Moral Licensing and Moral Cleansing in Contingent Valuation and Laboratory Experiments of Willingness to Pay to Reduce Negative Externalities

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Recent field experiments show that peer information can induce people to reduce their production of negative externalities. Related work in psychology demonstrates that inducing feelings of relative culpability can induce pro-social behavior. We use a contingent valuation and parallel lab experiment to explore patterns of responses to norm-based interventions. Asymmetric responses between those whose impacts are above or below the norm are found to be robust across decision settings. Substantial heterogeneity in responses is observed across a number dimensions not explored in large field experiments, raising questions about the universality of peer-information effects and the design of such programs.

Keywords: culpability; moral licensing; moral cleansing; contingent valuation; peer information; social preferences; externalities; green electricity

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Recent large-scale field experiments demonstrate that peer comparisons and social-norm nudges are effective tools for inducing the conservation of privately purchased goods that collectively create negative public externalities. Randomized residential electricity experiments that have monitored energy use and informed households of their personal consumption levels relative to a neighborhood norm provide evidence that energy consumers significantly reduce their energy consumption relative to a control group that does not receive such comparative information (Ayers et al. 2009; Allcott and Mullainathan, 2010; Costa and Kahn, 2010; Allcott, 2011). Such behavioral change-based interventions, as opposed to more traditional price instruments, can indeed be powerful, especially amongst specific groups of the population. Ferraro and Price (2011), for example, study the effects of providing non-price interventions for household water use and find that, in the short run at least, the social-comparison effect is equivalent to that which would be expected if average prices were to increase by 12 to 15 percent; in a study of residential electricity consumption, Ayers et al. (2009) estimate that non-price, peer comparison intervention induce the equivalent consumption response as a 17 to 29 percent price increase.

While the average treatment effect has been shown to be significant, it is apparent that there is variation in response patterns to norm-based interventions. Notably, in a localized study of 290 households, Schultz et al. (2007) demonstrate 3 average consumption households may actually increase their energy consumption when they are informed that their baseline consumption is below the average of their peer group. In this same study, high-energy users significantly decreased their electricity consumption levels relative to the baseline, as expected from the focus theory of normative behavior (Cialdini et al., 1991). This asymmetry in treatment effects has been replicated, to an extent, in large scale field experiments with observations ranging from 75,000 to 600,000 households. However, rather than observing a strong boomerang effect that increases consumption, there more commonly seems to be a zero, or muted negative, effect on consumption patterns of low-use households. Allcott (2011) estimates that social-norm treatment effects are not significantly different from zero for the lowest three deciles of baseline electricity users, but that there is a significant mean treatment effect in high-use households ranging from about -3.7% for the 8th decile to over -7% in the 10th decile. Ayers et al. (2009) similarly find no significant treatment effect on two out of lowest three deciles of baseline electricity use (the second decile had a significant treatment effect of approximately +1%), while consumption levels significantly decline by about -3% to -7% for the top three baseline energy deciles. In a regression framework, Ferraro and Price (2011) estimate that the "social norm effect for our high user group is approximately 94.1 percent greater (5.28 versus 2.72 percent relative reduction) than for our low user group – a difference that is significant at the $p<0.005$ level." In all, while strong boomerang effects may not be evident, there does appear to be an important asymmetry in responses to social-norm interventions between households with above and below norm consumption levels.

Moreover, although responsiveness to norm-based messages have been demonstrated in a number of domains (e.g. Frey and Meier, 2004; Cialdini et al., 2006; Salganik, Dodds, and Watts, 2006; Cai, Chen, and Fang, 2009) recent research in the energy-social norms literature suggests that non-pecuniary effects may not be as universal as previously thought. Different socio-economic groups may have heterogeneous responsiveness to peer information. In interpreting these results, Costa and Kahn (2010) argue that:

> "behavioral economists have underestimated the role that ideological heterogeneity plays in determining the effectiveness of energy conservation "nudges"… we find that liberals and environmentalists are more responsive to these nudges than the average person. In contrast, for certain subsets of Republican Registered voters, we find that the specific

> "treatment nudge" that we evaluate has the unintended consequence of increasing electricity consumption." (p. 2)

In this paper we show that such asymmetric and heterogeneous responsiveness is also manifested in contingent valuation and laboratory economics experiments in which we can control the normative information that the subject receives. Along the lines of Bateman et al. (2008), who demonstrated parallelism between contingent valuation responses and "inconsistencies…found in everyday decisions involving real commitments" (p. 125), we argue that evidence of convergent behaviors across methods lends validity to each. Further, the survey application allows us to explore whether heterogeneity in response patterns occurs in demographic and other respondent-specific dimensions not able to be explored in large-scale field tests. The laboratory experiment permits exogenous control of the individual's impact, avoiding possible endogeneity effects that may arise in field and contingent valuation studies.

The contingent valuation study calculates the carbon footprint of a nationally representative sample of consumers by asking questions about their energy-related consumption habits. A carbon footprint is defined to be the number of tons of carbon dioxide emissions an individual is personally responsible for based upon his or her energy consumption decisions in a given year. We then provide subjects in the treatment group information about how their carbon footprint compares to those of others in the study and elicit willingness to purchase green electricity to induce a feeling of relative culpability. In an effort to parallel the field contingent valuation study, the laboratory experiment has student subjects purchase "private commodities" (analogous to electricity) that generate a negative externality (analogous to pollution) for a group in which they are a member. A treatment group is given information about the private, pollution generating choices of others and the subjects are subsequently given an opportunity to contribute to a fund that would reduce the negative harm created by the externality. In the taxonomy of Harrison and List (2004) we present results from a framed field experiment coupled with a conventional laboratory experiment.

Beyond demonstrating convergent validity between field experiments, economic laboratory exercises, and contingent valuation responses and identifying further dimension of response heterogeneity to social- norm nudges, our research contributes to the broader literature on norm-based conservation incentives. First, in contrast to energy and water conservation in which the psychological cues and economics savings are mutually reinforcing, our contingent valuation study of willingness to pay for “green electricity” and laboratory experiment study of willingness to contribute to a public good involve tradeoffs between private costs and societal or group gains. As such, our work extends the work of Shang and Crosson, (2009) and Chen (2009) who show that some individuals are willing to bear additional monetary burdens in response to information about social norms. Second, much of the previous research on norm-based messaging has been confined to providing information about peer consumption in the domain of the desired conservation activity. For example, studies that seek to encourage towel re-use in hotels, provide information about towel re-use habits of others (Goldstein et al. 2008). At the same time some limited research suggests that social-norm information in one domain of decision-making affects decisions in other domains (Mazar and Zhong, 2010; Keizer et al., 2008). These studies have considered moral licensing—learning you are more moral in one domain makes you less moral in another—and moral cleansing—learning you are less moral in one domain makes you more moral in another. Our research speaks to both and finds an asymmetric response. This asymmetry could produce a “moral rebound” effect that limits the effectiveness of social-norm based policy interventions. Therefore, understanding such response patterns could significantly improve the design of interventions and explain the limited effectiveness of past trials. More mundanely, our design speaks directly to the effect of carbon footprint calculators on the demand for carbon offsets and green electricity.

Our main findings are that information about the behaviors of others influences public provision behavior in contingent valuation and lab experiments. The effect of social information is *asymmetrical*— the moral cleansing effect for individuals above the norm is larger than the moral licensing effect for those whose consumption and negative externality effects are below the perceived norm. Finally, we demonstrate that systematic heterogeneity in responses to social norm nudges extends substantially beyond the political/environmental dimensions explored in Costa and Kahn's field experiment. As we argue in the concluding section, these findings, in conjunction with emerging field research, raise questions about the universal efficacy of nudges vis-à-vis pricing incentives.

The remainder of this paper is organized as follows. In the following section we review previous economic and psychological conceptualizations of the notion of culpability or guilt in choice and valuation and how these concepts have been tested in laboratory and contingent valuation exercises. We then provide details on our experimental design and data. In the fourth section we provide empirical analyses of our experimental results with respect to asymmetry in response patterns above versus below norm respondents. The fifth section lends supporting evidence to the Costa and Kahn results, and expands the analysis of heterogeneity to demographic and respondent-specific characteristics available from survey data. Conclusions and discussion are provided in the final section.

Background and Experimental Design

Background on Culpability

In this research we explore how willingness to pay to prevent a public bad is affected by an individual's relative culpability, which we define to be the amount of social damage resulting from an individual's actions relative to damages cause by others.[1] Whereas the mechanisms that might induce conformity to a perceived social norm have been extensively studied in economics (see for example Bikhchandani, Hirshleifer, and Welch, 1992; Ellison and Fudenberg, 1993; Bernheim, 1994; Akerlof and Kranton, 2000; Glaeser and Scheinkman, 2003), the mechanism of culpability has received less attention. Guilt has been explored in the psychology literature (see Baumeister, Stillwell, and Heatherton, 1994 for a review). Perhaps most famously, Carlsmith and Gross (1969) induced guilt in subjects by having them administer electric shocks to another person, a confederate. Later, when subjects believe they have completed the experiment, they are asked to donate blood. Subjects who actually administered the shock are much more likely to agree to donate, relative to subjects who merely observed the shocking.

Building from psychological foundations and psychological game theory (see Geanakoplos, Pearce, and Stacchetti (1989), Charness, Dufwenberg and co-authors construct a general theory of guilt aversion in which decision-makers experience guilt if they believe they let others down (e.g. Dufwenberg and Lundhom, 2001; Charness and Dufwenberg, 2006, 2007; Battigali and Dufwenberg (2007)). With supportive results from "Trust Game" experiments,

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[1] Our focus is on relative culpability because pilot experiments found that information about one's absolute level of social damage without comparison to one's peers had no effect on behavior.

they propose that this general theoretical framework can be extended to specific instances, such as public goods games and social norms, where it seems plausible that decision-makers are affected by guilt. In doing so these authors take care to distinguish the role of guilt aversion from conformity: “A norm is a social moral expectation a definition of which acts people in society will judge as right or wrong...Too many authors use “norm” just to mean ``conformity in behavior''. (Dufwenberg and Lundhom, 2001, p. 510).

Andreoni’s (1995) prior research on public goods suggests that such motivations may depend on whether the provision of the public good is framed positively or negatively. In Andreoni (1995), two groups of subjects participated in strategically identical public goods provision games, but with two separate framings. In one, the experiment was framed as providing a public good so that subjects would be motivated by warm glow altruism; in the other, the experiment was framed as avoiding a public bad, so that subjects would be motivated by a desire to avoid a “cold prickle” of guilt. Sonnemanns et al. (1998) conduct a like set of experiments in a threshold provision setting, alternatively framing the experiments as provision to provide a public good and prevention of a public bad. In both the Andreoni and Sonnemanns et al. studies, the tendency to free ride was more prevalent in the negative framing. Similarly, Solnick and Hemenway (2005) present informal survey evidence where positional concerns matter more for public goods rather than for public bads.

In the specific area of environmental norms, Bamberg and Moser (2006) conduct a meta-analysis of the literature on psychological mechanisms that promote pro-environmental behavior, finding that both social norms and guilt are important correlates to pro-environmental attitudes and behavior. Clark, Kotchen and Moore (2003) find that participation in a green electricity program is correlated with self-reported altruism and pro-environmental attitudes as measured by the New Environmental Paradigm (NEP). Brouwer et al. (2008) test the "passenger pays principle" to find that air travelers' perceived responsibility for climate change, awareness of the environmental impact of flying, and the frequency of flying were all positively correlated with WTP for a per-flight carbon offset program. This notion of personal responsibility in creating public harm is an extension of what Kahneman (1993) refers to as an "outrage effect", in which people are willing to pay more to avoid an environmental problem if they think it is human-caused than if they think that it is an outcome of nature (Bulte et al., 2005). Kahneman (1993) and Brown et al. (2002), amongst others have demonstrated this "outrage effect" on contingent valuation responses.

Our experiments complement the aforementioned literature by honing in on the individual culpability in contingent valuation and public goods experimental settings. We use peer information to manipulate the norm in a sequential setting most similar to the framing experiments of Andreoni (1995) and Sonnemans et al. (1998). Rather than split "Provision of Public Good" and "Prevention of Public Bad" samples as done in these studies, however, we employ a sequential framework: in the first stage of the experiment, we observe private decisions in a negative externality setting; the second stage involves a public goods contributions game in which contributions mitigate the negative effects of decisions in the first stage. We expect two main outcomes. For those who learn they contribute more to the negative externality than the perceived norm, i.e. have positive relative culpability, we expect they will be more altruistic in the second. For those who experience negative culpability, by learning they contribute less to the negative externality than the perceived norm in the first stage, we expect they will be less altruistic in the second. We find support for both of these effects, but we find that the former dominates. All treatment groups behaved less altruistically than those who received no information at all. This "moral licensing" effect has been explored by Mazur and Zhong (2010) who find that those who are given the opportunity to purchase green goods are more likely to cheat on an exam.

Similarly, in one field experimental test of the “broken windows” effect Keizer et al. (2008) find that observing others violate one social norm makes subjects more likely to violate other social norms. Our results further demonstrate that the effect predominates in those pre-disposed to provide more public goods in the second domain—for example Democrats, replicating in a lab and contingent valuation context the findings of Costa and Kahn (2010) who observed that the affect is limited to Democrats in a field experiment on electricity conservation. We extend their work to show that the heterogeneous effect exists along other dimensions as well.

Contingent Valuation Experiment:

The broad objective of the contingent valuation survey was to gather information from participants that allowed us to calculate a carbon footprint for each respondent and then elicit their willingness to pay for a green electricity program given information about their own carbon footprint and, in some treatments, their carbon footprint relative to those of another survey participant. Participants for the online hypothetical survey were recruited through The StudyResponse Project, a nationwide panel of 95,574 people. The diversity of the sample, as seen in the summary statistics in Table 1 will be important for our analysis. Participants were chosen at random and emailed the URL for the survey. For completing the survey, participants received $5. 520 panelists were invited to participate, and we received 420 completed surveys for an 81% response rate.

There were four steps in the survey: I) Eliciting demographic questions to calculate the subject’s carbon footprint; II) Providing information about International Panel on Climate Change (IPCC) predictions on the impacts of climate change; III) Showing subjects their estimated annual carbon footprint based on the input they provided; and IV) Eliciting individual demand for green electricity. For the control treatment, subjects were not provided any information about the carbon footprint of others. All other subjects received information about the carbon footprint of "Others like you who took this survey". (See Figure 2)

Part I of the survey consisted of several web pages eliciting information about energy use, including housing characteristics (type, age, size of residence, and location), home energy use (monthly electric and gas bill expenditures, type of fuel used to heat house, whether the household generates or purchases electricity); automobiles (number, models, use of each vehicle) and transportation choices (use of public transportation, frequency of short and long domestic flights, frequency of international flights). Subjects were also asked about whether they purchased carbon offsets and if so, how many had they purchased. Only 31 subjects reported having purchased carbon offsets.

Subsequent to providing the above information, subjects were provided with three IPCC climate policy scenarios and their anticipated consequences as presented below in Figure 1. The purpose of this screen was two-fold. First, we wanted to make respondents aware of current climate projections and relative policy options ranging from "Business as Usual" to "Aggressive Emissions Reductions." To a certain extent, this information also served to induce an element of moral outrage for those concerned about climate change.

In Part III, respondents were provided with an estimate of the carbon generated from their use of utilities and transportation and, after accounting for offset purchases, their estimated carbon footprint ("the total amount of climate changing greenhouse gas emissions caused directly and indirectly by your household") in tons of carbon per year. Carbon footprints were calculated using two algorithms. If participants knew their electricity and heating expenditures, information about average electricity and fuel prices in each state were used to determine annual consumption of electricity and fuel. (If participants knew their fuel expenditures but not their fuel source for heating, a weighted average of all fuel sources for the state was used.) Annual consumption of electricity was then converted into CO2 emissions using the average CO2 intensity for each state. Fuel consumption was converted into CO2 emissions using information about CO2 intensity for each fuel type. If participants did not know their electricity and heating expenditures, we gathered information about their housing structure and compared it to information about average energy consumption for houses of similar age, type and size in their state, which was then used to calculate CO2 emissions as above. Information about fuel prices, generation mix and average household energy consumption was obtained from the Energy Information Administration of the Department of Energy.

Information about participants' cars and miles driven was directly computed based on combined city/highway fuel economy information from the EPA for every make, model and year of car from 1983 to 2009. For air travel, short flights were assumed to be 100 miles each way, long flights 750 miles, and international flights 4,250 miles. Carbon offsets reduced the carbon footprint by 168 pounds for every dollar spent, equivalent to prevailing rates at popular commercial carbon offset retailers.

Median estimated carbon emissions for the sample were 17.9 tons per household per year. For subjects in the control group, no other information was provided.[2] Individuals in the treatment groups were informed that "Others like you who took this survey in the past had a carbon footprint of xx tons per year" and whether their contribution was MORE or LESS than this value. The "xx" value was randomly assigned to be high (26 tons) or low (11 tons). For example, a subject with an estimated carbon footprint of 18 tons and was assigned to the "See

[2] In pilot experiments, we also compared the results of a control group where no information about carbon footprint was given to the current control group where the carbon footprint was given without peer comparison and found no significant difference in behavior.

Low" group would be told that "Others like you who took this survey in the past had a carbon footprint of 11 tons per year" and that "Your contribution to global warming is MORE than this average." Similarly, a like individual who was assigned to the "See High" treatment was "Others like you who took this survey in the past had a carbon footprint of 26 tons per year" and that "Your contribution to global warming is LESS than this average." 26 tons and 11 tons were selected because they were the average footprint from various pilot samples, that happened to be near the 25th and 75th percentile of the total sample. This ensures that on average about half of all of those treated were informed that they were relatively more culpable than others, while half received information that they were relatively less culpable. As will be discussed below, the difference between the subject's carbon footprint and the value associated with the reference individual provided a measure of relative culpability.

Given this information contingent values (CV) were elicited using a modification of a green electricity payment card used in Champ and Bishop (2001, 2006) in which individuals were given opportunities to buy blocks of energy measured in kilowatt hours. As shown in Figure 3 each block had a corresponding monthly and annual cost and estimated annual tons of CO2 averted based on information available from the Energy Information Agency of the Department of Energy.

In Part IV, debriefing and demographic questions were asked, along with ten questions designed to measure environmental concern drawn from the New Environmental Paradigm (NEP) scale (Dunlap and Van Liere, 1978; Dunlap et al. 2000.) This scale is widely used in the psychology and sociology literature to characterize an individual's environmental concern based on the extent to which they agree or disagree with various statements of environmental concern:

> "limits to growth, anthropocentrism, the fragility of the balance of nature, rejection of the idea that humans are exempt from the constraints of

> nature, and the possibility of an eco-crisis or ecological catastrophe. The response categories range between 1 and 5 so that high scores correspond to a stronger pro-environmental attitude than low scores (with the ordering reversed for the statements that reject the NEP-paradigm)" (Ek and Söderholm, 2008, p. 175)

Past studies of willingness to pay for green electricity have found the aggregated values across a series of NEP questions to be a significant, exogenous explanatory variable (Kotchen and Moore, 2007; Ek and Söderholm, 2008). We also asked subjects their political party identification, and political orientation on a Likert scale that ranged from "Very Liberal" to "Very Conservative".

Twelve observations in our data set were identified as outliers and excluded from analysis: ten of these observations were excluded because at least one component of their carbon footprint was much greater than the rest of the sample, often an order of magnitude more. These observations were unrealistically high values, appearing to be incorrectly entered responses as to miles driven, airline flights, carbon offsets purchased, or housing information. The other two observations are repeated surveys. Removing these twelve observations halves the mean of the reported carbon footprint and reduces the standard deviation by an order of magnitude.

Lab Experiment:

We endeavored to develop a parallel experimental economics laboratory in which subjects purchase "private commodities" (analogous to electricity) that generate a negative public externality (analogous to pollution) for a group in which they are a member. The subjects are subsequently given an opportunity to contribute to a fund that would reduce the negative harm created by the externality, akin, we believe to the opportunity to purchase green electricity.

Subjects (n=240) were recruited from a variety of undergraduate business and economics courses at Cornell University. Pen and paper experimental sessions were conducted in the Laboratory for Experimental Economics and Decision Research in cohorts ranging in size from 10 to 20. A session lasted approximately 45 minutes and average earnings were $14.41.

Subjects were randomly assigned into groups of five anonymous participants including themselves. Adapting Plott's (1983) seminal externality experiments, each individual was given a balance of $9 at the beginning of each of five rounds and a per-unit value (demand) function for a commodity that could be purchased at a cost of $1 (experimental dollars were converted to real dollars at a rate of $15 experimental = $1 real.) Subjects in each group were randomly assigned into high, low and medium demands and the choices offered to individuals were presented (see Appendix for full experimental instructions).

In addition to private return for each commodity unit purchased, subjects were informed that each unit purchased would impose a negative externality on the entire group,

> *Your group also shares a GROUP FUND. This group fund began with 300 experimental dollars, and at the end of the experiment, any dollars in this group fund will be divided equally between all members of the group. Your actions and the actions of other people in your group in Round 1 may have reduced the total amount of dollars remaining in the group fund.*
>
> *In Round [1-5], every unit of the commodity that you purchase decreases the number of experimental dollars in the group fund by 1.25. (Because there are five people in your group, every unit of the commodity that you purchase reduces the amount in the group fund by 0.25 dollars per person. Likewise, every unit of the commodity purchased by everyone else in the group reduces the amount in the group fund by 1.25 dollars and therefore costs everyone else 0.25 dollars.)*

Hence, the optimal private decision would be to purchase only those commodities with a value of $1.25 or higher. Examples were worked through with the entire session on a whiteboard at the front of the lab, and after each decision, subjects were asked to calculate and report their own private returns and the impacts of their private decisions on other members of the group. Subjects were asked to sum their commodity purchases over the first five rounds and write this number down on a "passing sheet" which was submitted to the experimental moderator. The experimental moderator passed these sheets back to other subjects, who were then asked to record their own total purchases and the amount of total purchases that they saw on the sheet that was passed to them. Those in the high culpability treatment received the sheet of someone else with low demand, those in the medium culpability treatment received the sheet of someone with medium demand, those in the low culpability treatment received the sheet of someone with high demand, and those in the control received their own sheet back again. As in the CV experiment, we dropped 14 out of 240 outliers from analysis on the assumption that they were not paying attention carefully to the rules of the game. These were the subjects that chose to consume more than what was even privately optimal (i.e. they consumed at levels where the private cost exceeded the private benefit).

Analysis and Results

Contingent Valuation Experiment

Our analyses of the contingent valuation and laboratory experiments break the sample into treatment and control groups. In the contingent valuation "Treatment" group, subjects were informed about the carbon footprints of "Others like [them] who took this survey in the past", with others like them corresponding to the "See Low" (n=111) and "See High" (n=84) information described previously. Similarly, the "Treatment" group in the Lab Experiment is organized by whether subjects were passed information from a subject with a "High" (n=63), "Medium" (n=29) or “Low” (n=62) induced demand. No such relative information was provided to the “Control” groups in the contingent valuation (n= 79) and lab (n =64) experiments.

Averages for the control and treatment groups are provided in Tables 1 for the contingent valuation experiments. In the contingent valuation experiment, the dependent values reported are annual willingness to pay for green electricity. As these data are not conditioned on other possible covariates, some caution should be taken in interpreting the treatment effects. However, it is particularly notable that in both cases, providing information appears to either not affect average contributions or has a negative effect relative to the control group. The high culpability (11 ton) inducement yielded the same average willingness to pay ($143.40) as the control ($143.33). The low culpability inducement led people to contribute less ($107.68). This would suggest that providing social norms tends to lower willingness to pay values. The average willingness to pay of the full treatment group was ($128.20). If these results generalize, then contingent valuation studies that fail to provide information about peers would provide higher values than studies that provide such information, regardless of whether the individual is higher or lower than the norm. Such a result corresponds to the “broken windows” effect that observing others violate one social norm makes subjects more likely to violate other social norms. (Keizer et al. (2008)

Columns (4) and (5) show the summary statistics divided by those who saw peer information lower (“saw low”) or higher (“saw high”) than themselves. While the willingness to pay in these columns cannot be cleanly interpreted because membership in saw high or saw low is endogenous and depends on own carbon footprint, dividing the dataset in this way will be useful when we turn to regression analysis to understand the asymmetry in behavior. However, we address the endogeneity directly in the lab experiment.

Econometric modeling reveals more about the structure of how subjects responded to the peer information. In modeling the responses to the contingent valuation experiment, the dependent variable we use is "extra cost per year." Given the discrete, ordered nature of the payment card response options, we extend Cameron's expenditure difference model (1988) to the interval modeling format developed in Cameron and Huppert (1989), wherein circling a particular threshold value provides the lower bound of a willingness to pay (WTP) interval bounded from above by the next cost point. Assuming a logistically distributed WTP function, and letting E(WTP) = γZ and var(WTP) = σ^2 yields the following log likelihood function:

$$Ln(L) = \sum_{i=1}^{n} \ln \left[F\left({(\gamma Z_i - t_{iU})}/{\theta} \right) - F\left({(\gamma Z_i - t_{iL})}/{\theta} \right) \right],$$

where F(.) indicates the logistic distribution, Z is a vector of covariates, t_{iU} is the upper bound of the interval selected, t_{iL} is the lower bound, and the scale parameter $\theta = \sigma\sqrt{3}/\pi$.[3] Throughout, robust standard errors are reported, based on the Huber-White heteroskedasticity-consistent-covariance-matrix estimator.

For the treatment group, we constructed a relative culpability variable measuring the difference between the subject's carbon and the "other" carbon footprint he/she was shown.

$$\text{Culpability} = \text{Own footprint} - \text{Observed footprint of others}$$

In specifications where we include the control group which had no information about their peers, we set culpability to zero on the assumption that people assume

[3] These regressions were also all done with OLS and Tobit (because of non-negativity constraints), as well as Huber-White (heteroscedasticity-consistent covariance matrix estimator) standard errors and (for the lab experiment) corrections for cluster-level standard errors. All these alternate models produced essentially the same results.

their footprint is about the same as others. However, since it is reasonable to assume that the effect of culpability differs depending on whether peer information was made available, we focus on the regression specifications that drop subjects in the control condition. In the regressions reported in Table 2, we also included controls for the subject's own carbon footprint (CO2 Footprint), the NEP scale response summed over the 10 Likert scale NEP questions (NEP)[4], and a self-reported political scale (Political Scale) variable extending from 0 (very liberal) to 6 (very conservative), which has been recoded into a binary variable for liberal political leaning at the median of the sample. These latter two variables comport with the environmental and political orientation variables in the Costa and Kahn study (2010). In addition, standard demographic and socio-economic variables of the type typically included in contingent valuation research (age, gender, children in household, income and education) are added as covariates.

Table 2 reports estimation results for Full Models with all the aforementioned covariates and Short Models with only a subset of the variables. The vector of covariates was organized into three sub-vectors: 1) Estimation Variables (Constant, Theta); 2) Culpability Measures (Relative Culpability > (<) 0; Relative Culpability, CO2 Total); and 3) Demographic Variables (NEP, Politics, Children, Age, Income, Education). For both the latter two groups the estimation strategy followed the pretest estimation procedure presented in Goldberger (1991) wherein Likelihood Ratio Tests were used to test the zero-null-vector hypothesis for the entire group (which was rejected in all cases). This was followed by a stepwise procedure in which the most insignificant coefficients were sequentially dropped. Coefficients were retained in the short model if their corresponding p values were

[4] The Cronbach alpha value for the subjects for the NEP questions was 0.7785, generally consistent with the literature, and indicating that the NEP is a coherent metric.

less than the cutoff value of 0.15. Further, CO2 Total was kept as a control variable in all estimations.

The econometric analysis reveals that though on average, those who received peer information were willing to contribute less than those who did not, people are indeed positively and significantly influenced by relative culpability—those who were induced to feel relatively more culpable were willing to pay more than those who were induced to feel relatively less culpable. Specifically, for each ton of CO2 a person is led to believe that she polluted more than others, her willingness to pay increases by $2.84 to $3.11. For context, the mean culpability score for someone who saw a lower footprint was 16.96 tons, and the mean WTP for the control group was $143.33. In addition to the estimation variables, culpability, and CO2 Total, Only the NEP covariate was retained in the Short Model.

To better reconcile the regression results with the aggregate effects, we interact binary variables for those with positive culpability scores (those who are induced to feel more culpable than others) and those with negative culpability scores (those who are induced to feel less culpable than others) with the relative culpability measure. This is referred to as "Conditional Culpability" in Table 2. Columns (4) and (5) present the results and find evidence that the impact of peer information is asymmetric. Those who are more culpable than those they observed significantly increase their WTP by $3.36-$3.60 for each ton of additional culpability. There is no significant effect of relative culpability for those who are less culpable than those they observed (p=0.326 in a z-test). Note that since we control for each individual's own CO2 footprint, the coefficient on culpability is identified off the exogenously assigned treatment group. There remains the concern that in this asymmetry, we are merely capturing the difference between those with high footprint and low footprint in a way that is not controlled for by the inclusion of the footprint variable (perhaps due to a nonlinear relationship). To address this concern, we rely on the results from the lab experiment where footprint is exogenously assigned.

Lab Experiment

In order to better isolate the effect of culpability we rely on the results of a context-free lab experiment in which an individual's impacts on the public good is an outcome of an induced demand for the private good. Since culpability depends only on own consumption levels and the observed consumption levels of others, the lab experiment allows a degree of exogenous control over both components.

Table 3 presents the summary statistics for the lab experiment. Note once again, that even though positive culpability was induced for two of the three treatment conditions, as before, all conditions yielded less (or at most equal) altruistic behavior than the control (3.41 tokens). On average, it appears that information on culpability leads to less altruistic behavior in both CV and experimental laboratory settings.

Since each unit of a subject's consumption choice generates negative externalities on others in the experimental session, we use their consumption choice as the analogue for "carbon footprint." Also, in order to ensure the exogeneity of the culpability variable, we use the expected target footprint he would have been induced to select if he were a completely self-interested rationally maximized individual given the treatment condition he was in (high demand, medium demand, low demand) instead of using the subject's actual own "footprint" minus footprint of others,. This measure is highly correlated with actual culpability ($\rho = 0.7799$), but ensures that the culpability score is exogenous and not correlated with subject characteristics like altruism, as is possibly the case in the CV experiment.

$$\text{Lab Culpability} = \text{Induced target footprint} - \text{Observed footprint of others}$$

Table 4 presents the maximum likelihood estimates using the same econometric model and estimation strategy as the one used for the CV experiment, with similar asymmetric patterns emerging. Relative culpability is not significant in the full sample, and indeed the only significant coefficient is that of the politics covariate. When the estimation separates those who were either above or below the norms shown, those with relatively high induced relative culpability provide significantly more to the public good in the short, but not the full model. There is an insignificant effect for those with less relative culpability. Note that we used a maximum likelihood model here to be consistent with the CV specification, but we also tested OLS, Tobit, and an IV specification where we used the exogenously assigned treatment group as an instrument for culpability in a reviewer's appendix. We also repeated those specifications, clustering by experimental group. These alternate specifications yielded largely similar results.

Note while the culpability variable is insignificant for the full sample, we again see the asymmetric effect when one sees higher others compared to seeing lower others in the short model.

Heterogeneity in Responsiveness to Norms

Costa and Kahn (2010) noted the heterogeneous effect of the peer information experiment on Democrats vs Republicans. We confirm their findings by dividing the data into self-identified "Democrats" (a relatively liberal party in the United States) and all others (Non-DEM). We extend their work by also considering heterogeneity in other dimensions, including liberal versus conservative, number of children, gender, age, income, education, and NEP score, available for the relatively diverse contingent valuation study. For each of these socio-economic dimensions, we partitioned our sample along the median, and ran the same estimation models as above for each partition. Summary statistics and correlation tables are found in the reviewer's appendix—note that although these demographic characteristics are correlated, the correlations are quite low.

We first note that our results are consistent with Costa and Kahn (2010). As shown in column (1) of Table 6, the coefficient on culpability for Democrats was positive and significant, indicating that such individuals are responsive to social norm nudges. Indeed, in the regression this parameter dominates in the sense that the coefficients for the other explanatory variables are not significant. As shown in Column (3), however, neither the coefficient for Culpability nor for the CO2 Footprint is significant: non-democrats are not affected by our culpability inducement. Yet, coefficients for NEP and Political Scale are significant and consistent with expectations in the Non-Dem regressions.

It is evident that this heterogeneity in response patterns extends to other dimensions. We find that culpability is effective for liberals but not non-liberals; for those with children but not for those without children; for men but not for women; for those older than 36.5 but not those younger; for those above approximately $50,000 for income but not for those below; for those with a college degree but not for those without; for those who are more environmental conscious (NEP score > 34.5)..

A possible explanation for the patterns in Tables 5 - 7 is that peer information nudges work on those already inclined to give, but do not work and may even backfire when preaching to those less inclined (see Meier (2007a, 2007b) for a brief summary of related work on the importance of heterogeneity). It is also possible that in the specific context of climate change, those who question the premise of whether climate change is happening may be unresponsive.

We should be careful to note that this heterogeneity analysis should be seen as exploratory and mostly provided to be suggestive for future work. However, the fact that such heterogeneity exists appears quite robust. Awareness of this heterogeneity is important for increasing the precision of estimates of the effect of peer information interventions, as well as for increasing the cost effectiveness of future norm based interventions.

Conclusions

Using a contingent valuation framed field experiment coupled with a conventional lab experiment to examine how peer information that induces culpability differs from peer information interventions based on conformity. We demonstrate that there is important heterogeneity in how altruism responds to such peer information. We find similar patterns of heterogeneity for both the online contingent valuation experiment and the context free lab experiment using a convenience sample. We find the culpability effect is larger when the information makes the subject feel good about themselves, then when the information makes them feel guilty. We also find suggestive evidence about the locus of the effect, by finding that the effect of culpability comes mostly from those who may be more inclined to. This result has potentially important implications for public policy. Strategies that induce culpability affect primarily individuals who are more inclined to reduce energy consumption in the first place. As a consequence, they are more likely to be cost-ineffective and should not be seen as a substitute for more traditional policies that could alter the behavior or the entire population Prospect Theory (Kahneman and Tversky 1979) may also explain this asymmetry in response. In our results, people whose footprints exceed the reference amount would find themselves in the loss domain, and weight such a loss more heavily than the gain of being below the reference amount. However, we leave a fuller development of such theoretical implications to future research.

Appendix

Appendix Table 1 Provides results for the OLS, Tobit, and IV specifications for the Lab Experiment results. The Tobit specification deals with the non-negativity constraint on the amount of public goods we allow each subject to provide. The IV uses the induced target culpability based on the exogenously assigned treatment group as an instrument for actual culpability, to ensure that the culpability variable is exogenous. In columns 1 and 2 of Table 9, the effects of culpability (the difference between a subject's own purchases in rounds 1 through 5 and the purchases of the subject whose information he or she saw) has no significant impact on purchases of the public good in round 6, even when we restrict our sample to just those subjects who received information not there own. However, when we split our sample between in those who self-identify as Democrats versus those who do not, we see a significant, positive impact on culpability and footprint on contribution, compared with those who do not self-identify as Democrats. (Compare columns three and four.) Columns five and six repeat this analysis of sub-sections of the data with a Tobit model, to check for biased estimates of coefficients due to censoring of allowed values of the contributions to the public good below zero. Finally, instrumental variables are used to control for the confounding effects of people who voluntarily purchase less than the privately optimal amount of the goods in rounds 1-5 on contribution to the public good. Such a subject would have already made a sacrifice by forgoing possible earnings in the first five rounds in order to cause less harm to the group, and thus their culpability is affected. By using a dummy for the type of subject (small or large) they received from as an instrument for culpability, we can control for this effect, and confirm the positive correlation between culpability and contribution to the public good for Democrats. Appendix Table 2 provides summary statistics and a correlation matrix for the demographic splits.

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Figure 1: Information about Climate Change Presented in On-Line Survey

Climate Options The IPCC has presented several options for reducing climate change, each with different final levels of carbon and impacts on the global climate:

	Business as Usual	Small Emissions Reductions	Aggressive Emissions Reductions
Mean Percent change in Carbon Emissions from 2000 to 2050	115% Increase	55% Increase	70% Decrease
Global Average Temperatures Increases	8.8-11 degrees (4.9-6.1 degrees Celsius)	7.2-8.8 degrees Fahrenheit (4-4.9 degrees Celsius)	3.6-4.3 degrees Fahrenheit (2-2.4 degrees Celsius)
Sea Level Increases	12-24 inches (0.3 - 0.6 meters) Millions at risk of coastal flooding	10-24 inches (0.26 - 0.6 meters) Millions at risk of coastal flooding.	Less than 17 inches (0.45 meters)
Extinction Risk	More than 40% of species face some risk	More than 40% of species face some risk	30% of species face some risk
Crops and Famine	Crop productivity is expected to decrease. Global food production is expected to decrease, causing an increased risk of famine.	Crop productivity is expected to decrease. Global food production is expected to decrease, causing an increased risk of famine.	Crop productivity may increase in some regions and decrease in others. Increased risk of famine in some areas.
Other effects	Increase in intensity and frequency of heat waves. Increased range for tropical diseases. Together, these will cause death and sickness, placing a substantial burden on health services.	Increase in intensity and frequency of heat waves. Increased range for tropical diseases. Together, these will cause death and sickness, placing a substantial burden on health services.	Increase in intensity and frequency of heat waves.

Figure 2: Information about Carbon Footprint presented in the survey.

The “low treatment” (11 tons) is shown below.

Reveal Carbon Footprint

Carbon Production from Utilities: 6.2 tons per year
Carbon Production from Tranportation: 1.4 tons per year
Carbon Offsets: 0 tons per year

Estimated Carbon Footprint: **7.6** tons per year

Your Carbon Footprint is the total amount of climate changing greenhouse gas emissions caused directly and indirectly by your household.

Others like you who took this survey in the past had a carbon footprint of **11** tons each year.

Your contribution to global warming is **LESS** than this average.

Figure 3: Elicitation Question for Contingent Valuation in On-Line Survey

Green Energy

Suppose your electric utility were to offer you renewable energy appropriate to your area. For example, wind, solar, geothermal, or tidal power could all be offered, depending on your geographic location. Choose the option you would like to purchase from the table below. (Information from the Energy Information Administration of the Department of Energy.)

	Size of Block	Extra Cost per Month	Extra Cost per Year	Tons of CO2 Averted per Year
○	0 kilowatt hours	$0.00	$0.00	0 tons
○	50 kilowatt hours	$2.80	$33.60	0.405 tons
○	100 kilowatt hours	$5.60	$67.20	0.81 tons
○	200 kilowatt hours	$11.20	$134.40	1.62 tons
○	300 kilowatt hours	$16.80	$201.60	2.43 tons
○	400 kilowatt hours	$22.40	$268.80	3.24 tons
○	500 kilowatt hours	$28.00	$336.00	4.05 tons
○	600 kilowatt hours	$33.60	$403.20	4.86 tons

Submit

Table 1: Summary Statistics for Contingent Valuation Experiment

	By Treatment Group			Treated: By Culpability	
	Control	Saw 11 Tons	Saw 26 Tons	Saw Low Footprint	Saw High Footprint
WTP (Average of lower bound of interval)	143.33	143.40	107.68	152.26	96.40
	(15.41)	(12.30)	(12.98)	(12.87)	(11.46)
CO2 Total	23.30	20.84	25.91	32.01	11.08
	(2.35)	(1.85)	(2.64)	(2.34)	(0.67)
Relative Culpability		9.84	-0.09	16.96	-9.38
		(1.85)	(2.64)	(2.18)	(0.72)
NEP	34.01	35.25	34.65	34.37	35.82
	(0.81)	(0.67)	(0.82)	(0.71)	(0.75)
Politics	0.75	0.75	0.66	0.73	0.69
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)
Children	0.58	0.50	0.53	0.63	0.36
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
Gender	0.57	0.49	0.49	0.47	0.52
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
Age	37.61	37.50	40.39	36.86	41.20
	(1.17)	(1.19)	(1.40)	(1.10)	(1.50)
Income	5.04	4.65	4.30	4.94	3.93
	(0.23)	(0.17)	(0.21)	(0.17)	(0.20)
Education	0.53	0.53	0.46	0.55	0.43
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
Democrat	0.41	0.46	0.34	0.41	0.40
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)
N=	79	112	83	111	84

Summary statistics for Not outliers, with no missing observations

Standard Errors in parentheses

CO2 Total: Total CO2 Footprint

Culpability: Total CO2 Footprint – (11 or 26 tons, depending on treatment)

NEP: Aggregate NEP value

Politics: Binary for liberal/conservative (1 if liberal)

Children: Binary for children in household

Gender: Binary for gender (1 if female)

Age: Age of respondent

Income: Household income in levels (0: <$10K, 1: $10K-$15K, 2: $15K-$25K, 3: $25K-$35K, 4: $35K-$50K, 5: $50K-$75K, 6: $75K-$100K, 7: $100K-$150K, 8: $150K-$200K, 9: >$200K

Education: Binary for education (1 if at least college education)

Democrat: Binary for party affiliation (1 if democrat)

Table 2: MLE Results for Contingent Valuation Experiment

	Control	Treated			
		Continuous Culpability		Conditional Culpability	
		Full Model	Short Model	Full Model	Short Model
Constant	-127.41	-35.04	-36.32	-32.97	-32.25
	(113.61)	(56.77)	(48.63)	(56.82)	(49.15)
Relative Culpability>0				3.60**	3.36**
				(1.52)	(1.52)
Relative Culpability<0				2.64*	2.34
				(1.50)	(1.50)
Relative Culpability		3.11***	2.84**		
		(1.17)	(1.17)		
CO2 Footprint	0.58	-0.89	-0.63	-1.25	-0.99
	(0.88)	(1.22)	(1.23)	(1.41)	(1.40)
NEP	7.41***	4.63***	5.16***	4.57***	5.08***
	(2.49)	(1.28)	(1.20)	(1.28)	(1.21)
Politics	5.98	9.93		9.84	
	(38.02)	(20.01)		(19.97)	
Children	9.25	26.07		27.50	
	(35.85)	(18.74)		(18.95)	
Gender	-23.46	-22.73		-22.20	
	(36.05)	(17.76)		(17.77)	
Age	0.42	1.12		1.12	
	(1.75)	(0.72)		(0.72)	
Income	-2.82	-8.51		-8.24	
	(8.56)	(5.56)		(5.58)	
Education	48.17	15.06		14.74	
	(35.55)	(18.48)		(18.47)	
Theta	80.46***	67.94***	69.38***	67.83***	69.26***
	(8.30)	(4.31)	(4.39)	(4.31)	(4.39)
Observations	79	195	195	195	195
Log Likelihood	-176.44	-424.85	-428.41	-424.72	-428.27

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All samples exclude outliers and observations with any missing samples

Table 3: Summary Statistics for Laboratory Experiment

	Contro l	Entire Sample By Induced Demand			Treated By Culpability		
		Small	Medium	Large	Saw Smaller	Saw Larger	Saw Same
Round6	3.31	2.38	2.79	3.42	2.81	2.78	0.83^
Purchases	(0.45)	(0.37)	(0.56)	(0.38)	(0.40)	(0.43)	(0.48)
Relative	n.a.	-5.78	3.55	11.23	-3.60	16.59	0.83
Culpability		(1.04)	(1.28)	(1.16)	(1.65)	(1.09)	(3.31)
Total Purchases	18.27	12.86	18.97	23.51	13.10	25.49	22.67
	(1.13)	(0.36)	(0.98)	(1.11)	(0.56)	(1.01)	(3.95)
NEP	24.23	22.70	25.67	24.26	22.63	24.94	25.33
	(0.72)	(0.60)	(1.10)	(0.64)	(0.60)	(0.89)	(2.43)
Liberal	0.58	0.68	0.48	0.54	0.63	0.53	0.67
	(0.06)	(0.06)	(0.09)	(0.06)	(0.06)	(0.07)	(0.21)
Democrat	0.45	0.54	0.42	0.44	0.55	0.41	0.50
	(0.06)	(0.06)	(0.09)	(0.06)	(0.06)	(0.07)	(0.21)
Obs	64	69	33	81	62	51	6

All samples are excluding “greater than ideal”: people whose purchases exceeded the private optimum and likely misunderstood the experiment. All samples also exclude any observations with any missing responses.

Standard errors in parentheses.

^: Value is less than control at $p<0.05$

Table 4: MLE Results for Laboratory Experiment

	Control	Treated			
		Continuous Culpability		Conditional Culpability	
		Full Model	Short Model	Full Model	Short Model
Constant	0.51	2.88*	1.71	2.42	2.00*
	(3.17)	(1.61)	(1.23)	(1.63)	(1.16)
Relative Culpability>0				0.06*	0.07*
				(0.04)	(0.04)
Relative Culpability<0				-0.03	-0.05
				(0.05)	(0.05)
Relative Culpability		0.02	0.02		
		(0.02)	(0.02)		
Total Purchases	-0.01	-0.01	-0.02	-0.01	-0.03
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
NEP	0.14	-0.05		-0.04	
	(0.10)	(0.05)		(0.05)	
Politics	0.71	0.87	0.93*	0.74	
	(1.03)	(0.56)	(0.56)	(0.56)	
Exp Dummies?	Yes	Yes	Yes	Yes	Yes
Theta	1.94***	1.55***	1.56***	1.54***	1.56***
	(0.20)	(0.12)	(0.12)	(0.12)	(0.12)
Obs.	64	119	119	119	119
Log Likelihood	-168.79	-290.11	-290.74	-289.27	-290.70

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

All samples are excluding "greater than ideal": people whose purchases exceeded the private optimum and likely misunderstood the experiment. All samples also exclude any observations with any missing responses.

Table 5: Summary Statistics Split by Demographic Subgroup for Contingent Valuation Experiment

	Liberal	Not Liberal	Children	No children	Male	Female	Age>36.5	Age<36.5
WTP, average of Lower Bound of interval	137.30	105.60	142.46	113.18	137.12	119.00	138.60	118.11
	(11.09)	(14.94)	(12.94)	(12.47)	(12.38)	(13.18)	(13.73)	(11.79)
Relative Culpability	6.13	4.34	10.86	0.09	9.53	1.58	0.09	10.97
	(1.91)	(2.81)	(2.18)	(2.17)	(2.61)	(1.68)	(1.37)	(2.72)
Total CO2	23.06	22.84	28.46	17.25	26.89	18.98	17.96	27.88
	(1.87)	(2.82)	(2.12)	(2.13)	(2.60)	(1.57)	(1.26)	(2.72)
NEP	36.43	31.43	34.66	35.35	34.10	35.92	36.22	33.81
	(0.61)	(0.81)	(0.70)	(0.76)	(0.69)	(0.76)	(0.77)	(0.68)
Politics	1.00	0.00	0.73	0.69	0.69	0.74	0.65	0.78
	(0.00)	(0.00)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Children	0.53	0.48	1.00	0.00	0.49	0.53	0.43	0.60
	(0.04)	(0.07)	(0.00)	(0.00)	(0.05)	(0.05)	(0.05)	(0.05)
Married	0.58	0.77	0.80	0.46	0.66	0.61	0.72	0.56
	(0.04)	(0.06)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Gender	0.51	0.45	0.51	0.47	0.00	1.00	0.51	0.47
	(0.04)	(0.07)	(0.05)	(0.05)	(0.00)	(0.00)	(0.05)	(0.05)
Income	4.56	4.36	5.07	3.91	4.62	4.39	4.35	4.65
	(0.16)	(0.24)	(0.17)	(0.19)	(0.21)	(0.17)	(0.18)	(0.20)
Age	37.22	42.46	36.84	40.72	38.38	39.08	49.67	28.12
	(1.01)	(1.86)	(0.99)	(1.53)	(1.31)	(1.26)	(0.83)	(0.50)
Education	0.52	0.45	0.54	0.45	0.59	0.41	0.43	0.57
	(0.04)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Democrat	0.53	0.13	0.42	0.40	0.31	0.51	0.42	0.40
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	139	56	100	95	99	96	96	99

Standard Errors in Parentheses

Table 5: Summary Statistics Split by Demographic Subgroup for Contingent Valuation Experiment (Cont.)

	Income>4.7	Income<4.7	At least college	No college	Dem	Not Dem
WTP Lower Bound	139.85	112.80	135.79	120.69	139.44	120.38
	(12.03)	(13.58)	(12.25)	(13.29)	(14.01)	(11.82)
Relative Culpability	10.46	(0.80)	9.71	1.56	4.90	6.11
	(2.27)	(1.92)	(2.60)	(1.73)	(2.38)	(2.12)
Total CO2	27.55	16.99	26.59	19.45	21.15	24.29
	(2.26)	(1.83)	(2.64)	(1.58)	(2.41)	(2.03)
NEP	34.30	35.92	34.67	35.32	36.41	34.01
	(0.62)	(0.87)	(0.68)	(0.78)	(0.71)	(0.71)
Politics	0.73	0.69	0.74	0.68	0.91	0.57
	(0.04)	(0.05)	(0.04)	(0.05)	(0.03)	(0.05)
Children	0.62	0.37	0.56	0.47	0.53	0.50
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)
Married	0.74	0.50	0.68	0.59	0.60	0.66
	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.04)
Gender	0.43	0.57	0.40	0.58	0.61	0.41
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Income	5.78	2.81	5.07	3.94	4.74	4.34
	(0.09)	(0.15)	(0.19)	(0.17)	(0.18)	(0.19)
Age	38.16	39.48	37.38	40.06	38.61	38.81
	(1.12)	(1.51)	(1.18)	(1.37)	(1.43)	(1.19)
Education	0.60	0.36	1.00	0.00	0.58	0.44
	(0.05)	(0.05)	(0.00)	(0.00)	(0.06)	(0.05)
Democrat	0.45	0.36	0.47	0.35	1.00	0.00
	(0.05)	(0.05)	(0.05)	(0.05)	(0.00)	(0.00)
Observations	111	84	97	98	80	115

Standard Errors in Parentheses

Table 6: MLE Results for Democrat/Non-Democrat Split: Contingent Valuation Experiment

	Democrat		Not Democrat	
	Full Model	Short Model	Full Model	Short Model
Constant	13.83	184.54***	-34.96	2.35
	(112.9)	(37.41)	(66.09)	(61.66)
Relative Culpability	5.577**	4.20**	1.942	2.00
	(2.277)	(2.02)	(1.386)	(1.40)
CO2 Footprint	-3.200	-2.28	0.334	0.42
	(2.162)	(2.04)	(1.479)	(1.49)
NEP	3.509		4.957***	5.42***
	(2.381)		(1.467)	(1.37)
Politics	-54.25		14.35	
	(53.74)		(22.38)	
Children	26.38		31.28	
	(30.26)		(24.19)	
Gender	24.78		-42.37*	-44.93**
	(30.86)		(22.86)	(21.55)
Age	5.145		-16.00***	
	(10.22)		(6.107)	
Income	1.305		0.951	-12.19**
	(1.377)		(0.838)	(5.87)
College	-3.200		24.09	
	(2.162)		(23.50)	
Theta	69.17***	73.40***	63.09***	64.07***
	(6.824)	(7.14)	(5.261)	(5.35)
Obs	80	80	115	115
Log Likelihood	-174.5	-178.38	-244.1	-246.22

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: MLE Results for Demographic Subgroups for Contingent Valuation Experiment (Full regression)

Subgroup	Culpability Coefficient
Liberal	3.33***
Not Liberal	2.58
Children	4.54***
No Children	1.88
Male	3.10**
Female	2.89
Age>36.5	5.14***
Age<36.5	1.10
Income>4.7	5.47***
Income<4.7	0.16
At least College	4.21***
Less than College	1.98
NEP>34.5	4.63**
NEP<34.5	1.55
Democrat	5.58**
Not Democrat	1.94

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1: Effect of Culpability on Contribution to a Public Good

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				Tobit		IV	
	Full	Treated	Dem	Not Dem	Dem	Not Dem	Dem	Not Dem
Relative	-0.0022	-0.00285	0.166***	-0.0352	0.240***	-0.0556	0.158**	0.0108
Culpability	(0.0287)	(0.0327)	(0.0521)	(0.0503)	(0.0710)	(0.0652)	(0.0705)	(0.0734)
Footprint	0.00119	0.0106	-0.230***	0.0488	-0.366***	0.0687	-0.220**	-0.00598
(Rounds1-5)	(0.0368)	(0.0534)	(0.0825)	(0.0807)	(0.114)	(0.105)	(0.102)	(0.0989)
NEP	0.0124	-0.0435	-0.145*	-0.0227	-0.213*	-0.0674	-0.143**	-0.0220
	(0.0470)	(0.0539)	(0.0818)	(0.0748)	(0.111)	(0.101)	(0.0707)	(0.0642)
Politics	-0.120	-0.192	-0.674*	-0.193	-0.783	-0.250	-0.665**	-0.245
	(0.166)	(0.200)	(0.368)	(0.316)	(0.498)	(0.406)	(0.316)	(0.279)
Session Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	4.128***	5.598***	12.14***	-0.197	10.37***	3.638	8.649***	5.322
	(1.554)	(2.034)	(3.350)	(3.866)	(3.731)	(4.340)	(2.838)	(3.335)
Obs	183	119	58	61	58	61	58	61
R-squared	0.064	0.177	0.474	0.246			0.474	0.232

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: Summary Statistics by Demographic Split

	Democrats	Not Dem	Liberal	Not Liberal	Children	No Child	Male	Female	Age>36.5	Age<36.5	Income >4.7	Income <4.7	At least a college degree	Less than a college degree	NEP>34.5	NEP<34.5	CO2<17.9	co2_total<17.9
WTP	137.72	116.25	133.94	101.94	139.62	109.69	131.82	117.43	135.39	114.17	134.12	111.47	131.74	117.92	138.68	110.91	147.69	106.40
	(13.94)	(11.20)	(10.75)	(14.35)	(12.68)	(11.91)	(12.03)	(12.72)	(13.15)	(11.51)	(11.45)	(13.49)	(11.95)	(12.76)	(12.92)	(11.69)	(14.21)	(10.62)
Relative Culp	4.69	5.49	5.78	3.66	10.52	(0.23)	8.79	1.45	0.02	10.28	9.57	(1.04)	9.18	1.28	2.46	7.86	19.38	(6.17)
	(2.36)	(2.01)	(1.84)	(2.72)	(2.13)	(2.07)	(2.52)	(1.63)	(1.35)	(2.64)	(2.15)	(1.92)	(2.52)	(1.68)	(2.18)	(2.12)	(2.62)	(0.80)
Total CO2	21.06	23.63	22.74	22.28	28.07	17.10	26.14	18.98	17.93	27.25	26.70	16.84	26.12	19.20	20.08	25.12	37.63	10.62
	(2.39)	(1.91)	(1.79)	(2.70)	(2.07)	(2.00)	(2.50)	(1.51)	(1.22)	(2.63)	(2.12)	(1.81)	(2.55)	(1.51)	(2.08)	(2.11)	(2.56)	(0.41)
NEP	36.54	34.23	36.49	31.76	34.86	35.44	34.27	36.04	36.30	34.02	34.61	35.92	34.89	35.40	40.94	29.47	34.58	35.60
	(0.71)	(0.68)	(0.59)	(0.81)	(0.69)	(0.73)	(0.67)	(0.74)	(0.74)	(0.67)	(0.60)	(0.87)	(0.67)	(0.76)	(0.44)	(0.43)	(0.76)	(0.68)
Liberal	0.91	0.58	1.00	0.00	0.73	0.70	0.68	0.74	0.64	0.79	0.73	0.68	0.74	0.68	0.80	0.62	0.73	0.70
	(0.03)	(0.04)	0.00	0.00	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)
Children	0.52	0.49	0.51	0.47	1.00	0.00	0.49	0.51	0.42	0.58	0.60	0.36	0.55	0.45	0.50	0.50	0.70	0.34
	(0.06)	(0.05)	(0.04)	(0.07)	0.00	0.00	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Married	0.59	0.65	0.57	0.76	0.80	0.45	0.63	0.61	0.72	0.53	0.72	0.49	0.66	0.59	0.61	0.64	0.78	0.50
	(0.05)	(0.04)	(0.04)	(0.06)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Female	0.62	0.41	0.51	0.44	0.50	0.48	0.00	1.00	0.52	0.47	0.44	0.56	0.40	0.59	0.57	0.42	0.42	0.55
	(0.05)	(0.04)	(0.04)	(0.07)	(0.05)	(0.05)	0.00	0.00	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Income	4.74	4.34	4.56	4.35	5.07	3.91	4.61	4.39	4.35	4.65	5.78	2.82	5.07	3.94	4.28	4.70	5.05	4.06
	(0.17)	(0.18)	(0.16)	(0.24)	(0.17)	(0.18)	(0.20)	(0.17)	(0.18)	(0.19)	(0.08)	(0.15)	(0.19)	(0.17)	(0.19)	(0.18)	(0.20)	(0.16)
Age	38.44	39.03	37.10	43.02	36.73	40.89	38.19	39.43	49.81	27.89	38.12	39.76	37.01	40.54	41.20	36.43	37.36	39.95
	(1.42)	(1.18)	(1.01)	(1.81)	(0.98)	(1.50)	(1.31)	(1.25)	(0.82)	(0.50)	(1.11)	(1.52)	(1.17)	(1.36)	(1.32)	(1.20)	(1.14)	(1.34)
Obss	81	124	146	59	103	102	104	101	102.00	103.00	120	85	101	104	102	103	91	114
Standard errors in parentheses																		

Appendix Table 3 Correlation Matrix:

	Culpability	Total CO2 Footprint	NEP	Liberal	Children	Female Gender	Income	Age	Democrat	At least a college degree
Relative Culpability	1									
Total CO2 Footprint	0.9985	1								
NEP	-0.0011	-0.0064	1							
Liberal	0.0227	0.0223	0.2884	1						
Children	0.1175	0.1233	-0.0966	0.0163	1					
Female Gender	-0.1048	-0.1095	0.1511	0.0122	0.0418	1				
Income	-0.0302	-0.0271	-0.0692	0.0379	0.264	-0.0937	1			
Age	-0.0432	-0.0471	0.1927	-0.142	-0.1762	0.0617	-0.0744	1		
Democrat	-0.0599	-0.0613	0.1764	0.3818	0.0164	0.13	0.0612	-0.0237	1	
At least a college degree	0.074	0.0766	-0.0482	0.0251	0.0894	-0.1904	0.3104	-0.1321	0.0551	1