition of property rights (to carbon emissions) from firms to the general population.

of their compliance obligation by purchasing "offsets" that support other types of approved carbon-dioxide-reducing projects.[7] Permits were initially traded at $22 per ton of CO_2-equivalent. However, after some volatility, equilibrium prices fell and eventually settled around a price of $12 to $13 a ton by 2014.

As noted in the introduction, the spatial distribution of carbon abatement activity does not affect the distribution of global benefits from carbon emissions reductions. Carbon dioxide is considered to be "globally uniformly mixing," meaning that it spreads evenly throughout the earth's atmosphere. It is the global concentration of carbon dioxide that determines the pace of global warming. Consequently, it does not matter which firm(s) choose to abate their carbon emissions; all that matters for climate change is the aggregate abatement. This feature of carbon-emissions means that there can be opportunities to decrease the overall cost of regulation by facilitating the allocation of abatement responsibility towards cost-minimizing locations.

The expected spatial pattern of abatement under a cap-and-trade program is determined by the spatial pattern in marginal abatement costs. Environmental justice concerns would be warranted if plants with higher marginal abatement costs are located in disadvantaged communities, and if changes in carbon dioxide emissions are strongly correlated with changes in local co-pollutants. However, if marginal abatement costs are uncorrelated with characteristics of the surrounding community, then it is unlikely that changes in the location of carbon emissions, attributable to cap-and-trade, will differentially change co-pollutants for low-income and minority communities.

[7]Common offset projects including preserving or planting forests and disposing of certain types of ozone-depleting gases which also have greenhouse effects. These projects may be located outside of the state.

Theory is ambiguous on which case will hold. There are three possibilities: (1) dirtier plants located in disadvantaged communities may not yet have taken full advantage of all available abatement technologies, implying that marginal abatement costs for these firms could be lower than for cleaner state-of-the-art plants located in wealthier non-minority communities; (2) dirtier plants located in low-income communities have higher emissions because they tend to have higher marginal abatement costs (often implicitly assumed by those who oppose cap-and-trade programs on EJ grounds); (3) there is simply no correlation between marginal abatement costs and the low-income and minority shares of surrounding communities. Unfortunately, it is not possible to observe marginal abatement costs directly in the available data and possible proxies are insufficiently informative, so it is not possible to simply observe which of (1) through (3) holds.

These environmental justice concerns have taken a central role in the debate about whether to renew California's cap-and-trade program after the expiration of AB32 in 2020. These concerns can also be seen explicitly in the legislation passed. The process to extend California's carbon cap-and-trade beyond 2020 began with the passage of a separate California Senate bill, SB32, in 2016, which mandated a forty percent reduction in carbon emissions below 1990 emission levels by 2030. In response to widespread EJ concerns, SB32 explicitly mandates that the emission reductions must be achieved in a "manner that benefits the state's most disadvantaged communities." AB 398, which was passed in 2017, established a more aggressive cap-and-trade system to achive these reductions.

In addition to the cap-and-trade "extension" in AB 398, a separate bill was passed (in conjunction with the extension) that mandates stricter regulation of local pollutants. As a condition for their support of the bill, industry groups demanded that no new GHG regulations could subsequently target entities already participating in the cap-and-trade program. This final condition was viewed as an attempt to forestall any traditional command-and-control regulations, such as plant-specific abatement targets or emissions limits, which had become a popular policy proposal in environmental justice circles to address their distributional concerns.

Thus the distributional effects of changes in co-pollutants have become an important part of both the political economy and legal obligations of California policy makers. A better understanding of the pattern of abatement due to California's cap-and-trade program is therefore of first-order importance to policy-makers.

Another factor that could affect the proper function of California's carbon market is the potential for out-of-state or out-of-country "leakages" of carbon emissions. Abatement of carbon emissions in California could be at least partially undone by increases in carbon emissions outside the state, because carbon pricing would make production outside of California relatively more profitable. Specifically to deter leakage, California freely allocates a portion of the total number of permits based on a firm's output and its efficiency relative to the industry. These criteria act as an output subsidy and encourage firms (and production) to stay in California instead of moving to an unregulated state.[8] Furthermore, California directly taxes the carbon content embodied in imports of electricity from other states.

[8]For a model of the role of output subsidies in preventing leakages see Fischer and Fox (2012)

Data and Empirical Strategy

My dataset consists of plant-level emissions from 2010 to 2016 for almost all power plants in the continental United States. Emissions data can be retrieved from the EPA's Clean Air Market Data (CAMD) which includes all generators with a capacity greater than 25 MW. These data are collected from continuous emission monitoring systems (CEMS) which record emissions data at an hourly frequency. All units must report CO_2, NO_x and SO_x emissions data. These emissions are flows measured at the point of emission from the plant and do not represent readings or imputations of concentrations from ambient pollution monitors. Wind speed and direction, temperature, precipitation and atmospheric chemistry, for example, will all affect the "fate and transport" of these emissions and eventual exposure of the population to the resulting ambient levels of pollution.

Data on the characteristics of electricity generators have been retrieved from the EPA's Emission and Generation Resource Integrated Database (eGRID). eGRID is an extensive database on both the environmental and the technical characteristics of U.S. electricity generators. These eGRID statistics are published every other year. In years without eGRID data, I assign plants the characteristics contained in the eGRID release from the previous year. Plant characteristics that I use in this analysis include the primary fuel type of the plant, the nameplate capacity, annual net generation, and the heat rate. Nameplate capacity refers to the amount of electricity an electrical plant can generate in a given period of time (usually in megawatts) and is a proxy for plant size. Heat rate is a measure of the plant's efficiency. It reflects how much energy is needed to generate one unit of electricity. Plants with *lower* heat rates are more efficient. Latitude and longitude coordinates for the plant's location are also drawn from eGRID.

My demographic data are drawn from the five-year moving average of the American Community Survey (ACS) data at the census-tract level.[9] To study the environmental justice impacts of the program, I focus on two variables: per-capita income and the proportion of the population which belongs to a minority group.[10] Demographic data for the neighborhood surrounding each plant are based on all census tracts which intersect a one-mile buffer centered on the plant's latitude/longitude location.

There are two natural control groups to consider for the California plants: the North American Electric Reliability Corporation's (NERC) Western Electricity Coordinating Council (WECC)[11] or the entire set of non-California power plants in the United States. The WECC enforces many federal regulations and writes rules to ensure power-plant compliance across its region. The regulations are designed to ensure equal access to transmission infrastructure and to minimize the chance of a wide-scale power failure. In addition, the WECC overlaps with the Western Interconnection. The Western Interconnection is one of three grid interconnections in the US and covers the portion of the United States that lies west of the Rocky Mountains. Figure 1 shows the geographical structure of

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[9]The values I use reflect the terminal year of the five-year window, not the midpoint. This is a necessary compromise because midpoint data for 2016 will not be available until the ACS data for 2018 are available. I also cannot use earlier years because the five-year ACS only became available in 2010. Data at census tract geography is not available in the one-year or three-year ACS due to confidentiality reasons.

[10]Following Fowlie et al. (2012) I use the proportion of residents who identify as African-American or Hispanic for the minority variable.

[11]The NERC is a non-profit collective of electricity-generating firms charged by the federal government with (a) ensuring reliability throughout the grid by ensuring compliance with federal regulations, and (b) collecting data.

the U.S. electricity grid. Technological constraints make it difficult for plants inside the Western Interconnection to transmit power to consumers outside of the interconnection and for firms outside the interconnection to transmit power in.[12] Thus plants inside the interconnection form a market, and experience similar market conditions and regulations.[13] By limiting the sample to the WECC, it is less likely that unobserved demand shocks across regions will bias the results.

The WECC, however, is an imperfect control group due to the limited number of plants that are available for use with the matching estimator as potential controls. Matches may therefore be of lower quality, in the sense that the control plants may differ in their covariates. The estimator may incorrectly estimate the counterfactual for the treated plant. Expanding the control group to include all U.S. plants outside of California could allow a greater pool of potential controls, thus increasing the chance that there are good matches with similar covariates for each treated unit. Additionally, a greater number of plants will provide more statistical power and decrease finite sample bias.

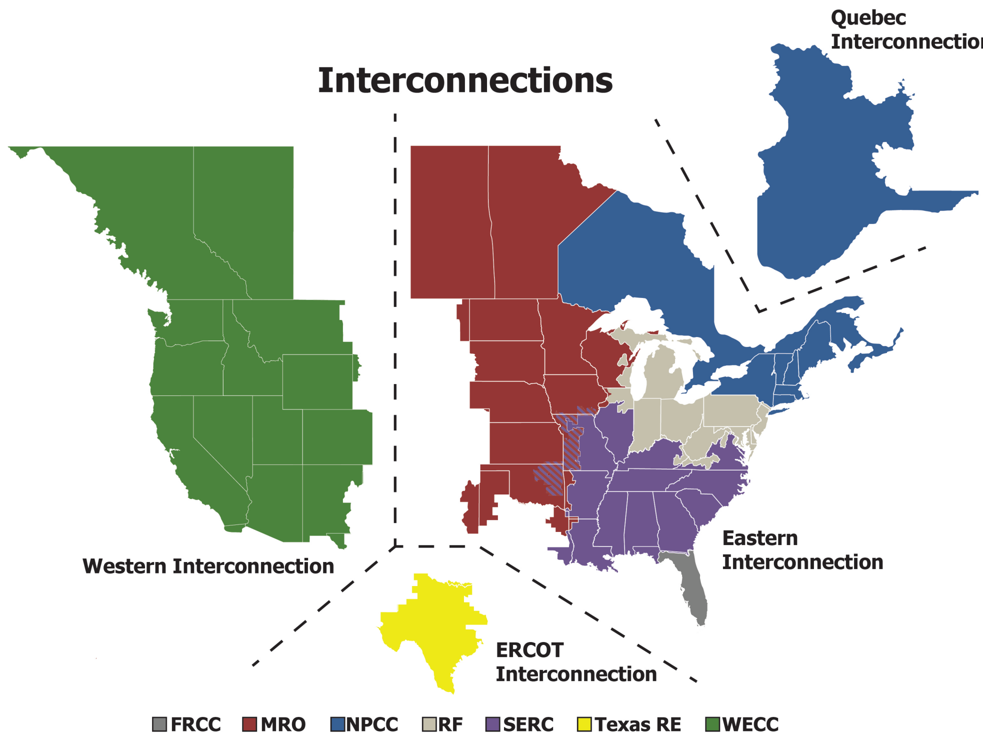
Given that there are plausible arguments for the choice of either of these control groups, I will show estimates using both of these groups. Qualitatively, the signs and magnitudes of the key parameter estimates do not change with the choice of control groups. The difference lies in the sizes of the parameter standard errors and therefore in the statistical significance of the results.

I aggregate the hourly raw emissions data for each plant to the cumulative yearly level for that plant because demographic data are only available at the

[12]In recent years there has been a push to better integrate the different interconnections. However, these projects are either in their early phases or relatively small.

[13]Note that I use a more-general definition of "market" here, where a market is the set of firms and buyers whose actions influence the price. Often when people speak of electricity markets, they are referring to a wholesale market administered by an Independent System Operator(ISO).

FIGURE 1.
Structure of the United States Grid



Notes: This figure shows the structure of the United States electricity grid. There are three separate interconnections and electricity can not be easily transferred between them. The chapter presents results for two control groups for California: plants in the Western Interconnection (WECC) and plants in the entire United States. Source: NERC.

annual level. The unit of observation is thus the plant-year. I merge the eGRID data on plant characteristics with the CAMD emissions data using the Office of Regulatory Information System PLant (ORISPL) codes. Plants without eGRID information must be dropped from the sample. Given that all California power plants primarily use natural gas instead of coal or oil, I limit the sample to plants for which the primary fuel type is natural gas. The panel is unbalanced. For the plants whose emissions are not observed in every year, most have a few observations at the beginning of the time-period and are missing emission data in all future observations. This pattern is consistent with a plant closing down, so I replace the missing emission data values with zeroes.[14] In total, I drop 263 plants due to missing eGRID or demographic data This leaves me with a total of 68 eligible control plants from the WECC, and 646 eligible control plants from the entire United States. There are 85 treated plants in the California sample.

Table 1 shows summary statistics for California and the two candidate control groups. California plants, as a group, are different from the control plants. They tend to be cleaner and smaller. The communities surrounding the California plants are more diverse and have more income than the communities surrounding the rest of the WECC plants. However, average income levels in communities surrounding plants across the entire U.S. outside of California do not differ from income levels in the communities surrounding California plants.

Figure 2 plots trends in the emissions for the treatment and control groups for both NO_x and SO_x and for both the regional and national-level control groups. WECC control plants for NO_x and the national level controls for SO_x seem to provide somewhat plausible control groups as it is arguable that the assumption of parallel prior trends seems to hold. For the other two sets of trends, it seems like the parallel trends assumption is violated.

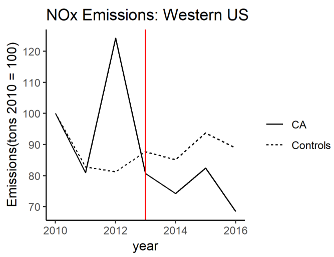
[14] Estimates dropping these plants can be found in Appendix A and are qualitatively similar. An alternative explanation is that some of these plants are high-cost "peakers" that operate only rarely when electricity prices are very high. If this explanation is true, replacing the missing emission data with zeroes would be appropriate.

TABLE 1.
Summary Statistics

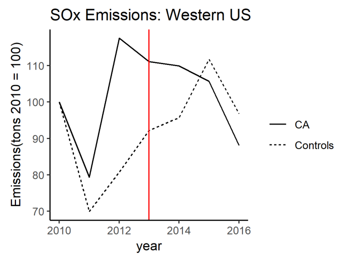
	CA	WECC	EntireUS	CA-WECC	CA-EntireUS
Plant Emissions					
NO_x Emissions (Tons/Year)	39.0	107	197	-68.3^{***}	-158^{***}
	(125)	(157)	(507)		
SO_x Emissions (Tons/Year)	2.27	3.17	29.7	-0.893^{***}	-27.4^{***}
	(4.54)	(4.04)	(375)		
CO_2 Emissions (Thousands of Tons/Year)	412	521	613	-109^{***}	-201^{***}
	(616)	(602)	(933)		
Plant Attributes					
Nameplate Capacity (MWh)	432	483	571	-51.3^{*}	-140^{***}
	(564)	(410)	(554)		
Heat Rate	9.67	9.45	13.0	0.220	−3.28
	(3.15)	(2.66)	(145)		
Annual Net Generation	0.916	1.11	1.29	-0.193^{**}	-0.372^{***}
	(1.46)	(1.35)	(2.06)		
Community Characteristics					
Proportion Minority	0.494	0.321	0.266	0.174^{***}	0.229^{***}
	(0.247)	(0.215)	(0.233)		
Per-Capita Income (Thousands of Dollars)	25.0	21.2	24.7	3.87^{***}	0.319
	(11.9)	(5.09)	(7.86)		
n	680	544	5,168		
Average Within-Plant Standard Deviations Over Time					
Proportion Minority	0.008	0.007	0.012	0.001	−0.004
Per-Capita Income	1.36	1.27	1.40	0.085	−0.041
Number of Plants	85	68	646	17	−561

Notes: Summary statistics for natural-gas-fired power plants. The treatment group is California. Two possible control groups shown are the WECC (which includes Arizona, California, Colorado, Idaho, Montana, New Mexico, Oregon, Utah, Washington, and Wyoming) and all non-California gas-fired plants in the U.S. (Standard errors in parentheses.)

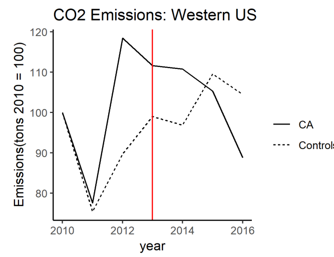
FIGURE 2.
Emissions for Treatment and Control Plants, 2009–2016



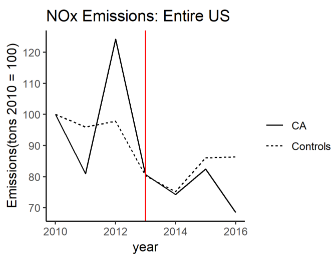
(a) NO_x Western U.S.



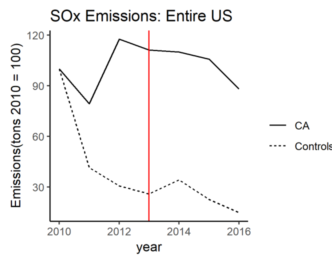
(b) SO_x Western U.S.



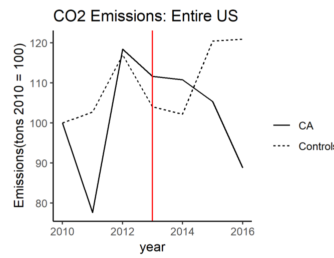
(c) CO_2 Western U.S.



(d) NO_x Entire U.S.



(e) SO_x Entire U.S.



(f) CO_2 Entire U.S.

Notes: This figure shows historical emissions for the treated plants as well as the historical emissions for all control plants.

The most simplistic approach to answering the research question poised in this chapter would be to compare the mean emissions before and after the program. Many of the EJ groups concerned about cap-and-trade in California implicitly make such an argument and cite research such as Cushing et al. (2016) who follow this method. If I were to replicate this approach with my data, I would find a statistically insignificant decrease of 10.2 tons a year in NO_X and a statistically significant increase of .062 tons per year for SO_x. However, there are major concerns about the validity of this simple approach. It is impossible to separate the effect of the program from changes in co-pollutant levels that would have occurred anyway. To get proper estimates of the program's *causal* impact, we need to find a proper control group that would allow us to estimate what would have happened at the California plants under the no-program counterfactual.

The most natural way to construct a counterfactual would be to estimate a simple difference-in-differences model using plants outside of California as a control group. However, the differing pre-trends should lead us to approach these simple difference-in-differences results with caution. Table 2 shows estimates from such a difference-in-differences model with the WECC plants acting as the control group. Rudimentary difference-in-differences specifications imply that the California carbon program caused co-pollutants to increase. Parameter estimates for the change in SO_x emissions imply an increase of more than five standard deviations, which is implausibly large. The inclusion of state-by-year time trends causes the results to lose statistical significance. This suggests that the simple difference-in-differences results (showing an increase in co-pollutants) are *not* properly controlling for economic shocks that differentially impact the treatment and control groups, and that the model does a poor job of controlling for differences in covariates across treatment and control groups. Even in the most defensible specification, including state-specific time trends, the standard errors are large and the thus the parameters are imprecisely estimated. A more sophisticated method of selecting the appropriate control group could both reduce bias and shrink standard errors by controlling better for differing pre-trends.

Nearest-Neighbor Matching Estimator

To construct a more-valid control group, I turn to a nearest-neighbor matched difference-and-difference estimator. There is a large literature concerning the properties and implementation of this matching estimator (Heckman et al.

TABLE 2.
Effects on Co-pollutants of California's
Carbon Cap and Trade: Non-Matched Difference-in-Difference (2010 - 2016)

	Dependent variable:							
	NOx Emissions (tons)				SOx Emissions(tons)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA	-126^{***}	-149^{***}			-24.7^{*}	-16.4^{**}		
	(22.1)	(25.1)			(13.0)	(8.2)		
CA × PerCapitaIncome		11.7^{*}	−0.4	1.6		2.3	−1.7	6.4
		(6.8)	(6.9)	(7.8)		(1.6)	(9.5)	(13.4)
CA × ProportionMinority		1,610	811^{*}	713		356	793	587
		(991)	(443)	(507)		(235)	(1,330)	(1,320)
Post	-50.0^{***}	-59.0^{***}	-32.0^{***}	−53.4	-23.1^{**}	-21.8^{**}	−9.0	11.1
	(10.5)	(12.5)	(9.4)	(44.6)	(10.4)	(9.7)	(12.6)	(27.2)
Post × PerCapitaIncome		12.0	5.5	10.8		1.5	−6.3	0.1
		(10.0)	(8.4)	(9.0)		(1.3)	(8.5)	(11.3)
Post × ProportionMinority		159.0	825.9	867.1		−413.0	1,150	793
		(434)	(550)	(592)		(420)	(1,350)	(1,310)
CA × Post	26.1^{**}	28.9^{**}	35.4^{**}	50.2	21.9^{**}	19.6^{**}	19.1	−2.3
	(12.9)	(13.8)	(14.4)	(46.4)	(10.8)	(9.5)	(14.3)	(28.9)
CA × Post ×PerCapitaIncome		-35.7^{*}	−0.7	−7.6		−5.0	7.0	0.2
		(18.6)	(8.4)	(9.3)		(3.6)	(8.7)	(11.4)
CA × Post ×ProportionMinority		$-3{,}200^{*}$	−767	−852		−175	−1,110	−761
		(1,902.6)	(532.0)	(649.7)		(563.3)	(1,335.0)	(1,309.1)
Constant	30.7	−42.8			−0.8	12.7		
	(26.1)	(57.6)			(11.2)	(30.4)		
PerCapitaIncome		1.5	−0.6	−1.9		−0.3	1.0	−6.6
		(1.4)	(6.4)	(7.3)		(0.7)	(9.2)	(13.0)
ProportionMinority		149.2^{**}	−528.9	−400.0		−23.3	−794.7	−569.4
		(62.2)	(338.8)	(331.8)		(38.3)	(1,329.8)	(1,315.1)
Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes
State Specific Time Trends	No	No	No	Yes	No	No	No	Yes
Observations	5,236	5,221	5,221	5,221	5,236	5,221	5,221	5,221
Note:								*p<0.1; **p<0.05; ***p<0.01

Notes: This table shows parameter estimates from a panel diff-in-diff for the effect of California's carbon cap-and-trade program conditional on the plants surrounding demographics. Controls include Primary fuel type and nameplate capacity. All interactions are demeaned. Standard errors, in parentheses, are clustered at the plant level.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

(1997), Heckman et al. (1998), Abadie (2005), Abadie and Imbens (2006), Abadie and Imbens (2011), Fowlie et al. (2012)). The estimator matches treated units with their nearest neighbors, where "distance" is decided by similarity in covariates. By limiting the control group to the nearest neighbors of the treated plants, dissimilar control plants (which may bias or obscure the effect of the program) are removed from the estimation. The advantage of a semi-parametric approach, compared to the standard difference-in-difference model, is that it allows more flexibility in terms of the functional form of the relationship between the treated and control groups when estimating treatment effects.

Following the potential outcome framework from Rubin (1973), suppose that each plant has two potential levels of emissions based on its inclusion in a carbon pricing program like the one in California. Let $Y_{i,t}(1)$ be the emissions from plant i in time period t under carbon pricing, and let $Y_{i,t}(0)$ represent the emissions under the no-carbon-pricing counterfactual. Let $D_i = 1$ if plant i was actually subject to the California cap-and-trade and $D_i = 0$ if it was not. I wish to estimate the average treatment effect on the treated (ATT)

$$ATT = E[Y_{i,t}(1) - Y_{i,t}(0)|D_i = 1] \tag{2.1}$$

For the California plants, the econometrican observes only $Y_{i,t}(1)$ The challenge is to find a consistent estimator for $Y_{i,t}(0)$. The semi-parameteric matching estimator uses the M nearest neighbors in covariate space to estimate $Y_{i,t}(0)$. In other words, the estimator constructs a matched control group for each Californian plant. The distance between plant attributes is measured by the Mahalanobis norm, which scales the measured difference in each plant attribute by the standard deviation of that attribute. This means a higher penalty is assigned to plants which differ in attributes that do not have much variation than to plants that differ in attributes that vary widely across the sample.

It can be shown that the ATT can be consistently estimated as:

$$\frac{1}{N_{\text{treat}}}\left[\sum_{i\in\mathcal{I}_{\text{treat}}}(Y_{i,t_1}(1)-Y_{i,t_o}(0))-\frac{1}{M}\sum_{j\in\mathcal{J}(i)}(Y_{j,t_1}(0)-Y_{j,t_0}(0))\right] \tag{2.2}$$

where $\mathcal{I}_{\text{treat}}$ is the set of treated plants and $\mathcal{J}_i$ is the set of the M closest matches to plant i from the group of control plants. Y_{i,t_0} is average emissions of plant i in the pre-treatment period ($t = t_0$) and Y_{i,t_1} indicates the average emissions of plant i after the introduction of the cap-and-trade program ($t = t_1$).

I also implement the finite bias adjustment suggested in Abadie and Imbens (2011). This approach uses OLS to adjust for remaining differences in covariates between the treated entity and the matched control group.

To test for adverse environmental justice effects of the program, I run the following regression:

$$\begin{aligned}\Delta\text{Emissions}_i &= \beta_0+\beta_1 Treat+\beta_2 Treat\times PropMinority\\ &+\beta_3 Treat\times PerCapitaIncome+\gamma X_i+\eta_{\mathcal{J}(i)}+\epsilon_i\end{aligned} \tag{2.3}$$

where the X_i are a set of plant-level controls and $\eta_{\mathcal{J}(i)}$ is a match-group fixed effect. The regression thus compares each treated unit to the within-group variation from the set of control plants chosen by the semi-parametric matching estimator. The inclusion of interaction terms captures how changes in co-pollutants vary systematically with demographic characteristics of the surrounding community.

Synthetic Control Approach

As an additional robustness check, I also estimate a synthetic control model. The synthetic control approach provides a data-driven method for choosing a control group. Instead of finding controls based on covariate similarity, as in matching, a synthetic control model finds the linear combination of controls that best approximates (by minimizing the mean-square prediction error) the path of California emissions in the before-treatment periods. This linear combination of controls can then be used as a counterfactual, or "synthetic control," for the treated entity. The effect of the policy is the difference between the actual post-treatment outcome for the treated entity and the outcome predicted by the synthetic control.

Given that available synthetic control algorithms are designed to analyze only one treated entity, I aggregate the California power plant emissions data to the state level and use average per-plant emissions as my outcome variable. The synthetic control approach makes no provisions for heterogeneous treatment effects. However, to test for environmental justice concerns, I can find the program's effect on low-income and high-minority share communities by limiting the sample to include only these groups.

Results

Matching Estimator Results

Table 3 shows the estimate of the ATT computed by the semi-parametric matching estimator. The first column of results shows the estimates for the program's effect on NO_x. The second column of results shows the estimates for the program's effect on SO_x. Lastly the third column shows estimates for the program's effect on carbon dioxide (CO_2). Rows 1 and 2 show estimates using plants in the western United States as a control group. Rows 3 and 4 show estimates allowing all (non-California) plants in the United States to be used as potential controls. Rows 2 and 4 use the Abadie and Imbens (2011) finite-bias adjustment, whereas Rows 1 and 3 do not. Estimates with and without the bias adjustment are qualitatively similar.

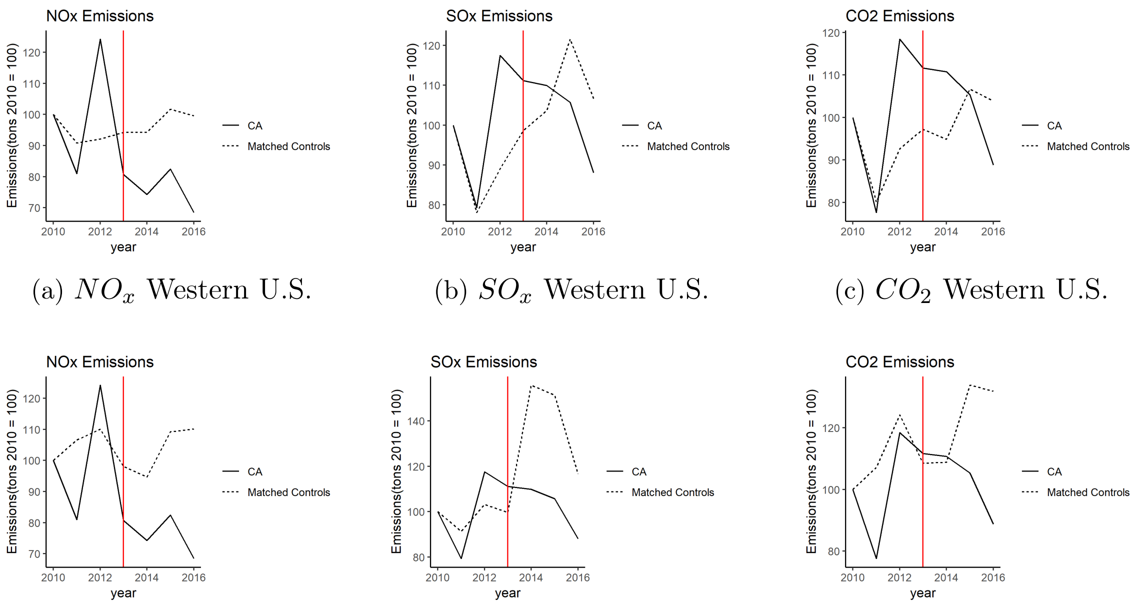
TABLE 3.
Average Treatment Effect on the Treated Estimates From Matched Difference-in-Difference For California's Carbon Cap-and-Trade on Co-pollutants

Donor Pool	NO_x	SO_x	CO_2	N-Treated	N-Control
Western U.S.	−24.1	−0.859	−19.9	85	68
	(17.9)	(0.553)	(40.3)		
Western U.S. (With Bias Adjustment)	−25.4	−0.834	−38.0	85	68
	(17.8)	(0.514)	(45.5)		
Entire U.S.	−22.9**	−1.60**	−79.4**	85	646
	(10.9)	(0.794)	(33.9)		
Entire U.S. (With Bias Adjustment)	−29.8***	−1.43	−91.4***	85	646
	(11.2)	(1.44)	(35.5)		

Notes: This table shows parameter estimates from a nearest-neighbor matched difference-in-difference estimator. Distance is computed using the Mahalanobis norm based off of efficiency (heat rate), nameplate capacity and past emission histories.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Regardless of the control group used, the signs of the key parameter estimates in Table 3 suggest that, on average, California's cap-and-trade program has decreased both carbon dioxide and co-pollutants. However, the effect is statistically significant only when the control plants outside California are drawn from the entire rest of the country. This difference could reflect the fact that, with a larger sample size and better quality matches, there is greater statistical power and thus smaller standard errors. Even if the difference in standard errors is not due to sample size, there is no evidence that the program, on average, has had an adverse effect on co-pollutant emissions.

Figure 3 plots emissions of the treated plants both in California and for those non-California plants that have been selected at least once as a control. The comparability of the pre-trends seems to have improved somewhat, particularly for SO_x.



(a) NO_x Western U.S. (b) SO_x Western U.S. (c) CO_2 Western U.S.

(d) NO_x Entire U.S. (e) SO_x Entire U.S. (f) CO_2 Entire U.S.

FIGURE 3.
Emissions for Treated Plants (CA) and Plants Used For Matching

Notes: This figure shows historical emissions for the treated plants as well as the historical emissions for all control plants that were matched at least once to a treated plant.

Table 4 shows estimates, for equation 3, for heterogeneous treatment effects by race and income. The first three columns show estimates for NO_x, SO_x, and CO_2 respectively using the WECC as a control group. Columns 4, 5, and 6 show estimates for NO_x SO_x, and CO_2 respectively, allowing every natural-gas plant in the US, outside of California, to be used in matching.

TABLE 4.
Heterogeneous Treatment Effects

	Emissions (Tons/Year)					
	Western US NOx	Western US SOx	Western US CO2	Entire US NOx	Entire US SOx	Entire US CO2
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−1,670	−18.8	3,660	2,990	913	2,910
	(2,570)	(80.6)	(6,270)	(2,180)	(2,190)	(2,780)
Proportion Minority	47.0	0.74	−88.1	−26.9	39.8	−141**
	(118)	(2.62)	(238)	(59.7)	(64.9)	(59.3)
Per-Capita Income	−2.44	−0.11	−12.7	0.13	0.90	−2.73
	(3.51)	(0.10)	(8.56)	(1.86)	(1.54)	(1.75)
Treat× Proportion Minority	−72.3	−0.80	165	123	38.0	121
(Adverse EJ $\implies coef > 0$)	(111)	(3.55)	(276)	(88.8)	(89.3)	(114)
Treat× Per-Capita Income	3.43	0.09	18.9*	2.71	1.33	1.81
(Adverse EJ $\implies coef < 0$)	(3.99)	(0.11)	(10.19)	(3.30)	(2.79)	(3.31)
Constant	9.22	2.69	441	27.6	30.8	175***
	(83.7)	(3.53)	(296)	(76.5)	(72.7)	(63.6)
Observations	153	153	153	731	731	731

Notes: This table shows estimates of heterogeneous treatment effects. Estimates are computed from a regression of changes in co-pollutant emissions on demographic variables and a match-group fixed effect.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

For the purposes of this chapter, the California cap-and-trade program will have had an adverse environmental justice impact if disadvantaged socioeconomic groups are disproportionately affected (i.e. if they see lesser pollution reductions or greater increases in pollution than the rest of the population). The coefficients of interest for the environmental justice implications of the policy are those on the interaction terms (*Treat* $\times$ *Proportion Minority*) and (*Treat* $\times$ *Per-Capita Income*). These coefficients show how changes in emissions have differed with the racial composition and income of the surrounding communities. Any statistically significant results for the estimates of these coefficients would therefore suggest that the gains or losses in co-pollutant levels do not accrue evenly across income and race.

To test for adverse environmental justice impacts on minority communities, the relevant coefficient to consider is the one on (*Treat* $\times$ *Proportion Minority*). A positive estimate for this coefficient would imply that minority communities have seen additional, disproportional, increases in co-pollutants as a result of the policy and that the EJ concerns about the program are supported by the data. A negative coefficient estimate would imply that minority communities have seen additional *decreases* in emissions as a result of the policy. The signs of the estimates for this coefficient vary by pollutant and control group. However, none of the six estimates are statistically significant and thus I cannot reject the null hypothesis that gains from this policy are *unrelated* to the racial composition of the surrounding communities.

To test for the presence of environmental justice concerns along the income dimension, we need to examine the coefficient on (*Treat* $\times$ *Per-Capita Income*). Concerns about adverse environmental justice effects would be valid if the estimates for this coefficient were negative. This would mean that lower-income communities saw larger increases, or smaller decreases, in emissions than higher-income communities. The signs of all of the estimates are positive suggesting that the policy had a progressive impact. Only one of these estimates — for CO_2 using the WECC as a control group — is statistically significant. However, because carbon is uniformly mixing, the distribution of carbon emissions has no spatially differentiated effect on local welfare. For the other five estimates, I cannot reject the null hypothesis that emission reductions are unrelated to income.

To summarize: a statistically significant result for either of the key coefficients would imply that the pattern of abatement gains differs systematically with the racial and income characteristics of the community. However, across all pollutants and all control groups, I find no evidence of adverse environmental justice impacts, with respect to either race or income, as a result of California's cap-and-trade program.

Robustness Checks

Table 5 displays several robustness checks designed to address possible threats to identification.[15] For comparison, the first two rows of the table reproduce the (bias-adjusted) estimates of the effects of the program from Table 3.

Leakages. One concern is that the program may have affected electricity generators outside of California, thereby contaminating the controls. This could occur if, for instance, the increased cost of carbon made unregulated electricity outside of California more attractive to buyers inside California, causing "leakages." Given that the structure of the grid makes it difficult to transfer power outside

[15]For brevity I only display the ATT estimates. The heterogeneous treatment effects estimates are also robust and can be found in Appendix A.

TABLE 5.
Robustness Checks: ATT Estimates of Program Effect

Control Group	NO_x	SO_x	CO_2	N-Treated	N-Controls
Western US (With Bias Adjustment)	-25.4	-0.834	-38.0	85	68
	(17.8)	(0.514)	(45.5)		
Entire US (With Bias Adjustment)	-29.8^{***}	-1.43	-91.4^{***}	85	646
	(11.2)	(1.44)	(35.5)		
No Western States	-31.3^{***}	-1.48	-90.5^{***}	85	578
	(11.4)	(1.50)	(34.0)		
No 2012 West	-25.4	-0.751	-22.5	85	68
	(17.2)	(0.525)	(48.2)		
No 2012 Entire US	-34.6^{***}	-1.67	-92.3^{**}	85	646
	(13.3)	(1.58)	(36.2)		
Post San Onofre (2012) Only: West	-26.9	-1.28^{**}	-114^{**}	85	68
	(20.4)	(0.546)	(46.1)		
Post San Onofre (2012) Only: Entire U.S.	-26.2^{***}	-1.31^{**}	-95.4^{***}	85	646
	(8.38)	(0.659)	(39.0)		

Notes: This table shows various robustness checks. The first two rows repeat the results shown in Table 3. The third row drops all WECC plants from the control group to test for spillovers. The fourth and fifth row show estimates when the year 2012 is dropped from the sample to test for anticipation effects. The last two rows assess how the estimates are affected by the closure of the San-Onofre nuclear power plant.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

of the Western Interconnection, I can test for spillovers by eliminating all WECC plants as potential national-level controls. The key estimates remain qualitatively similar, with overlapping confidence intervals.

Anticipatory Effects. There may also be some concern that the results may be biased by anticipatory effects in the lead-up to the introduction of the regulation. Initial permit allocations were determined in part by a firm's historical record of emissions, so firms may have had an incentive to increase their emissions right before the start of the program. There does seem to be an increase in emissions around the program start date, but this increase also occurs for both control groups. To test whether these anticipation effects are significantly biasing my estimates, I drop from the sample the year immediately preceding the program (2012) since emissions in that year where used to determine permit allocations. I then rerun the estimator. The results for this robustness test are displayed in rows 4 and 5 of Table 5. The resulting estimates are qualitatively similar and, if anything, suggest that the original estimates may understate the NO_x emission reductions due to the program.

San Onofre Closure. In January 2012, the San Onofre nuclear power plant permanently closed. San Onofre was a large source of electricity to California that provided eight percent of in-state electricity generation. Its closure increased the demand for electricity from natural gas plants. As documented in Davis and Hausman (2016), transmission constraints made it difficult for plants to replace the lost generation and therefore San Onofre's closure resulted in an increase in emissions and a change in the spatial distribution of electricity generation across the state. Given that matches are made, in part, based on a plant's emission history between 2010 and 2012, this change could lower the quality of the matches if a plant's pre-closure emissions history no longer predicts the plant's post-closure behavior. To assess the effect of this closure on the matches made by the estimator, I explore a specification that uses observations only after the San Onofre closure, but still before the start of the cap-and-trade program to construct the matches. The results, in the last two rows of Table 5, remain qualitatively similar.

Placebo Tests. One concern is that my results could be due to an overfitting of the model by my choice of covariates in a way that produces a spurious statistically significant result. Figure 4 shows results from 50,000 placebo tests, where "treatment" status is randomized across all units in the sample. The estimates for the actual set of treated plants from Table 3 are marked by the thin vertical line. If the estimator is not overfitting, or otherwise downward biasing the results, the distribution of estimated placebo treatment effect sizes should be centered around zero. This is what occurs.

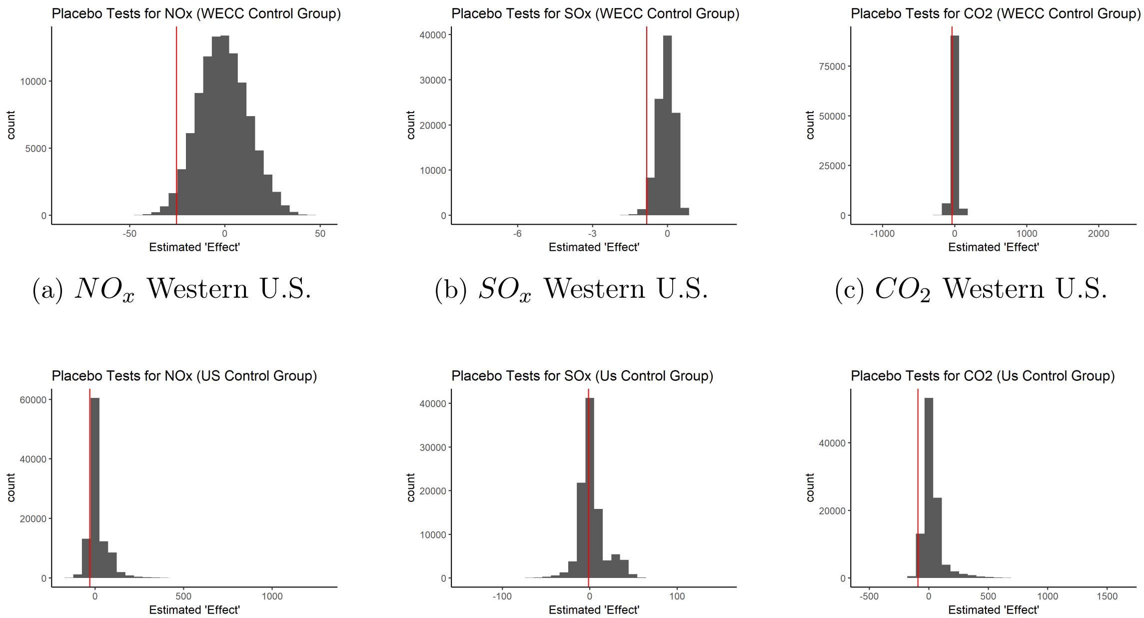
Additionally in Appendix A I show results which include a measure of RPS stringency and controls for weather.

Synthetic Control Method Results

Figure 5 plots the results of the synthetic control model. The data have been disaggregated to the monthly frequency. Actual California emissions are shown by the solid line and the synthetic control is shown by the dotted line.[16] Treatment effects can be computed by examining the distance between the two lines. Table 6 shows yearly averages of these estimated treatment effects. The estimated effect is much lower than the effect estimated from the matching estimator and

[16] Weights for the synthetic control can be found in Appendix A.

FIGURE 4.
Placebo Test for Semi-Parametric Matching Estimator



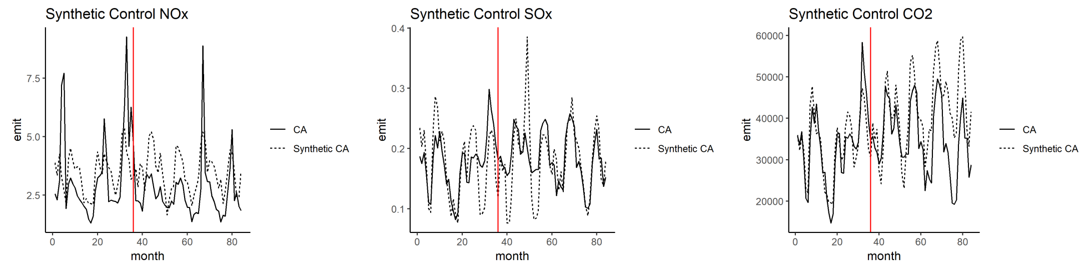
(a) NO_x Western U.S. (b) SO_x Western U.S. (c) CO_2 Western U.S.

(d) NO_x Entire U.S. (e) SO_x Entire U.S. (f) CO_2 Entire U.S.

Notes: This figure shows ATT estimates from a placebo test of the semi-parametric nearest-neighbor estimator. "Treatment" status was randomly assigned to a subset of plants from the entire sample. The vertical red line shows the estimate calculated from designating the actual treated plants as treated.

is frequently close to zero. There is, however, no evidence of a damaging increase in co-pollutants.

Inference in synthetic control models is done through a permutation test as suggested in Abadie et al. (2010). The basic idea of this test is, if the policy had an effect on the outcome variable of interest, that there should be an increase in the distance between the synthetic control and the actual outcome for the treated entity after the start of the program. Recall that, the synthetic control is constructed from those states whose ,weighted, average minimizes the distance between the synthetic control and the pre-treatment outcome. If the policy



(a) NOx (b) SO_x (c) CO_2

FIGURE 5.
Synthetic Control Plots

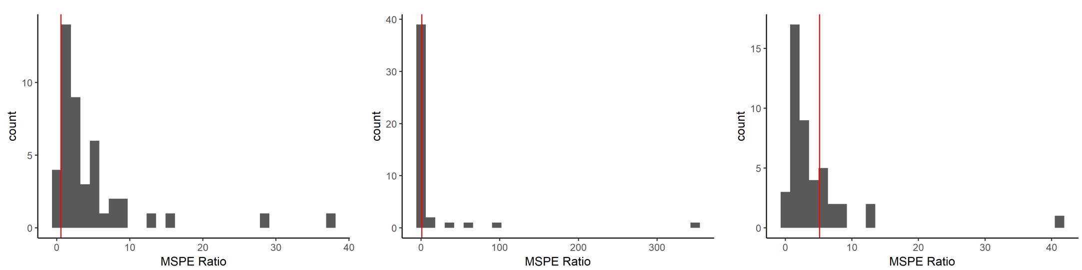
Notes: This figure shows historical emissions from California plants (solid line) and the imputed counterfactual emissions from the "synthetic control" (dotted line). The distance between the actual and synthetic control emissions shows the estimated effect of the policy. The vertical red line marks the start of California's carbon cap-and-trade policy.

has an effect on the outcome variable of interest, the policy will change the relationship between the outcome for the treated entity and the outcome for the synthetic control. In this case, the goodness-of-fit between the synthetic control and the actual data will deteriorate after the start of the program. This "forecast deterioration" can be quantified by calculating the ratio of the mean-squared prediction error (MSPE) of the synthetic control *after* the treatment period to the MSPE *before* the treatment period. A higher MSPE ratio signifies that the program caused a greater deterioration in the goodness of fit, indicating that the estimate is statistically significant.

How large the MSPE ratio of a given synthetic control needs to be, to achieve statistical significance, is determined by comparing it to the distribution of placebo MSPE ratios. In this permutation test, a placebo synthetic control model is estimated for each control unit wherein that unit is artificially designated as the treated entity. If the MSPE ratio for the actual treated entity is above the $(1 - \alpha)$

percentile of all the placebo MSPE ratios, we reject the null hypothesis that the policy had no effect at the α significance level.

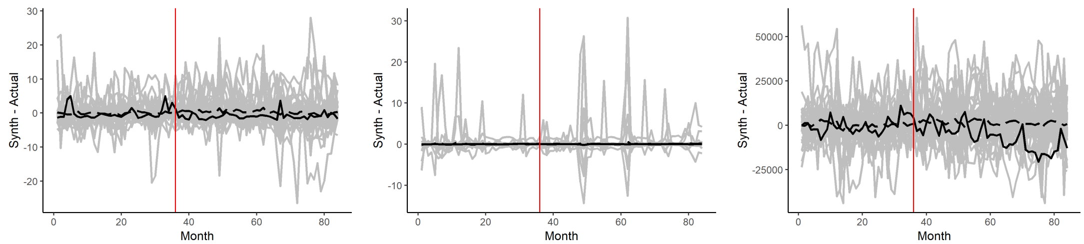
Figure 6 shows the result of this permutation test. The pre-post MSPE ratio for California is in the middle of the distribution of the placebo estimates for both pollutants, suggesting that the policy had no statistically significant effect on co-pollutant emissions. Equivalently, but more intuitively, the permutation tests can be shown by plotting the implied treatment effects for California and the placebos as in Figure 7. The dark black line shows the implied treatment effect (difference between the actual and the synthetic California) for California, where the grey lines show the implied treatment effect for each placebo test. The dotted black line shows the average of all placebos. This form of the permutation test makes it clear that California does not significantly deviate from the emissions behavior of any other state in the post-treatment period, although it does tend to respond a little more than the average of the placebos.



(a) NOx (b) SO_x (c) CO_2

FIGURE 6.
Synthetic Control Permutation Tests: MSPE Ratios

Notes: This figure shows the ratio of the mean square prediction error (MSPE) after and before the introduction of the cap-and-trade program. A high MSPE ratio indicates a departure from the synthetic control after the introduction of treatment. California's MSPE is shown by the red line. The fact that California is not an outlier implies that the synthetic control estimates are not statistically significant.



(a) NOx (b) SO_x (c) CO_2

FIGURE 7.
Permutation Test: Estimated Treatment Effects, Placebo Versus Treated

Notes: This figure shows the estimated treatment effects (Actual - Synthetic Control) for both California (black) as well as placebo estimates for each control state (grey). The dotted line shows the average of the placebos. For clarity a few outliers have been dropped. Figures with outliers can be found in Appendix A.

Table 6 also shows estimates of the cap-and-trade program on low-income and high-minority-share communities. None of the estimates are statistically significant. Similarly, Figure 8 shows synthetic control results when the sample is restricted to include only plants in low-income communities, high-minority-share communities and communities which are both low-income and high-minority share. Lastly, Figure 9 shows the placebo estimates for the restricted sample. The estimates of the program's effect for all restricted samples are small, close to zero and statistically insignificant.[17] Although these comparisons of synthetic control estimates between various samples lack the formal statistical testing of the matching estimator, these results suggests that abatement patterns across plants have not differed for plants in communities with lower-than-average incomes or higher-than-average minority shares, when compared to the state as a whole.

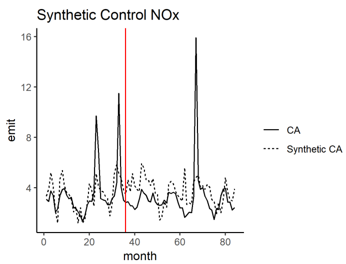
[17]MSPE ratios for the restricted samples can be found in Appendix A

TABLE 6.
Synthetic Control Estimates

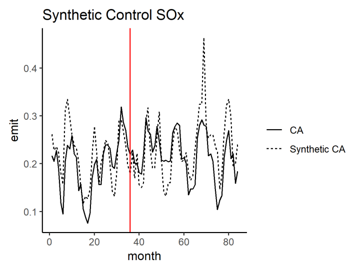
	2013	2014	2015	2016	Implied P-Value
			Full Sample		
NO_x	-1.25	-0.851	-0.723	-0.812	(0.911)
SO_x	0.0197	0.012	-0.0113	-0.00854	(0.533)
CO_2	-1.22	-2.70	-8.70	-14.1	(0.244)
			Low-Income Communities Only		
NO_x	-1.57	-0.227	0.0559	-0.497	(0.513)
SO_x	0.0182	0.0221	-0.0454	-0.0564	(0.543)
CO_2	1.66	2.58	-4.11	-8.27	(0.600)
			High-Minority-Share Communities Only		
No_x	-1.57	-0.47	-0.907	-2.07	(0.909)
SO_x	0.00703	0.0307	-0.0497	-0.0887	(0.818)
CO_2	-2.18	10.8	-9.12	-13.5	(0.727)
			Both EJ Groups Only		
NO_x	-1.24	-0.378	-0.152	-1.35	(0.65)
SO_x	0.0299	0.0574	-0.022	-0.0623	(0.85)
CO_2	81.6	13.2	-6.88	-11.8	(0.65)

Notes: This table shows estimates, using the synthetic control method, of the effect of California's cap-and-trade program for each year. Only low-income communities and only high minority share communities only include communities with income (minority share) below(above) the California median. Inference is done using a permutation test. The p-values from these tests are displayed in parenthesis. Weights for the synthetic control are shown in Appendix A.

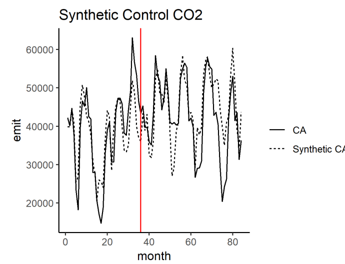
Low-Income Communities Only



(a) NOx

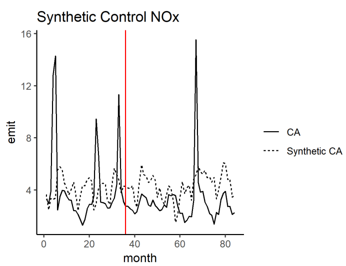


(b) SO_x

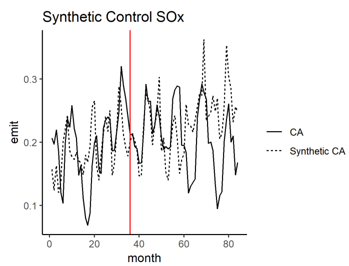


(c) CO_2

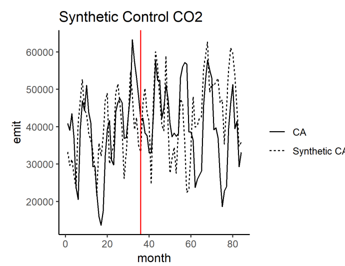
High-Minority-Share Communities Only



(d) NOx

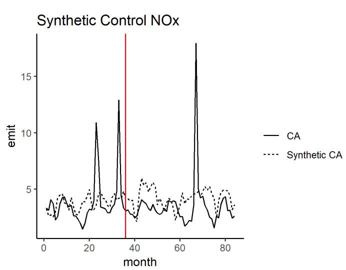


(e) SO_x

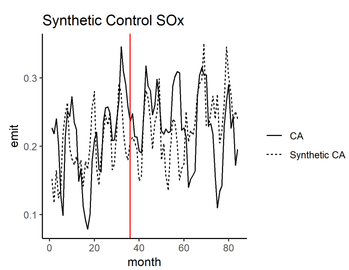


(f) CO_2

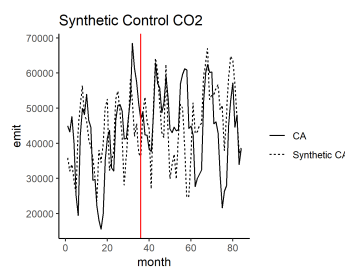
Low-Income *And* High-Minority-Share Communities Only



(g) NOx



(h) SO_x

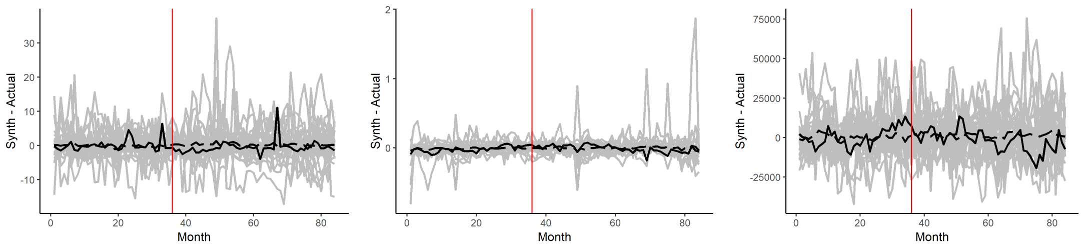


(i) CO_2

FIGURE 8.
Treatment Effect Heterogenity: Actual vs. Synthetic Control

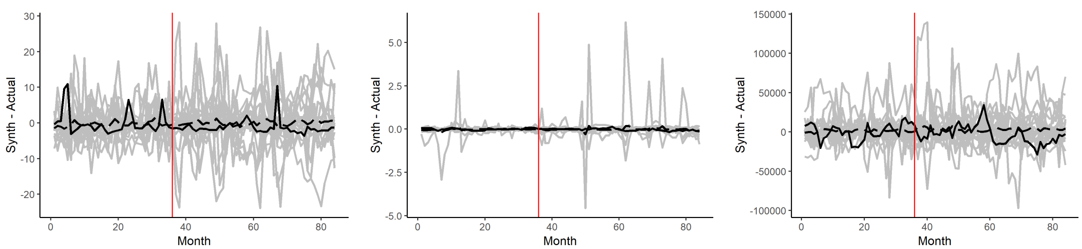
Notes: This figure shows historical emissions from California plans (solid line) and the imputed counterfactual emissions from the "synthetic control" (dotted line) for various subsamples of the data. The vertical red line marks the start of California's carbon cap-and-trade policy.

Low-Income Communities Only



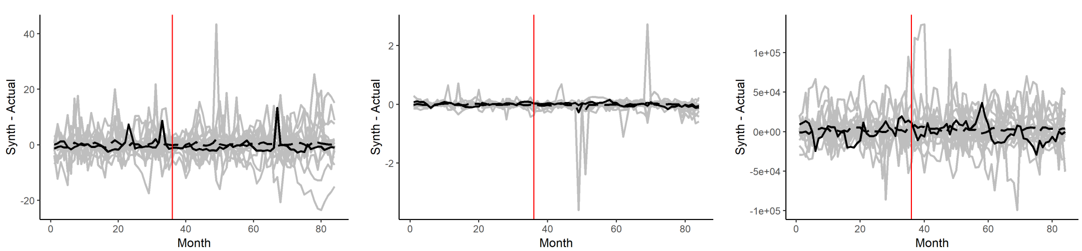
(a) NOx (b) SO_x (c) CO_2

High-Minority-Share Communities Only



(d) NOx (e) SO_x (f) CO_2

Low Income *And* High Minority Share Communities Only



(g) NOx (h) SO_x (i) CO_2

FIGURE 9.
Treatment Effect Heterogenity: Placebos versus Treated

Notes: This figure shows estimated treatment effects for placebo estimates (grey) and California (black), for various subsamples of the data. The dotted line shows the mean of the placebo treatments.

Caveats and Directions for Future Research

My data sources and statistical approaches have several limitations and qualifications. First, my analysis focuses on just one sector, electricity generation, and does not take into account the effect of carbon cap-and-trade on the distribution of pollutants from other sectors. Within-state electricity generation represents only eleven percent of California's overall greenhouse gas emissions. The other major contributing sectors are transportation (39 percent) and other industrial sources (23 percent). The external validity of my results depends on the degree of similarity in the pattern of abatement in the electricity generation sector compared to other sectors that also fall under the carbon cap-and-trade program. It is worth noting, though, that the concerns of environmental justice activists often rely on general arguments that are not specific to any one sector. Future work could expand this analysis to address changes in the distribution of co-pollutant abatement in other industries.

Given the relatively short period of time that has passed since the implementation of this carbon trading program, my results reflect only the short-run responses of firms and cannot describe possible long-run effects. For instance, the cap-and-trade program could affect plant entry and exit decisions differently for clean and dirty plants. Cap-and-trade could encourage cleaner plants to open, or dirtier plants to close. Thus, long-term co-pollutant abatement patterns may be different from short-term abatement patterns.

The California carbon cap-and-trade program was announced many years before it became law, so there may be reason to believe that there was enough lead time for firms to invest in abatement capital and other anticipation measures. Unfortunately, no data exist concerning this behavior, so I am unable to characterize fully the extent of relevant capital investment that occurred before or during my sample period. If there was a wave of anticipatory investment before the start of the program, the direction and degree of bias in my estimates would depend on how the pattern of that investment is related to local income and demographic characteristics.

Finally, my results may not generalize in a straightforward fashion to other carbon-dioxide trading programs. California's inventory of power plants was already much cleaner relative to the average grid. This property reflects both a lack of coal generation and longstanding tighter air quality regulations in general. Carbon cap-and-trade may lead to more co-pollutant abatement in a jurisdiction that relies more heavily on dirty fuels like coal, when coal could be easily replaced by generally cleaner fuels like natural gas.

Conclusion

Environmental justice advocates have expressed grave concerns about the potential negative effects of California's carbon cap and trade program on co-pollutants. Oversimplified analytical approaches can indeed lead to the impression that co-pollutants have increased as a result of the introduction of the program. In this chapter, however, I address these EJ concerns using modern econometric program-evaluation methods and detailed data from the electricity sector. I use both a semi-parametric matching estimator and synthetic control approach to construct relevant control groups for California's treated plants in a systematic data-driven way.

Co-pollutant abatement will be determined by the spatial pattern of carbon abatement which is in turn determined by spatial pattern of marginal abatement costs for carbon emissions. I find no evidence of increases in co-pollutant emissions, or that the changes in these emissions have varied systematically with the race or income of the communities surrounding these electricity generating plants. This suggests that marginal carbon abatement costs do not differ systematically with the sociodemographic characteristics of the communities that surround these plants. Additionally, I find some evidence (although this inference is not robust to all plausible specifications) that the program may actually have caused a decrease in average co-pollutant emissions.

Further work should examine the impact of carbon cap-and-trade programs in other industries, and in other settings, to ascertain whether these results hold more generally. Additionally, as more years of data accumulate, researchers will be able to explore the longer-term impacts of the program, and growing sample sizes will permit greater precision in the key parameter estimates.

For the electricity sector, at least, my results suggest that policymakers have no apparent basis for worrying about environmental justice concerns relating to California's carbon cap-and-trade program.

CHAPTER III

DETERMINANTS OF WILLINGNESS TO PAY FOR INTERNAL CARBON PRICING PROGRAMS

Introduction

Note this chapter contains previously unpublished coauthored material (with Trudy Ann Cameron and Steve Mital). I contributed to the design and development of the survey, conducted a large share of the pre-testing, contributed to the data analysis and wrote the majority of the chapter. Trudy Ann Cameron contributed to the writing of the survey instrument and analysis of the data. Steve Mital provided important feedback on the survey instrument and was an important source of institutional knowledge.

Given that the U.S. has stepped away from any plans to price carbon at a national level, either through a carbon tax or a carbon cap-and-trade program, policymakers, climate advocates and others have expressed hope that voluntary non-governmental programs can substitute, at least in part, for the federal government's lack of a coordinated climate change mitigation policy. More than 500 U.S. businesses have implemented voluntary internal carbon pricing (ICP) programs which charge internal company divisions and individual projects for the carbon emissions they generate. Firms institute internal carbon pricing for several reasons. Some firms see it as a way to signal their commitment to sustainability, while others view it as a way to raise revenue for green energy projects. Other firms view internal carbon pricing as a means to prepare for the adoption of potential mandatory carbon government pricing policies in the future, either by enacting their own fee, or using an estimate of future carbon prices to make long-term decisions about cost-effective combinations of fixed and variable inputs (i.e. capital equipment and fuel choices) that would be relatively more cost-effective under a future carbon price, even though they might not be today. These strategies could also be used by academic institutions, non-profit organizations, and the public sector, as argued by Barron and Parker (2018b).

These internal carbon pricing programs have yet to receive much attention from the formal economics literature. The extent to which individuals are willing to pay to support these programs, or how preferences vary with program design, is relatively unknown. A better understanding of individual preferences for internal carbon-pricing would increase our sense of when and where private climate change mitigation programs may be acceptable to stakeholders. In addition, understanding preferences over the design of private carbon-pricing programs may also clarify some aspects of how people might react to alternative designs for eventual governmental carbon-pricing programs (at either the state or the national level).

A few universities have begun to experiment with internal carbon pricing. Most notably, Yale has recently introduced an internal carbon price in the form of a building energy fee, as described in Gillingham et al. (2017). Universities offer a rich setting to study individuals' preferences for carbon pricing programs, as they are large institutions consisting of several administrative divisions and many types of stakeholders who are likely to have varying preferences concerning alternative designs of internal carbon pricing programs. However, these programs are still rare, so existing evidence remains limited.

We conduct a stated-preference survey using an advisory referendum format, yielding a sample of ICP program preferences for approximately 1,000 respondents (including students, faculty and staff) at a large public university. In each choice scenario, respondents are asked to consider either one or two hypothetical carbon-pricing programs, along with a status quo alternative (with no program and no out-of-pocket costs). Programs vary in the emission reductions they achieve, and the unavoidable cost of the program to the respondent, as well as by the initial incidence of their costs across the university's population and how the collected revenues would be spent. These choices are then used to estimate a Random Utility Model (RUM) that is used to recover willingness to pay (WTP) for carbon reductions as a function of program attributes and respondent characteristics.

Increasing attention has been paid to equity issues in the implementation of carbon-pricing programs more generally. In this context, individuals may have different views of two programs that cost the same and deliver the same reductions in carbon emissions, depending on how the costs are borne across stakeholder groups, and how the revenues produced by carbon pricing are distributed across alternative uses. For example, uniform lump sum fees for everyone may be perceived as less "fair" than a fee schedule that reflects a "polluter pays" principle. In our choice scenarios, the funding for carbon emissions reduction projects can be raised, to varying extents, through simple lump-sum carbon fees on students, faculty and staff, through carbon fees on university-paid air travel, through charges on emissions generated through building energy use, or through state-government support funded by taxpayers. The revenue can be spent, to varying extents, for on-campus carbon reduction projects, for off-campus carbon "offsets," or it can be "recycled" back into academic programs.[1]

A natural concern is that the subset of stakeholders who respond to a survey about internal carbon pricing programs may differ systematically from the stakeholder population as a whole. Fortunately, we have access to conformable individual-level administrative data, for both respondents and non-respondents, which allow us to correct for systematic sample selection.[2] Having basic data on both respondents and non-respondents allows us to create a statistical model of survey response propensity. We construct a measure of each respondent's deviation from the average response propensity in our random sample from the university population who were invited to take the survey. This de-meaned response propensity is allowed to affect the estimated marginal utility of all program attributes, and we then simulate the WTP measures that would be expected, had everyone in the usable sample had a response propensity equal to the mean in the invited population.

Additionally, we strive to make our estimated WTP function useful for benefit transfer purposes. Other universities that might consider internal carbon pricing programs may have systematically different stakeholders from those at the university where our study was conducted. Our WTP function depends on the distribution of incomes, political attitudes, and other demographic and climate-related extreme-weather experience variables. It will thus ultimately be

[1]Of course, the case with 100% of the funding raised from state taxpayers and 100% of the spending devoted to academic programs would not be an internal carbon pricing program at all, just government-funded higher education. We do not include extreme mixes such as these in our program design.

[2]A protocol for identity-redaction protected campus subjects, and all data for this study are stored on a FERPA-compliant server.

possible for us to simulate the demand for specific types of internal carbon-pricing programs within the range spanned by our randomized design, at other universities with mixes of stakeholders that differ from the mix at the university where we fielded our survey.

The chapter is organized as follows. Section 3.2 reviews the context for internal carbon pricing and briefly discusses the prior literature. Section 3.3 lays out our empirical strategy. Section 3.4 presents the results of our estimated choice model. Section 3.5 concludes and discusses potential future directions for research.

Institutional Setting And Prior Literature

Over 500 companies in the U.S., as of 2017, had established internal carbon pricing (ICP) programs, and at least another 700 planned to enact a program within the next two years CDP (2017). These ICP programs have taken several different different forms.[3] The most simple ICP program involves a carbon levy on individual divisions, which is then used to fund carbon reduction programs. ICPs can also be used to meet emission reduction goals for the institution or merely as a trial run for an anticipated future mandatory government carbon pricing program. Institutions may also consider adding accounting charges based on the anticipated lifetime emissions of new (or replacement) buildings, equipment or technologies under consideration.

Universities have similar goals in the use of internal carbon pricing, seeking to use these programs as a way, simultaneously, to reduce emissions and/or raise money for future carbon-reduction projects. Universities may also see internal carbon pricing as a way to develop a reputation for sustainability as a means to

[3] For a full discussion of ICP programs in the private sector see Ahluwalia (2017) and Camuzeaux and Medford (2016)

attract students, to advance the university's mission, or as a way to educate their students about sustainability and carbon pricing.

It is technologically cost-prohibitive to meter accurately all carbon emissions related to a university campus, so we choose instead to focus this study on two major source of emissions: air travel and building energy use. On most campuses, these tend to be some of the largest sources of carbon emissions, with building heating often being the largest. For the university surveyed in this study, building heat accounts for about 48 percent of total estimated carbon emissions, and university-sponsored air travel accounts for about 13 percent. Air travel and building energy use also tend to be the carbon sources for which universities have the most information about the origin and quantity of their carbon emissions. Yale University's program is perhaps the most well-known current example of a university internal carbon pricing program (see Gillingham et al., 2017). In the Yale program, campus building carbon emissions are "taxed" and the revenue is refunded, according to their relative performance, to occupants. Other programs are being explored at Swarthmore and Smith Colleges.

Little academic work has addressed individual preferences for internal carbon pricing. In contrast, there is an extensive literature that has used stated preference methodologies to examine the demand for climate change mitigation for national-scale climate mitigation policies.

For national-level climate policies, Lee and Cameron (2008), and Cai et al. (2010) explore preferences concerning the distribution of costs of climate-change mitigation programs across groups, and the perceived distribution across country groups of the benefits of these programs (i.e. avoided damages). Groh and Ziegler (2018) finds that individuals prefer a "polluter-pays approach" over a distribution following an "ability to pay" approach which in turn is preferred to a scenario that distributes costs equally across households. Brannlund and Persson (2012) find evidence for distributional preferences within a developing country. The literature clearly demonstrates that distributional consequences have a strong influence on people's willingness to bear the costs of climate change mitigation programs more broadly.

Prior work has established that individuals are skeptical of revenue recycling. Carattini et al. (2017) examine consumers' preferences for carbon pricing programs using voting data from a Swiss carbon-tax referendum. They find that an important determinant of opposition to carbon taxes is concern about negative distributional effects from the carbon tax. Voters are skeptical of alternative revenue recycling plans and prefer that revenues be spent directly on pro-environmental programs, such as green energy or R&D. These voters, however, can be influenced to support revenue recycling more enthusiastically if they are provided with more-comprehensive information about changes in carbon emission levels as a result of the tax. Brannlund and Persson (2012), Sonnenschein and Smedby (2019) and Rotaris and Danielis (2019) all find evidence that WTP for emission charges increases if revenues are specifically "earmarked" for emission-reduction projects. Baranzini and Carattini (2017) show that individuals tend to ignore the incentive effects of carbon taxation.

Also relevant to this paper is Baranzini et al. (2018). Through a choice experiment, these authors study preferences for international versus domestic forestry-based carbon offsets. Participants are more likely to choose international offsets after being reminded about their relative cost-effectiveness. Information treatments that remind participants about co-benefits or monitoring concerns seem to show no effect.

Our survey also contributes to our understanding of consumer demand for clean energy and energy financing. In the broader literature, several papers have examined green energy demand, including Ma et al. (2015), and Conte and Jacobsen (2016). These papers find that consumers express a positive WTP for green energy that tends to increase with education level, to be higher for women than for men, and that this demand often seems to include a "warm glow effect," where consumers wish to buy at least low levels of renewable-derived electricity, but are less willing to pay for higher levels.

Survey Design and Analytical Framework

Description of Survey

Our survey was administered electronically, using the Qualtrics survey platform, in two waves—one in the late Spring of 2018 and one in the Fall of 2018. Our respondents are randomly selected from the set of all students, employees, staff and administrators affiliated with the university.[4] The survey invitation states that the university is seeking input about whether, and how, to implement an internal price on carbon and that the responses to the survey will be used by university administrators as they decide whether such a program should be implemented. Respondents are offered a five-dollar incentive in the form of a digital gift certificate to the campus store. On average, the survey took about twenty minutes to complete, although some respondents chose to study the

[4] In the spring wave, we excluded graduating seniors because their affiliation with the university was ending and their "votes" would have no consequences for them, personally.

optional background information in considerably more detail. In total, we collected 1052 responses, of which 997 were fully usable, representing a 9.4% response rate.

A detailed description of the structure of the entire survey, and one instance of the randomized survey instrument, are provided in the Appendices to this paper.[5] We sought to incorporate current best practices for stated-preference survey design, as documented in Johnston et al. (2017). Here, we review just the key features. The core of our survey is a set of "program choice" tasks. Respondents are offered the opportunity to express their preferences (i.e. to "vote") on their most-preferred alternative from a choice set that includes either one or two specific internal carbon-pricing programs versus No Program. Each alternative is described in terms of a common set of attributes, with the No Program alternative representing the status quo. The key attributes of each internal carbon-pricing program are the percentage-point net reduction in carbon emissions that the program is projected to achieve, and the unavoidable annual cost to the respondent. But we also focus on the fact that internal carbon-pricing programs can be implemented in a wide variety of different ways. We direct our respondents' attention to the distributional consequences of the different programs, in terms of both (a) how the costs of the program would be borne, and (b) how the revenue raised by these programs might be spent.

We define the default program as one which would be funded by an across-the-board "average carbon fee" charged to all students and employees of the university. The revenue to be raised, in this default case, would also be spent entirely on internal carbon-reduction projects within the university. However, it is not yet clear how the other details of any prospective ICP program would be

[5]The survey instrument can be found online by clicking on this link

settled. Thus, we designed our survey to permit an assessment of how individual willingness to pay for carbon emissions reductions might vary systematically with differences in the way the costs are borne and differences in the way the revenues are used. We allow the cost of the program to be funded in four distinct ways. In addition to (a) a flat carbon fee on all students and employees, funds can be raised through (b) a fee on university-sponsored air travel, (c) a charge for the carbon emissions of campus buildings, or (d) by relying on funds raised from the state's taxpayers. Besides spending the revenue raised for (a) on-campus carbon-reduction projects, some of the revenue could go towards (b) off-campus "carbon offsets," or (c) some revenue could be recycled in the form of spending on academic programs. All choice sets offered to each respondent are randomly populated, in advance, with different mixes of program attributes, so every copy of the digital survey "instrument" is essentially unique. The only constraints are that programs offering higher carbon reductions generally cost more money, and the difference in cost between any pair of programs offered in the same choice set is constrained to be at least five dollars.[6]

The survey begins with an extensive tutorial. Respondents are given information about the university's current carbon emissions and about internal carbon pricing programs in general. Each respondent's degree of familiarity with

[6]Our randomizations are not D-efficient due to complexity that arises from the fact that the cost shares and revenue shares must both sum to one. Additionally we wish to explore non-linearities in functional forms which would be more difficult with traditional D-efficient designs that weight choices towards extremes in attribute space and thus may have trouble distinguishing functional forms. We note that consumer rationality is sometimes tested by offering pairs of programs where one program is both less costly and more effective. However, we elect to forgo such choice sets in favor of more cases where we force people to make tradeoffs. When one program strictly dominates another in terms of cost and effectiveness, one risks having the survey respondent wonder whether they are being tricked. Of course, sufficiently negative *distributional* consequences of a cheaper program that produces greater carbon reductions could overwhelm its cost advantage, but we will be able to infer the circumstances where this might happen from our parameter estimates.

existing governmental carbon pricing programs is elicited. The choice task and each program attribute are explained in detail. Throughout the tutorial we check the respondent's understanding through frequent questions. Misconceptions are corrected. After the choice tasks, we collect information on stated attention to attributes, perceptions of research bias, history of exposure to potentially climate-related disasters, responses to a four-question version of the so-called "Six Americas" classification of climate attitudes (as described in Maibach et al. (2011), as well as a series of questions to collect potentially relevant individual-level sociodemographic information not available in the administrative data provided by the university's Office of Institutional Research.

Empirical Strategy

We follow standard stated-preference choice modeling procedures and use our survey data to estimate a random utility model (RUM) of consumer preferences. We assume that U_{jt}^i is the unobserved utility level anticipated by respondent i from internal carbon-pricing program j on choice occasion t. We assume that this indirect utility consists of a systematic component, V_{jt}^i, which can be expressed as a function of the stated attributes of program j (and selected characteristics of respondent i) and estimated parameters, plus a random component that summarizes all other unmeasured factors that affect utility, ϵ_{jt}^i. This random component is assumed to be known to the respondent who is making the program choice, so that the respondent is fully able to discern the best alternative from their own perspective, but this random component is unobserved by the researcher and therefore contributes an error term to the model.

The systematic component of the level of anticipated indirect utility under any given ICP program is assumed to depend on the respondent's annual household income, Y^i, minus the unavoidable annual cost of the program to that person, C^i_{jt} The key program attribute, other than its cost, is the level of the carbon-reduction benefit expected from the program, B^i_{jt} (measured as a percentage-point reduction in university carbon emissions). However, programs also differ in the shares of their costs borne in ways other than as a fee charged to all students and university employees, denoted as the vector $CostShares^i_{jt}$. If all of these "other" shares are simultaneously zero, the cost of the program in question will be borne entirely as an annual fee charged to all students and employees.

Programs also differ in the shares of the revenue they raise that will be used for things other than internal carbon-emissions reduction programs, denoted as the vector $ExpShares^i_{jt}$. As with the cost shares, if all of these other expenditure shares for a particular program are simultaneously zero, all of the revenue raised by that program will be spent exclusively on internal carbon-reduction programs.

Homogeneous preferences

In our simplest specification, the anticipated indirect utility from a program depends only upon the respondent's income, the program's cost and benefits, and the two vectors of non-default shares of program costs and program revenues. The simplest version of the indirect utility function, for estimation using a standard conditional logit algorithm, is:

$$U^i_{jt} = V^i_{jt} + \epsilon^i_{jt} = \alpha(Y^i - C^i_{jt}) + \beta B^i_{jt} + CostShares^i_{jt}\gamma + ExpShares^i_{jt}\delta + \eta^i_{jt} \quad (3.1)$$

In logit-based binary or multiple discrete choice models suitable for analyzing people's responses to the choice questions posed in our survey, it is assumed that the *relative* anticipated indirect utility levels of the different alternatives drive the choices made by individuals. Every choice task in this study includes No Program as an alternative, indexed as $j = 0$. The No Program alternative involves no cost, no benefits, and thus no issue of the distribution of either the costs or the revenues. Thus $U_{0t}^i = \alpha(Y^i) + \eta_{0t}^i$. The difference in anticipated utility between alternative j and the No Program alternative can then be written as:

$$(U_{jt}^i - U_{0t}^i) = \alpha(-C_{jt}^i) + \beta B_{jt}^i + CostShares_{jt}^i \gamma + ExpShares_{jt}^i \delta + \epsilon_{jt}^i \qquad (3.2)$$

where $\epsilon_{jt}^i = \eta_{jt}^i - \eta_{0t}^i$. In this linear and additively separable specification for utility, individual household incomes conveniently drop out of the utility differences.[7]

The model in equation (3.2) involves several fixed but unknown preference parameters, including α, the marginal utility of net income, and β, the marginal utility of a percentage-point reduction in carbon emissions, as well as *vectors* of fixed parameters: γ, which conveys the marginal utility (or disutility) of the shares of program costs borne in ways other than just a flat carbon fee imposed on all members of the university community, and δ, which conveys the marginal utility (or disutility) of the shares of revenues spent on things other than just on-campus carbon reduction projects.

[7]Given that it is always difficult to determine which fraction of household income represents disposable income that might be allocated to the object of choice, many researchers find it convenient to specify anticipated indirect utility as additively separable in income, so that the level of income drops out of the model. While utility is unlikely to be linear in income overall, researchers typically rely on a locally linear approximation when annual program costs can be considered to be relatively small compared to annual income.

If we assume that preferences are homogeneous, or that the estimated marginal utility parameters apply to a "representative consumer," it is possible to back out of the estimated preference function an expression for (a) the representative consumer's willingness to pay for a program with specified coefficients, as well as (b) this consumer's marginal willingness to pay for incremental amounts of each attribute. Maximum annual willingness to pay for a given carbon-pricing program is assumed to be that unavoidable yearly cost that would make this representative individual just indifferent between paying that amount and gaining the benefits from that program, or not paying and forgoing those benefits. Specifically, this yearly cost would make the utility-difference in equation (3.2) equal to zero. We can impose this equality and solve for the implied annual cost:

$$0 = \alpha(-C^i_{jt}) + \beta B^i_{jt} + CostShares^i_{jt}\gamma + ExpShares^i_{jt}\delta + \epsilon^i_{jt} \quad (3.3)$$

$$WTP^i_{jt} = C^{*i}_{jt} = (1/\alpha)\left[\beta B^i_{jt} + CostShares^i_{jt}\gamma + ExpShares^i_{jt}\delta + \epsilon^i_{jt}\right] \quad (3.4)$$

At the zero mean of the symmetrically distributed error term, this expression would be simple to calculate. However, it must be remembered that the estimated maximum likelihood parameters are random variables that are asymptotically jointly normally distributed. Given that α is not constrained to be strictly positive, zero is a potential value for this parameter and the analytical expected value is therefore undefined. Many researchers, however, elect to build up a sampling distribution for the value of the implied willingness-to-pay (WTP) function. Using the so-called Krinsky-Robb technique, we make 10,000 draws from the asymptotically joint normal distribution of the maximum likelihood parameters. We combine each independent draw for a set of parameter vectors with the specified levels of the attributes of a given program, namely its percentage-point carbon reduction, B^i_{jt}, along with its non-default shares of costs, $CostShares^i_{jt}$, and its non-default shares of expenditures, $ExpShares^i_{jt}$, to calculate one point estimate of WTP. Over the 10,000 different draws, we build up a sampling distribution for the 10,000 resulting WTP estimates, and report the mean and 5th and 95th percentiles of this distribution to convey a sense of the central tendency for total willingness to pay for such a program, as well as an approximate 90 percent confidence interval for this WTP estimate.

For the marginal willingness to pay for different attributes, for example, a one percentage-point increase in the size of the carbon reduction, our homogeneous-preferences model implies that:

$$\frac{\partial WTP^{*i}_{jt}}{\partial B^i_{jt}} = \frac{\partial C^{*i}_{jt}}{\partial B^i_{jt}} = \frac{\beta}{\alpha} \tag{3.5}$$

Correspondingly, for share k of each of the three possible non-default cost shares and the two possible non-default expenditure shares, the elements of the two vectors of marginal WTP estimates take the form:

$$\begin{aligned} \frac{\partial WTP^{*i}_{jt}}{\partial CostShare^i_{kjt}} &= \frac{\gamma_k}{\alpha} \\ \frac{\partial WTP^{*i}_{jt}}{\partial ExpShare^i_{kjt}} &= \frac{\delta_k}{\alpha} \end{aligned} \tag{3.6}$$

The presence of α in each denominator likewise means that a sampling distribution of estimates for each marginal WTP should likewise be built up using draws from the joint distribution of the estimated parameters, and means and 5th and 95th percentiles reported to convey a sense of the precision with which these quantities are estimated.[8]

Heterogeneous preferences

We can also generalize the model to allow preferences to vary systematically across individuals with different characteristics. We wish to allow our model to be useful for benefit transfer exercises to other universities that differ in the mix of people that make up their populations (provided that the distribution of people's characteristics has roughly the same support). This requires us to estimate models that explain preference heterogeneity as an explicit function of observable individual characteristics. One of the most popular alternatives for modeling heterogeneous preferences, random-parameters mixed logit models) allow heterogeneity to be unobserved. Mixed logit models estimate, instead, a small number of parameters that describe the specific distribution of specific preference parameters (across the population represented in the sample), among a family of distributions assumed by the researcher. When benefit-transfer exercises are anticipated, however, there is no basis upon which to forecast the possibly different locations and scales of these random preference parameters in the "policy population" to which the model is to be transferred. It is preferable to be able to capture observed heterogeneity to the fullest extent possible.

[8] We note that there exists a user-written program in Stata to calculate, by several methods, *marginal* willingness-to-pay point estimates and standard errors associated with a conventional conditional logit specification where the index is linear in variables. However, this Stata program does not seem to be able to calculate interval estimates for total WTP for programs consisting of specified levels of the full set of attributes. Just knowing the marginal WTP estimates for each attribute and their standard errors is insufficient, because non-zero correlations among the various marginal utility parameters are ignored. Total WTP is a linear combination of correlated random parameters, so the covariances among these parameters must be recognized.

Let Z_{it} be a vector of individual characteristics. We can then introduce heterogeneity by interacting the individual characteristics with the program characteristics:

$$U_{jt}^i - U_{0t}^i = -(\alpha' Z_i)C_{jt}^i + (\beta' Z_i)B_{jt}^i + (\gamma' Z_i)DistCosts_{jt}^i + (\delta' Z_i)DistSpend_{jt}^i + \epsilon_{jt}^i \tag{3.7}$$

In this more-general model, the marginal WTP for a one-percent-point reduction in carbon would be:

$$MWTP_{jt}^i = \frac{\hat{\beta}' Z_i}{\hat{\alpha}' Z_i} \tag{3.8}$$

Response/Non-Response correction

It is always a concern, in surveys, that the unexplainable component of response rates (due to unobserved heterogeneity that affects response propensities) may be correlated with with the unexplained component of respondents' WTP, such that systematic sample selection bias may therefore distort the estimates. To correct at least partially for sample selection bias, we estimate a model of propensity to respond and use the de-meaned fitted response propensities as ad hoc controls in our model. Rigorous Heckman-style correction models require that the error term in the selection equation and the error term in the "outcome" model be distributed joint normal. This condition is not satisfied when the outcome equation is a conditional-logit choice model for multiple alternatives.

Through an agreement with the University's Office of Institutional Research, we have access to a wide variety of standard administrative data on all invited respondents to the survey. For students, this dataset includes the zip code for the respondent's high school, which we take as a proxy for the location of the neighborhood in which they came of age (and presumably formed some of their opinions about climate change). We treat this zip code as corresponding to each student's "permanent address." For each university employee, we use the zip code of their current residence, taking advantage of the fact that there are some very different communities within commuting distance of the university, where political ideologies differ systematically.

We convert to zip-code extents a wide variety of data on proportions of the population in different categories. These data are drawn from the American Community Survey (originally at the census tract level), from David Leip's Election Atlas for the 2016 Presidential election (originally at the county level), and from the League of Conservation Voters (originally at the congressional-district level). We use a very large selection of these variables to predict response propensities. This huge number of candidate regressors necessitates the use of variable selection techniques. The reported results use a linear probability model with stepwise selection for variable selection. The remaining variables are then used to estimate a probit model to explain response propensities and to calculate response probabilities. We have also estimated our selection equation using LASSO methods for variable selection. The results appear to be qualitatively similar, so we use the stepwise approach for the selection equation in this paper.[9]

[9]LASSO results are available upon request.

Results

Estimated Sample Selection Model

Table 7 gives descriptive statistics for the set of regressors with estimated coefficients that appear to be robustly statistically significant after our variable-selection process. Given the huge number of candidate variables among all of the zip-code proportions in different groups, and the administrative data about individual members of the campus community, it was not possible to estimate a maximum likelihood probit model for the complete set. Ordinary least squares, applied to a linear probability model (LPM), however, can handle many more regressors. We winnowed the universe of potential regressors using LPM. We then employed the surviving variables in a binary probit specification to yield the fitted response propensities employed subsequently, in de-meaned form, as ad hoc "individual attributes" that are permitted to shift the marginal utility for each program attribute used in our main models.

Table 8 provides the parameter estimates for this model. The choice experiments presented in our survey were complex and participation was completely optional, and our monetary incentives were relatively meager, so our 9.4% response rate is not surprising. Invited respondents are *more* likely to complete our survey if their permanent-address zip code has:

- More housing lacking a complete kitchen
- More people commuting via public transit
- More people employed in retail

TABLE 7.
Descriptive statistics: Response-nonresponse model

	mean	sd
1=respondent; 0=nonrespondent	0.094	
zip pr No vehicles available	0.049	0.034
zip pr Housing lacking complete kitchen	0.006	0.008
zip pr Commute any public transit	0.055	0.030
zip pr Commute 45 to 59 min	0.038	0.036
zip pr Industry retail trade	0.124	0.026
zip pr Industry arts/enter/recr/accom/food	0.100	0.023
1=female	0.534	
1=Black or African American	0.019	
1=Hispanic or Latino	0.089	
1=Nonresident alien	0.090	
1=employee: student employee	0.195	
1=have employee home organization	0.540	
1=organization: Athletics	0.027	
1=organization: Arch and Allied Arts	0.006	
1=organization: Education	0.030	
1=organization: PhysEd and Rec	0.015	
1=organization: Music and Dance	0.011	
1=organization: Housing	0.075	
1=student: Other	0.084	
1=department: environmental studies	0.015	
1=department: journalism and communications	0.074	
1=department: law	0.013	
1=department: music	0.016	
Observations	10520	

Invited respondents are *less* likely to complete the survey if their permanent address zip code has:

- More households with no vehicle available
- More people commuting a long way to work (45 to 59 minutes)
- More people employed in arts, entertainment, recreation, accommodations or food industries

TABLE 8.
Response-nonreponse model estimates; persistently significant explanatory variables (weighted estimates)

Explanatory variables	Estimate	Std. Err.
zip pr No vehicles available	-2.681***	(0.857)
zip pr Housing lacking complete kitchen	12.59***	(3.101)
zip pr Commute any public transit	2.183**	(0.858)
zip pr Commute 45 to 59 min	-2.418***	(0.669)
zip pr Industry retail trade	2.885***	(1.025)
zip pr Industry arts/enter/recr/accom/food	-2.611***	(0.907)
1=female	0.145***	(0.0357)
1=Black or African American	-0.521***	(0.183)
1=Hispanic or Latino	-0.143**	(0.0671)
1=Nonresident alien	-0.359***	(0.0785)
1=employee: student employee	-0.187***	(0.0533)
1=have employee home organization	0.483***	(0.0442)
1=organization: Athletics	-0.481***	(0.125)
1=organization: Arch and Allied Arts	-0.655**	(0.277)
1=organization: Education	-0.405***	(0.107)
1=organization: PhysEd and Rec	-0.295*	(0.157)
1=organization: Music and Dance	-0.473**	(0.187)
1=organization: Housing	-0.241***	(0.0738)
1=student: Other	-0.146**	(0.0647)
1=department: environmental studies	0.402***	(0.123)

Continued on next page

Table 8 – continued from previous page

1=department: journalism and communications	-0.185**	(0.0804)
1=department: law	0.269**	(0.135)
1=department: music	0.294**	(0.141)
Constant	-1.631***	(0.198)
No. Survey Invitations	10520	
Max. log-likelihood	-3113.14	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

At the individual level, based on our person-specific administrative data, invited respondents are *more* likely to complete the survey if they are:

– Female

– A university employee, rather than a student

– A student in Environmental Studies, the Law School, or Music

Individuals are *less* likely to complete the survey if they are:

– Black or African American, or Hispanic or Latino

– A non-resident alien

– A student employee

– A university employee affiliated with Athletics, Architecture and Allied Arts, Education, Physical Education or Recreation, Music and Dance, or Housing

– A student in something other than one of the nine major Schools and Colleges in this university (but not "undeclared")

– A student in Journalism and Communications

Estimated Choice Model

If preferences were homogeneous, our model would have only eight parameters: the coefficient on the size of the carbon reduction, the coefficient on the cost of the program, the five coefficients capturing the distribution of costs/benefits, and a coefficient on a status-quo indicator. The distributional features of the program are captured by the four cost shares borne by different groups, and the three shares describing how the revenue would be used. Relative to the default cost share (for a common carbon fee charged to all members of the university community), there are coefficients on the three other shares of costs. Relative the default expenditure share (spent on carbon projects at the university) there are coefficients on the other two non-default shares of revenue. As is standard practice in the modeling of data from choice experiments, we also include a "status quo" indicator for all choice occasions.

It is, however, a maintained hypothesis that we need to allow for non-zero "nuisance" parameters associated with each person's deviation from the invited sample's mean response propensity. Thus we always interact each program attribute with this demeaned response propensity. When we simulate zero for all demeaned response propensities, then, we are implicitly simulating a situation where everybody in our estimating sample shares the same response propensity and this propensity is the mean in the entire invited sample, which represents the population of interest (up to some sampling weights that control for a higher proportion of employees than students in the spring sampling wave).

Given that the marginal utility of every feature in our offered ICP programs may vary systematically with respondent characteristics, the same characteristic may persist as being influential in more than one interaction with the program attributes. Thus Table 3 displays descriptive statistics for each of the dimensions of respondent heterogeneity that have a persistently significant effect and survive our winnowing process. These summary statistics are also displayed next to each parameter estimate in the next table, Table 9 displaying the choice model, to emphasize the importance of individual heterogeneity for the total WTP for a given program.

Table 10, which spans two pages, reports our preferred specification for respondents' preferences among the wide range of randomly designed ICP programs proposed across the different (essentially unique) survey instruments used in our study. The first thing to note is that the fitted demeaned response propensities, from the model in Table 2, have a persistently statistically significant effect on the marginal utility of the unaviodable cost to respondents, the cost share borne by taxpayers, the share spent on academic and on the status quo indicator variable. (In cases where interaction variables have persistently statistically insignificant coefficients, we drop those interactions.)

The parameter estimates in Table 10 are an intermediate step on our way to exploring the model's implications for total and marginal WTP amounts for different programs and for different people. Thus we will not reiterate each of those parameter estimates. Instead, we note that the table is structured so that each of the eight basic program attributes is followed by that attribute's interactions with selected respondent characteristics (either for their permanent address zip code, or individually from administrative data). Any interaction term bearing a positive coefficient suggests that the marginal utility from the attribute in question increases when that zip-code proportion is larger, or when the individual indicator is "switched on."

TABLE 9.
Descriptive statistics: Heterogeneity in choice model

	mean	sd
Permanent-address zip code proportions:		
zip pr Asian alone (.027)	0.027	0.031
zip pr Black alone (.009)	0.009	0.016
zip pr Cmt 15-19 min (.199)	0.199	0.028
zip pr Cmt 30-34 min (.092)	0.092	0.032
zip pr 25+, some coll, no degr (.285)	0.285	0.031
zip pr Heat fuel oil, kero (.009)	0.009	0.024
zip pr Inc 100K-150K (.11)	0.110	0.039
zip pr Inc 15K-25K (.141)	0.141	0.033
zip pr Inc 150K-200K (.037)	0.037	0.019
zip pr Inc lt 10K (.072)	0.072	0.020
zip pr Moved; from abroad (.004)	0.004	0.004
zip pr Moved; dif cty, sme st (.041)	0.041	0.015
zip pr Moved; same cty (.094)	0.094	0.028
zip pr No vehicle avail (.046)	0.046	0.026
zip pr Hous-multi-unit (.107)	0.107	0.098
zip pr Ind constr (.06)	0.060	0.010
zip pr Ind publ admin (.041)	0.041	0.011
zip pr Ind ret trade (.127)	0.127	0.023
zip pr Hsng incompl kitch (.006)	0.006	0.008

Continued on next page

Table 9 – continued from previous page

zip pr Hsng incompl plumb (.002)	0.002	0.003
zip pr No phone service (.023)	0.023	0.006
zip pr Green votes 2016 Pres elect.	0.031	0.005
Administrative data–individual characteristics		
1=individual's age known	0.751	0.433
demean indiv. age, if known	0.127	10.416
1=have gender	0.997	0.055
1=female (.601)	0.601	0.490
1=Non-white (.38)	0.380	0.486
1=stu: Design (.066)	0.066	0.249
1=stu: Other (.083)	0.083	0.276
1=stu: Undeclared (.044)	0.044	0.205
1=dept: Biology (.033)	0.033	0.179
1=dept: Bus admin (.063)	0.063	0.243
1=dept: Env studies (.025)	0.025	0.156
1=empl: Officer of admin (.143)	0.143	0.351
1=empl: Career non-tenure (.058)	0.058	0.234
1=empl: Athletics (.023)	0.023	0.126
1=empl: Business (.027)	0.027	0.137
1=empl: Facilities (.03)	0.03	0.144
1=empl: Design (.044)	0.044	0.174
1=empl: Classified staff (.144)	0.144	0.352

Continued on next page

Table 9 – continued from previous page

1=empl: Library (.041)	0.041	0.168
1=empl: Student employee (.186)	0.186	0.389
Survey Data - Individual Characteristics		
=1 if have hhld inc	0.876	0.330
hhld inc ('000) if known	47.025	60.268
1=perceive anti-ICP bias (.032)	0.032	0.176
1=perceive pro-ICP bias (.445)	0.445	0.497
1=somew/very conserv (.087)	0.087	0.282
1=somew/very liberal (.673)	0.673	0.469
1=12 mos: Severe winter (.138)	0.138	0.346
1=12 mos: Heat wave (.422)	0.422	0.494
1=extr weath: any harm (.615)	0.615	0.487
1=fall 2018 wave (.41)	0.410	0.492
Observations	997	

For every program for which we will calculate WTP, the status-quo indicator will be set to zero. Given this, we will not discuss this parameter in the WTP simulations below, but will comment here on those respondent characteristics that affect their preference for the status quo, rather than any ICP program, regardless of the attributes of that program.

TABLE 10.
Final specification, displayed in wide format (Omitted categories: those not included in the specification, by factor)

	Estimate	Std.Err.
Unavoid cost to resp. ($22 to $232)	-0.00946***	(0.00112)
× demeaned resp propensity	0.00122**	(0.000511)
Pct-point C reduction (10 to 50)	-0.0591*	(0.0350)
× Pct-point C reduction (10 to 50)	-0.000419**	(0.000178)
× zip pr Asian alone (.027)	0.176*	(0.0983)
× zip pr Moved; dif cty, sme st (.041)	-0.478***	(0.146)
× zip pr 25+, some coll, no degr (.285)	0.185*	(0.107)
× zip pr No vehicle avail (.046)	-0.167*	(0.0962)
× zip pr Hsng incompl plumb (.002)	-1.536**	(0.744)
× zip pr No phone service (.023)	0.458	(0.331)
× zip pr Green votes 2016 Pres elect.	-0.794*	(0.476)
× 1=have gender	0.0986***	(0.0185)
× 1=female (.601)	-0.00703*	(0.00392)
× 1=empl: Business (.027)	0.0247*	(0.0129)
× 1=empl: Library (.041)	-0.0306**	(0.0132)
× 1=12 mos: Severe winter (.138)	-0.0155***	(0.00566)
× 1=perceive pro-ICP bias (.445)	-0.0100**	(0.00450)
× =1 if have hhld inc (.889)	0.00490	(0.00595)

Continued on next page

Table 10 – continued from previous page

× hhld inc ('000) if known	0.0000531	(0.0000341)
Cost shr air trav fees (0 to .5)	-0.0647**	(0.0301)
× Cost shr air trav fees (0 to .5)	-0.000423***	(0.000102)
× zip pr Inc lt 10K (.072)	0.277***	(0.0993)
× zip pr Inc 15K-25K (.141)	-0.178**	(0.0739)
× zip pr Hsng incompl plumb (.002)	1.235**	(0.611)
× zip pr Hsng incompl kitch (.006)	-0.439	(0.269)
× zip pr Cmt 15-19 min (.199)	0.272***	(0.0781)
× 1=have gender	0.0491*	(0.0263)
× 1=empl: Athletics (.023)	-0.0173	(0.0111)
× 1=empl: Business (.027)	-0.0102	(0.00740)
× 1=empl: Facilities (.03)	-0.0267*	(0.0144)
× 1=12 mos: Severe winter (.138)	0.00807**	(0.00384)
Cost shr bldg en fees (0 to 1)	0.0148***	(0.00365)
× zip pr Cmt 30-34 min (.092)	-0.0929***	(0.0320)
× 1=empl: Athletics (.023)	-0.0124	(0.00830)
× 1=empl: Design (.044)	-0.0130***	(0.00390)
× 1=somew/very liberal (.673)	0.00614***	(0.00237)
Cost shr taxpayrs (0 to .2)	0.0322	(0.0228)
× zip pr Inc 100K-150K (.11)	-0.576***	(0.201)

Continued on next page

× zip pr Inc 150K-200K (.037)	1.115***	(0.388)
× zip pr Hsng incompl plumb (.002)	2.469**	(0.979)
× zip pr Heat fuel oil, kero (.009)	-0.282*	(0.145)
× zip pr Ind constr (.06)	-0.613*	(0.328)
× zip pr Ind publ admin (.041)	0.812***	(0.282)
× 1=empl: Athletics (.023)	-0.0457*	(0.0266)
× 1=empl: Design (.044)	-0.0324*	(0.0179)
× 1=empl: UGS (.02)	0.0540***	(0.0198)
× 1=stu: Design (.066)	0.0239*	(0.0143)
× 1=stu: Other (.083)	0.0374***	(0.0129)
× 1=dept: Bus admin (.063)	-0.0377***	(0.0117)
× 1=dept: Env studies (.025)	0.0277*	(0.0163)
× demeaned resp propensity	-0.00384**	(0.00182)
Spend shr acad prog (0 to .3)	-0.0766***	(0.0263)
× zip pr Black alone (.009)	0.562***	(0.146)
× zip pr 25+, some coll, no degr (.	0.303***	(0.0818)
× zip pr Ind publ admin (.041)	-0.604***	(0.217)
× 1=Non-white (.38)	-0.0104*	(0.00554)
× 1=individual's age known (1)	0.00399	(0.00636)
× demean indiv. age, if known	-0.000547*	(0.000303)
× 1=empl: Career non-tenure (.058)	-0.0164	(0.0104)
× 1=empl: Athletics (.023)	0.0456**	(0.0186)

Continued on next page

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× 1=empl: Health, Counsel. (.021)	-0.0671***	(0.0181)
× 1=extr weath: any harm (.615)	0.0123***	(0.00471)
× 1=perceive anti-ICP bias (.032)	-0.0397***	(0.0149)
× demeaned resp propensity	0.00662**	(0.00272)
Spend shr offsets (0 to .5)	0.0123	(0.0172)
× zip pr Hous-multi-unit (.107)	-0.133***	(0.0395)
× zip pr No vehicle avail (.046)	0.332**	(0.133)
× zip pr Hsng incompl kitch (.006)	-0.596	(0.374)
× zip pr Heat fuel oil, kero (.009)	0.156*	(0.0908)
× zip pr Ind ret trade (.127)	-0.125	(0.114)
× 1=individual's age known (1)	0.00934*	(0.00522)
× 1=empl: Career non-tenure (.058)	-0.0263***	(0.00981)
× 1=empl: Health, Counsel. (.021)	0.0194	(0.0126)
× 1=dept: Bus admin (.063)	0.0122*	(0.00677)
× 1=fall 2018 wave (.41)	-0.00990***	(0.00383)
Status quo w/ no prog	-5.334***	(1.256)
× zip pr Moved; same cty (.094)	9.709***	(3.157)
× zip pr Moved; from abroad (.004)	-72.56***	(21.73)
× zip pr Inc 15K-25K (.141)	6.849**	(3.097)
× 1=have gender	4.466***	(1.099)
× 1=empl: Classified staff (.144)	0.851***	(0.239)

Continued on next page

Table 10 – continued from previous page

× 1=empl: Officer of admin (.143)	0.580**	(0.232)
× 1=empl: Student employee (.186)	-0.534***	(0.190)
× 1=empl: Design (.044)	-1.287***	(0.449)
× 1=stu: Undeclared (.044)	-0.373	(0.302)
× 1=dept: Biology (.033)	-0.555*	(0.325)
× 1=extr weath: any harm (.615)	0.283*	(0.155)
× 1=fall 2018 wave (.41)	-0.272*	(0.155)
× 1=12 mos: Heat wave (.422)	-0.387***	(0.143)
× 1=perceive pro-ICP bias (.445)	0.434**	(0.177)
× 1=somew/very liberal (.673)	-0.607***	(0.192)
× 1=somew/very conserv (.087)	0.768**	(0.302)
× demeaned resp propensity	0.125*	(0.0657)
No. alternatives	12466	
Max. log-likelihood	-6047.73	
Clustering	caseid	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Specifically, a respondent is more likely to prefer NO program if:

- They are an administrator or staff member, instead of a student.
- They identify as somewhat or very conservative
- Their permanent address is in a low-income zip-code
- They have recently experienced any harm from an extreme weather event

In contrast, a respondent is more likely to prefer ANY program, regardless of its characteristics, if:

- They are affiliated with the college of design, the biology department or have not yet declared their major.
- They have recently experienced a heatwave
- They identify as somewhat or very liberal
- They are from a zip-code with a high share of people who have recently moved from a foreign country.

WTP Simulations for Specific Programs and Specific Individuals

We now discuss some specific willingness-to-pay results from the estimated choice model. The fitted WTP function depends both the attributes of the ICP program in question, and on numerous respondent characteristics (that may enter the model in more than one place). To illustrate how WTP (willingness to bear the costs of) ICP programs depends on specific program attributes or specific respondent characteristics, we will explore several notable cases.

Table 11 summarizes these illustrations of the scope of the influence of program attributes and respondent characteristics. As we discuss Table 11 we will consider one panel at a time.

TABLE 11.
Heterogeneity in WTP by program attributes and respondent characteristics

1. By percentage-point carbon reduction

(40% carbon reduction, student/employee fees only, spend revenues on carbon projects only)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

10	53.54***	4.96***
	(35.69, 73.26)	(3.47, 6.57)
15	77.33***	4.56***
	(52.82, 104.19)	(3.35, 5.85)
20	99.14***	4.16***
	(69.35, 131.46)	(3.21, 5.17)
25	118.96***	3.77***
	(85.39, 155.45)	(3.02, 4.56)
30	136.80***	3.37***
	(100.61, 175.45)	(2.72, 4.06)
35	152.65***	2.97***
	(115.08, 192.61)	(2.21, 3.75)
40 (benchmark case)	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
45	178.41***	2.18**
	(140.84, 218.48)	(0.91, 3.42)
50	188.32***	1.78*
	(150.93, 227.81)	(0.2, 3.32)

2. By proportion of costs borne as air travel fees (vs. student/employee fees)

(40% carbon reduction, spend revenues on carbon projects only)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

0 share	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
0.10 share	195.96***	"
	(155.50, 239.90)	
0.20 share	216.36***	"
	(173.49, 263.20)	
0.30 share	227.70***	"
	(183.60, 276.29)	
0.40 share	229.99***	"
	(185.75, 279.17)	

Continued on next page

Table 11 – continued from previous page

0.50 share (out-of-sample)	223.24***	"
	(179.14, 271.95)	
0.60 share (out-of-sample)	207.44***	"
	(161.84, 258.20)	

3. By proportion of costs borne as building energy fees (vs. student/employee fees)

(40% carbon reduction, spend revenues on carbon projects only)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

0 share	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
0.20 share	198.43***	"
	(157.63, 242.51)	
0.30 share	212.62***	"
	(170.00, 258.94)	
0.40 share	225.64***	"
	(181.52, 274.33)	
0.60 share	248.15***	"
	(201.54, 299.97)	
0.80 share	265.97***	"
	(216.11, 321.47)	
1.00 share	279.08***	"
	(224.89, 340.26)	

4. By proportion of costs borne by state's taxpayers (vs. student/employee fees)

(40% carbon reduction, spend revenues on carbon projects only)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

0 share	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
0.10 share	169.03***	"
	(129.90, 210.49)	
0.20 share	171.53***	"
	(130.50, 215.07)	
0.30 share (out-of-sample)	174.03***	"
	(129.85, 221.67)	

5. By proportion of revenues spent on academic programs (vs. carbon-reduction programs)

(40% carbon reduction, costs borne as student/employee fees only)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Continued on next page

Table 11 – continued from previous page

0 share	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
0.10 share	161.41***	"
	(122.62, 202.21)	
0.20 share	156.30***	"
	(115.06, 199.31)	
0.30 share (second benchmark)	151.18***	"
	(106.51, 197.54)	
0.40 share (out-of-sample)	146.07***	"
	(97.34, 196.43)	

6. By proportion of revenues spent on carbon offsets (vs. carbon-reduction programs)

(40% carbon reduction, costs borne as student/employee fees only)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

0 share	166.53***	2.58***
	(128.58, 206.73)	(1.59, 3.55)
0.10 share	182.39***	"
	(139.41, 228.59)	
0.20 share	198.26***	"
	(140.58, 260.52)	
0.30 share (second benchmark)	214.12***	"
	(137.12, 296.91)	
0.40 share	229.99***	"
	(132.02, 334.91)	
0.50 share	245.86***	"
	(126.07, 373.79)	
0.60 share (out-of-sample)	261.72***	"
	(119.37, 413.41)	

7. By carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30

(40% carbon reduction, student/employee fees only, spend revenues on carbon projects only)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

10	55.44***	4.96***
	(37.47, 75.16)	(3.47, 6.57)
15	79.23***	4.56***
	(54.62, 106.18)	(3.35, 5.85)
20	101.04***	4.16***
	(71.26, 133.39)	(3.21, 5.17)

Continued on next page

Table 11 – continued from previous page

25	120.86***	3.77***
	(87.16, 157.45)	(3.02, 4.56)
30	138.70***	3.37***
	(102.56, 177.62)	(2.72, 4.06)
35	154.55***	2.97***
	(116.94, 194.59)	(2.21, 3.75)
40 (initial benchmark case)	168.43***	2.58***
	(130.33, 208.83)	(1.59, 3.55)
45	180.31***	2.18**
	(142.67, 220.53)	(0.91, 3.42)
50	190.22***	1.78*
	(152.93, 229.93)	(0.2, 3.32)

8. By deviations from sample mean household income
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

-64.818 (min = 15)	144.59***	1.98**
	(100.83, 190.55)	(.81, 3.13)
-57.318 (20th %ile = 22.5)	147.35***	2.05***
	(104.57, 192.60)	(0.91, 3.17)
-17.31 (40th %ile = 62.5)	162.06***	2.42***
	(123.04, 203.58)	(1.39, 3.43)
0 (at mean = 79.8)	168.43***	2.58***
	(130.33, 208.83)	(1.59, 3.55)
7.68 (60th %ile = 87.5)	171.25***	2.65***
	(133.52, 211.31)	(1.68, 3.61)
32.68 (80th %ile = 112.5)	180.44***	2.88***
	(143.42, 220.14)	(1.95, 3.81)
145.18 (max = 225)	221.82***	3.91***
	(178.36, 270.75)	(2.85, 4.99)

9. By deviations from sample mean age
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

-3.33 (min = 18)	168.48***	2.58***
	(130.38, 208.88)	(1.59, 3.55)
-1.33 (20th %ile = 20)	168.43***	"
	(130.34, 208.84)	
-.331 (40th %ile = 21)	168.41***	"
	(130.32, 208.82)	
0 (mean = 21.33)	168.41***	"
	(130.32, 208.81)	

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3.668 (60th %ile = 25)	168.33***	"
	(130.27, 208.75)	
15.6685 (80th %ile = 37)	168.07***	"
	(130.06, 208.52)	
51.6685 (max = 73)	167.29***	"
	(129.31, 207.56)	

10. By perceived bias of researchers
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

No perceived bias	168.43***	2.58***
	(130.33, 208.83)	(1.59, 3.55)
Perceived pro-ICP bias	128.71***	1.58**
	(88.88, 169.37)	(0.48, 2.62)
Perceived anti-ICP bias	70.60	0.13
	(-24.26, 163.78)	(-2.44, 2.51)

11. By respondent's experience with extreme weather in last 12 months
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

No extreme weather events	168.43***	2.58***
	(130.33, 208.83)	(1.59, 3.55)
Drought	111.11***	1.14
	(48.50, 173.20)	(-.57, 2.77)
Extreme cold	121.85***	1.40
	(72.88, 171.83)	(-0.05, 2.75)

12. Any prior experience with extreme-weather harm?
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

No prior harm	168.43***	2.58***
	(130.33, 208.83)	(1.59, 3.55)
Some prior harm	168.88***	"
	(130.72, 209.30)	

t footnote1
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Panel 1: Percentage-point carbon reduction

In this illustrative case, the program involves the default distributional shares—student/employee fees only, and revenues spent on campus carbon projects only. Respondent characteristics are set at the sample means and baseline categories for indicator variables. This panel illustrates that WTP responds to the scope of the program. Furthermore, the marginal WTP for an additional percentage-point carbon reduction declines over the range of the illustration, from about \$5 per year at a 10% reduction, down to just \$1.78 at a 50% reduction. There is diminishing marginal WTP as the program becomes more aggressive, although MWTP does not become negative within the range of our choice set designs.

A natural question one may ask with this model is what is the implied WTP for a ton of carbon dioxide reduction? Since WTP varies by individual respondent characteristics, it is not accurate to speak of a single number which can be applied to every member of the university population. However, for illustrative purposes, suppose every member of the campus community was identical to the person described in Section 1 of Table 11. Consider the benchmark 40% carbon reduction, which corresponds to the university changing their physical plant from natural gas to green energy. The mean WTP for this type of person, \$167, can be multiplied by the roughly 28,100 people at the university and divided by the 24,000 metric tons (from the original 61,000 metric ton estimate of university emissions that was described to respondents). In this example, this would lead us with an average WTP, per ton of carbon dioxide reduced, of \$195. Importantly, however, this person is not the mean member of the university's population of stakeholders, but merely an illustration.

Panel 2: Baseline ICP Program; Proportion of the costs borne as air travel fees

Each of these next simulations corresponds to a 40% reduction in campus carbon emissions. Now we divert the costs increasingly away from the default category of student/employee fees, and increase the share of costs borne as air travel fees. This is still the same "person" with the same basic characteristics as panel 1. Total WTP for an ICP program increases with the share of the costs borne as air travel fees, but only up to the maximum 40% share used in the experimental design. Total WTP is diminishing in this feature over the range of programs used in this study, and the statistically significant coefficient on the quadratic term in the proportion of costs borne as air travel fees means that total WTP, for programs above the range included in our choice-sets, would be predicted to decrease as a result of additional increases in air-travel fees. However, this is merely an out-of-sample forecast. Within the range of our data, all we can say is that there is diminishing marginal utility from an increasing share via air-travel fees.

Panel 3: Baseline ICP Program; Proportion of costs borne as building energy fees

This is the dimension most similar to the program at Yale University. Ceteris paribus, our benchmark person is willing to bear higher personal costs for an ICP program if that program results in a greater share of costs borne via building energy fees. Our choice set designs included up to a 100% share of costs borne this way, to allow us to benchmark WTP in this campus population against the Yale example. For this arbitrary person, total WTP ranges from $167 per year for the default share scenario, up to $279 per year if all costs are borne via building energy fees. This is consistent with a preference for a "polluter pays" approach.

Panel 4: Baseline ICP Program; Proportion of costs borne by the state's taxpayers

We find additional evidence of distributional preferences in the estimates relating to the proportion of costs paid by taxpayers in the fitted WTP values shown in this panel. As the amount of funds provided by the state's taxpayers increases, the WTP increases from $167 to $174 when taxpayer contributions are raised from the program baseline to cover 30 percent of the program's costs. One may be concerned that the decreasing WTP reflects a false perception of respondents that programs with a higher share of taxpayer support may reflect reductions in the cost of the program to respondents. We find this explanation unlikely due to the small size of the change in WTP in response to increases in taxpayer support. An agent who thought they could avoid paying in programs with higher levels of taxpayer support should be willing to pay an amount for this taxpayer support equal to the savings. However each 10 percent step in Table 5 results in an increase in WTP far less than 10 percent suggesting that respondents incorrect perceptions of program costs are unlikely to be the only reason behind the sign of the estimates.

Panel 5: Baseline ICP Program; Proportion of revenues spent on academic programs

The estimates in this panel show how our benchmark individual responds to a portion of the revenue raised by the ICP being spent on academic programs, which we view as a form of revenue recycling. When spending on academic programs increases to 30 percent, from the program where all revenues are spent on on carbon reduction projects, the WTP falls to $151. This suggests that, all else equal, respondents would prefer the revenues to be spent on some form of carbon reduction.

Panel 6: Baseline ICP Program; Proportion of revenues spent on carbon offsets

Estimates in this panel show the tradeoff our benchmark individual is willing to make between off-campus carbon-reduction projects (in the form of offsets) and on-campus carbon reduction projects. WTP is increasing in the proportion of the revenue spent on offsets. Total WTP increases to $245 when half of the money is spent on offsets, from $167 when all of the money is spent on on-campus carbon projects. We find no evidence that respondents have a systematic preference for local reductions (i.e. on campus projects) instead of distant projects (i.e. offsets).[10]

Panel 7: ICP Program with mixed shares: Percentage-point carbon reductions

For our baseline program, with all costs borne as a flat fee on all students and employees, and with all revenues spent on carbon-reduction programs at the university, the only types of heterogeneity that can affect willingness to pay are those respondent characteristics that shift the marginal utility of a percentage-point carbon reductions. As is common in choice experiments, we constrain the marginal utility of net income to be constant across respondents. It is therefore necessary to select programs that have non-zero values for the non-default shares if we are to explore the sensitively of WTP for an ICP program to other respondent characteristics that shift only the marginal utilities of some of these non-default shares. For illustration, then, we now examine a program with 20% of costs borne as air travel fees, 30% of costs borne as building energy fees, and 20% of costs borne by the state's taxpayers (leaving 20% to be borne in the form of a flat fee on all students and employees). Analogously, this new benchmark ICP program

[10]Our survey did not mention co-pollutants. If the survey had mentioned co-pollutants we might have seen a preference for local emission reductions. These results should therefore be interpreted as holding local air quality constant.

will feature 30% of its revenues spent on academic programs, and 30% of its revenues spent on off-campus carbon offsets. This leaves just 40% being spent for on-campus carbon projects.

For this program, increases in the amount of carbon reductions can be compared directly to Panel 1. Total WTP for a 10% carbon reduction changes minimally, from $53.54 to $55.44, and Total WTP for a 50% carbon reduction goes from $188 to $190. The positive and negative effects of shifting the different shares more or less cancel out. The action, in this case, is going to stem from different respondent characteristics.

Panel 8: ICP Program with mixed shares: Respondent household income

Income is measured coarsely in our survey, and is treated as no more than an indicator for preferences that vary with socioeconomic status, rather than as an accurate measure of disposable income. In the estimated model, income enters as a shifter of marginal utilities in the form of deviations from the sample mean, so that a zero value for the interaction implies a model that applies for someone with mean household income. The estimated model suggests that ICP programs are a normal good. Willingness to pay increases with household income.

Panel 9: ICP Program with mixed shares: Respondent age

Given that we control for so many other respondent characteristics, any heterogeneity attributed solely to age has essentially disappeared.

Panel 10: ICP Program with mixed shares: Perceived researcher bias

It is difficult to field a survey about a topic and to succeed in leaving respondents with the perception that the research team is indifferent about the results. In the process of survey design, the goal is to have as many people as possible respond that the survey seemed "unbiased," but it is fairly typical to have an imbalance in the tails. For this survey, people who garnered the impression that the research team had a pro-ICP bias were willing to pay about $40 less per year for this benchmark ICP program than people who perceived that the survey was unbiased. Relatively few respondents felt that the survey was biased against ICP programs.

There is some question about the exogeneity of this variable, of course. People who take a dim view of the necessity for dealing with climate change might unsurprisingly be more inclined to feel that *any* survey that discusses climate change programs is biased in favor of these programs. We leave the observed values of this respondent characteristic this attribute in our specifications as we simulate predicted WTP amounts, therefore, rather than simulating (counterfactually) that everyone perceives no bias.

<u>Panel 11: ICP Program with mixed shares: Extreme weather in last 12 months?</u>

The salience of climate change mitigation policies can be expected to depend on recent experience with extreme weather events on the part of the respondent, since such events are increasingly attributed to climate change. This panel shows that respondents who have experienced extreme cold in the last year seem to be less willing to pay for carbon reductions through a campus ICP program. Less intuitive, however is the finding that respondents who have experience drought in the last year are also less willing to pay for carbon reductions. One possibility is that drought may be correlated with time spent in rural agricultural areas over the last year. We may be picking up "time spent in red counties" via the drought indicator, but this apparent anomaly may warrant further study.

Panel 12: ICP Program with mixed shares: Prior experience with extreme-weather harm

This respondent characteristic was intended to capture the individual's lifetime exposure to extreme-weather events, as well as the exposure of family and friends. However, it seems that extreme-weather harm over a longer time-span, or to other people known to the respondent, has no significant effect on WTP for this particular program.

Campuswide Distributions of WTP for Specific Programs

Our model can be used to compute the distribution of WTP across the entire campus community. We generate and display the entire distribution, as opposed to limiting our analysis to simple summary statistics such as the mean or median, and perhaps the variance in WTP. Knowledge of the entire distribution for a specifically configured ICP program, across the entire campus community, can be very useful to policymakers as they assess the level of support for that type of program. The ability to consider the entire distribution of fitted WTP amounts allows the policy-maker to understand, for a program with a given per-person cost, what fraction of the campus community is predicted to be accepting of such a program. Those individuals whose WTP exceeds the cost of the program would be predicted to vote "yes" for such a program, if it were put to a vote. Analyses that yield only the WTP of the "median voter" on campus may have difficulty separating programs that only achieve a narrow majority of support from those which are uncontroversial. Additionally, the WTP distribution can be used to assess equity concerns about the distribution of benefits from the program.

A key point to appreciate is that if we were to model preferences as homogeneous, there would be just one estimate of WTP for any given program, based solely on the attributes of that program. The *distribution* of WTP amounts for a given program stems entirely from all the heterogeneity, across the campus community, in respondent characteristics.

Campuswide distribution of WTP for our baseline ICP program

Figure 10 shows the distribution of fitted WTP values across the sample of all respondents, weighted to reflect, as well as possible, the demographic distribution of the university. This WTP corresponds to our baseline program that raises all revenue from lump-sum fees and spends it all for on-campus carbon-reduction projects that achieve a 40% reduction in university emissions. The distribution of total WTP amounts (which can be interpreted as "support" for the specified program) is unimodal with a long right tail.

For comparison, Figure 11 shows the same baseline program along with a program with identical revenue and spending shares but instead achieves half the emission reductions. The program with the smaller carbon reduction has a more compact distribution suggesting that preference heterogeneity becomes more important, the larger the scope of the ICP program.

Figure 12 shows distributions of campus-wide WTP amounts for programs that vary from the baseline ICP program by changing either the non-default cost shares or the non-default revenue shares, one at a time. In general, the shape of the distribution does not noticeably change due to differences in program design.

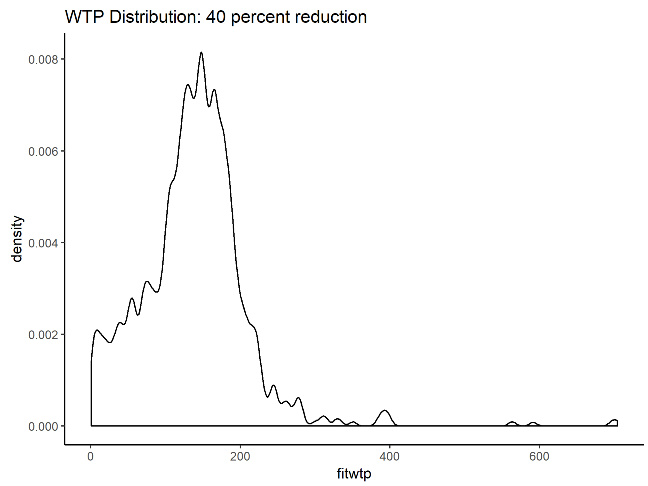


FIGURE 10.
Distribution (weighted) across the university population of expected individual WTP amounts for a program that produces a 40% reduction in carbon emissions, where the costs are borne entirely as a flat fee on all students and employees, and where all of the revenues are spent on carbon-reduction projects

Instead changes in WTP due to changes in program attributes seems to be best described as a change in the mean of the WTP distribution.

Figure 13 shows the distribution of WTP for our second program that includes all sources of revenue and all shares of spending. This distribution of WTP tends to be more diffuse than the distribution fo the baseline program. This can be expected, given that different segments of the university population have different views about the means of raising and using revenues across the different programs.

Campuswide Distributions of WTP Within Distinct Stakeholder Groups

For any given program, instead of just showing the entire marginal distribution of WTP amounts across the whole university community, we can split our sample into specific groups of interest. Figure 14 shows the distribution of

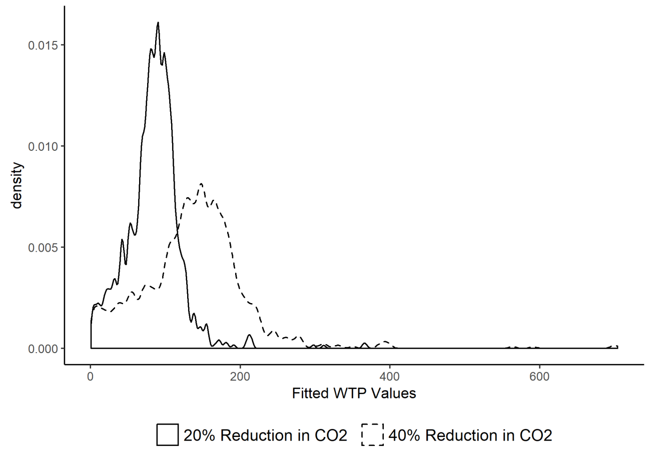
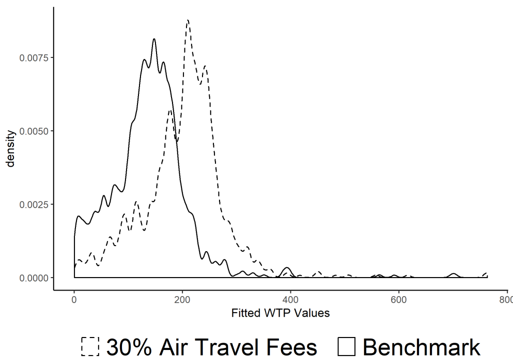


FIGURE 11.
Distribution (weighted) across the university population of expected individual WTP amounts for programs that produce a 40% and a 20% reduction in CO_2. Both programs raise all of their money from lump-sum student/staff fees and spend all revenue on on-campus carbon reduction projects.

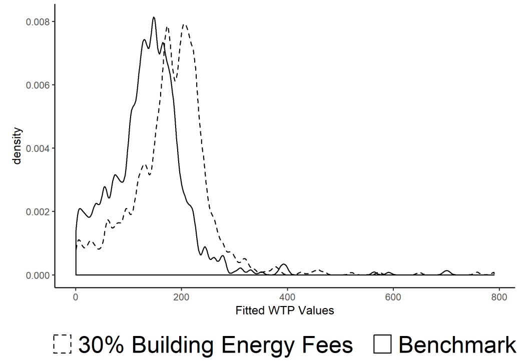
WTP sorted according to respondents' self-reported political ideology. Figures 15 and 16 show the distribution sorted by self-reported income and by home zip-code income levels, respectively. Finally, Figure 17 shows the WTP distribution for students and non-students, and Figure 18 shows WTP distributions for two of the better -represented academic departments in our sample, business and environmental studies. The examples we use to illustrate the different distributions of total WTP for a given program across different groups have been arbitrarily selected, as they are intended to serve merely as an illustration of the results our model is capable of generating.

Conclusions and Directions for Further Research

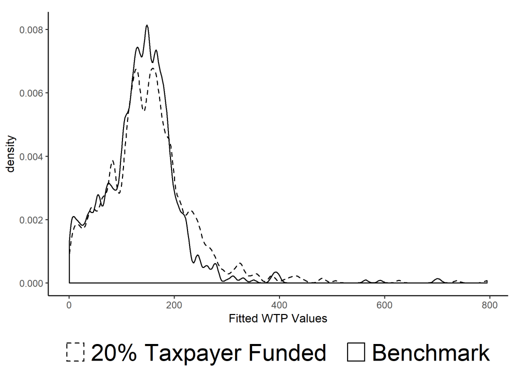
In this paper, we describe the findings from a stated-preference survey, in the form of an advisory referendum, designed to explore preferences for internal



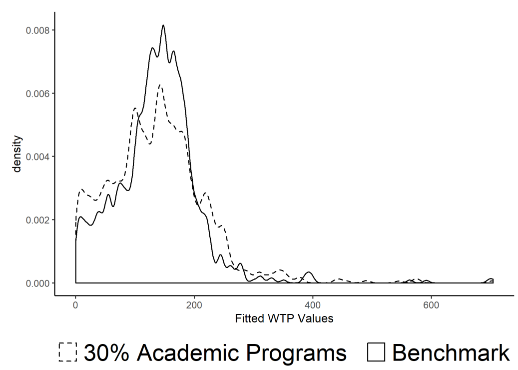
(a) 30% Air Travel Fees



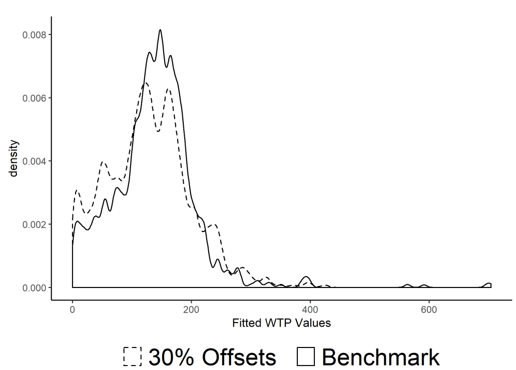
(b) 30% Building Energy Fees



(c) 20% Taxpayer Funding



(d) 30% to Academic Spending



(e) 30% Offsets

FIGURE 12.
Distribution across the university population of expected individual WTP for programs with various spending and revenue shares.

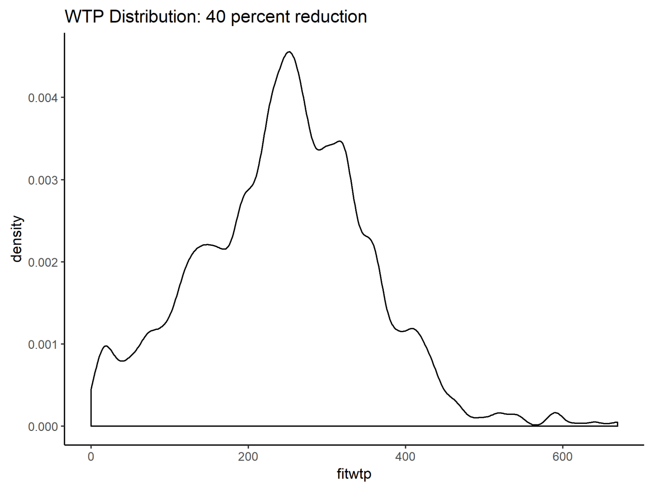


FIGURE 13.
Distribution (weighted) across the university population of expected individual WTP amounts for a program that produces a 40% reduction in carbon emissions, where the costs are borne: 20% as a flat fee on everyone, 30% as air travel fees, 30% as building energy fees, and 20% by Oregon taxpayers, and revenues are spent 40% on carbon reduction projects, 30% on academic programs, and 30% on carbon offsets.

carbon pricing programs at a university. Our estimates suggest that there exists substantial support for local climate action, with predicted individual WTP amounts exceeding or matching many estimates of the social cost of carbon. This suggests that support for non-governmental programs is substantial enough to justify significant emission reductions, at least for institutions whose stakeholder preferences can be adaquately captured by the same explanatory variables we use in this study.

We also find substantial evidence that program *design* influences demand for internal carbon pricing. Respondents have preferences over the initial incidence of the program's costs. They prefer programs where costs are linked to emissions. Even for programs with the same cost to the individual, we find that support is higher when taxpayers across the state are perceived as sharing the burden

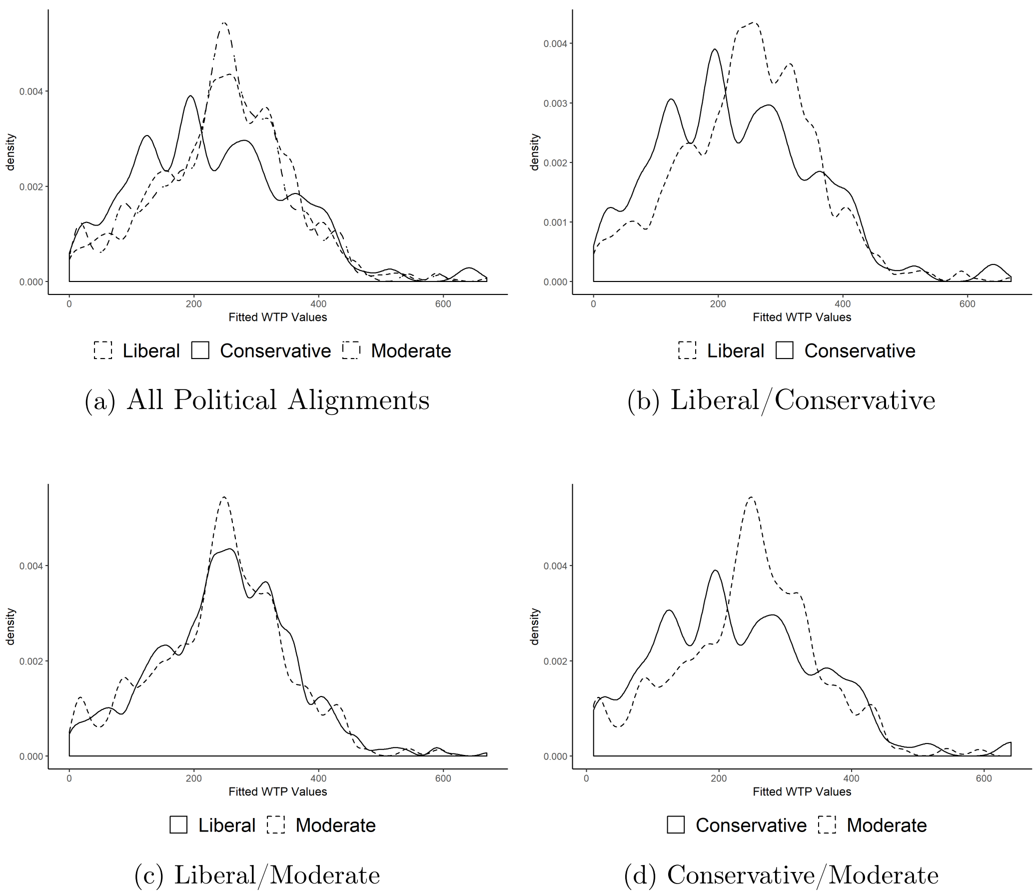


FIGURE 14.
Distribution (weighted) across the university population of expected individual WTP amounts for self-identified conservative, moderates, and liberals.

of the carbon reduction program. Additionally we find that revenue recycling lowers support for these programs. There is no evidence of a preference for carbon reduction projects to be local, instead of being achieved through offsets.

Our survey and choice model are designed to facilitate benefit transfer exercises across universities. The next chapter will use a variation on our estimated model to construct WTP estimates for another (stylized) university from publicly available data. These estimates, and the method more generally, would give

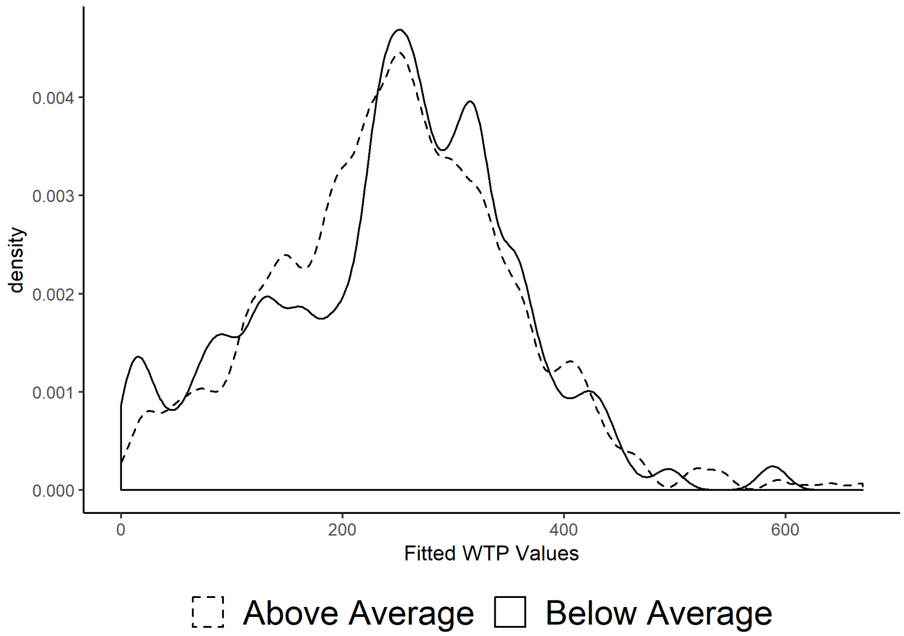
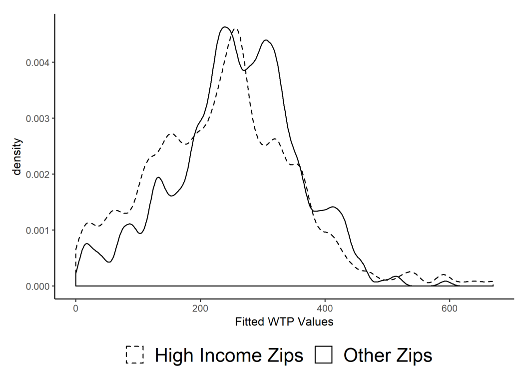


FIGURE 15.
Distribution (weighted) across the university population of expected individual WTP amounts shown separately for respondents with above average and below average self-reported income.

(a) Low Income Zip-Codes



(b) High Income Zip-Codes

FIGURE 16.
WTP Distributions by Origin Zipcode Household Income

Notes: Distribution (weighted) across the university population of expected individual WTP amounts shown separately for individuals from zip codes with above and below average shares of high and low income households respectively.

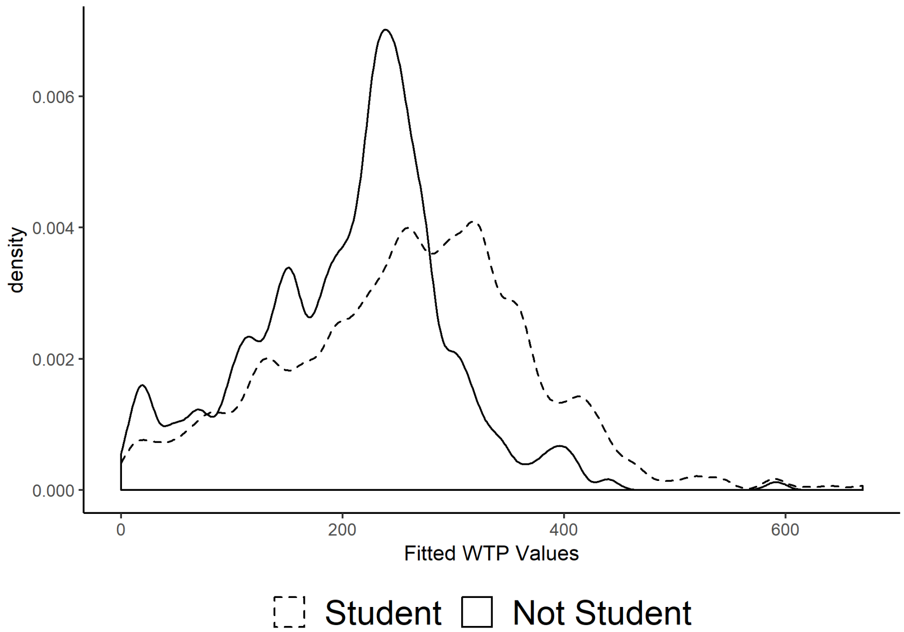
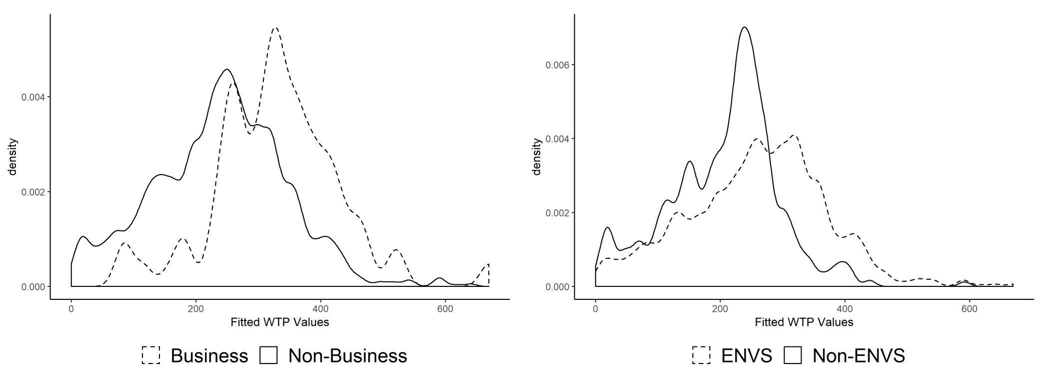


FIGURE 17.
Distribution across the university population of individual WTP amounts shown separately for individuals who are students versus those who are not



(a) Business Department

(b) ENVS Program

FIGURE 18.
WTP Distributions by Selected University Department

Notes: Distribution (weighted) across the university population of expected individual WTP amounts shown separately for individuals for individuals in the business school and environmental studies program.

university administrators and sustainability coordinators a means of assessing support without the expense of fielding a new survey and new analysis themselves.

Our model can estimate the entire WTP distribution across the campus community, allowing the amount and degree of support for a given program to be assessed. Future work could explore ways of further utilizing this information by perhaps formally ranking distributions using tools from the inequality literature. Future work could also explore the extent to which our results generalize to other types of institutions in both the private and public sector.

CHAPTER IV

ASSESSING SUPPORT FOR UNIVERSITY INTERNAL CARBON PRICING USING BENEFIT TRANSFER METHODS

Introduction

Note this chapter contains previously unpublished coauthored material (with Trudy Ann Cameron). I contributed to the development of the methodology and wrote the chapter. Trudy Ann Cameron contributed to the development of the methodology and analysis of the data.

Many universities see a commitment to sustainability as an important institutional value, and emission reductions as an important societal goal. Roughly 680 American universities have signed the American College and University President's Climate Commitment to become carbon-neutral. A self-imposed charge on a university's carbon emissions, known as an internal carbon price (ICP) is one way an university can accomplish its emissions-reduction goals by incentivizing changes in behavior and generating funds for emission-reduction projects. ICPs have been used by (or are planned for) schools such as Yale, Swathmore and Smith.

However, there is little guidance on how high an internal carbon price can be. University administrators must balance the costs of their sustainability goals with commitments to other priorities. New fees for sustainability could prove unpopular if they are set too high and provoke a backlash, especially in times when students are already facing increasing tuition and fees. However, carbon fees that are set substantially below the campus willingness to pay could leave the university without adequate funds to accomplish desired carbon reduction goals. Correctly instituting an internal carbon pricing program requires a careful evaluation of the amount of support among various stakeholder groups in the campus community.

A formal survey-based evaluation of support in the university community as described in Walch et al. (2019) will provide the most comprehensive ex ante assessment of stakeholder support. However, surveys are expensive and time-consuming. The cost of a de novo survey may be more than many universities are willing to bear, and may therefore be a barrier to evaluating the implementation of an ICP. The expense of conducting a new study to assess stakeholder support is a not unique to this setting and it is a common problem in environmental economics. A variety of methods have been described to "transfer" benefit estimates (or better yet, benefit functions) from a previously conducted study into a new setting to allow the assessment of the WTP for an unstudied, but similar resource.

In this paper, we present a way for university administrators to estimate support for internal carbon pricing on their campus without the need to field their own original stated-preference survey. With only the type of administrative data that all universities should have readily available—such as information about student ages, genders and permanent addresses—we describe how willingness to pay estimates for an arbitrary school can be computed using the estimated discrete choice preference parameters from an existing stated-preference survey of a different large public university described in Walch et al. (2019). Our estimated model provides the distribution of WTP for a specified set of individuals and a given ICP program for both our surveyed university and a new un-surveyed university. ICP programs are allowed to differ, consistent with the range of ICP programs in the original survey, in their scope and the way their revenue is raised and spent.

As an illustration of our method we calculate estimates for the distribution of willingness to pay for a fictitious large public university in Kentucky. This university differs in many respects from the originally surveyed university, such that using simple direct "value transfer" of the average WTP calculated for the original surveyed institution would be very unlikely to produce a valid estimate for our fictional Kentucky university. However, the approach we advocate is a benefit function transfer which allows WTP predictions to be adjusted for known differences in the distribution of demographics across the study sample and this new university. These adjustments are valid as long as the joint distribution of stakeholder characteristics at the surveyed university substantially *overlaps* the joint distribution of stakeholder characteristics at the university receiving the transferred WTP function. Stated more simply, for each individual in the fictitious university in Kentucky, there must be someone roughly similar at the surveyed university, even if the numbers and proportions of such people differ substantially between the two universities.

Our paper draws on a large literature about "benefits transfer" including Johnston (2007), Bateman et al. (2011), Colombo and Hanley (2015), Johnston and Duke (2015), Smith (2018), and McConnell and Siikamäki (2018). Additionally, our paper also adds to our collective understanding of the provision of public goods by non-government entities by predicting the demand for institutional climate change mitigation. Our method may prove useful to sustainability professionals and other university administrators when considering the size and design of an internal carbon pricing program at their own institutions.

The chapter is structured as follows. Section 4.2 discusses background on internal carbon pricing as well as briefly reviewing some existing approaches to benefits transfer. Section 4.3 explains our method of benefits transfer in addition to discussing our data. Section 4.4 presents results of a benefit transfer to our fictitious University in Kentucky. Section 4.5 concludes.

Background

Internal carbon pricing (ICP) programs take a variety of forms. Existing university ICP programs have taken the form of building energy fees, as described in Gillingham et al. (2017) or "shadow" carbon prices used solely for internal decisions about long-run capital expenditures. Air travel fees, meant to account for some of the carbon dixoide emissions released in travel, are also somewhat common. There is no universally accepted way to determine the appropriate size of a carbon fee.[1] Furthermore, the size of a carbon price is only one feature of an ICP. A university must decide what sources of emissions the fee should cover, as well as how to spend the revenue. In practice, universities face limits on what sources of emissions they can effectively meter. This means that all ICPs will necessarily cover an incomplete portion of emissions on a given campus. A university also faces multiple choices about the use of the revenue raised via an ICP. Options range from revenue recycling, to investing the proceeds in projects that reduce campus emissions, to purchasing carbon offsets.

Benefit transfer approaches arise out of the need to carry out benefit-cost analysis of various policies in the absence of both the funds and time to carry out

[1]See Barron and Parker (2018a) for a discussion of approaches universities have taken.

a complete original analysis.[2] Generally, a simple benefit transfer takes an existing valuation of some amenity from a so-called "study site" and applies it to a new "policy site." The transfer is deemed successful if the values from the study site are a good approximation of the values that would likely be found if an actual study were to be conducted. The transfer error is the deviation of the transferred value from the actual value.[3]

There are broadly two approaches to benefits transfer: a point-value transfer and a model transfer.[4] A value transfer takes the estimated WTP in one setting and transfers it to another setting with little or no adjustment. The "value of statistical life" is perhaps the best-known example of a value transfer approach in environmental economics. A model transfer approach, on the other hand, transfers over the full set of parameters from an estimated structural econometric willingness-to-pay model. The original estimation results in an equation which maps a set of attributes of the valued good, and individual characteristics, into a predicted WTP value. A model-transfer approach is generally seen as superior in that it allows for systematic adjustment for any differences in the environmental good in question and any differences in the distribution of the characteristics of the affected population between the original study site and the policy site.

The error in benefit transfer has been found to rely on a variety of factors related to the nature of the value being transferred, the entity for which the

[2]See Newbold et al. (2018) for a discussion of the history and current use of benefit-transfer in current policy making.

[3]In practice, it is usually impossible to calculate the error in any one example of benefit transfer without conducting a new study at the policy site. Given that the whole point of a benefit transfer exercise is to avoid doing a full valuation study at the policy site, validation like this is very rarely done when conducting a benefit transfer. However, benefit transfer errors are frequently studied for the purpose of methodological improvement by choosing policy sites where existing studies have already been done.

[4]Model transfer is sometimes called function transfer.

transfer is being constructed, as well as numerous methodological choices the researcher can make. Generally, the literature has confirmed that model transfer generally leads to a smaller transfer error, that stated preference surveys tend to lead to less error (Kaul et al. (2013)), and that similarity between transfer and policy sites is important (Johnston (2007)). Reducing the amount of error in benefit transfer and establishing best practices remains an active area of research.

Empirical Strategy and Data

The original institution where we conducted the survey, namely our "study site", is a large flagship public university. However, this study site is in a more liberal state than conservative-leaning Kentucky, and the study-site university does not contain a medical school or other professional schools (other than a business school and a law school).

Description of Survey for Study Site

The original study, described fully in Walch et al. (2019) consists of a stated-preference discrete choice experiment which is cast in terms of an advisory referendum. In each of several choice tasks, students, staff and faculty are asked to choose (a) between two programs and a no-program status quo alternative, or (b) between a single program and a no-program status-quo alternative. Programs vary in the size of the emissions reduction they achieved, in their cost, in their means of raising revenue (a lump-sum fee, emission fees on air travel, emission fees on building energy use, or taxpayer funds), and in their use of these revenues (divided between on-campus carbon projects, off-campus projects in the form of offsets, or spending that constitutes revenue recycling in the form of additional funds for academic programs). Additionally, in the original survey, we elicited information on each respondent's self-reported political ideology, history of experience with extreme weather, and attitudes towards climate change.

Administrative data are obtained on both respondents and non-respondents and used to estimate a response/non-response model to adjust for potential systematic selection into the sample that might be correlated with WTP. The "permanent address" of each respondent and non-respondent invitee were obtained from these confidential administrative records and used to link each individual to zip-code level demographic statistics from the U.S. Census's American Community Survey (ACS). A formal response/non-response model was estimated and used to adjust for systematic non-response.

In total, we collected approximately 1,000 completed responses. These individuals, collectively, made a total of approximately 5,500 choices. Across each individual survey instrument all choice sets and attribute levels were randomized and essentially unique. We use these results to estimate a random utility model (RUM) using a conditional logit algorithm. Our results show substantial support for an internal carbon pricing program. WTP estimates for the study sample, which vary depending on the design of the program, are generally above most accepted values of the social cost of carbon. Across programs that involve the same individual cost to the respondent, we find that respondents prefer programs with emission charges and where taxpayers also share a portion of the burden. Revenue recycling decreases support for an ICP program.

Description of Transfer Methodology

We build a fictional large public university in Kentucky to illustrate the potential of our model for use in benefit function transfer. The reasons for choosing the Kentucky are somewhat arbitrary, but Kentucky does have certain advantages for our illustration. Like our study site, the "policy site" is imagined to be a large public flagship university. The population of our study university, however, is drawn from a mostly liberal-leaning state (although some regional heterogeneity exists) which contains little energy resource extraction. In contrast, our fictional university in Kentucky is located in a mostly conservative state with ties to the substantial coal industry located in the region. This allows us to assess the construct validity of our initial estimates (i.e. is predicted WTP higher and lower in the places you would expect it to be). This exercise is also designed to highlight the sensitivity of our estimates to the political environment where a university is located.

The confidential administrative data we received from the study site university are not automatically available to us for other institutions, unless we can persuade the university to provide these confidential data. We are therefore missing individual-level administrative data on students, faculty and staff, age, gender and department/division of the university. Of course, the identical data we collected only via the original survey will not be available unless we replicate our survey at the other site. Thus we also lack the information we elicited at the end of the survey itself, such as a respondent's personal political ideology, income, or history of experiences with extreme weather.

We must therefore find (1) some way to approximate the individual information we have collected in our original survey at the study site, (2) estimate a model only using that information at the study site, and (3) transfer the estimates to our policy site, the fictitious university in Kentucky. Luckily, there is a large amount of publicly available data on the origins of students who attend the *actual* University of Kentucky. We draw basic information, including the number of students, the proportion of students from each state, and the numbers of faculty and staff from the University of Kentucky website and the National Center for Education Statistics' Integrated Postsecondary Education Data System (IPEDS). We use these data to guide our construction of an approximate population of a fictitious university that roughly matches the actual University of Kentucky on these dimensions.

We populate the groups of faculty, non-faculty employees, and students by drawing people and their basic attributes from the corresponding category in the study sample. The individual characteristics derived from the "permanent address" zip code data, however, is changed to better reflect the characteristics of the actual University of Kentucky population.

Students are assigned to permanent address zip-codes (proxided by the zip-code of the high school they attended before they came to University). The neighborhood characteristics for that zip-code are assumed to apply. We first draw a sample of students equal in size to the number of in-state students from the University of Kentucky from the universe of Kentucky zip codes. The probability of a student being drawn from a given zip-code is proportional to that zip code's population of residents with "some college education." We also have data on the number of out-of-state students from each of the ten most common source states for out-of-state students which allows us to assign zip codes for out-of-state students in a similar fashion. Absent any other information, students are then randomly assigned to departments so that the relative proportions of students in each department matches the share of students assigned to that department at the study site university.[5]

Faculty and staff residential locations are drawn using a procedure similar to that used for students. We assume that the relevant zip codes from which to sample faculty and employees are those within commuting distance from the university which we define as zip codes whose centroids are within 30 miles of campus. We then draw from each zip code in proportion to the percent of people in each zip code with the commute time sufficient for them to commute to the university. We sample in proportion to those who have graduate degrees (for the faculty). We furthermore do the same for non-faculty employees but instead sample in proportion to the share of zip-code residents with some college education.

We estimate the model to be transferred from the study university to the policy university using only the choice data, from the survey, variables available from the ACS zip-code data, 2016 voting data, and LCV data, plus some information from the confidential administrative records such as gender, university department and role. The model we estimate is an abbreviated version of the model desrcibed in Walch et al. (2019), where the vectors of z_i are more limited:

[5]University of Kentucky has a medical school and several professional programs that our study university does not have. This prevents us from exactly exactly matching the student body of University of Kentucky and motivates our approach here. An alternative approach would be assume that all medical and other professional students have similar WTP to the law and MBA students at our study site.

$$U^i_{jt} - U^i_{0t} = -(\alpha' Z_i)C^i_{jt} + (\beta' Z_i)B^i_{jt} \tag{4.1}$$
$$+ (\gamma' Z_i)DistCosts^i_{jt} + (\delta' Z_i)DistSpend^i_{jt} + \epsilon^i_{jt}$$

where U^i_{jt} is the WTP for ICP program j for individual i, U^i_{0t} is the utility of the (no-program) status-quo. B^i_{jt} is the carbon reductions from program j, C^i_{jt} is the cost of the program to individual i. Z_i is a vector of individual characteristics drawn from the census ACS data. $DistCosts$ and $DistSpend$ are vectors describing the distribution of costs and benefits, as described above, for the ICP program. ϵ^i_{jt} is the difference of two type-1 Extreme Value errors. The marginal WTP for a given attribute can be found by dividing the coefficient on that attribute by α the marginal utility of income.

We take the estimated model and calculate WTP estimates for the fictitious campus in Kentucky by evaluating the estimated WTP function for each "individual" at the Kentucky campus. Standard errors for each individual are computed using the Krinksy-Robb method.[6] We will present estimates for the entire distribution of WTP, for selected programs, for this fictional Kentucky university.

Our approach does have limitations. We are only able to approximate the true student body, and our assumptions about the orgin zip code of students from Kentucky and other nearby states may lead to some error. Of course, we cannot verify the accuracy of our WTP estimates without carrying out an actual survey of

[6]The Krinksy-Robb method constructs standard errors by drawing estimates from the sampling distribution of the parameters. It is necessary because the support of the sampling distribution for the marginal utility of income contains zero.

the University of Kentucky to achieve an estimate of the true value. Some amount of error is unavoidable without access to the true student data. After all, benefit transfer is not preferable to a formal survey in terms of accuracy. Its advantage lies in its feasibility.

Results

Table 12 shows descriptive statistics, including both means and median, for the variables included in the estimated choice model for the study site and fictional Kentucky university. Table 13 shows the coefficients of the choice model estimated using only those explanatory variables from the original study university for which we can construct values for the fictionalized Kentucky university. Most variables are statistically different between the study sample and the transfer sample, confirming that a simple value transfer is unlikely to work well. A model is needed to adjust for these differences.

We can calculate the entire WTP distribution of the fictionalized campus. However, because we must rely on census data at the zip code level, the resolution of our distributions is much coarser than if we had access to the (restricted) administrative data or survey responses. Because of this, the histograms exhibit less heterogeneity (i.e. fewer unique values) than those generated from models with a wider variety of regressors. There are only as many unique WTP values as there are unique sets of zipcodes and administrative characteristics.

TABLE 12.
Compare means of relevant measures of sample heterogeneity

	Means			Medians		
	Study Sample	Fictitious UK	p	Study Sample	Fictitious UK	p
zip pr: Asian alone	.0273	.0141	0	.0182	.00779	0
zip pr: Native HI, etc., alone	.00201	.00057	0	.000564	0	0
zip pr: Aged 5 or less	.0438	.0268	0	.0462	.026	0
zip pr: Aged 50-54	.0373	.0314	1.19e-33	.0426	.032	0
zip pr: 25+, some coll, no degr	.285	.203	0	.299	.197	0
zip pr: 25+, assoc degr	.0823	.0753	9.06e-31	.0814	.0745	0
zip pr: 25+, bach degr	.17	.159	4.33e-06	.151	.164	.000662
zip pr: ltd English	.0104	.0158	1.24e-13	.0047	.00669	1.45e-08
zip pr: ltd English, Asian/Pac Isl lang.	.00376	.0047	.00106	.00124	.000428	0
zip pr: Inc lt 10K	.072	.0946	0	.072	.0847	0
zip pr: Inc 10K-15K	.0914	.062	0	.096	.06	0
zip pr: Inc 15K-25K	.141	.126	2.47e-26	.155	.126	0
zip pr: Inc 25K-35K	.101	.12	0	.0964	.117	0
zip pr: Inc 35K-50K	.153	.149	2.14e-06	.16	.154	2.82e-09

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Table 12 – continued from previous page

	Means			Medians		
	Study Sample	Fictitious UK	p	Study Sample	Fictitious UK	p
zip pr: Inc 75K-100K	.113	.106	1.06e-11	.106	.101	0
zip pr: 4 rooms, fewer	.232	.281	0	.22	.258	0
zip pr: Moved-in 1980-89	.0603	.0764	0	.0586	.0731	0
zip pr: Hsng incompl kitch	.00563	.00881	9.26e-14	.00245	.00633	0
zip pr: Hsng incompl plumb	.00191	.0043	1.59e-33	.00112	.00199	0
zip pr: No phone service	.0225	.0309	0	.0207	.0279	0
zip pr: Hse val 100K-150K	.119	.237	0	.126	.234	0
zip pr: Hse val 200K-300K	.262	.131	0	.256	.122	0
zip pr: Hse val 500K-1M	.0489	.0306	5.15e-23	.0398	.0155	0
zip pr: Cmt drive alone	.736	.814	0	.758	.823	0
zip pr: Cmt any carpool	.0861	.0989	7.72e-36	.0816	.0973	0
zip pr: Cmt any pub tran	.0556	.0104	0	.0619	.00302	0
zip pr: Cmt 10-14 min	.202	.144	0	.222	.145	0
zip pr: Cmt 15-19 min	.199	.174	7.58e-37	.201	.175	0
zip pr: Cmt 20-24 min	.128	.148	2.34e-36	.122	.145	0

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Table 12 – continued from previous page

	Means			Medians		
	Study Sample	Fictitious UK	p	Study Sample	Fictitious UK	p
zip pr: Cmt 25-29 min	.0548	.0615	7.81e-14	.0519	.0578	2.21e-10
zip pr: Cmt 40-44 min	.0199	.0312	0	.0141	.0277	0
zip pr: Cmt 60-89 min	.0393	.0436	.000192	.0411	.0322	1.27e-14
zip pr: Cmt 90 min up	.0124	.0213	0	.00819	.0174	0
zip pr: Heat electr	.621	.53	0	.664	.567	0
zip pr: Heat solar	.000352	.000246	.00536	0	0	7.62e-08
zip pr: Ind wh trade	.0203	.0233	1.59e-14	.0149	.0242	2.66e-15
zip pr: Ind transp	.0403	.0567	0	.0398	.0543	0
zip pr: Ind infor	.0199	.014	0	.0187	.0129	0
zip pr: Ind financ	.0605	.0541	1.45e-12	.0606	.0492	0
zip pr: Ind professl	.105	.0827	0	.102	.0777	0
zip pr: Ind art/enter	.0995	.0881	5.21e-28	.0987	.0858	0
zip pr: with Dem repr	.955	.113	0	1	0	0
1=Non-white	.38	.44	.000185	0	0	.000185
1=individual's age known	.751	.71	.00481	1	1	.00481

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Table 12 – continued from previous page

	Means			Medians		
	Study Sample	Fictitious UK	p	Study Sample	Fictitious UK	p
	109	620	0	40.8	400	0
1=U.S. citizen	.694	.914	0	1	1	0
1=not U.S. citizen	.0562	.0858	.000927	0	0	.000928
1=Employee	.712	.641	3.37e-06	1	1	3.38e-06
1=empl.grp: Career non-tenure	.0582	.0536	.528	0	0	.528
1=empl.grp: Classified staff	.144	.15	.656	0	0	.656
1=empl.grp: Courtesy appt	.011	.0294	.000649	0	0	.00065
1=empl.grp: Student employee	.186	.151	.00309	0	0	.00309
1=empl.org: Arch, Allied Arts	.00301	.00643	.18	0	0	.18
1=empl.org: Facilities	.0211	.0269	.262	0	0	.262
1=empl.org: Design	.0311	.0223	.065	0	0	.065
1=empl.org: Library	.0291	.0194	.0307	0	0	.0307
1=empl.org: Music and Dance	.00903	.0108	.597	0	0	.597
1=empl.org: UGS	.014	.0135	.894	0	0	.894
1=empl.org: VPFA, VPSL	.00903	.00792	.699	0	0	.699

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Table 12 – continued from previous page

	Means			Medians		
	Study Sample	Fictitious UK	p	Study Sample	Fictitious UK	p
1=Student	.698	.638	.0000915	1	1	.0000916
1=stu.sch: Nat. Sci.	.159	.132	.0118	0	0	.0118
1=stu.dept: Bus admin	.0632	.0773	.0993	0	0	.0993
1=stu.dept: Educ studies	.012	.0121	.978	0	0	.978
1=stu.dept: Env studies	.0251	.0118	.000161	0	0	.000161
1=stu.dept: Music	.0191	.0123	.058	0	0	.058
1=stu.dept: Sociol	.00903	.00909	.984	0	0	.984

TABLE 13.
Parameter Estimates for Final Transfer Model

	Estimate	Standard Error
1=Preferred program		
Program's cost to household (22 to 232)	-0.00919***	(0.00119)
× demeaned response propensity (.193)	0.00538**	(0.00247)
Percentage-point C reduction (10 to 50)	-0.919***	(0.158)
× Percentage-point C reduction (10 to 50)	-0.000460***	(0.000176)
× zip pr: Race: Asian alone (.028)	0.446***	(0.130)
× zip pr: Race: Native HI, etc., alone (.002)	1.706**	(0.668)
× zip pr: Aged 5 yrs or less (.044)	-1.413***	(0.478)
× zip pr: Aged 50-54 yrs (.037)	1.067**	(0.481)
× zip pr: Educ 25+: some college, no degree (.285)	0.495***	(0.147)
× zip pr: Educ 25+: associate degree (.082)	0.722***	(0.259)
× zip pr: Educ 25+: bachelors degree (.171)	-0.217**	(0.0875)
× zip pr: ltd English, Asian languages (.004)	-1.606***	(0.576)
× zip pr: Incomes: lt 10K (.071)	0.443***	(0.163)
× zip pr: Incomes: 10K-15K (.091)	-0.878***	(0.224)
× zip pr: Incomes: 15K-25K (.14)	0.593**	(0.239)
× zip pr: Incomes: 25K-35K (.1)	-1.123***	(0.267)
× zip pr: Incomes: 35K-50K (.153)	-0.423***	(0.148)

Continued on next page

× zip pr: Incomes: 75K-100K (.114)	0.702***	(0.187)
× zip pr: Housing with: incomplete kitchen (.006)	-1.260***	(0.438)
× zip pr: Housing with: incomplete plumbing (.002)	-2.490***	(0.771)
× zip pr: Housing with: no phone service (.023)	2.511***	(0.462)
× zip pr: Commute by: drive alone (.737)	0.239**	(0.0932)
× zip pr: Commute by: carpool (.087)	0.453***	(0.146)
× zip pr: Commute by: public transit (.055)	0.710***	(0.228)
× zip pr: Commute time: 10-14 min (.2)	0.744***	(0.154)
× zip pr: Commute time: 15-19 min (.199)	0.754***	(0.159)
× zip pr: Commute time: 20-24 min (.129)	0.202*	(0.120)
× zip pr: Commute time: 25-29 min (.055)	0.879***	(0.237)
× zip pr: Commute time: 40-44 min (.02)	1.180***	(0.355)
× zip pr: Commute time: 60-89 min (.039)	0.463*	(0.257)
× zip pr: Commute time: 90 min up (.013)	1.294***	(0.410)
× zip pr: Heating: electricity (.618)	0.115***	(0.0426)
× zip pr: Heating: solar (0)	9.506***	(2.139)
× zip pr: Industry: wholesale trade (.021)	0.836**	(0.370)
× zip pr: Industry: transportation (.041)	1.245***	(0.313)
× zip pr: Industry: information (.02)	2.634***	(0.544)
× zip pr: Industry: finance etc. (.06)	0.421*	(0.238)

Continued on next page

× zip pr: Industry: professional (.105)	-1.037***	(0.203)
× zip pr: Industry: arts/entertain/recr. (.099)	0.500***	(0.174)
× zip pr: Democratic representative	-0.0369***	(0.0139)
× 1=Individual's age known (1)	0.00243	(0.0117)
× Demeaned indiv. age, if known	-0.00192***	(0.000497)
× (Demeaned indiv. age) squared	0.0000609***	(0.0000180)
× 1=not U.S. citizen (.074)	0.0209***	(0.00807)
× 1=Employee (.677)	0.0286***	(0.00834)
× 1=empl.grp: Classified staff (.122)	-0.0207***	(0.00702)
× 1=empl.grp: Student employee (.214)	-0.0260***	(0.00827)
× 1=empl.org: Arch, Allied Arts (.004)	0.154***	(0.0322)
× 1=empl.org: Library (.041)	-0.0378***	(0.0137)
× 1=Student (.741)	0.00461	(0.0109)
× 1=stu.sch: Nat. Sci. (.176)	-0.0119**	(0.00562)
Cost share: air-travel fees (0 to .5)	0.0469***	(0.00736)
× Cost share: air-travel fees (0 to .5)	-0.000426***	(0.000101)
× zip pr: Dwellings sized: 4 rooms or fewer (.23	-0.0530**	(0.0222)
× zip pr: Housing with: incomplete plumbing (.00	1.004**	(0.506)
× 1=Employee (.677)	-0.00171	(0.00348)
× 1=empl.org: Arch, Allied Arts (.004)	-0.312***	(0.0562)

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Table 13 – continued from previous page

× 1=empl.org: Facilities (.026)	-0.0406***	(0.0148)
Cost share: building energy fees (0 to 1)	0.00918***	(0.00190)
× 1=Employee (.677)	0.00148	(0.00241)
× 1=empl.grp: Courtesy appt (.009)	-0.0151**	(0.00627)
× 1=empl.org: Arch, Allied Arts (.004)	-0.260***	(0.0439)
× 1=empl.org: Design (.046)	-0.0129***	(0.00431)
× 1=empl.org: Music and Dance (.013)	0.0355**	(0.0144)
× 1=empl.org: VPFA, VPSL (.012)	0.0221*	(0.0117)
Cost share: taxpayers (0 to .2)	-0.000156	(0.0103)
× zip pr: ltd English (.011)	0.483**	(0.205)
× 1=Employee (.677)	-0.000563	(0.00796)
× 1=empl.org: Arch, Allied Arts (.004)	-1.147***	(0.191)
× 1=empl.org: UGS (.022)	0.0580***	(0.0218)
× 1=Student (.741)	0.00327	(0.00790)
× 1=stu.dept: Educ studies (.013)	0.0844***	(0.0182)
× 1=stu.dept: Env studies (.028)	0.0303*	(0.0158)
× 1=stu.dept: Music (.021)	-0.0408**	(0.0177)
Spend share: academic programs (0 to .3)	0.00251	(0.00799)
× zip pr: Race: Asian alone (.028)	-0.159**	(0.0759)
× 1=Non-white (.374)	-0.0177***	(0.00523)

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Table 13 – continued from previous page

× 1=Employee (.677)	0.00428	(0.00552)
× 1=empl.org: Arch, Allied Arts (.004)	-0.754***	(0.0993)
× 1=Student (.741)	0.00597	(0.00589)
× 1=stu.dept: Sociol (.01)	0.0659**	(0.0308)
Spend share: carbon offsets (0 to .5)	0.0133	(0.0215)
× zip pr: Move-in years: 1980-89 (.06)	0.486***	(0.154)
× zip pr: Housing with: incomplete kitchen (.006	-0.537*	(0.275)
× zip pr: House values: 100K-150K (.119)	-0.140**	(0.0710)
× zip pr: House values: 200K-300K (.261)	-0.0722*	(0.0431)
× zip pr: House values: 500K-1M (.05)	-0.153**	(0.0657)
× 1=Employee (.677)	-0.00189	(0.00416)
× 1=empl.grp: Career non-tenure (.046)	-0.0200*	(0.0110)
× 1=empl.org: Arch, Allied Arts (.004)	0.183***	(0.0358)
× 1=empl.org: VPFA, VPSL (.012)	0.0779***	(0.0264)
× 1=Student (.741)	0.00222	(0.00475)
× 1=stu.dept: Bus admin (.07)	0.0189***	(0.00642)
× 1=stu.dept: Music (.021)	0.0339**	(0.0154)
Status quo, no program	0.159	(0.482)
× zip pr: Moved: same county (.094)	8.036**	(3.606)
× zip pr: Moved: from abroad (.004)	-106.4***	(23.41)

Continued on next page

Table 13 – continued from previous page

× zip pr: Heating: no fuel (.005)	57.30*	(33.84)
× 1=Employee (.677)	0.371	(0.322)
× 1=empl.grp: Courtesy appt (.009)	-2.671***	(0.547)
× 1=empl.grp: Student employee (.214)	-0.741***	(0.257)
× 1=empl.org: Arch, Allied Arts (.004)	-39.31***	(5.492)
× 1=empl.org: Design (.046)	-1.123**	(0.456)
× 1=empl.org: VPFA, VPSL (.012)	3.564***	(1.341)
× 1=Student (.741)	-0.389	(0.269)
× 1=stu.dept: Bus admin (.07)	1.192***	(0.323)
× 1=stu.dept: Biology (.036)	-0.882**	(0.357)
Max. log-likelihood	-6088.67	
No. respondents	997	
No. choices	5252	
No. alternatives	12466	

t Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We now compare the distributions of predicted WTP for the same program, for the two universities, to give an idea of what the model is capable of computing. Of course, the distribution for any given program (within the sample range), split according to the values of any individual variable in the data could be computed if so desired. The few we show are selected for the purpose of illustration.

Figure 19 and Figure 20 show the overall distribution of WTP for the entire campus of the policy site and study site university, respectively, for a program that (a) achieves a 40% emission reduction, (b) is funded entirely by lump-sum fees and (c) spends all of its revenue on on-campus projects. The median WTP for this type of program differs substantially between the two schools. The median WTP for this type of program at the study site university is \$134 versus \$83 at the fictionalized Kentucky university. Furthermore, the fictionalized Kentucky university has a substantial proportion of individuals who are not willing to pay any amount of money for the program. The differing political environment and other variables of the two universities seems to be an important factor determining the level of support for ICPs.

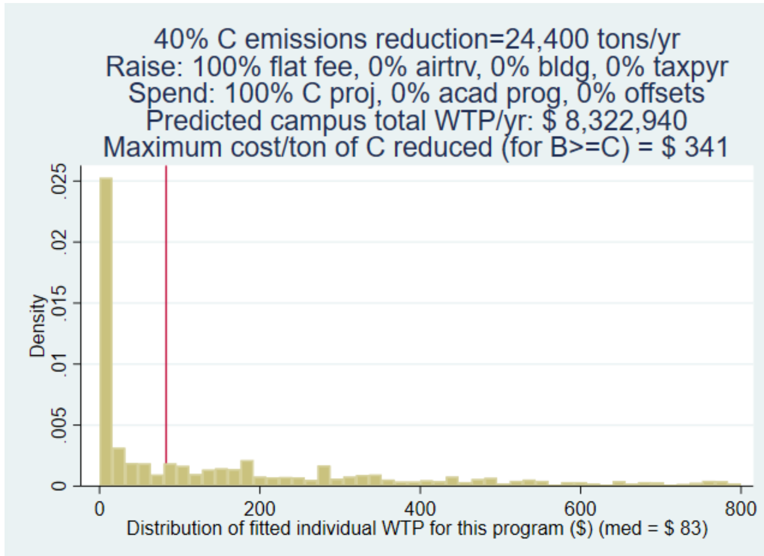


FIGURE 19.
WTP Distribution for the entire campus at the *policy site* university. All revenue raised from lump-sum fees, and spent on carbon reduction projects. Data comes from home zip-code characteristics. Homezip codes selected proportionally to proportion with at least some college education.

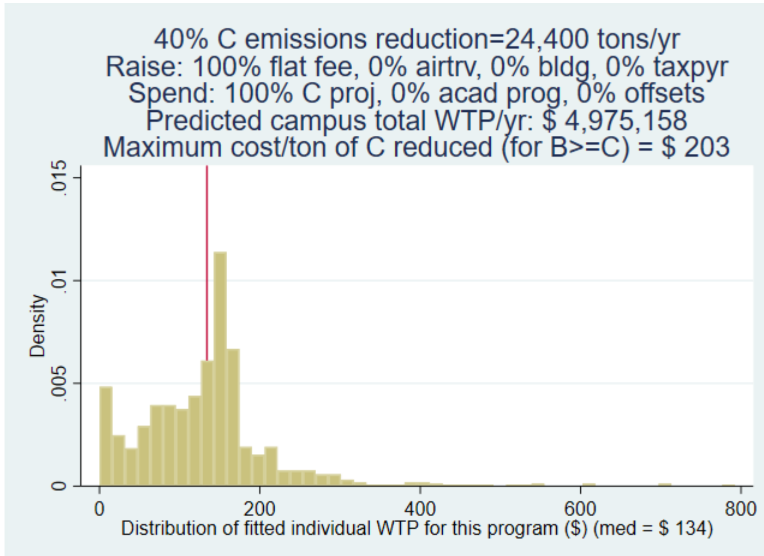


FIGURE 20.
WTP Distribution for the entire campus at the *study site* university. All revenue raised from lump-sum fees, and spent on carbon reduction projects. Data comes from home zip-code characteristics.

Figures 21 and 22 show the distribution for a program which (a) achieves a 40 percent emission reduction (b) raises 20 percent of its revenues from lump-sum fees, 30 percent from air travel fees, 30 percent from building energy fees and 20 percent from taxpayers, and (c) spends 40 percent of its revenue on on-campus projects, 30 percent on academic programs and 30 percent on offsets. Median WTP still differs substantially between the two schools, in the study site university the median is equal to $243, as compared to to $158 in the policy site university. The shape of the distributions differ remarkably. The policy site school is concentrated close to zero with a long right tail, where the study site school is more symmetric.

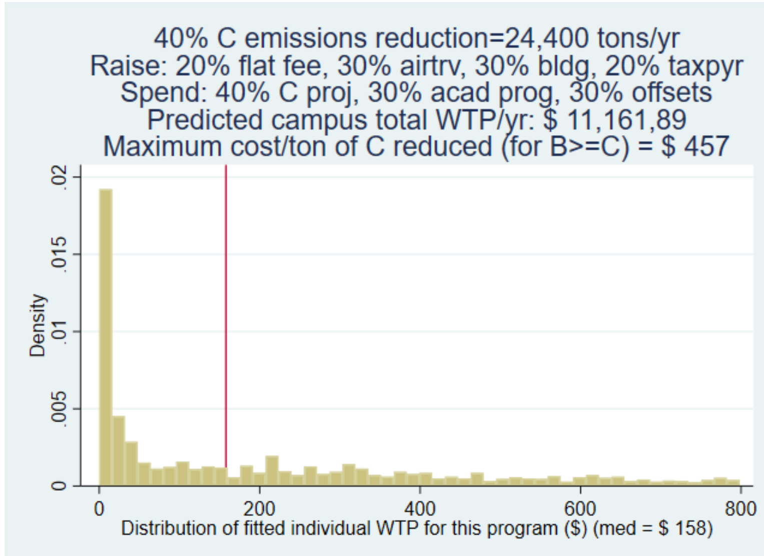


FIGURE 21.
WTP for the entire campus at the *policy site* university. Program raises its revenue from all 4 cost categories and spends it on all 4 revenue categories. Data comes home zip-code characteristics. Homezip codes selected proportionally to proportion with at least some college education.

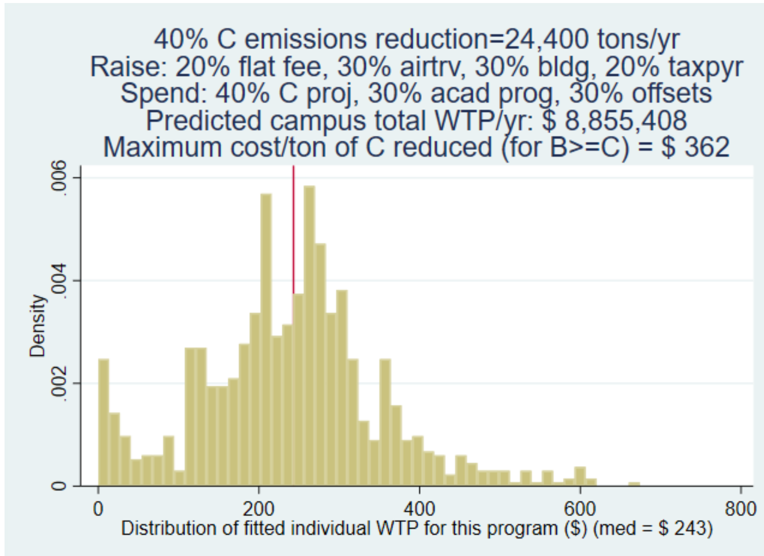


FIGURE 22.
WTP for the entire campus at the *study site* university. Program raises its revenue from all 4 cost categories and spends it on all 4 revenue categories. Data comes home zip-code characteristics.

We next compute WTP distributions for both campuses for subsets of the campus community. Figure 23 and Figure 24 shows WTP distributions partitioned according to whether the individual is or is not a student. Figure 25 and Figure 26 shows the distribution of WTP across individuals "from" zip-codes split according to the proportion with some college education. Figure 27 and Figure 28 show individuals split into subsamples according to their terciles in zip-code average commute time.

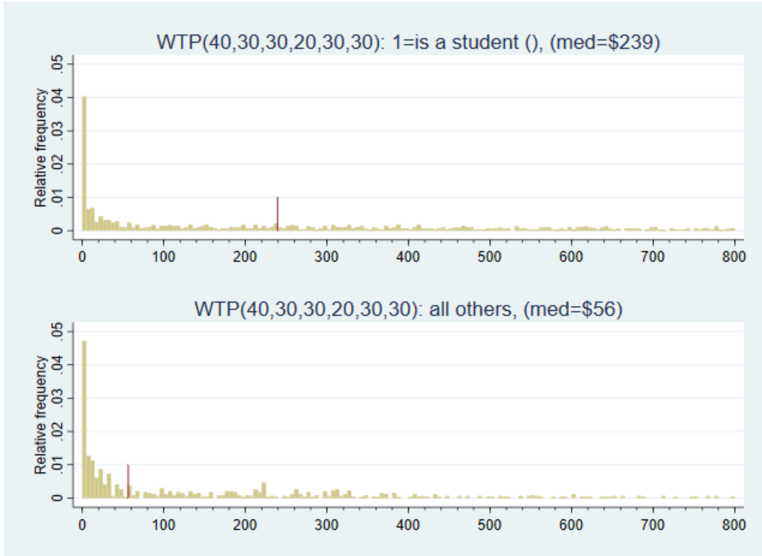


FIGURE 23.
Students vs. Non-Students Policy Site University: WTP distribution split by whether the individual is, or is not, a student. See the notes to figure 3 for further details.

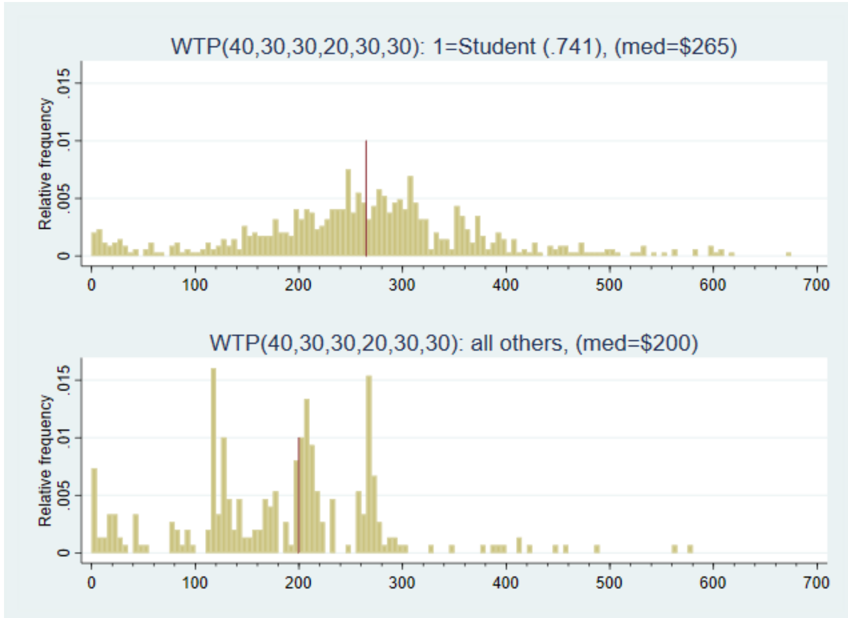


FIGURE 24.
Students Vs. Non-Students Study Site University: WTP distribution split by whether the individual is, or is not, a student. See the notes to figure 4 for further details.

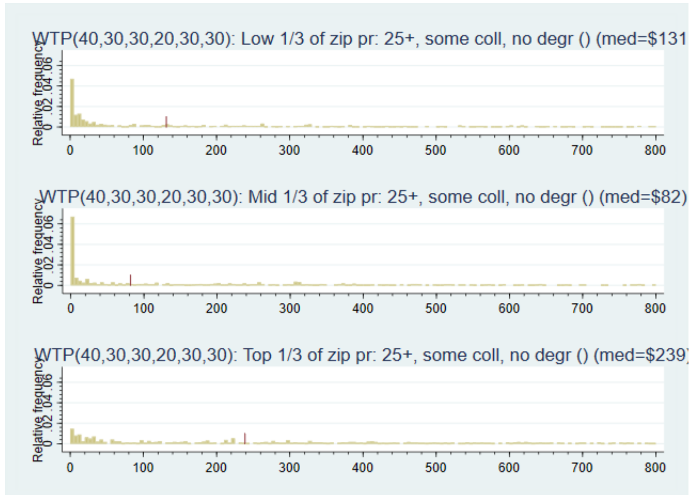


FIGURE 25.
Proportion with Some College, Policy University: WTP Distribution for the policy site split by whether the individual is from a zip-code in the bottom, middle or top tercile of the proportion of residents with at least some college. See the footnote to Figure 3 for more details.

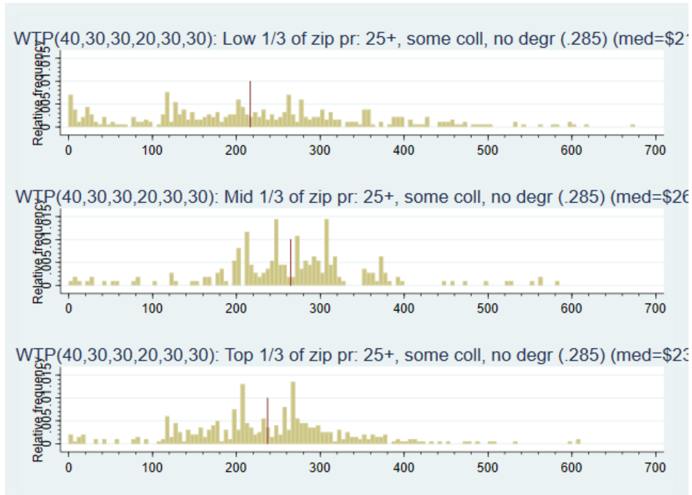


FIGURE 26.
Proportion with Some College, Study University: WTP Distribution for the policy site split by whether the individual is from a zip-code in the bottom, middle or top tercile of the proportion of residents with at least some college. See the footnote to Figure 4 for more details.

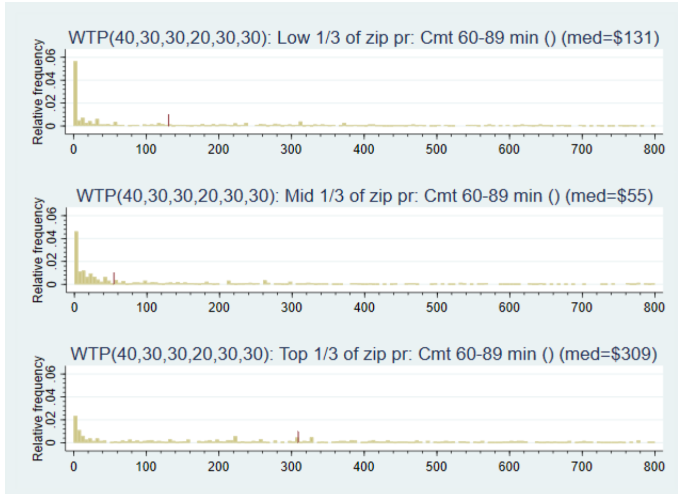


FIGURE 27.

Proportion with Long Commutes: Policy Site University: WTP distribution at the policy site university split by whether people are from a zip-code in the bottom, middle, or top tercile of the proportion of people with a commute between 60 and 89 minutes. See the footnote to Figure 3 for further details.

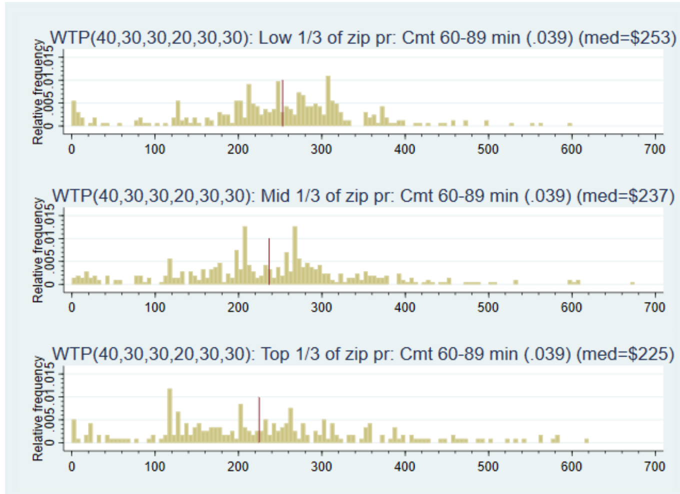


FIGURE 28.
Proportion with Long Commutes: Study Site University: WTP distribution at the study site university split by whether people are from a zip-code in the bottom, middle, or top tercile of the proportion of people with a commute between 60 and 89 minutes. See the footnote to Figure 4 for further details.

Conclusion

In this paper we show how the coefficients from a choice model, estimated using data from a stated-preference survey on internal carbon pricing at a university can be used for benefit-transfer exercises. The estimates are constructed by drawing a synthetic set of university stakeholders from census data to evaluate an estimated WTP model that allows WTP to vary as a function of individual characteristics and ICP program attributes. Such a method may be useful to university policy-makers who are considering an internal carbon price and want a way to evaluate support for the program in a way that does not involve the expense, time or expertise needed to carry out a de novo stated-preference survey.

This paper also evaluates the sensitivity of our estimates of WTP distributions for a single study university, as in Walch et al. (2019), to predict WTP at another university in a very different sociopolitical environment.

We find that the different regions from which the populations of the two institutions are drawn do indeed affect the sizes of our WTP estimates. For the model estimated at our liberal-leaning study university, the median WTP for our baseline program is $134. In our more conservative leaning fictional university in Kentucky the WTP for the baseline program is $83. Future work could explore a wider range of universities to assess the level of support for internal carbon pricing across the country, given that political ideologies and socio-demographics among students and employees will vary from state to state and from campus to campus.

Our benefit transfer exercise is unfortunately somewhat limited by certain characteristics of our study university. The study university lacks a medical school, an engineering program, as well as several other professional programs. Because of this, we are not able to characterize more specifically the distribution of WTP on campuses where participation in these programs may be correlated with ICP preferences. Future work may explore carrying out a survey for at least one university with these professional programs to support benefit transfer exercises for universities that feature these populations. For other reasons, our method may not be entirely applicable to small private schools, and future work could likewise carry out a survey of a small private school to allow us to construct estimates of WTP for people associated with these types of higher-education institutions. Lastly, additional work could characterize and explore the sources of transfer error by conducting a fresh stated-preference survey at another institution and comparing the benefit transfer results to the results estimated on data from a comprehensive stated-preference survey.

APPENDIX A

APPENDIX TO CHAPTER 2

Figure 29 show the results, in the form of mean square prediction error (MSPE) ratios, of the permutation test for the synthetic control estimates for low-income, high-minority-share, and communities that are classified as both low-income and high-minority-share. The MSPE ratio for California is shown by the vertical line. The fact that the Californian MSPE ratio is smaller than the MSPE ratio of many placebo estimates implies that the synthetic control estimates are statistically insignificant.

Table A1 shows the non-zero weights on emissions in other states used to construct the synthetic control for California. The synthetic control is calculated by taking a weighted average of these control-state emissions.

Table A2 shows ATT estimates from the nearest-neighbor matched difference-in-differences estimator with an alternative sample definition where plants that do not appear every year are dropped.[1] In the chapter, plants who appear in the first year but not in subsequent years are assigned zero emissions for the missing years. The estimates do not change for the Western U.S. and are

[1] In total, I exclude 1,768 observations from 368 plants in the overall sample. The plants I exclude tend to be smaller than the plants included in the sample. Among the controls, the excluded plants tend to be larger emitters although the plants excluded from the California sample emit at levels similar to the included California plants. Energy efficiency measures for dropped and included plants are similar across both the California and the control sample.

qualitatively similar for the models which draw their control units from the entire U.S.

Table A3 shows the estimates for heterogeneous treatment effects from the same sample. The estimates remain qualitatively similar.

Table A4 shows estimates which include weather variables, such as temperature, as controls. While the signs and the magnitudes of the estimates are similar, the coefficients are no longer statistically significant except for the coefficient on NO_x in the model which draws controls from the entire U.S. and utilizes the Abadie and Imbens (2011) finite-bias adjustment. Table A5 show the heterogeneous treatment effect estimates. Coefficients for co-pollutant heterogeneity remain unchanged. The coefficients for CO_2 becomes statistically significant. However, the location of CO_2 emissions has no effect on the locations of the damages so this change does not matter economically.

Table A6 shows estimates from a specification including the change in state renewable energy certificate (REC) requirements across the treatment date as a statistical control. A REC signifies the right to claim a certain quantity of renewable energy generation for the purpose of complying with state level Renewable Portfolio Standard (RPS) requirements. If RPS requirements changed as the cap-and-trade program went into effect, my estimates may be biased. Controlling for changes in RPS stringency, the key paramter estimates are similar in sign and magnitude but lose statistical significance. Table A7 shows the heterogeneous treatment effect estimates. They are qualitatively similar except for the coefficient for CO_2 which is now statistically significant in the model which draws its controls only from plants in the Western U.S.

Table A8 shows nearest-neighbor matched difference-in-difference estimates for the California carbon cap-and-trade program where "distance" is computed using the simple propensity score instead of the Mahalanobis norm. The magnitudes of the estimates are qualitatively similar and the p-values are somewhat lower, resulting in estimates with a greater degree of statistical significance. Table A9 shows the heterogeneous treatment effect results obtained from these alternative matches. Like my preferred specification, using the Mahalanobis norm, I cannot reject the null hypothesis that average emission changes are invariant to the race and income composition of the surrounding communities associated with the plants (except in the case of SO_x in minority communities for the model which draws controls from the Western U.S.). This propensity score specification implies that a greater share of reductions in SO_x occurred in minority communities.

Table A10 shows the estimates for the heterogeneous treatment effects associated with the robustness checks in Table 5. The results are qualitatively similar to those in Table 4 with the exception that the coefficient for SO_x in the "San Onofre closing" robustness check now implies a disproportionate decrease in SO_x in minority communities. The decrease in CO_2 in the no-2012 robustness check also becomes statistically significant, but this heterogeneity is not economically important because CO_2 is uniformly mixing.

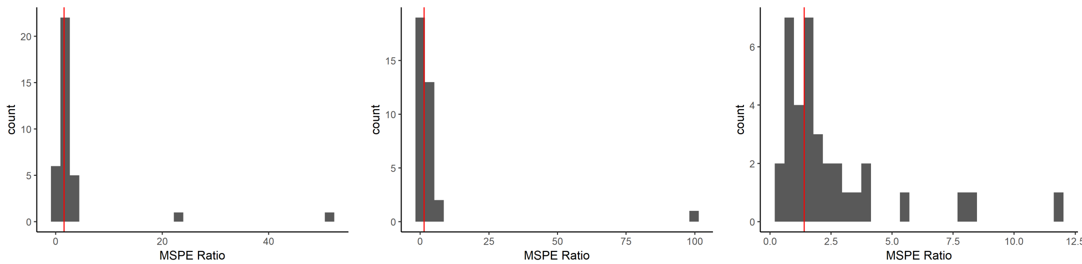
Lastly, Table A11 and Table A12 show how the key results vary with the choice of the number of nearest neighbors. The results are likewise qualitatively similar to those presented in the body of chapter 2.

TABLE A1.
Synthetic Control Weights

Full Sample						Low Income Only					
NO_x		SO_x		CO_2		NO_x		SO_x		CO_2	
State	Weight	State	Weight	State	Weight	State	Weight	State	Weight	State	Weight
NV	0.1145	AZ	0.04616	AZ	0.0976	CT	0.0037	AZ	0.0082	AL	0.0364
OR	0.1284	DE	0.0004	KY	0.0008	GA	0.0017	CT	0.0002	OH	0.201
RI	0.1183	GA	0.0002	NH	0.0001	KY	0.0321	IL	0.2132	OR	0.0407
WY	0.6388	ID	0.0062	NY	0.0002	LA	0.0112	LA	0.0007	UT	0.498
		IL	0.0004	OH	0.026	MS	0.0221	ME	0.2031	WA	0.2238
		KY	0.4247	OR	0.1287	NE	0.632	NV	0.0067		
		MA	0.0003	PA	0.0599	NJ	0.0029	OH	0.0067		
		ME	0.1018	UT	0.3431	NM	0.0013	OR	0.1908		
		MT	0.0144	WY	0.3435	NV	0.0813	PA	0.109		
		NV	0.0188			OR	0.1347	SC	0.0001		
		OH	0.0002			PA	0.0001	WA	0.0043		
		OR	0.1075			WI	0.0772	WI	0.0436		
		PA	0.0007								
		WA	0.0239								
		WI	0.2534								
		WY	0.0002								
High Minority Share Only						Both EJ Groups					
NO_x		SO_x		CO_2		NO_x		SO_x		CO_2	
State	Weight	State	Weight	State	Weight	State	Weight	State	Weight	State	Weight
CT	0.3393	AL	0.1608	AL	0.1435	IL	0.0007	AL	0.5375	AL	0.4027
FL	0.0186	AZ	0.0204	CT	0.2646	IN	0.0547	AZ	0.0176	AZ	0.1938
IL	0.0999	CT	0.0001	IL	0.0003	KS	0.0079	CT	0.0002	CT	0.0641
IN	0.0722	DE	0.4385	KS	0.3597	KY	0.0019	IL	0.0435	FL	0.0092
NV	0.4699	IL	0.4385	MI	0.0064	MS	0.0165	IN	0.0423	IN	0.0272
		IN	0.0502	SC	0.0985	NM	0.0324	KS	0.0001	MI	0.007
		KS	0.0001	UT	0.1271	NV	0.0015	KY	0.012	NY	0.2013
		KY	0.0256			NY	0.6815	MI	0.0002	SC	0.0945
		SC	0.0177			SC	0.0574	MS	0.0001	UT	0.0002
		TX	0.03618					NJ	0.0002		
		UT	0.0001					NM	0.0711		
								NV	0.1789		
								SC	0.0363		
								TX	0.0594		
								UT	0.0002		

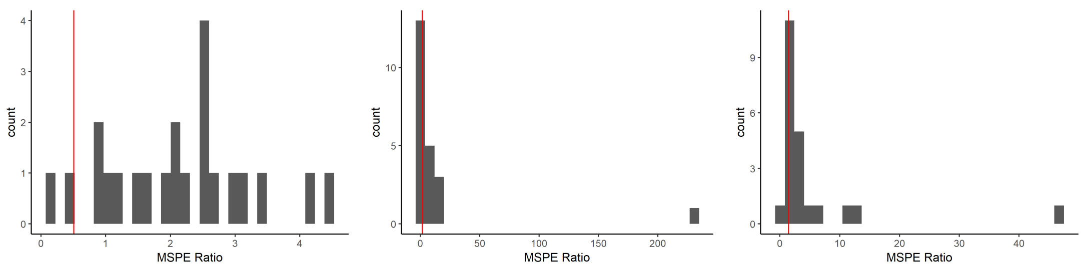
Notes: This table shows the weights used to construct the synthetic California used as a counterfactual. Weights were chosen to minimize the mean square prediction error for the pre-treatment period.

Low-Income Communities Only



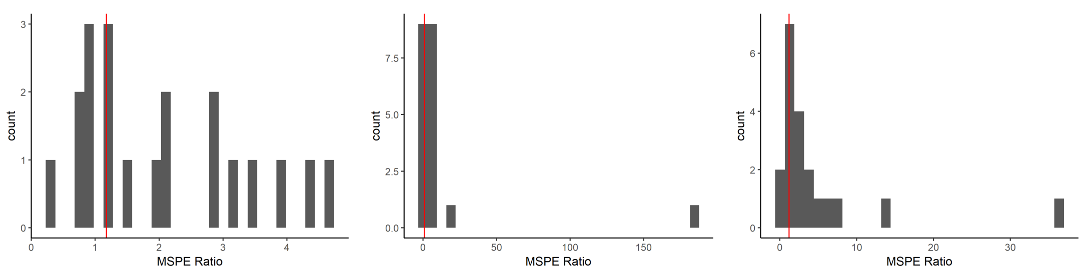
(a) NOx (b) SO_x (c) CO_2

High-Minority-Share Communities Only



(d) NOx (e) SO_x (f) CO_2

Low-Income *And* High-Minority-Share Communities Only



(g) NOx (h) SO_x (i) CO_2

FIGURE 29.
Treatment Effect Heterogenity: MSPE Ratios

Notes: This figure shows mean square prediction (MSPE) ratios for various subsamples of the data. See Figure 5 for more detail.

TABLE A2.
ATT Estimates from Alternative Sample Definition Where Plants That Do Not Appear Every Year Are Dropped

Donor Pool	NO_x	SO_x	CO_2	N-Treat	N-Control
Western U.S.	-24.1	-0.859	-19.9	85	68
	(17.9)	(0.553)	(40.3)		
Western U.S. (With Bias Adjustment)	-25.4	-0.834	-38.0	85	68
	(17.8)	(0.514)	(45.5)		
Entire U.S.	-24.6^{**}	-1.61^{**}	-81.8^{**}	85	623
	(11.0)	(0.797)	(33.7)		
Entire U.S. (With Bias Adjustment)	-31.3^{***}	-1.43	-93.4^{***}	85	623
	(11.3)	(1.44)	(35.2)		

Notes: This table shows parameter estimates from a nearest-neighbor matched difference-in-difference estimator under a different sample definition. All plants that do not appear every year in the sample are dropped to balance the panel. See the footnote for Table 3 for further information.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A3.
Heterogeneous Treatment Effects from Alternative Sample Definition Where Plants That Do Not Appear Every Year Are Dropped

	Emissions (Tons/Year)					
	Western US NOx	Western US SOx	Western US CO2	Entire US NOx	Entire US SOx	Entire US CO2
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−1,670	−18.8	3,660	3,010	921	2,870
	(2,570)	(80.6)	(6,270)	(2,240)	(2,250)	(2,870)
Proportion Minority	47.0	0.74	−88.1	−33.9	39.8	-138^{**}
	(118)	(2.62)	(238)	(60.5)	(67.7)	(59.2)
Per-Capita Income	−2.44	−0.11	−12.73	−0.15	0.82	-2.95^{*}
	(3.51)	(0.10)	(8.56)	(1.90)	(1.59)	(1.77)
Treat× Proportion Minority	−72.3	−0.80	165	124	38.5	120
(Adverse EJ $\implies coef > 0$)	(111)	(3.55)	(276)	(91.5)	(91.9)	(118)
Treat× Per-Capita Income	3.43	0.09	18.9^{*}	2.78	1.45	1.79
(Adverse EJ $\implies coef < 0$)	(3.99)	(0.11)	(10.19)	(3.33)	(2.81)	(3.30)
Constant	9.22	2.69	441	36.3	32.7	188^{***}
	(83.7)	(3.53)	(296)	(78.4)	(75.5)	(65.5)
Observations	153	153	153	708	708	708

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: This table shows estimates of heterogeneous treatment effects using the alternative sample definition discussed in the footnote for Table A2. See Table 4 for additional information.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A4.
ATT Estimates From Matching Estimator Including Weather Variables

Donor Pool	NO_x	SO_x	CO_2	N-Treated	N-Controls
Western U.S.	−24.1	−0.312	−15.2	85	68
	(17.4)	(0.392)	(41.1)		
Western U.S. (With Bias Adjustment)	−26.3	−0.377	−30.7	85	68
	(16.5)	(0.428)	(47.8)		
Entire U.S.	−19.3	−0.735	−33.5	85	646
	(15.1)	(0.910)	(42.8)		
Entire U.S. (With Bias Adjustment)	−27.5*	−0.586	−50.9	85	646
	(14.6)	(1.84)	(47.1)		

Notes: This table shows estimates of the average treatment effect-on-the-treated (ATT) where pre-treatment weather variables are included in the matching criteria. See the footnotes to table 3 for further details.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A5.
Heterogeneous Treatment Effects: Weather Variables Used For Matching

	Emissions (Tons/Year)					
	Western US NOx	Western US SOx	Western US CO2	Entire US NOx	Entire US SOx	Entire US CO2
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−2,030	−22.7	3,390	3,550	2,610	9,780**
	(2,506.28)	(76.03)	(6,011.52)	(4,470.08)	(3,934.19)	(4,884.31)
Proportion Minority	74.1	1.08	−101	−41.2	28.6	−158***
	(110)	(2.72)	(244)	(56.2)	(59.3)	(57.7)
Per-Capita Income	−0.74	−0.15	−11.02	0.30	0.73	−3.15*
	(4.55)	(0.15)	(9.09)	(1.73)	(1.47)	(1.71)
$Treat\times$ Proportion Minority	−87.7	−0.95	153	145	107	402**
	(109)	(3.35)	(265)	(183)	(161)	(200)
$Treat\times$ Per-Capita Income	1.60	0.13	16.4	5.58	4.15	9.84*
	(5.12)	(0.18)	(10.30)	(5.18)	(4.57)	(5.07)
Constant	−41.1	3.52	372	19.1	28.1	175***
	(106)	(4.73)	(303)	(70.6)	(67.4)	(62.3)
Observations	153	153	153	731	731	731

Notes: This table shows estimates for heterogenous treatment effects when pre-treatment weather variables are included in the matching criteria. See the footnote to Table 4 for additional information.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A6.
ATT Estimates: Matching Criteria Includes State RPS Requirements

Donor Pool	NO_x	SO_x	CO_2	N-Treated	N-Control
Western U.S.	−26.5	−0.866	−20.0	85	68
	(19.2)	(0.598)	(43.2)		
Western U.S. (With Bias Adjustment)	−28.0	−0.841	−38.7	85	68
	(19.3)	(0.560)	(49.0)		
Entire U.S.	−16.9	−1.10	−44.2	85	646
	(16.6)	(1.25)	(51.9)		
Entire U.S. (With Bias Adjustment)	−24.6	−1.19	−60.8	85	646
	(16.8)	(1.31)	(54.1)		

Notes: This table shows estimates of the average treatment on the treated (ATT) for the California cap-and-trade system that use the change in Renewable Portfolio Standard stringency, measured by changes in renewable energy certificate (REC) obligations, over the sample in the matching criteria.

TABLE A7.
Heterogeneous Treatment Effects: Matching Includes State RPS Standards

	Emissions (Tons/Year)					
	Western US NOx	Western US SOx	Western US CO2	Entire US NOx	Entire US SOx	Entire US CO2
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−2,970	−77.5	3,180	3,880	1,410	542
	(3,150)	(73.4)	(6,220)	(3,680)	(3,560)	(5,300)
Proportion Minority	82.4	0.88	−83.1	−16.9	37.5	-103^{*}
	(125)	(2.51)	(215)	(53.2)	(56.7)	(55.9)
Per-Capita Income	−1.50	−0.12	−10.85	0.47	0.94	−1.70
	(3.34)	(0.09)	(8.13)	(1.68)	(1.39)	(1.65)
$Treat\times$ Proportion Minority	−129	−3.40	143	159	58.1	24.2
	(137)	(3.22)	(273)	(150)	(146)	(217)
$Treat\times$ Per-Capita Income	2.84	0.02	16.35	4.92	2.76	0.67
	(3.79)	(0.10)	(10.34)	(4.70)	(4.27)	(4.83)
Constant	−14.2	2.76	341	13.9	22.1	130^{**}
	(84.9)	(2.90)	(251)	(66.3)	(61.7)	(57.1)
Observations	153	153	153	731	731	731

Notes: This table shows heterogeneous treatment effects for a matching estimator that includes state Renewable Portfolio Standards. See the footnotes to Table A6 and Table 4 for further infromation.

TABLE A8.
ATT Using The Propensity Score for Matching

DonorPool	NO_x	SO_x	CO_2	N-Treated	N-Control
Western U.S.	−22.5	−0.573	-93.1^{**}	85	68
	(15.0)	(0.435)	(31.8)		
Western U.S. (With Bias Adjustment)	-27.4^{*}	-0.735^{**}	-108^{**}	85	68
	(14.3)	(0.310)	(49.5)		
Entire U.S.	-34.6^{***}	-1.85^{***}	-143^{***}	85	646
	(12.9)	(0.554)	(28.2)		
Entire U.S. (With Bias Adjustment)	-34.3^{**}	-1.96^{**}	-148^{***}	85	646
	(13.5)	(0.776)	(43.2)		

Notes: This table shows results from a matched difference-in-difference estimator where the nearest-neighbors are found using the propensity score instead of the Mahalanobis norm.

TABLE A9.
Heterogeneous Treatment Effects: Propensity Score Matching

	Emissions (Tons/Year)					
	Western US NOx	Western US SOx	Western US CO2	Entire US NOx	Entire US SOx	Entire US CO2
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−3,400	−153*	−9,110	2,330	446	4,530
	(2,940)	(85.0)	(5,710)	(3,050)	(2,650)	(3,910)
Proportion Minority	157	2.58	200	−18.7	50.3	−119**
	(135)	(1.57)	(172)	(55.4)	(59.9)	(56.6)
Per-Capita Income	−2.42	−0.04	0.39	0.74	1.07	−2.02
	(3.21)	(0.07)	(5.27)	(1.80)	(1.51)	(1.80)
$Treat\times$ Proportion Minority	−148	−6.70*	−398	95.8	18.6	188
	(128)	(3.74)	(251)	(124)	(108)	(160)
$Treat\times$ Per-Capita Income	1.52	−0.04	−3.57	2.74	2.37	4.58
	(3.65)	(0.11)	(6.74)	(3.82)	(3.37)	(3.80)
Constant	−19.8	−0.32	88.3	−17.3	5.13	107*
	(101)	(2.83)	(189)	(67.1)	(64.9)	(59.9)
Observations	153	153	153	731	731	731

Notes: This table shows estimates of the heterogeneous treatment effects using the matches obtained from propensity score matching obtained from the estimator described in Table A8.

TABLE A10.
Robustness Table for Heterogeneous Treatment Effects

	Dependent variable:											
	Entire US			No Western States			No 2012			Post Closing		
	NO_x	SO_x	CO_2	NO_x	SO_x	CO_2	NO_x	SO_x	CO_2	NO_x	SO_x	CO_2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$Treat$	2,990	913	2,910	3,840	1,090	4,390	3,510	1,560	7,220**	780	−1,330*	−5,170
	(2,180)	(2,190)	(2,780)	(2,640)	(2,690)	(3,070)	(2,270)	(2,820)	(3,250)	(2,120)	(798)	(3,500)
$ProportionMinority$	−26.9	39.8	−141**	−34.3	43.7	−154**	−46.7	45.8	−166**	−13.8	38.0	−40.5
	(59.7)	(64.9)	(59.3)	(66.9)	(72.3)	(65.5)	(62.3)	(80.2)	(64.7)	(57.5)	(23.3)	(55.2)
$PerCapitaIncome$	0.13	0.90	−2.73	0.34	1.05	−2.60	−0.08	1.06	−3.08	1.11	0.91	−1.66
	(1.86)	(1.54)	(1.75)	(1.99)	(1.67)	(1.86)	(1.96)	(1.93)	(1.94)	(1.75)	(0.66)	(1.58)
Treat× **ProportionMinority**	123	38.0	121	155	44.6	179	144	64.9	298**	32.0	−54.3*	−208
(Adverse EJ $\implies coef > 0$)	(88.8)	(89.3)	(114)	(106)	(108)	(124)	(92.5)	(115)	(133)	(86.6)	(32.6)	(143)
Treat× **PerCapitaIncome**	2.71	1.33	1.81	3.53	1.95	3.66	3.66	2.12	4.87	−0.02	−1.42	−0.90
(Adverse EJ $\implies coef < 0$)	(3.30)	(2.79)	(3.31)	(3.70)	(3.22)	(3.43)	(3.50)	(3.61)	(3.67)	(2.77)	(1.00)	(3.24)
Constant	27.6	30.8	175***	22.7	28.2	181***	49.0	51.2	192***	−32.6	−44.1	123**
	(76.5)	(72.7)	(63.6)	(82.1)	(79.3)	(68.2)	(80.0)	(90.1)	(70.2)	(72.9)	(28.4)	(60.6)
Observations	731	731	731	663	663	663	731	731	731	731	731	731

Notes: This table gives estimates of heterogeneous treatment effects for each of the robustness checks described in Table 5.

TABLE A11.
ATT Robustness to Choice of Number of Neighbors

	M=2	M=3	M=4	M=5
Western U.S. NO_x	-27.5	-25.4	-26.3	-26.1^{*}
	(17.8)	(17.8)	(16.4)	15.7
Western U.S. SO_x	-0.961	-0.834	-0.747	-0.687*$
	(0.587)	(0.514)	(0.461)	(0.411)
Western U.S. CO_2	-31.9	-38	-43.1	-43.1
	(45.8)	(45.5)	(45.4)	(40.9)
Entire U.S. NO_x	-28.2*	-29.8***	-30.4***	-31.4***
	(11.7)	(11.2)	(11.0()	(12.0)
Entire U.S. SO_x	-1.65	-1.43	-1.28	-1.40
	(1.66)	(1.44)	(1.35)	(1.34)
Entire U.S. CO2	-71.3*	-91.4***	-79.4**	-81.8**
	(38.5)	(35.5)	(32.5)	(33.5)

Notes: This table shows how estimates change with the number of plants used to construct the counterfactual outcome.

TABLE A12.
Robustness to Number of Nearest Neighbor's: Heterogeneous Treatment Effects

	$M = 2$	$M = 3$	$M = 4$	$M = 5$
NO_x Western U.S.				
$Treat \times ProportionMinority$	−126.2	−178.5	−121.1	−98.1
	(150.6)	(138.9)	(159.1)	(122.6)
$Treat \times PerCapInc$	4.327	3.863	2.439	3.683
	(3.926)	(3.665)	(3.355)	(3.771)
NO_x Entire U.S.				
$Treat \times ProportionMinority$	113.8	117.4	131.0	108.1
	(89.75)	(109.1)	(111.2)	(110.0)
$Treat \times PerCapInc$	1.325	1.625	2.397	1.594
	(3.216)	(3.432)	(3.535)	(3.390)
SO_x Western U.S.				
$Treat \times PropMinority$	1.231	−1.434	−0.076	−1.068
	(2.870)	(3.065)	(2.997)	(3.574)
$Treat \times PerCapInc$	0.036	0.059	0.012	0.083
	(0.074)	(0.081)	(0.085)	(0.139)
SO_x Entire U.S.				
$Treat \times PropMinority$	−77.54	−86.49	−44.56	−60.18
	(105.7)	(108.2)	(119.4)	(106.2)
$Treat \times PerCapitaIncome$	−1.876	−2.008	−1.645	−1.072
	(2.725)	(2.925)	(2.788)	(2.830)

Notes: For the models of heterogenous treatment effects, this table shows how the estimates change with the number of plants used to construct the counterfactual outcome.

APPENDIX B

APPENDIX TO CHAPTER 2

Survey Design

Basic components of the survey

Oath-taking. The survey begins with an "oath-taking" page, where the respondent is asked to confirm that they will "thoughtfully provide" their best answers to each question in the survey.

Social priorities. Respondents are asked to check their three highest personal priorities from a randomly ordered list that includes "Conserve natural resources," "Improve education,", "Improve public health," "Prevent climate change," "Prevent violence, crime," and "Reduce poverty, hunger."

Background information. Respondents are reminded about fossil fuels and greenhouse gases of human origin, that almost all climate scientists agree that emissions from human activities are causing Earth's climate to change, but that some people remain unconvinced. They are then quizzed about the geographic scope of carbon impacts from a university (and incorrect perceptions are corrected). Carbon pricing is introduced as an incentive to reduce carbon emissions that will simultaneously create a revenue stream. Existing government-run carbon-pricing schemes are reviewed, and respondents are quizzed about their awareness of discussions in Washington and Oregon about possible carbon-pricing programs (including state-wide cap-and-trade). Internal carbon-pricing programs by roughly 500 individual U.S. businesses are outlined, along with the reasons firms give for embarking on these programs (followed by a quiz about which of these reasons were included on the previous page). Respondents are reminded that the benefits of carbon emissions reductions are global, but a number of ways in which a university might benefit from instituting such a program are suggested. It is noted that these effects are not guaranteed, but are possibilities.

A university carbon-pricing program. The survey reviews how it would be difficult to price all carbon emissions from a university, so the focus would be on energy use in buildings and on university-sponsored air travel. It is noted that no specific program is currently being proposed, so that the survey will describe a range of different possible programs, each described in terms of the overall reduction in net carbon emissions, how the costs would be shared, how the money raised by the program would be spent, and what would be the unavoidable cost to the individual. We emphasize that the programs are designed so that some programs are small, others are moderate, and some may seem like just too much. We then use the individual's own specific variant of "Program A" as a training example, as we explain in detail how to interpret the program summaries that are used in each choice set the individual will consider. First, however, respondents are reminded that they will always have the option to vote for "No Program." Reasons are suggested why reasonable people may choose that alternative in some or all cases. The programs are also described as remaining in effect indefinitely.

However, if the federal government or the state implement a mandatory carbon-pricing program, the university's program would be re-evaluated.

Review of the specific university's circumstances. Before the choice tutorial section begins, respondents are reminded about the basic facts of their university's carbon footprint, including the number of students and the number of faculty and staff. The most recent estimate of the university's carbon footprint (not counting the carbon content of other purchased products) is estimated in metric tons of carbon dioxide equivalent emissions. Building energy use and air travel are noted explicitly, in terms of the total annual emissions and the percent of total university-related carbon emissions.

Choice set tutorial. Due to randomization at the individual level, every respondent has a unique set of programs making up their choice sets. We use the first alternative in the first choice set to illustrate how the respondent is asked to interpret the information in each choice set "summary table." The benefit information appears first, by itself.

The second feature of every internal carbon-pricing program concerns information about how costs are shared. For public universities, these costs are shared four ways, and this information is displayed as an additional set of four rows in the table. Each share, as it is discussed on its own page, is highlighted in yellow in the table. Option additional information is provided in pop-up "modals" that appear superimposed on the main screen, so that respondents do not have to change browser windows.[1] Pre-testing of the survey identified a couple of points of potential confusion on the part of respondents. For example, some thought that air travel fees would also be paid by foreign students when they went home to visit their families. A quiz question checked for this mis-perception and corrected it if necessary. Other pre-test subjects were confused about whether they could avoid the cost of the carbon-pricing program if the share borne via student/employee fees was zero. If they believe this, they are reminded that everyone affiliated with the university would bear costs via building energy use fees, even if they were not charged directly.

The third feature of each program is a summary of how the revenues raised by the program are to be spent. The dominant form of spending is on internal carbon-reduction projects, and several possible examples are outlined. Another use would be for a variety of academic programs, for undergraduates, graduate students and/or faculty, for teaching or research. The third potential use of the revenues is described as "to pay for offsets." Offsets are explained, and respondents are asked to assume that there are "no legal or political considerations that would prevent your institution from spending money on high-quality verifiable carbon offsets."

The final program feature is the cost per year, "all told, after you have done what you can to adapt to the program." Respondents are asked to assume that they will pay these costs for as long as they remain with the university, and

[1] The survey was designed to be feasible on the screen of a mobile device, as well as on a computer or tablet.

are reminded that these may be direct fees or indirect costs that filter down to everyone who benefits from the use of campus buildings, including residence halls, or via higher air-travel costs for other programs that end up affecting you if they are covered by higher fees and/or reductions in other services.

The final pages of the tutorial section caution people that they should fully consider their future expenses, and should think very carefully about what they would have to give up, if the program in question were to be put in place at their institution. This is the "cheap talk" component of the preparation for program choices. They are also reminded that the university plans to use the results of the study to help decide whether to implement a carbon-pricing program and, if so, what type. This is the "consequentiality" component of the preparation for program choices. Finally, respondents are reminded that they should consider each policy choice independently, as though the options in each choice scenario are the ONLY ones being offered. They should vote as they would if these were real and secret ballots, and they should feel free to vote "no" if the program(s) in question would be just too costly.

Choice tasks. The first choice task consists of just Program A versus No Program (replicating the attributes for Program A used in this respondent's tutorial section. The second task consists of just Program B versus No Program (with Program B's new set of attributes).

The third choice is a three-way choice between new Program C, new Program D, and No Program. If they choose either of Programs C or D, their next choice branches to a choice between the non-chosen Program alternative and No Program. Then each respondents is offered another three-way choice between new Program E, new Program F, and No Program, again with a followup question (if either of Programs E or F is chosen) between the non-chosen Program alternative and No Program.

If No Program is chosen in any of these choice sets, the respondent is asked for reasons why they preferred the No Program alternative. Some of these reasons are "economic" reasons why they preferred No Program (for example: "Program C would cost me more than I would want to pay," "I did not approve of the way the costs of Program C would be shared," "I did not approve of the ways the money from program C would be spent," "I did not believe that the benefit to the university of Program C justified its cost to me," "I did not believe that the global benefits of Program C justified its cost to me.". But one of the offered reasons suggests some form of scenario rejection: "The mix of features described for Program C did not seem believable." Respondents were given the opportunity to specify other reasons as well. Choices where an individual gave a reason for choosing No Program that suggested scenario rejection will cause those choices to be omitted from the analysis.

We made a conscious effort to reduce the burden of the survey for people who strongly object to carbon-pricing programs. Respondents who chose No Program in the first choice set were asked a follow-up question if they indicated that their reasons for choosing No Program included that the benefits to the university (or the global benefits) did not justify the cost. If they checked a box indicating that they "did not like Program A, but there might be some type of program, at some cost low enough for me, for which I could possibly vote "Yes," they were allowed to continue with the rest of the choice sets. But they were also given an opportunity to check instead that "Carbon-pricing programs are a BAD idea. It would not matter how the program is set up. I would not vote "Yes" for ANY carbon-pricing program!" These respondents were then skipped to the end of the choice tasks, and we will mark them as preferring "No Program" in all of the subsequent choice tasks. This strategy is designed to limit the attrition of anti-carbon-pricing respondents prior to the end of the survey.

Debriefing. After making their program choices, respondents were asked to think back and check those program attributes that were especially important to them. This information will help us assess attribute non-attendance. If a respondent voted for No Program in every choice set, they were given a list of reasons to consider why they might have chosen that way, including "These choice tasks were just too difficult for me to process." and "I am not convinced that climate change is actually happening." and "Even if climate change is actually happening, I don't believe that anything we do (or don't do) will make any real difference." Also offered were "I don't think universities produce enough carbon emissions to matter. Instead, heavy industries should be required to cut back," "I would be hurt by the effect of the program on my livelihood or the cost of my education," "I would be hurt by the effect of the program on the cost of university-paid air travel that is important to me."

Personal exposure to climate change impacts. Respondents are invited to indicate whether they have ever lived, for more than a few month in total, in places that are exposed to specific different types of climate-related risks (including "in a developing country with limited preparedness for natural disasters," where they are then subsequently asked whether this experience was a result of a study-abroad program). Respondents are then asked if they, or any close family members or friends, have been personally harmed to different degrees by weather-related hazards. They are then asked about their experience, if any, with specific extreme weather events over the last 12 months (to check for any "recency" effects).

Perceived researcher bias. Respondents were asked "Overall, the wording of this survey made it seem that the researchers conducting this study really wanted me to choose..." The options included "some carbon-pricing program, rather than No Program," "No Program, rather than some carbon-pricing program," "The best alternative for me, personally, based on all of the features of the programs," and "Not sure/couldn't tell." The goal in survey design is to have the majority of people choose one of the last two options.

Climate change attitudes. We included, at this point in the survey, a set of five questions about "global warming" developed by researchers at Yale University, for which there is existing evidence about the relative frequency of these climate attitudes in the general population of the U.S.

Sociodemographics. The survey collects information about gender, the respondents main role at the university (and any secondary roles), age, race, ethnicity, educational attainment, and employment status. Finally, we inquire about the respondent's political views (including an explicit "prefer not to say" option) and their household's income bracket.

Randomizations

The survey template is populated according to a set of "parameters" specific to the university. These parameters include strings to identify the university and its state, the total number of students, total number of faculty and staff, the year of the last carbon inventory (or approximate inventory), the estimated total emissions due to the operation of the university (not including carbon embodied in purchased inputs other than the fuel for the physical plant and transportation), the type of heating fuel, the carbon emissions related to district heating, the percent of emissions attributed to district heating, the carbon emissions due to air travel and the percentage of emissions due to air travel, and the nature of the incentive for survey participation.

Most universities will have basic demographic data on file for everyone affiliated with the university. If key variables are available from administrative data, and therefore do not need to be elicited from survey respondents, some respondent effort can be saved. Thus the parameters for the survey include indicators for whether there is available administrative data for gender, age, race, ethnicity and educational attainment.

Given that the shares of total percentage points of carbon emissions reduction must sum to one, and that the shares in which the proceeds of an internal carbon-pricing scheme might be spent must also sum to one, it was more difficult than usual to pursue a d-optimal design for the mix of attributes among the choice sets. We elected instead to randomize the portfolio of shares for each potential carbon-pricing program, and then to follow up by pairing these portfolios to eliminate pairs of programs where one program dominates the other by having both greater carbon-reduction benefits and lower cost. We wished to force respondents to trade off between basic benefits and costs. While it is possible that one program might dominate the other on these two dimensions, yet be less preferred because of its distributional consequences, we did not wish to risk too many of these likely easier choices.

The design options for the choice sets where as follows:

- Percentage point reductions in carbon emissions: 10, 15, 20, 25, 30, 35, 40, 45, 50
- Distribution of program costs:
  * Percent of program cost borne as student/employee fees: 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100
  * Percent of program cost borne as air travel fees: 0, 10, 20, 30, 40, 50
  * Percent of program cost borne as building energy fees: 0, 10, 20, 30, 40, 50, 60, 70, 80, 90, 100
  * Percent of program cost borne by the state's taxpayers: 0, 10, 20
- Distribution of program revenues (Spring 2018 Survey Wave):

    * Percent of revenues spent on internal carbon-reduction projects: 50, 60, 70, 80, 90, 90, 100, 100
    * Percent of revenues spent on academic programs: 0, 10, 20, 30
    * Percent of revenues spent on carbon offsets: 0, 10, 20, 30
  - Distribution of program revenues (Fall 2018 Survey Wave):
    * Percent of revenues spent on internal carbon-reduction projects: 20, 30, 40, 50, 60, 70, 80, 90, 90, 100, 100
    * Percent of revenues spent on academic programs: 0, 10, 20, 30
    * Percent of revenues spent on carbon offsets: 0, 10, 20, 30, 40 50

For the overall benefits of the program (percentage-point carbon reduction), one value is drawn randomly from the list. For the distribution of program costs, and also for the distribution of program benefits, the design algorithm draws one value randomly from each list in the set and calculates whether the total sums to 1.0. If yes, that mix of shares is accepted as viable; if no, another set of shares is randomly drawn and their total is calculated. The process continues until a valid set of shares is produced.[2]

For complete orthogonality among program attributes, it might seem preferable to draw the cost of each program independently from that program's

[2]In the Spring 2018 design, we specifically limited the possible shares to that range of values most likely to be relevant in any prospective real program for the university in question. In the Fall 2018 design, we extended the range used for the distribution of program revenues, to see if these more-extreme values induced a measurable reaction among respondents who received these designs. In the Spring 2018 design, people were not particularly responsive to expenditure on carbon offsets, and only students appeared to respond systematically to expenditure on academic programs.

attributes. However, we wished to avoid scenario rejection due to implausible combinations of program benefits and program costs. Thus we constructed program costs that would be systematically related to program benefits, but also incorporate a uniformly distributed random component. The random component for costs is drawn from the distribution: -3, -2, -1, 0, 1, 2, 3. Unavoidable program costs per year (to the individual) are then constructed from a formula that includes an intercept (set at 40 for students and 20 for employees), a cost per percentage-point reduction in carbon emissions (set at 3.0), and a scale factor that multiplies the random component for costs (set at 14). After randomization, any cost per year less than 10 is set to 10, and any cost greater than 250 is set at 250.

The number of programs to generate is based on the number of email addresses in the sample in question. While only three pairs of programs are eventually used in each person's survey, we build ten two-policy choice sets per person and utilize the first three pairs of programs that do not fail the inclusion criteria. These criteria include the "no dominance in terms of both higher carbon emissions and lower costs for one of the alternatives in a pair" and "the difference in costs between the two alternatives should be at least $5 per year." (Costs are rounded to the nearest whole dollar.)

Response-nonresponse Modeling

When respondents can choose whether or not to begin or complete a survey when they are invited to participate (i.e. in almost every voluntary survey context), it is important to question whether the sample of responses that is sufficiently complete to be included in estimation can be argued to be representative of the population of interest. Any given invitee's propensity to show up in the final estimating sample may be correlated with the value of the outcome variable of interest for that person—in this case, willingness-to-pay for carbon reductions via an internal carbon pricing program. It is vitally important to assess whether observable individual characteristics, including proxies for the environment within which the individual's preferences for carbon-pricing programs may have evolved, appear to have any bearing on the individual's decision about whether to participate fully in the survey.

The set of invitees was randomly drawn from the student sample and from the employee sample, albeit at slightly different rates from each group. In this study, due in part to the survey's launch just before the end of the Spring quarter, response rates were only on the order of 10 percent. This may be due in part to the modest incentive payment for each response (a five-dollar electronic gift card for the campus shop). A response rate this low does not necessarily imply that the sample will be non-representative. But nothing can be assumed, ex ante.

To model response/nonresponse propensities, it is necessary to have common explanatory variables available for both respondents and non-respondents.

By prior arrangement with the university's Office of Institutional Research, we designed an elaborate procedure to connect all invited respondents to administrative data held by the university and to zip-code level information associated with employees via their current zip-code and with students via the zip-code of the high-school they attended prior to their admission to the university. Our goal with these zip-code level variables is to proxy for the "neighborhood" in which the individual may have developed their preferences with respect to climate change policies and carbon program. By zip code, we connect each individual to Census data from the American Community Survey (using the census-tract-to-zip-code crosswalk from Department of Housing and Urban Development). We also connect each zip code to David Leip's US Election Atlas, with its election results at the county level for every county in the U.S., for the 2012 and 2016 Presidential elections. Finally, we connect the centroid of each zip code to its corresponding Congressional District and merge in data from the League of Conservation Voters to capture the voting record of that district's representative on environmental legislation.

Our goal in response/nonresponse modeling is to capture systematic heterogeneity in each invited respondent's propensity to provide a completed survey for our use in estimation. To this end, we specify an ordinary probit model, with the binary outcome defined as 1 = completed survey and 0 = nonresponse or incomplete survey. We have explored two strategies for determining a parsimonious specification for the response/nonresponse model: (1) a conventional binary probit, subjected to backwards stepwise deletion of explanatory variables that are not statistically significant, and (2) LASSO models that employ a penalty function that help to zero-out the coefficient on explanatory variables that are both statistically insignificant and which contribute little to explaining variation in the outcome.

Binary probit with stepwise deletion

It would be ideal to be able to estimate the response propensity model simultaneously with the program choice models described in the body of this paper. As yet, there is no available full-information maximum likelihood estimator that can accomplish this task, either for conventional conditional logit specifications or when random-parameters mixed-logit or latent-class models are in play. Instead, we take a crude approach to assessment and correction of potential nonresponse bias in our estimated preference parameters.

We estimate an ad hoc probit specification that uses all available variables to explain systematic differences in response/nonresponse propensities. These propensities are interpreted to be the fitted "index" for the probit model. We then calculate the average of these fitted index values across all invited respondents (using exogenous weights to control for the different proportions of students and employees that were invited). For each person, we then calculate the deviation of their individual response propensity from this overall average in the target population (from which the invited sample was drawn at random). Then we estimate our choice models using only the sample of respondents. However, we allow each basic preference parameter in these models to vary systematically with the deviation of that individual's response propensity from the population mean response propensity. By including these controls, it is possible, subsequently, to simulate what would have been the basic preference parameters had everyone in the estimating sample had a response propensity exactly equal to the mean among the invited respondents drawn as a stratified random sample from the university's overall population. This "counterfactual" holds when everyone's "deviation from the mean response propensity" is exactly zero. As a practical matter, we can just ignore the coefficients on these deviations and pay attention to the "base" coefficients, which hold when all of the deviations are set to zero.

LASSO models

In the presence of a large number of variables, there is a danger of finding statistical relationships between variables that exist merely due to chance and do not reflect the actual data generating process. One approach to limit over-fitting is to use regularization, a technique where a penalty is assigned to the inclusion of variables. This penalty decreases the model variance due to variable selection and thus will produce lower levels of prediction error than simpler methods of mode selection.

For the response/non-response model we use a form of regularization known as Lasso.[3] The probability of response is modeled by estimating a logit with a penalty term in the likelihood function equal to the sum of the absolute value

[3]Lasso is an abbreviation for Least Absolute Shrinkage and Selection Operator

of each coefficient. We therefore want to find a vector of β's that maximize the following log-likelihood function

$$\sum_{i=1}^{N} \left[y_i(\beta x_i) - log(1 + e^{\beta x_i}) \right] - \lambda \sum_{j=1} K|\beta_j|$$

where y_i is equal to one if the individual responded to the survey and is zero otherwise and λ is a tuning parameter that determines the level of penalty imposed on coefficient size.

The use of an absolute value specification of the penalty function has the advantage of making corner solutions likely, which means that in practice estimated coefficients are zero and variables are dropped from the model. Thus lasso selects the variables which are most predictive of response status and drops those with limited predictive power.

We select the value of λ using cross-validation techniques.[4]. A candidate grid of λ values is specified and the sample is divided into several subsets. Each subset is "held-out" of the sample and the model is estimated on the remaining data for each value of λ. A measure of model fit[5] is then computed using each holdout sample. The value of λ we use for the estimates in paper is the one with the best average score across the various holdout samples.

[4]We estimate all lasso models using the R package glmet

[5]In this case the deviance, equal to two times the negative of the log-likelihood function

Choice Model Parameter Estimates

TABLE A13.
How surviving interaction terms affect utility parameter estimates (weighted, sw probit selection model); base case=zero value for nuisance interactions; persons= 965, choices= 5084 (Omitted categories: those not included in the specification, by factor)

	Base (homog.)	+Base × selected (Minimal)	pruned
1=chosen alt			
Unavoid cost to resp. (22 to 232)	-0.0126*	-0.00869***	-0.00876***
	(0.00664)	(0.00112)	(0.00112)
× demeaned resp propensity	0.00105		
	(0.00106)		
Pct-point C reduction (10 to 50)	0.0335	0.0640***	0.0568***
	(0.0253)	(0.0165)	(0.0170)
× Pct-point C reduction (10 to 50)		-0.000347*	-0.000340*
		(0.000182)	(0.000181)
× zip pr Asian alone (.034)		0.378***	0.381***
		(0.0969)	(0.0970)
× zip pr Native HI, etc., alone (.002)		-1.080**	-1.027*
		(0.514)	(0.526)

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Table A13 – continued from previous page

× zip pr Moved; dif cty, sme st (.041)	-0.326**	-0.299**
	(0.153)	(0.152)
× zip pr 25+, grad/prof degr (.114)	-0.199***	-0.191***
	(0.0719)	(0.0720)
× zip pr Hsng incompl plumb (.002)	-1.094*	-1.197*
	(0.634)	(0.635)
× zip pr Cmt 60-89 min (.042)	0.330*	0.340*
	(0.180)	(0.179)
× 1=empl: Arch, Allied Arts (.004)	0.135***	
	(0.0325)	
× 1=empl: Business (.026)	0.0222	0.0219
	(0.0141)	(0.0142)
× 1=empl: Library (.037)	-0.0316**	-0.0310**
	(0.0133)	(0.0132)
× 1=dept: Gen soc sci (.012)	-0.0341*	
	(0.0177)	
× 1=dept: Jour/Comm (.045)	-0.0206*	-0.0203

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		(0.0124)	(0.0125)
× 1=12 mos: Severe winter (.136)		-0.0149***	-0.0141**
		(0.00565)	(0.00569)
× 1=perceive pro-ICP bias (.443)		-0.0100**	-0.00986**
		(0.00457)	(0.00457)
× 1=perceive anti-ICP bias (.028)		-0.0207*	-0.0213*
		(0.0119)	(0.0119)
× demeaned hhld inc ('000) if known		0.0000763**	0.0000647*
		(0.0000300)	(0.0000341)
× demeaned resp propensity	-0.00318		
	(0.00400)		
Cost shr air trav fees (0 to .5)	-0.00190	-0.0853***	-0.0816***
	(0.00910)	(0.0306)	(0.0302)
× Cost shr air trav fees (0 to .5)		-0.000444***	-0.000433***
		(0.000105)	(0.000104)
× zip pr Inc lt 10K (.07)		0.284***	0.287***
		(0.104)	(0.104)

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× zip pr Inc 75K-100K (.113)	0.393***	0.375***
	(0.125)	(0.122)
× zip pr Hsng incompl plumb (.002)	0.737	0.754
	(0.480)	(0.483)
× zip pr Cmt 15-19 min (.195)	0.266***	0.257***
	(0.0787)	(0.0780)
× 1=empl: Athletics (.022)	-0.0223*	-0.0210*
	(0.0129)	(0.0127)
× 1=empl: Arch, Allied Arts (.004)	-0.279***	
	(0.0586)	
× 1=empl: Business (.026)	-0.0133	-0.0127
	(0.00867)	(0.00862)
× 1=empl: Facilities (.029)	-0.0268*	-0.0262*
	(0.0140)	(0.0141)
× 1=empl: Health, Counsel. (.019)	0.0226	
	(0.0154)	
× 1=12 mos: Tornado (.035)	-0.0142*	-0.0143*

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Table A13 – continued from previous page

		(0.00809)	(0.00823)
× 1=12 mos: Severe winter (.136)		0.0113***	0.0105**
		(0.00422)	(0.00424)
× demeaned resp propensity	0.00229		
	(0.00144)		
Cost shr bldg en fees (0 to 1)	0.0106	0.0137***	0.0140***
	(0.00669)	(0.00399)	(0.00398)
× zip pr Cmt 30-34 min (.096)		-0.0715**	-0.0731**
		(0.0364)	(0.0363)
× zip pr Heat solar (.001)		1.273**	1.315**
		(0.625)	(0.634)
× 1=empl: Courtesy appt (.01)		-0.0194***	
		(0.00682)	
× 1=empl: Arch, Allied Arts (.004)		-0.228***	
		(0.0438)	
× 1=empl: Design (.047)		-0.0134***	-0.0133***
		(0.00442)	(0.00446)

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× 1=empl: Music and Dance (.017)		0.0326***	
		(0.00978)	
× 1=empl: Health, Counsel. (.019)		0.0171*	
		(0.00916)	
× 1=dept: Jour/Comm (.045)		-0.0108**	-0.0105*
		(0.00545)	(0.00546)
× 1=somew/very liberal (.683)		0.00470*	0.00459*
		(0.00246)	(0.00243)
× demeaned resp propensity	-0.000248		
	(0.00105)		
Cost shr taxpayrs (0 to .2)	0.0228	0.273***	0.262***
	(0.0166)	(0.0770)	(0.0756)
× zip pr Nonfamily hhld (.45)		-0.0875*	-0.0748
		(0.0521)	(0.0516)
× zip pr Inc 100K-150K (.114)		-0.578***	-0.543**
		(0.215)	(0.212)
× zip pr Inc 150K-200K (.041)		1.056**	1.013**

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	(0.440)	(0.438)
× zip pr Cmt 5-9 min (.149)	-0.270*	-0.262*
	(0.154)	(0.152)
× zip pr Cmt 15-19 min (.195)	-0.547***	-0.530***
	(0.198)	(0.195)
× zip pr Cmt 40-44 min (.022)	-1.311**	-1.312**
	(0.556)	(0.551)
× zip pr Cmt 60-89 min (.042)	-0.778**	-0.767**
	(0.314)	(0.309)
× 1=empl: Athletics (.022)	-0.0663**	-0.0565*
	(0.0321)	(0.0291)
× 1=empl: Arch, Allied Arts (.004)	-1.027***	
	(0.203)	
× 1=empl: Design (.047)	-0.0385**	-0.0392**
	(0.0190)	(0.0191)
× 1=empl: Music and Dance (.017)	0.0400	
	(0.0252)	

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× 1=empl: UGS (.022)		0.0628***	0.0561***
		(0.0182)	(0.0186)
× 1=stu: Design (.067)		0.0305**	0.0278*
		(0.0153)	(0.0153)
× 1=stu: Other (.083)		0.120***	0.123***
		(0.0429)	(0.0425)
× 1=dept: Comm educ (.082)		-0.0722*	-0.0796*
		(0.0436)	(0.0433)
× 1=dept: Couns psych (.009)		0.0667***	
		(0.0226)	
× 1=dept: Educ studies (.012)		0.0998***	
		(0.0169)	
× 1=dept: Env studies (.025)		0.0362**	0.0314*
		(0.0162)	(0.0164)
× demeaned resp propensity	-0.00267		
	(0.00265)		
Spend shr acad prog (0 to .3)	0.00916	-0.000698	-0.00793

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	(0.0160)	(0.0226)	(0.0238)
× zip pr Nonfamily hhld (.45)		0.0645*	0.0614*
		(0.0369)	(0.0369)
× zip pr Cmt 5-9 min (.149)		0.113**	0.106*
		(0.0561)	(0.0563)
× zip pr Heat solar (.001)		-4.083***	-4.139***
		(1.231)	(1.237)
× 1=Non-white (.365)		-0.0136**	-0.0122**
		(0.00541)	(0.00553)
× 1=individual's age known (1)			0.00690
			(0.00638)
× demean indiv. age, if known		-0.000601*	-0.000655**
		(0.000316)	(0.000311)
× 1=empl: Athletics (.022)		0.0355	0.0323
		(0.0219)	(0.0219)
× 1=empl: Arch, Allied Arts (.004)		-0.662***	
		(0.111)	

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× 1=empl: Music and Dance (.017)		-0.0437***	
		(0.0146)	
× 1=empl: Health, Counsel. (.019)		-0.0495***	
		(0.0158)	
× 1=dept: Gen soc sci (.012)		0.0279	
		(0.0190)	
× 1=dept: Sociol (.01)		0.102***	
		(0.0283)	
× 1=extr weath: any harm (.607)		0.0116**	0.0125***
		(0.00484)	(0.00479)
× demeaned resp propensity	-0.00138	-0.00750***	-0.00707***
	(0.00251)	(0.00214)	(0.00219)
Spend shr offsets (0 to .5)	-0.0108	-0.106***	-0.108***
	(0.0120)	(0.0327)	(0.0326)
× zip pr Asian alone (.034)		-0.244**	-0.252**
		(0.100)	(0.0998)
× zip pr Moved; same cty (.093)		0.196**	0.223***

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	(0.0771)	(0.0760)
× zip pr Hous-multi-unit (.119)	-0.0469	-0.0473
	(0.0293)	(0.0291)
× zip pr Hsng incompl kitch (.006)	-0.743**	-0.673*
	(0.350)	(0.345)
× zip pr No phone service (.022)	1.401***	1.244***
	(0.360)	(0.347)
× zip pr Heat electr (.591)	0.0501*	0.0440
	(0.0287)	(0.0287)
× zip pr Heat fuel oil, kero (.011)	0.226**	0.210**
	(0.102)	(0.0983)
× zip pr Ind manuf (.089)	0.153*	0.149*
	(0.0831)	(0.0825)
× zip pr Dem votes 2016 Pres elect.	0.0797*	0.0889**
	(0.0451)	(0.0451)
× Avg 2017 LCV score, pop wtd	-0.000222*	-0.000192
	(0.000133)	(0.000132)

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× 1=empl: Career non-tenure (.052)		-0.0266***	-0.0220**
		(0.00996)	(0.00996)
× 1=empl: Officer of admin (.134)		-0.0159**	-0.0151**
		(0.00637)	(0.00632)
× 1=empl: Arch, Allied Arts (.004)		0.173***	
		(0.0370)	
× 1=empl: Health, Counsel. (.019)		0.0284**	
		(0.0122)	
× 1=empl: VPFA, VPSL (.01)		0.0783**	
		(0.0338)	
× 1=dept: Bus admin (.068)		0.0238***	0.0236***
		(0.00672)	(0.00669)
× 1=dept: Music (.02)		0.0424***	0.0426***
		(0.0163)	(0.0155)
× 1=dept: Sociol (.01)		-0.0307**	
		(0.0146)	
× demeaned resp propensity	0.00144		

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	(0.00191)		
status quo w/ no prog	-0.218	0.734	0.569
	(0.741)	(1.057)	(1.051)
× zip pr Moved; same cty (.093)		11.33***	11.61***
		(4.028)	(4.022)
× zip pr Moved; from abroad (.004)		-75.93***	-72.54***
		(22.27)	(22.34)
× zip pr Inc 25K-35K (.098)		9.879	10.38
		(6.323)	(6.387)
× zip pr Hous-mobile (.066)		-5.577**	-5.708**
		(2.625)	(2.607)
× zip pr Cmt 15-19 min (.195)		-7.565**	-7.743**
		(3.693)	(3.691)
× zip pr Cmt 25-29 min (.055)		-11.45*	-10.52*
		(6.297)	(6.296)
× zip pr Ind oth serv (.062)		13.19*	13.33*
		(6.829)	(6.825)

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Table A13 – continued from previous page

× 1=empl: Classified staff (.136)	0.568**	0.661***
	(0.239)	(0.233)
× 1=empl: Courtesy appt (.01)	-2.475***	
	(0.585)	
× 1=empl: Graduate employee (.084)	-0.563**	-0.495*
	(0.276)	(0.277)
× 1=empl: Student employee (.186)	-0.656***	-0.634***
	(0.182)	(0.185)
× 1=empl: Arch, Allied Arts (.004)	-34.24***	
	(5.759)	
× 1=empl: Design (.047)	-1.321***	-1.294***
	(0.465)	(0.461)
× 1=empl: Education (.036)	0.932*	0.953*
	(0.509)	(0.518)
× 1=empl: Health, Counsel. (.019)	1.682**	
	(0.720)	
× 1=empl: VPFA, VPSL (.01)	2.575**	

Continued on next page

	(1.119)	
× 1=stu: Business (.068)	0.778**	0.824***
	(0.314)	(0.312)
× 1=stu: Undeclared (.042)	-0.554*	-0.466
	(0.296)	(0.296)
× 1=dept: Biology (.033)	-0.720**	-0.669**
	(0.338)	(0.338)
× 1=dept: Jour/Comm (.045)	-1.268***	-1.189***
	(0.414)	(0.415)
× 1=extr weath: any harm (.607)	0.289*	0.303*
	(0.156)	(0.156)
× 1=12 mos: Heat wave (.432)	-0.390***	-0.392***
	(0.144)	(0.144)
× 1=perceive pro-ICP bias (.443)	0.465**	0.473***
	(0.181)	(0.180)
× 1=somew/very liberal (.683)	-0.607***	-0.611***
	(0.197)	(0.196)

Continued on next page

Table A13 – continued from previous page

× 1=somew/very conserv (.086)		0.680**	0.700**
		(0.293)	(0.295)
× demeaned resp propensity	0.0516		
	(0.117)		
× =1 if have hhld inc (.882)			0.00517
			(0.00591)
No. alternatives	12069	12069	12069
Max. log-likelihood	-6355.36	-5772.24	-5827.61
Clustering	caseid	none	caseid
Base case WTP (40% C red)	106.29	294.66	259.36
Implied lower CI	22.72	140.72	105.12
Implied upper CI	189.87	448.60	413.60

t standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Additional WTP Simulations

TABLE A14.
Heterogeneity in WTP by program attributes and respondent characteristics

13. Zip code proportions, interquartile heterogeneity: pc15to19min

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pc15to19min	168.24***	2.58***
	(130.16, 208.62)	(1.59, 3.55)
Upper quartile: pc15to19min	168.64***	"
	(130.56, 209.08)	

14. Zip code proportions, interquartile heterogeneity: pinc_150_199

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pinc_150_199	168.20***	"
	(130.11, 208.59)	
Upper quartile: pinc_150_199	168.30***	"
	(130.22, 208.68)	

Continued on next page

15. Zip code proportions, interquartile heterogeneity: pinc_75_99

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pinc_75_99	152.05***	2.17***
	(112.79, 193.88)	(1.12, 3.21)
Upper quartile: pinc_75_99	171.35***	2.65***
	(133.13, 211.93)	(1.66, 3.63)

16. Zip code proportions, interquartile heterogeneity: pinc_lt_10

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pinc_lt_10	168.39***	2.58***
	(130.30, 208.80)	(1.59, 3.55)
Upper quartile: pinc_lt_10	168.58***	"
	(130.48, 209.05)	

17. Zip code proportions, interquartile heterogeneity: psub_kitchen

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Continued on next page

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: psub_kitchen	168.60***	"
	(130.51, 209.01)	
Upper quartile: psub_kitchen	168.34***	"
	(130.26, 208.72)	
18. Zip code proportions, interquartile heterogeneity: psub_plumbing		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: psub_plumbing	185.35***	3.00***
	(145.71, 227.50)	(2.00, 4.02)
Upper quartile: psub_plumbing	170.68***	2.63***
	(132.60, 211.10)	(1.65, 3.61)
19. Zip code proportions, interquartile heterogeneity: pasian		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		

Continued on next page

Lower quartile: pasian	150.35***	2.12***
	(111.47, 191.65)	(1.06, 3.15)
Upper quartile: pasian	165.43***	2.50***
	(127.62, 205.47)	(1.50, 3.47)

20. Zip code proportions, interquartile heterogeneity: pc20to24min

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pc20to24min	153.73***	2.21***
	(114.36, 194.61)	(1.15, 3.23)
Upper quartile: pc20to24min	190.57***	3.13***
	(145.21, 238.97)	(2.00, 4.27)

21. Zip code proportions, interquartile heterogeneity: pc60to89min

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pc60to89min	144.36***	1.97***
	(103.62, 186.69)	(0.9, 3.01)

Continued on next page

Upper quartile: pc60to89min	176.24***	2.77***
	(136.46, 218.42)	(1.75, 3.79)

22. Zip code proportions, interquartile heterogeneity: ped_bachelor

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: ped_bachelor	192.08***	3.17***
	(150.43, 237.41)	(2.15, 4.22)
Upper quartile: ped_bachelor	170.20***	2.62***
	(132.11, 210.60)	(1.63, 3.60)

23. Zip code proportions, interquartile heterogeneity: pc30to34min

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pc30to34min	168.48***	2.58***
	(130.37, 208.90)	(1.59, 3.55)
Upper quartile: pc30to34min	168.42***	"
	(130.32, 208.82)	

Continued on next page

24. Zip code proportions, interquartile heterogeneity: pmvabroad		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: pmvabroad	168.34***	"
	(130.27, 208.72)	
Upper quartile: pmvabroad	168.53***	"
	(130.42, 208.92)	
25. Zip code proportions, interquartile heterogeneity: pmvsameco		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: pmvsameco	168.53***	"
	(130.41, 208.98)	
Upper quartile: pmvsameco	168.36***	"
	(130.32, 208.74)	
26. Zip code proportions, interquartile heterogeneity: pblack		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		

Continued on next page

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pblack	168.22***	"
	(130.15, 208.60)	
Upper quartile: pblack	168.41***	"
	(130.31, 208.81)	

27. Zip code proportions, interquartile heterogeneity: pinform

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pinform	168.16***	"
	(130.09, 208.50)	
Upper quartile: pinform	168.54***	"
	(130.43, 208.97)	

28. Zip code proportions, interquartile heterogeneity: pnonfamily_hhlds

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Continued on next page

Lower quartile: pnonfamily_hhlds	168.29***	"
	(130.23, 208.68)	
Upper quartile: pnonfamily_hhlds	168.61***	"
	(130.47, 209.09)	

29. Zip code proportions, interquartile heterogeneity: pprofsnl

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pprofsnl	168.52***	"
	(130.42, 208.94)	
Upper quartile: pprofsnl	168.46***	"
	(130.37, 208.89)	

30. Zip code proportions, interquartile heterogeneity: pc45to59min

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pc45to59min	168.61***	"
	(130.47, 209.09)	

Continued on next page

Upper quartile: pc45to59min	168.42***	"
	(130.32, 208.81)	

31. Zip code proportions, interquartile heterogeneity: pmanuf

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: pmanuf	168.35***	"
	(130.28, 208.77)	
Upper quartile: pmanuf	168.46***	"
	(130.39, 208.90)	

32. Zip code proportions, interquartile heterogeneity: psub_telephone

(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables

Lower quartile: psub_telephone	168.36***	"
	(130.27, 208.75)	
Upper quartile: psub_telephone	168.45***	"
	(130.34, 208.85)	

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33. Zip code proportions, interquartile heterogeneity: pinc_15_24		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: pinc_15_24	168.43***	"
	(130.33, 208.83)	
Upper quartile: pinc_15_24	168.43***	"
	(130.33, 208.83)	
34. Zip code proportions, interquartile heterogeneity: pc40to44min		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: pc40to44min	168.64***	"
	(130.53, 209.04)	
Upper quartile: pc40to44min	168.28***	"
	(130.21, 208.68)	
35. Zip code proportions, interquartile heterogeneity: pconst		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		

Continued on next page

Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: pconst	168.50***	"
	(130.40, 208.94)	
Upper quartile: pconst	168.38***	"
	(130.29, 208.75)	
36. Zip code proportions, interquartile heterogeneity: ppubadmin		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		
Lower quartile: ppubadmin	168.27***	"
	(130.22, 208.68)	
Upper quartile: ppubadmin	168.53***	"
	(130.41, 208.93)	
37. Zip code proportions, interquartile heterogeneity: pwhlsale		
(40% carbon reduction, revenue shares = 20,30,30,20, spending shares = 40,30,30)		
Characteristics: mean response propensity, means of other continuous variables, baseline categories for other indicator variables		

Continued on next page

Table A14 – continued from previous page

Lower quartile: pwhlsale	168.11***	"
	(130.03, 208.46)	
Upper quartile: pwhlsale	168.84***	"
	(130.76, 209.30)	

t footnote1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Construct Validity Assessment

Systematic differences in preferences according to elicited climate change attitudes

Our survey elicited responses to a number of questions designed to assess various dimensions of the respondent's attitudes towards the problem of climate change. These attitudinal questions tend to be multiple choice. We rely on ordinary stepwise regression methods to narrow down a complete set of interaction terms for all of the non-cost program attributes, reporting here only those factors that remain statistically significant at the 10% level. The program attributes are completely orthogonal, except for a logical correlation between the percentage-point carbon reduction and the program cost. Likewise, the answers to the attitudinal questions are categorical and mutually exclusive, ensuring that they are also orthogonal. There may, of course be other omitted variables that may distort the apparent relationship between of any attitudinal factor on marginal utilities, but our goal in this section is merely to explore whether the marginal utilities implied by our program choices are consistent with the attitudes that people express when asked directly about aspects of climate change.

Tables A15–A18 duplicate the questions developed by Anthony Leiserowitz and his collaborators in their "Six Nations" of climate change attitudes. These questions mirror their shortest format.

Table A15 reveals that respondents exhibit a higher marginal utility from each percentage-point of carbon reduction if they feel that global warming is "extremely important." In addition, at the bottom of the table, the negative effect of this attitude on the individual's preference for the status-quo alternative means that the utility derived from any carbon reduction program, regardless of its scope, increases monotonically with the perceived importance of global warming. People who feel that global warming is extremely important are less enthusiastic about programs that charge air-travel fees or building energy fees, so they prefer programs that impose a uniform fee on everyone in the organization. In terms of spending shares, anyone who feels that climate change is somewhat important, very important, or extremely important would prefer to have all of the revenues spent on carbon reduction projects, rather than on academic programs or even on carbon offsets. These findings are all intuitively plausible.

Analogously, Table A16 explores the relationship between the respondents degree of worry about global warming and the marginal utilities implied by their program preferences. The results for this attitude are very similar. Being very worried about the problem increases the marginal utility from each percentage-point of carbon reduction and also increases the utility from any carbon-reduction program, regardless of its attributes, again in a monotonic fashion.

The results in Table A17, concerning the respondent's expectation of future harm to be experienced personally from climate change, suggest that those people who expect either a great deal of personal harm or modest personal harm are less likely to prefer the status quo alternative (no program), and thus more likely to prefer any carbon-reduction program, regardless of its attributes. The omitted category in this case is "Don't know," so there is a category for the expectation of "no personal harm," and this expectation increases preferences for the status quo (no program) alternative by a very large amount relative to the "Don't know" category.

Other-regarding preferences are elicited directly with the question about expected harm to future generations of people as a result of global warming. The omitted category is again "Don't know." Expecting a great deal of harm to future generations increases the marginal utility from each percentage-point carbon reduction in a program. This attitude, as well as an expectation of moderate harm to future generations, also corresponds to a higher utility from any program (as opposed to the status quo with no program). Respondents who expect that future generations will not be harmed at all have lower marginal utilities from carbon reductions.

We include an additional question from some of the earlier "Six Nations" work concerning the respondent's climate attitudes relative to their friends. This potential shifter of climate change preferences is summarized in Table A19. Compared to a respondent whose global warming views are shared by all of their friends, people whose global warming views are shared by fewer and fewer of their friends seem to derive less and less marginal utility from carbon reductions. For individuals having no friends that share their views, the marginal utility from an additional unit of carbon reduction become negative. However, this group consititues only about 1% of our sample (or about ten people), so this finding might not be robust in a larger sample.

In Table A20, we explore the relationship between a respondent's perception of a pro-ICP or anti-ICP on the part of the research team. Ideally, the wording of the survey would leave the impression that the research team is neutral, and we worked hard to have the survey instrument appear agnostic. However, given the political controversy in the U.S. about climate change, simply explaining the majority views of climate scientists can be perceive by some types of respondents as a bias in favor of carbon pricing. After all, if the research team did not care about climate change, why would they be doing this survey? It is likely that perceptions of research bias reflect as much the respondent's own attitudes about the importance or unimportance of dealing with climate change.

A slight majority of respondents perceived the survey to be unbiased or they couldn't tell. Only about 3% felt the research team wanted them to vote against the carbon pricing program, but 45% of respondents felt the research team wanted them to vote in favor of some carbon pricing program, rather than none at all. As expected, respondents who thought the research team wanted them to vote for some carbon-pricing program appear to derive less marginal utility from each percentage point reduction in carbon emissions, and they are markedly more likely to prefer the status quo over any program. Somewhat surprisingly, those thirty or-so-people who perceived that the researchers wanted them to choose no program make choices that imply that they derive essentially zero marginal utility per percentage point carbon reduction. However, they share with those perceiving no bias the baseline negative marginal utility from the status quo. Like everyone else, this group derives some positive utility from a carbon pricing program. Their preferred alternative just doesn't depend on the size of the carbon reduction.

Systematic differences in preferences as a function of initial misconceptions

Table A21 assesses how preferences appear to differ according to a respondent's errors in the tests of comprehension that we included during the tutorial portion of the survey. These questions were:

- Over how wide a geographic area will any negative effects of these carbon emissions eventually be felt? ["C global effects"]
- For what reasons might a private company (or an institution like a university) consider setting up an internal carbon-pricing program? ["Reasons for ICPs"]
- Many out-of-state and foreign students are far away from their families while they are at university. If the university's carbon-pricing program involves an airtravel carbon fee, will these students have to pay a carbon fee when they fly home to visit their families? ["C fees stud. trav"]
- Suppose a specific carbon-pricing program does not involve any direct student/employee "fees" for carbon. Consider a student who is not part of a team or group for which the university typically pays for air travel. Will that student be able to completely avoid the cost of that carbon-pricing program? ["unavoid. of fees"]

Anyone who answered any of these questions incorrectly was treated with a second and more detailed explanation, in an effort to correct these misperceptions before they began the choice tasks.

People who were unaware that carbon was a global pollutant have lower marginal utilities for each percentage-point carbon reduction. Those who were unaware that privately paid travel by students would not be subject to any air-travel fees were less in favor of programs that involved greater percentages of the cost borne via air travel fees, but they were more likely to approve of the revenues being spent on carbon offsets, and more likely to prefer any program relative to the status quo.

Those who did not pay sufficient attention to the list of reasons why private companies institute ICP programs to recognize that every reason on the offered list had been cited on the previous screen felt differently about how the revenues would be spent. They were less in favor of programs that spend more of the revenues on academic programs and more in favor of programs that allow greater spending on carbon offsets.

Finally, about 25% of respondents who did not initially understand that even without a flat fee on everyone at the institution, they could still bear the costs of carbon pricing through building energy fees and/or as state taxpayers. These respondents were less inclined to favor costs being borne as building energy fees and derived lower utility for increases in the proportion of revenues spent on academic programs.

Systematic differences in preferences with highest-priority social goals

At the beginning of the survey, respondents were asked to identify their three highest-priority social goals from a list that was randomized for each respondent. This list included:

- Prevent climate change
- Improve education
- Prevent violence, crime
- Conserve natural resources

- Improve public health
- Reduce poverty, hunger

Respondents who identified the prevention of violence and crime as one of their three highest priorities derive a lower marginal utility from additional percentage-points of carbon reduction. Those who indicated improvements to education as one of their three highest priorities did not share the disutility that others derived from the proportion of revenues spent on educational programs. Prioritizing climate change prevention, the conservation of natural resources, and the reduction of poverty and hunger were each associated with greater utility derived from any ICP program, regardless of its attributes.

Systematic differences in preferences by Spring and Fall survey waves

We oversampled employees in the Spring 2018 wave of the survey because the term was nearing an end and we did not want final exams to impinge on students' attention to the survey. In the fall, we oversampled students and invited relatively fewer employees to take the survey. As a result, it is entirely possible that the average preferences of the group with relatively more employees (Spring) would be different than the average preferences of the group with relatively more students (Fall). The differences reported in Table A23 tend to disappear when we control for other attributes that differentiate these two survey waves, but we provide these results for completeness. The Fall sample with its higher proportion of students disapproves of greater shares of program revenues being spent on carbon offsets, and is more inclined to prefer any program over the status quo.

TABLE A15.
Persistently statistically significant parameter estimates for interactions with answers to "How important is the issue of global warming to you personally?" sw, pr(.10) (Omitted category: Not at all important)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00850***	(0.00120)
× demeaned resp propensity	0.00309***	(0.00116)
Percentage-point C reduction (10 to 50)	0.0119**	(0.00463)
× 1=GW extremely important (.439)	0.0205***	(0.00408)
× demeaned resp propensity	-0.00877**	(0.00354)
Cost share: air-travel fees (0 to .5)	0.0145***	(0.00226)
× 1=GW extremely important (.439)	-0.00525*	(0.00316)
× demeaned resp propensity	0.00176*	(0.000973)
Cost share: building energy fees (0 to 1)	0.0114***	(0.00154)
× 1=GW extremely important (.439)	-0.00353*	(0.00214)
Cost share: taxpayers (0 to .2)	0.00877***	(0.00306)
Spend share: academic programs (0 to .3)	0.0234*	(0.0125)
× 1=GW extremely important (.439)	-0.0309**	(0.0128)
× 1=GW very important (.327)	-0.0270**	(0.0131)
× 1=GW somewhat important (.179)	-0.0238*	(0.0137)
× demeaned resp propensity	0.00627**	(0.00250)
Spend share: carbon offsets (0 to .5)	0.0226**	(0.0107)
× 1=GW extremely important (.439)	-0.0261**	(0.0110)
× 1=GW very important (.327)	-0.0246**	(0.0110)
× 1=GW somewhat important (.179)	-0.0206*	(0.0118)
× demeaned resp propensity	-0.00414*	(0.00214)
Status quo, no program	3.181***	(0.556)
× 1=GW extremely important (.439)	-3.922***	(0.574)
× 1=GW very important (.327)	-3.271***	(0.537)
× 1=GW somewhat important (.179)	-1.994***	(0.554)
× demeaned resp propensity	0.277***	(0.0933)
Max. log-likelihood	-6591.27	
No. respondents	1052	
No. choices	5547	

Continued on next page

Table A15 – continued from previous page

No. alternatives	13165

t standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A16.
Persistently statistically significant parameter estimates for interaction terms with answers to "How worried are you about global warming?" sw, pr(.10) (Omitted category: Not at all worried)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00854***	(0.00119)
× demeaned resp propensity	0.00280**	(0.00118)
Percentage-point C reduction (10 to 50)	0.0101**	(0.00510)
× 1=GW very worried (.587)	0.0181***	(0.00440)
× demeaned resp propensity	-0.00729**	(0.00356)
Cost share: air-travel fees (0 to .5)	0.0156***	(0.00259)
× 1=GW very worried (.587)	-0.00597*	(0.00305)
× demeaned resp propensity	0.00176*	(0.000959)
Cost share: building energy fees (0 to 1)	0.00930***	(0.00109)
Cost share: taxpayers (0 to .2)	0.00866***	(0.00316)
× 1=GW not very worried (.057)	-0.0312*	(0.0174)
Spend share: academic programs (0 to .3)	0.0482***	(0.0159)
× 1=GW very worried (.587)	-0.0527***	(0.0160)
× 1=GW somewhat worried (.325)	-0.0520***	(0.0163)
× 1=GW not very worried (.057)	-0.0644***	(0.0197)
× demeaned resp propensity	0.00549**	(0.00256)
Spend share: carbon offsets (0 to .5)	0.00303	(0.00304)
× 1=GW very worried (.587)	-0.00710*	(0.00365)
× demeaned resp propensity	-0.00343*	(0.00185)
Status quo, no program	3.655***	(0.689)
× 1=GW very worried (.587)	-4.198***	(0.695)
× 1=GW somewhat worried (.325)	-3.329***	(0.690)
× 1=GW not very worried (.057)	-1.718**	(0.806)
× demeaned resp propensity	0.235***	(0.0871)
Max. log-likelihood	-6614.00	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses

Continued on next page

Table A16 – continued from previous page

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A17.

Persistently statistically significant parameter estimates for interaction terms with answers to "How much do you think global warming will harm you personally?" sw, pr(.10) (Omitted category: Don't know)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00792***	(0.00117)
× demeaned resp propensity	0.00293**	(0.00114)
Percentage-point C reduction (10 to 50)	0.0192***	(0.00405)
× demeaned resp propensity	-0.00753**	(0.00366)
Cost share: air-travel fees (0 to .5)	0.0110***	(0.00162)
× demeaned resp propensity	0.00180*	(0.000934)
Cost share: building energy fees (0 to 1)	0.00904***	(0.00107)
Cost share: taxpayers (0 to .2)	0.00799***	(0.00305)
Spend share: academic programs (0 to .3)	-0.00374	(0.00249)
× demeaned resp propensity	0.00597**	(0.00255)
Spend share: carbon offsets (0 to .5)	-0.00137	(0.00180)
× demeaned resp propensity	-0.00317*	(0.00176)
Status quo, no program	0.427**	(0.191)
× 1=GW personal harm-great deal (.258)	-1.053***	(0.205)
× 1=GW personal harm-mod. amount (.475)	-0.552***	(0.180)
× 1=GW personal harm-not at all (.033)	2.674***	(0.613)
× demeaned resp propensity	0.231***	(0.0863)
Max. log-likelihood	-6796.08	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A18.
Persistently statistically significant parameter estimates for interaction terms with answers to "How much do you think global warming will harm future generations of people?" sw, pr(.10) (Omitted category: Don't know)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00848***	(0.00118)
× demeaned resp propensity	0.00302***	(0.00111)
Percentage-point C reduction (10 to 50)	-0.0000452	(0.00659)
× 1=GW future harm-great deal (.830)	0.0239***	(0.00615)
× 1=GW future harm-not at all (.012)	-0.0991**	(0.0436)
× demeaned resp propensity	-0.00775**	(0.00359)
Cost share: air-travel fees (0 to .5)	0.0112***	(0.00163)
× demeaned resp propensity	0.00209**	(0.000944)
Cost share: building energy fees (0 to 1)	0.00918***	(0.00108)
Cost share: taxpayers (0 to .2)	0.00790**	(0.00308)
Spend share: academic programs (0 to .3)	-0.00318	(0.00252)
× demeaned resp propensity	0.00568**	(0.00253)
Spend share: carbon offsets (0 to .5)	-0.00145	(0.00184)
× demeaned resp propensity	-0.00340*	(0.00186)
Status quo, no program	1.459***	(0.396)
× 1=GW future harm-great deal (.830)	-1.727***	(0.395)
× 1=GW future harm-mod. amount (.096)	-0.780*	(0.449)
× demeaned resp propensity	0.245***	(0.0870)
Max. log-likelihood	-6710.84	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A19.
Persistently statistically significant parameter estimates for interaction terms with answers to "How many of your friends share your views on global warming?" sw, pr(.10) (Omitted category: All friends share views)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00766***	(0.00115)
× demeaned resp propensity	0.00302***	(0.00108)
Percentage-point C reduction (10 to 50)	0.0227***	(0.00425)
× 1=friends share GW view-none (.01)	-0.0750***	(0.0253)
× 1=friends share GW view-a few (.1)	-0.0154***	(0.00592)
× 1=friends share GW view-some (.211)	-0.0107**	(0.00450)
× demeaned resp propensity	-0.00850**	(0.00344)
Cost share: air-travel fees (0 to .5)	0.0126***	(0.00170)
× 1=friends share GW view-some (.211)	-0.00750**	(0.00310)
× demeaned resp propensity	0.00151*	(0.000890)
Cost share: building energy fees (0 to 1)	0.0102***	(0.00117)
× 1=friends share GW view-some (.211)	-0.00497**	(0.00219)
Cost share: taxpayers (0 to .2)	0.00741**	(0.00304)
Spend share: academic programs (0 to .3)	-0.00342	(0.00242)
× demeaned resp propensity	0.00613**	(0.00245)
Spend share: carbon offsets (0 to .5)	-0.000287	(0.00183)
× demeaned resp propensity	-0.00379**	(0.00192)
Status quo, no program	-0.0382	(0.131)
× demeaned resp propensity	0.223***	(0.0837)
Max. log-likelihood	-6888.06	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A20.
Persistently statistically significant parameter estimates for perceived researcher bias, based on responses to "Overall, the wording of this survey made it seem that the researchers conducting this study really wanted me to choose: some carbon-pricing program, no program, the best alternative for me personally, not sure/count't tell", sw, pr(.10) (Omitted category: No perceived bias on the part of the researchers)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00758***	(0.00115)
× demeaned resp propensity	0.00304***	(0.00112)
Percentage-point C reduction (10 to 50)	0.0227***	(0.00442)
× 1=Perceive pro-ICP bias (.452)	-0.00866**	(0.00408)
× 1=Perceive anti-ICP bias (.034)	-0.0225**	(0.0105)
× demeaned resp propensity	-0.00810**	(0.00353)
Cost share: air-travel fees (0 to .5)	0.0106***	(0.00161)
× demeaned resp propensity	0.00207**	(0.000951)
Cost share: building energy fees (0 to 1)	0.00894***	(0.00107)
Cost share: taxpayers (0 to .2)	0.00742**	(0.00299)
Spend share: academic programs (0 to .3)	-0.00345	(0.00245)
× demeaned resp propensity	0.00594**	(0.00254)
Spend share: carbon offsets (0 to .5)	-0.00103	(0.00184)
× demeaned resp propensity	-0.00352*	(0.00188)
Status quo, no program	-0.302**	(0.142)
× 1=Perceive pro-ICP bias (.452)	0.563***	(0.166)
× demeaned resp propensity	0.233***	(0.0822)
Max. log-likelihood	-6875.96	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A21.
Persistently statistically significant parameter estimates for interaction terms with incorrect answers to knowledge/comprehension questions during the tutorial section of the survey; sw, pr(.10) (Omitted category: Gave correct response to knowledge/comprehension question)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00755***	(0.00113)
× demeaned resp propensity	0.00287***	(0.000984)
Percentage-point C reduction (10 to 50)	0.0211***	(0.00400)
× 1=dnk: C global effects (.134)	-0.0252***	(0.00565)
× demeaned resp propensity	-0.00838***	(0.00319)
Cost share: air-travel fees (0 to .5)	0.0124***	(0.00169)
× 1=dnk: C fees stud. trav (.148)	-0.00924**	(0.00411)
× demeaned resp propensity	0.00178**	(0.000890)
Cost share: building energy fees (0 to 1)	0.00978***	(0.00121)
× 1=dnk: unavoid. of fees (.253)	-0.00384*	(0.00230)
Cost share: taxpayers (0 to .2)	0.00665**	(0.00303)
Spend share: academic programs (0 to .3)	0.00494	(0.00321)
× 1=dnk: Reasons for ICPs (.333)	-0.0150***	(0.00458)
× 1=dnk: unavoid. of fees (.253)	-0.0124**	(0.00499)
× demeaned resp propensity	0.00527**	(0.00257)
Spend share: carbon offsets (0 to .5)	-0.00613***	(0.00215)
× 1=dnk: Reasons for ICPs (.333)	0.00746**	(0.00366)
× 1=dnk: C fees stud. trav (.148)	0.00793*	(0.00473)
Status quo, no program	0.00550	(0.132)
× 1=dnk: C fees stud. trav (.148)	-0.527**	(0.221)
× demeaned resp propensity	0.235***	(0.0733)
Max. log-likelihood	-6882.85	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A22.
Persistently statistically significant parameter estimates for interactions with indicators for respondent's choices of three highest-priority social goals, among randomly ordered options of: prevent climate change, improve education, prevent violence/crime, conserve natural resources, improve public health, reduce poverty/hunger; sw, pr(.10) (omitted category: no priority on any category of problem)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00825***	(0.00117)
× demeaned resp propensity	0.00314***	(0.00115)
Percentage-point C reduction (10 to 50)	0.0237***	(0.00426)
× 1=Prioritize crime (.291)	-0.0130***	(0.00437)
× demeaned resp propensity	-0.00837**	(0.00348)
Cost share: air-travel fees (0 to .5)	0.0115***	(0.00162)
× demeaned resp propensity	0.00178*	(0.000966)
Cost share: building energy fees (0 to 1)	0.00916***	(0.00107)
Cost share: taxpayers (0 to .2)	0.00786***	(0.00304)
Spend share: academic programs (0 to .3)	-0.00877**	(0.00399)
× 1=Prioritize education (.606)	0.00878*	(0.00497)
× demeaned resp propensity	0.00660***	(0.00251)
Spend share: carbon offsets (0 to .5)	-0.000832	(0.00190)
× demeaned resp propensity	-0.00445**	(0.00220)
Status quo, no program	1.031***	(0.214)
× 1=Prioritize nat resour (.477)	-0.359**	(0.147)
× 1=Prioritize climate chng (.594)	-1.157***	(0.150)
× 1=Prioritize poverty (.571)	-0.394***	(0.147)
× demeaned resp propensity	0.280***	(0.0937)
Max. log-likelihood	-6761.60	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A23.
Persistently statistically significant parameter estimates for membership in the Fall 2018 wave of the survey (with proportionately more students than faculty, compared to the Spring 2018 wave); sw, pr(.10) (Omitted category: respondent belongs to Spring 2018 sample)

1=Preferred program	Estimate	Std. Err.
Program's cost to household (22 to 232)	-0.00765***	(0.00114)
× demeaned resp propensity	0.00286***	(0.00108)
Percentage-point C reduction (10 to 50)	0.0180***	(0.00398)
× demeaned resp propensity	-0.00730**	(0.00351)
Cost share: air-travel fees (0 to .5)	0.0109***	(0.00159)
× demeaned resp propensity	0.00181*	(0.000927)
Cost share: building energy fees (0 to 1)	0.00891***	(0.00106)
Cost share: taxpayers (0 to .2)	0.00708**	(0.00301)
Spend share: academic programs (0 to .3)	-0.00351	(0.00244)
× demeaned resp propensity	0.00619**	(0.00244)
Spend share: carbon offsets (0 to .5)	0.00303	(0.00312)
× 1=Fall 2018 survey wave (.432)	-0.00722**	(0.00351)
× demeaned resp propensity	-0.00358*	(0.00187)
Status quo, no program	0.114	(0.145)
× 1=Fall 2018 survey wave (.432)	-0.372**	(0.149)
× demeaned resp propensity	0.212***	(0.0814)
Max. log-likelihood	-6937.00	
No. respondents	1052	
No. choices	5547	
No. alternatives	13165	

t standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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