BRIDGING THE CLIMATE IMPACT INFORMATION GAP BETWEEN INDIVIDUALS AND FIRMS: AN EMPIRICAL APPROACH

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Abstract

Using a novel policy design, this paper uses primary data from four jurisdictions to measure the willingness to pay of individuals when the environmental impact information asymmetry between them and firms is hypothetically eliminated. Though noisy and varying depending on the context, the results indicate that individuals exert an economically and statistically significant higher willingness to pay when they are certain that a firm has lesser environmental adverse impact. Complimenting this evidence, theoretical models suggest that this policy design has more potential to incentivise firms to invest in climate abatement compared to other policies, and might induce a race to the top where all firms try to reduce their negative externalities on the environment. If further research brings more robust data and analysis, this policy intervention can deal with two market failures at once.

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Keywords: information asymmetry, negative externalities, information economics, climate performance score, empirical evidence

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Authorship Declaration

I hereby declare and confirm that this thesis is entirely the result of my own work except where otherwise indicated. I acknowledge the supervision and guidance I have received from Professor Pierre Bentata. This thesis is not used as part of any other examination and has not yet been published.

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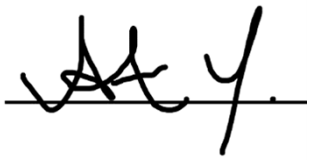

To my older brother Omer Faruk, the wisest of the wise, and to my younger brother Ibrahim Akin, the nicest of the nice.

Contents

1 INTRODUCTION

1.1 Short Motivation

In search for policies that would reduce the negative externalities imposed by market actors on the environment, a wide variety of legal instruments (from Pigouvian taxation to subsidies and from command-and-control to cap-and-trade systems) have been implemented by regulators across the world. All of these interventions have their inherent limitations ranging from distorted incentives to provide wrong information, killing the incentives to invest in research to find better production methods, difficulties in finding out the marginal social cost, inflexibilities, and political economy problems (Kolstad, 2000, pp.141-146). Empirically analysing 96 U.S. environmental policy changes, Hahn et al., 2024 find that revenue raisers such as taxes and cap-and-trade systems are good for raising revenue and also have societal positive effects, nudges can be effective but are contingent on the context, and certain subsidies such as directly displacing the dirty electricity production have good returns (pp. 37-51). Instead of these policies, the approach in this paper is to find a mechanism, that would exploit the power of information: Providing credible information to individuals and consequently incentivizing firms.

Information, however, is complex, difficult to measure-observe-and-verify, and most of the time incomplete and too costly to acquire (Stigler, 1961). Given these specifications of information, policy designs and interventions resolving the informational gap have shown to be effective in many different settings, from reduction in price dispersion, the elimination of waste, and adherence to the Law of One Price (R. Jensen, 2007) to citizens becoming more aware about the aid programs they are entitled to and accordingly bargain with local officials (A. Banerjee et al., 2015). Inspired by these research, and following the research agenda of environmental law and economics to find market mechanisms ensuring the creation, distribution, and utilisation of environmental information (Faure and Partain, 2019, p.27), this paper attempts to theoretically and empirically show that a novel policy design, the Climate Performance Score (CPS) scheme, may eliminate the information asymmetry between the individuals and firms in terms of the firms' environmental impact,

and as a by-product of the intervention, incentivize firms to invest in climate abatement and reduce their negative externalities imposed on the environment. CPS, therefore, could be an exception to the widespread acknowledgment in applied economics that using one instrument for multiple economic problems would render sub-optimal results and that "N" market failures require "N" instruments (De Geest, 2013).

Before this theoretical and empirical attempt, however, the paper will first show that there are enough reasons for being sceptical about the Corporate Sustainability Reporting Directive, the main policy of the European Union to reduce the information asymmetry in this context, and then will introduce the policy proposal of this research. Both of these setting will be analysed in the Theoretical Framework which will be followed by the research design an empirical results.

1.2 Why Corporate Sustainability Reporting Will Not Work

Corporate Sustainability Reporting (CSR) has a much wider scope than environmental impact, but throughout this paper it will be assumed that it has the potential to provide a somewhat credible information in that context too. Although the Directive prescribes the individuals as one of the ultimate beneficiaries who would be "adequately informed", the very nature of reporting itself, being a multi-faceted complex document, may inhibit the goal to reduce the information asymmetry between the parties. To overcome this, the European Commission adopted the European Sustainability Reporting Standards (ESRS). However, apart from the general disclosure requirements, ESRS is based on a materiality assessment which determines whether an impact is material enough for the stakeholders for reporting, though this is conducted by the firm too. As Barr et al., 2008 suggests, the goal of disclosure should be to improve the quality of information, otherwise simply adding information would be unlikely to work, and the authors also indicate that the information should be well understood and convey salient information that help the decision-maker act optimally (p.6).

Then the question becomes what would be the incentives of the firms in terms of disclosures. Admittedly, this is a difficult question to answer without seeing the first

CSR reports, but the experience from the modernized rules brought by the Securities and Exchange Commission (SEC) in 2018 and 2019 could be used for comparison. In the first empirical analysis of these regulations, Ho, 2020 finds that there is more of an underreporting issue and corporations are failing to identify emerging environmental or social risks as material so that important risk-related information is not being adequately disclosed (p.70). The underreporting problem due to the subjective materiality assessment is also indicated by American Institute of Certified Public Accountants (2016), and the Sustainability Accounting Standards Board (2016) which analysed 600 published reports. On the other hand, another empirical investigation points toward the unintended consequences of regulation-led disclosure and suggests that, though reports increase the quality of decision making until a certain threshold, stakeholders face an information overload after this threshold and the decision-making quality decreases with additional information (Impink et al., 2022).

Furthermore, even if the optimal level of information was somehow determined, would these data be reliable in the first place? A study analysing 709 indicators by using data provided by the six major ESG rating firms finds that the disagreement among raters is substantial as the rating correlations for the same firm by different agencies range from 0.38 to 0.71 with an average of 0.54 (Berg et al., 2022, p.8). Furthermore, utilizing a similar methodology, Dimson et al., 2020, find the pairwise correlations to be 0.45 (p.4). One might argue that this result could be stemming from weighing and simple differences in calculations. However, the researchers find the majority of the difference coming from measurement divergence with a contribution of 56% to the divergence, while scope divergence follows (38%) and weight divergence has minimal effect (6%) (Berg et al., 2022, p.4). What is even more striking is that the measurement divergence is in part driven by the "halo effect" (explaining 15% of the variation after controlling for other factors) which means that "*a firm receiving a high score in one category is more likely to receive high scores in all the other categories from that same rater*" suggesting that measurement divergence is not only randomly distributed noise (Berg et al., 2022, pp.18-20). European Securities and Markets Authority (ESMA), 2022 also conclude that divergence between

different ratings is significant (p.18). Unfortunately, the solution may not be simply disclosing more because by employing different estimations and using data from three largest ESG rating providers for 30,700 firm-year observations, Christensen et al., 2022 find that the greater ESG disclosure, the greater ESG rating disagreement becomes, and more disagreement on the outcomes rather than the inputs (pp.22-30), supporting the argument that there is immense measurement divergence between raters.

The result of all of these findings could be incentivizing companies for sending unreliable signals about their ESG performance rather than improving them (Larcker et al., 2022, p.6). Furthermore, similar to the halo effect but maybe even more striking, the empirical analysis of Tang et al., 2022 concludes that companies owned by the same institutional investors as their ESG rater receive higher ESG ratings after controlling for firm characteristics under different estimations (p.2). There is enough evidence to be sceptical about the reliability of information provided in these reports even if the optimal level of disclosure were to be found.

Apart from widespread strategic greenwashing (Lyon and Maxwell, 2011) which might be the optimal strategy for a firm (Wu et al., 2020), a recent Carbon Disclosure Project, 2023 shows that European companies classify 92% of their emissions coming from their value chain [sold products (57%) and purchased goods and services (17%)], including upstream and downstream emissions (pp.32-33). However, only 37% of these emissions are currently being addressed by their decarbonization measures (Carbon Disclosure Project, 2023, p.33). There is an apparent lack of a counterfactual but looking at proxy indicators in terms of climate change, it is clear that reporting had no clear positive effect on reaching environmental goals; the investor impact on real environmental outcomes seems to be minimal (Kölbel et al., 2020), 421 out of 593 ESG labelled equity funds and 72 out of 130 climate-themed equity funds (two categories hold over US330 billion in total net assets) are not even aligned with the Paris Agreement (InfluenceMap, 2021), ESG fund portfolio firms have more environmental law violations than non-ESG funds and again the former exhibiting worse performance with respect to carbon emissions in terms of both raw emissions output and emissions intensity (Raghunandan and Rajgopal, 2022, p.3)

and both atmospheric CO2 (Lindsey, 2024) and energy consumption (Enerdata, 2024) have been on the rise. Another research notes that when companies get an upgrade for their rating, on average, only 26% of all improvements are coming from environmental impact improvements, without affecting substantial practices such as an actual reduction in carbon emissions (Larcker et al., 2022, p.6). It could be that the focus on measurement and reporting has been only delaying the much-needed structural transformations (Pucker, 2021).

Given this scrutiny, one might think that companies do not spend too much on reporting. According to U.S. Securities and Exchange Commission, 2022 own estimations firms would have to spend up to $\$640,000$ for the initial compliance an incur an average annual cost up to $\$530,000$ every year (p.21439). In line with these estimations, a study conducted by ERM Sustainability Institute, 2022 shows that companies are, on average, spending $\$533,000$ annually on climate-related disclosure, while the figure for investors is $\$1,372,000$ to make more informed decisions. Moreover, the International Data Corporation (IDC), 2023 finds that ESG business services spendings (only one part of the cost) will reach 65 billion $ in 2027 from 37.7 billion in 2023. There seems to be room for an inefficiency debate.

Still, no matter how optimal the information disclosures or how accurate the data might become, it is not clear whether an individual would search for the report, read it thoroughly, understand the firm's climate impact, and be able to compare firms producing the same good in terms of their environmental impact. As indicated by Grether et al., 1986, individuals are aware that the costs of making these interfirm comparisons are too high compared to the gains (p.277) which may lead them to ignore the information. At the end individuals might decide to satisfice, not optimize, because of the costs of *acquiring information* and *processing information* (Grether et al., 1986, p.287). After all, "*how is a consumer to interpret Patagonia's statement that making one of its fleece jackets generates 20 pounds of CO2, or Levi's disclosure that production and laundering of a pair of 501 jeans will add 48.9 grams of phosphorous to freshwater or marine environments? Unlike with temperature or calories, consumers have no intuitive reference point that helps them*

understand many measures of environmental impact." (Pucker, 2021).

Undoubtedly reporting will be an important soft regulation going forward, potentially reduce the comparability problem [Cardoni et al., 2019, Amel-Zadeh and Serafeim, 2017], and will most likely become better after some experimentation. However, the argument in this paper is that the complexities in reporting and the costs attached to acquiring simple but accurate information calls for a different policy to reduce the information asymmetry.

1.3 Policy Proposal: The Climate Performance Score Scheme

Climate Performance Score (CPS), a measure that is thought to be on a scale between A and F, is a yet hypothetical but perfect indicator, showing how much adverse environmental impact a firm has. In an ideal world, it would be based on a thorough analysis of the subject firm's processes, energy usage, type of energy utilized, greenhouse gas emissions, what kind of logistics they use, the type and amount of waste they cause; simply anything that is known to have an impact on the environment and climate change. In addition, the measure factors the impact of a firm's intermediaries within their supply chain, as well as the externalities caused by consumption into the subject firm's score, so that everything is accounted for. Therefore, different from many labels, CPS is not a product specific label. Instead, it is an all-encompassing score given to the firm. Naturally, companies have to invest in climate abatement technologies, change their processes, energy resources, their logistics system or even their suppliers, if they would like to get a higher score.

Correspondingly, a CPS policy would be deemed as an information regulation that aims for eliminating the information asymmetry between the firms and the individuals. Hence, it has the advantage of being less interventionist (especially compared to the policies commonly used) where the cost-benefit analysis is left to the individual, may enhance consumer choice, and still effectively solve the externality issue (Ogus, 2004, pp.121-134) However, for CPS to be successful, there shouldn't be any political economy problems. In the empirical design, therefore, the participants were told that, being an objective and perfect indicator in terms of a company's climate impact, CPS is rendered by an international scientific group without any government, institution, or politician's

involvement.

One might argue that CPS is similar to eco-labels. Albeit some of the research having mixed results (Wang et al., 2021; Behavioural Insights Team and Department for Energy Security and Net Zero, 2023; Waechter et al., 2015), empirical evidence showing that eco-labels and green products are affecting purchasing behaviour (Tully and Winer, 2014; Sammer and Wüstenhagen, 2006) certainly motivates this study further. However, these two schemes would be completely different because eco-labels imply the energy efficiency of that particular good, whereas CPS is an all-encompassing firm score. As an analogy, producing electric cars does not necessarily make a firm environmentally friendly, without accounting for car manufacturing, battery production, and their whole supply chain. Furthermore, eco-labels have a direct financial benefit for individuals in the form of lowered bills, while knowing the environmental impact of a firm through CPS and making the purchasing decision based on that has no returns, if anything, would most likely have a negative financial return. Nevertheless, this piece of information might give an extra utility benefit, as explained in the next chapter.

2 THEORETICAL FRAMEWORK

The first step for developing the models is to determine the maximization problems of the individuals and the firms. In this regard, the models assume that individuals have a preference in terms of how much climate impact the firms have; they may be indifferent about this impact but there must be a preference, for that variable to be included in the indirect utility function. Accordingly, if the individual knows that the climate impact of the firm is small, a higher extra utility is extracted from consuming that product, instead of other products produced by firms with more adverse climate impact. That said, getting this information is costly and thus the cost of acquiring climate impact information must also be incorporated into the utility function.

Consequently, the indirect utility function v for individual i can be presented as the following:

$$\nu_i[\alpha(q), P, \beta(EI), \delta(EI)] = \alpha(q_k^x) - q_k^x \cdot P_k^x - \beta(EI_k) + \delta(EI_k) \tag{1}$$

where α represents a concave utility function on quantities consumed from product x produced by firm k, q the individual's consumption amount and P the unit price of x charged by k, β the information cost function for learning the environmental impact of firm k, and δ a decreasing utility function, where individuals get less utility by consuming firm k's product, as the environmental impact of k increases.

On the other hand, the profit function requires the addition of climate abatement investment costs as well as the costs attached to conveying environmental impact information. However, it is not assumed that firms have a preference in terms of being environmentally friendly or not; they are just trying to maximize their profits as microeconomic theory proposes. Correspondingly, the profit function for producing a single unit of x can be derived as follows:

$$\pi_k^x[P, C(x), \gamma(EI), \theta(EI)] = P^x - C(x) - \frac{\gamma(EI)}{Q^\forall} - \frac{\theta(EI)}{Q^\forall} \tag{2}$$

where P represents the price, C the cost function for producing x, γ the environmental impact abatement investment costs, and θ the costs attached to conveying/signalling environmental impact information. The last two cost terms are adjusted by all the goods produced by firm k as these investments and signalling are made for all goods produced by the firm (if the firm produces only x, $\forall = x$).

From the individual's perspective, if they are indifferent in terms of a company investing in climate abatement or not, the conventional indirect utility function applies and the last two terms in Equation 1 are disregarded. However, other individuals might be willing to pay more for the same good, if the change in the extra utility gain from knowing the environmental impact of the firm and consuming their product weakly offsets both the price difference and the cost of acquiring this information. In other words, holding income and quantity (and therefore the base utility) fixed, a higher price might be welcomed by the individual if the following condition holds:

$$\delta(EI)^1 - P^1 - \beta(EI)^1 \geq \delta(EI)^0 - P^0 - \beta(EI)^0 \tag{3}$$

$$= \delta(EI)^1 - \delta(EI)^0 \geq P^1 - P^0 + \beta(EI)^1 - \beta(EI)^0$$

In return, firms would have an incentive to invest in climate abatement technologies or to change their processes if they are compensated by not losing profit from their product, after incurring the investment costs but charging a higher price. Simply put, the total change in the revenue must be weakly higher than the new cost configuration, which can be demonstrated as follows:

$$\Delta Q^{x'} \cdot P^{x'} \geq \Delta C(x') + \Delta \frac{\gamma(EI)}{Q^{\forall'}} + \Delta \frac{\theta(EI)}{Q^{\forall'}} \tag{4}$$

In this framework, allocative efficiency can be ensured only if the individuals can be informed about a firm's environmental impact in a credible and minimal costly way. The next two sections will analyse whether CSR or CPS could achieve that.

2.1 The World with Corporate Sustainability Reporting

Although conveying credible climate impact information for firms is challenging (Hou et al., 2023, p.2) and Section 1.2 outlined the reasons to be sceptical about CSR's potential to reduce both the information asymmetry between the parties, and the negative externalities imposed by firms to the environment, the assumption employed here is that the report provides a somewhat credible signal than without mandatory reporting, which leads to a reduced signalling cost. In return, the acquisition and processing of this information is also becoming less costly for the individuals. Consequently, the information acquisition cost for individuals becomes $\beta(EI) - \varepsilon_1$ and the total signalling cost for firms $\theta(EI) - \varepsilon_2$, where $\varepsilon_z > 0$, $z \in \{1, 2\}$.

The information problem and whether firms have an incentive to invest in climate abatement technologies in this setting can be analysed with a game theoretic signalling model where the informed party (i.e. the firm) makes the first move with its investment decision. In this context, it will be assumed that nature exogenously and randomly

assigns individuals as climate caring or uncaring with a probability distribution where the respective probabilities are (λ) and ($1-\lambda$). On the other hand, individuals have a prior belief that firms choose to be climate friendly or unfriendly with another probability distribution with respective probabilities (μ) and ($1-\mu$). One omitted factor in this setting is the market structure and competition type, to keep the framework simple.

It is important to note that the probability distributions are unknown by the parties, which leads to an infinite belief space (Maschler et al., 2020, pp.397-398), and that neither individuals nor firms can tell the different types apart. The decision of the firm is to whether or not invest in climate abatement considering the signalling power of the report, while maximizing profits; whereas the individual assesses the signal and decides to buy the good or not while maximizing utility. The strategy nodes, assignments, and outcomes of this sequential game are depicted in Figure 1. Following Harsanyi, 1995, it will be assumed that both players know their own payoff functions, but they do not know each other's payoff functions (p.293), leading to a game with incomplete information. Also, as individuals cannot necessarily tell from the reports whether a firm chooses to be climate friendly (and thereby invest in climate abatement) or not with certainty, its a game with imperfect information (pp.296-297).

Using the notation in Equation (1), the base utility of consuming the product without factoring any other variable can be simplified as $\alpha - P = \sigma =:$ *base utility*. The climate uncaring individuals always get σ because $\sigma \geq 0$ and they don't benefit from the extra utility factor θ, thus don't incur the information acquisition cost. For the climate caring individual to buy the good, however, their expected utility from consumption has to be higher than their outside option of 0.

Coherent with the notations in Equation (1), individuals get one of the following payoffs:

$$\{0\}, \quad \{\sigma\}, \quad \{\sigma + \delta - (\beta - \varepsilon_1)\}, \quad \{\sigma + \delta - \big((\beta - \varepsilon_1) \cdot \omega\big)\} \text{ where } \omega > 1.$$

The reason behind adding ω is the assumption that when firms do not invest in climate abatement, their signal becomes even noisier, which increases the cost of acquiring this

information for climate caring individuals.

On the other hand, using the notation in Equation (2), the base profit for the firm for one unit of good can be simplified to:

$$P^x - C(x) = \Phi = \text{base profit.}$$

If the good is not sold and the firm does not make any climate abatement investments, the firm only incurs the cost $C(x)$ which is represented by:

$$C(x) = \mathbb{C} = \text{base loss.}$$

The firm always prefers the consumer to buy, as $\Phi > 0 > \mathbb{C}$.

Using Equation (4), the base profit is represented with Φ' when the firm makes the investment as they would update the price now. Moreover, the per product cost of this investment as well as the cost of signalling has to be incorporated. Therefore, the two outcomes if the firm invests and indeed sells the product are

$$\left\{\Phi' - \frac{\gamma}{Q^\forall} - \frac{\theta - \varepsilon_2}{Q^\forall}\right\}$$

and

$$\left\{\Phi' - \frac{(\gamma \cdot \zeta)}{Q^\forall} - \frac{\theta - \varepsilon_2}{Q^\forall}\right\}$$

(where $\zeta > 1$), respectively for environmentally friendly and unfriendly firms. The logic behind adding ζ is similar to ω because it is assumed that the cost of investing in climate abatement is higher for environmentally unfriendly firms than environmentally friendly firms.

In the bad scenario, if the firm invests but cannot sell their good, the base loss is exacerbated with the investment and signalling costs:

$$\mathbb{C} - \frac{\gamma}{Q^\forall} - \frac{\theta - \varepsilon_2}{Q^\forall} = \mathbb{C}'$$

for environmentally friendly firms and

$$\mathbb{C} - \frac{(\gamma \cdot \zeta)}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}} = \mathbb{C}''$$

for environmentally unfriendly firms. Because of $\zeta > 1$, $\mathbb{C}'' < \mathbb{C}' < \mathbb{C} < 0$ and the environmentally unfriendly firms make a bigger loss if they invest and cannot sell their product.

Lastly, the firm incorporates the probability λ of facing a climate caring individual in the market, and likewise individuals employ their prior belief μ about the distribution of environmentally friendly and unfriendly firms, which is why μ is also factored into their decision. Given the information and the assumptions, the purchasing and investment decision can be calculated based on the first-order expectations of the players Harsanyi, 1995.

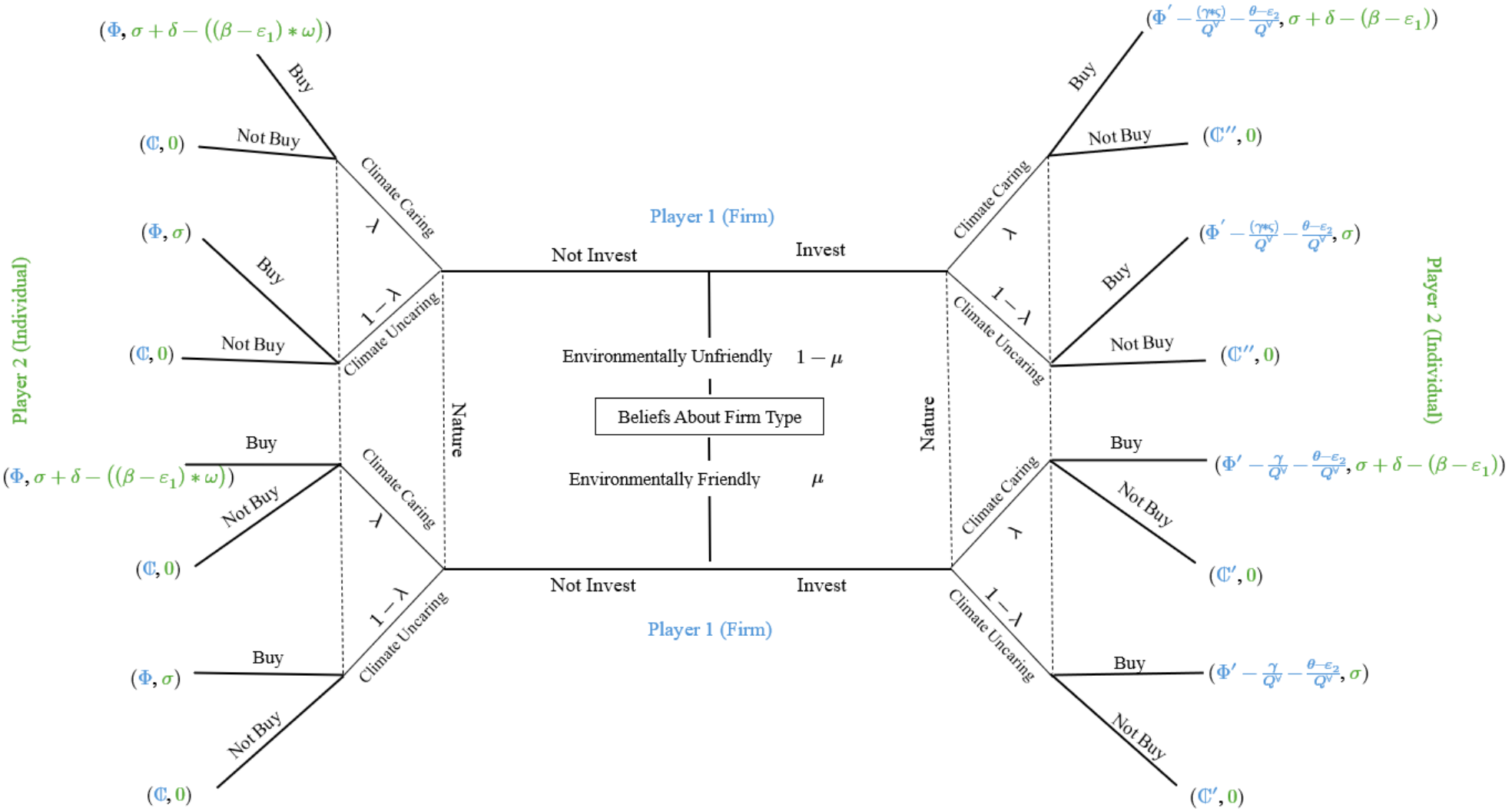

Figure 1: Signalling Game with Mandatory Reporting

As mentioned, the climate uncaring individual always participates in the market due to the assumption of $\sigma \geq 0$, but the participation constraints for the climate caring individual are given below: Equation (5) when the firms invest in climate abatement and Equation (6) when they do not. As Equation (6) is a subset of Equation (5), due to $\omega > 1$, only Equation (5) will be used moving forward.

$$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu \geq 0 \tag{5}$$

$$(\sigma + \delta - ((\beta - \varepsilon_1) \cdot \omega)) \cdot \mu \geq 0 \tag{6}$$

Correspondingly, (i) the higher the base utility from consuming the good, (ii) the higher the additional utility generated from knowing the environmental impact of the firm based on the inference from the noisy signal, (iii) the higher the prior belief on the proportion of environmentally friendly firms (μ), (iv) the lower the information acquisition cost, and relatedly (v) the higher the cost reduction (ε_1) provided by mandatory reporting, the higher the probability that the climate caring individual participates in the market.

On the other hand, in terms of making the climate abatement investment, the firm must consider the unknown parameter λ (the proportion of climate caring individuals) and is incentivized for the investment only if the following equations in Table 1 hold, for which the formal proofs are provided in Appendix 7.1.

Although seemingly tedious, the solutions to these equations can be found with elementary calculus when the figures are known, while the intuitions are easy to follow as well. According to Equation (7) and Equation (8), the solution is relatively simple when the climate caring individuals have a higher utility than their outside option and participate in the market, if the noisy signal justifies it. In that case both environmentally friendly and unfriendly firms invest in climate abatement if the increase in their profit margin per unit is higher than the investment and signalling costs per unit. This is of course more difficult for environmentally unfriendly firms as their investment cost is exacerbated by $\zeta > 1$, so that the right-hand side of the equation gets bigger and therefore

Table 1: The Climate Abatement Investment Decision for the Firm (with Signalling)

Panel A.	**For Environmentally Friendly Firms**
Climate caring individuals participate:	$\Phi' - \Phi \geq \frac{\gamma}{Q^{\forall}} + \frac{\theta - \varepsilon_2}{Q^{\forall}}$ ..... (Equation 7)
where participation requires	$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu \geq 0$
Climate caring individuals don't participate:	$(1 - \lambda)\left(\Phi' - \Phi - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}\right) \geq \lambda(\mathbb{C} - \mathbb{C}')$
when	$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu < 0$
Panel B.	**For Environmentally Unfriendly Firms**
Climate caring individuals participate:	$\Phi' - \Phi \geq \frac{\gamma \cdot \zeta}{Q^{\forall}} + \frac{\theta - \varepsilon_2}{Q^{\forall}}$ ..... (Equation 8)
where participation requires	$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu \geq 0$
Climate caring individuals don't participate:	$(1 - \lambda)\left(\Phi' - \Phi - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}\right) \geq \lambda(\mathbb{C} - \mathbb{C}'')$
when	$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu < 0$

the profit margin for them has to increase more than for environmentally friendly firms.

If, on the other hand, climate caring individuals do not participate, the investment incentive gets very unlikely. Because for the investment to happen the profit made from climate uncaring individuals (who buy the good in any case) must be bigger than the lost incurred from not being able to sell the good to the climate caring individuals. As $\mathbb{C}'' < \mathbb{C}' < \mathbb{C} < 0$, the right-hand side of the equations are relatively high positive numbers and it is questionable whether the new profit margin minus all the costs can dominate this figure, while this is also contingent to the proportion of climate caring and uncaring individuals.

All these contingencies open room for the equilibrium to be a separating one where different type of firms opt for different strategies, a pooling equilibrium where different types select the same strategy, or a semi-separating/pooling equilibrium where at least one type of firm employs a mixed strategy (Osborne and Rubinstein, 1994, p.238).

As demonstrated above, the participation constraint

$$(\sigma + \delta - (\beta - \varepsilon_1)) \cdot \mu \geq 0$$

is crucial for the investment incentive to emerge. Consequently, the signalling power of the sustainability reports immensely affects the outcome not only because they could reduce the information acquisition cost $(\beta - \varepsilon_1)$, but also because they reduce the signalling cost $\left(\frac{\theta - \varepsilon_2}{Q^{\forall}}\right)$ for the firm and thereby increase the probability of the new profit margin to be at least weakly higher than the investment and signalling costs.

Admittedly, the rest of the analysis is subjective but one of the theoretical hypotheses of this paper is that CSR can only lead to a *pooling equilibrium* in which no firm invests in climate abatement and "pool" together on the "not invest" side of the tree. That must be so because the current mandatory reporting schemes has almost no signalling power not only because of all the caveats explained under Section 1.2, but also because it is difficult to imagine that an individual would look at these 70-80 pages long[1] reports from each company for every purchasing decision, find the relevant information, construct a benchmark, analyse all the variables, and come to conclusion without incurring at least a substantial opportunity cost. Furthermore, as outlined in Section 1.2, if reports issued by different agencies do not even show a correlation and if the disagreement increases as the disclosure increases, it is difficult to think that the reporting scheme could reduce the signalling cost for the firms either. Nevertheless, empirical evidence is needed.

Under these circumstances the information asymmetry problem is not resolved, all firms have pooled for not investing in climate abatement, and no firm has an incentive to deviate from this equilibrium. Consequently, both market failures persist and even if the result is a Perfect Bayesian Equilibrium (Maschler et al., 2020, p.418), it cannot be welfare maximizing since all potential trades would most likely not happen (Belleflamme and Peitz, 2015, pp.297-299).

[1] Carter et al., 2022 analysed 200 sustainability reports from S&P 500 companies published in 2022 and found an average length of 77 pages.

2.2 The World with Climate Performance Score

As explained under Section 1.3, CPS is a perfect indicator showing a firm's environmental impact, which completely eliminates the information asymmetry between the two parties, as individuals now see how a firm performs in terms of climate friendliness in an easy, intuitive, and comparable way. In terms of Equation (1) and Equation (2), the biggest contribution of CPS is taking the information acquisition and processing cost $\beta(EI)$ (which was the main hindrance in the previous section for climate caring individuals to participate and as a consequence, for the firms to invest in climate abatement) out from the equation. In addition, firms cannot/don't have to send signals about their climate impact, which takes out $\frac{\theta(EI)}{Q^{\forall}}$ from Equation (2). Correspondingly, the maximization problems simplify as follows:

$$\nu_i[\alpha(q), P, \delta(CPS)] = \alpha(q_k^x) - q_k^x \cdot P_k^x + \delta(CPS_k) \tag{9}$$

$$\pi_k^x[P, C(x), \gamma(CPS)] = P^x - C(x) - \frac{\gamma(CPS)}{Q^{\forall}} \tag{10}$$

where the additional utility function δ and the γ investment cost function now take CPS as their variable. The higher the CPS of firm k, the more utility is extracted by the individual; and the higher the desired CPS gets, the higher the investment cost for the firm.

Holding income and quantity fixed, the individual welcomes a higher price for the same product if the utility derived from knowing that the firm has a higher CPS can offset the price difference as shown in Equation (11), while the firm will compare the change in profit after having a higher CPS and the costs attached to achieve that [Equation (12)].

$$\delta(CPS)^1 - P^1 \geq \delta(CPS)^0 - P^0 \tag{11}$$

$$= \delta(CPS)^1 - \delta(CPS)^0 \geq P^1 - P^0$$

$$\Delta Q^{x'} \cdot P^{x'} \geq \Delta C(x') + \Delta \frac{\gamma(CPS')}{Q^{\forall'}} \tag{12}$$

With the information related costs out of consideration, the game theoretic model also substantially simplifies. In this setting, the individuals don't have to form any subjective prior beliefs about the distribution of firms because CPS provides enough information. However, the firm still doesn't know the distribution of individuals in terms of climate caring levels and again nature assigns individuals as climate caring or uncaring with the same probability distribution as before. Similar to the previous settings, neither of the players know one another's payoff functions, leading to a game with incomplete information. However, thanks to CPS, now individuals can observe the previous moves of the firms (whether they have invested or not, given their CPS), and therefore players enjoy perfect information (Harsanyi, 1995, pp.296-297).

In a sequential game context where obtaining CPS is mandatory for all firms, the first action taker is again the firm which must decide on whether to invest in climate abatement and thereby get a higher CPS, while maximizing profit. The individual with precise information, on the other hand, decides to buy the good or not while maximizing utility. Following the notation used in Figure 1, the strategy nodes, assignments, and outcomes of this sequential game are depicted in Figure 2:

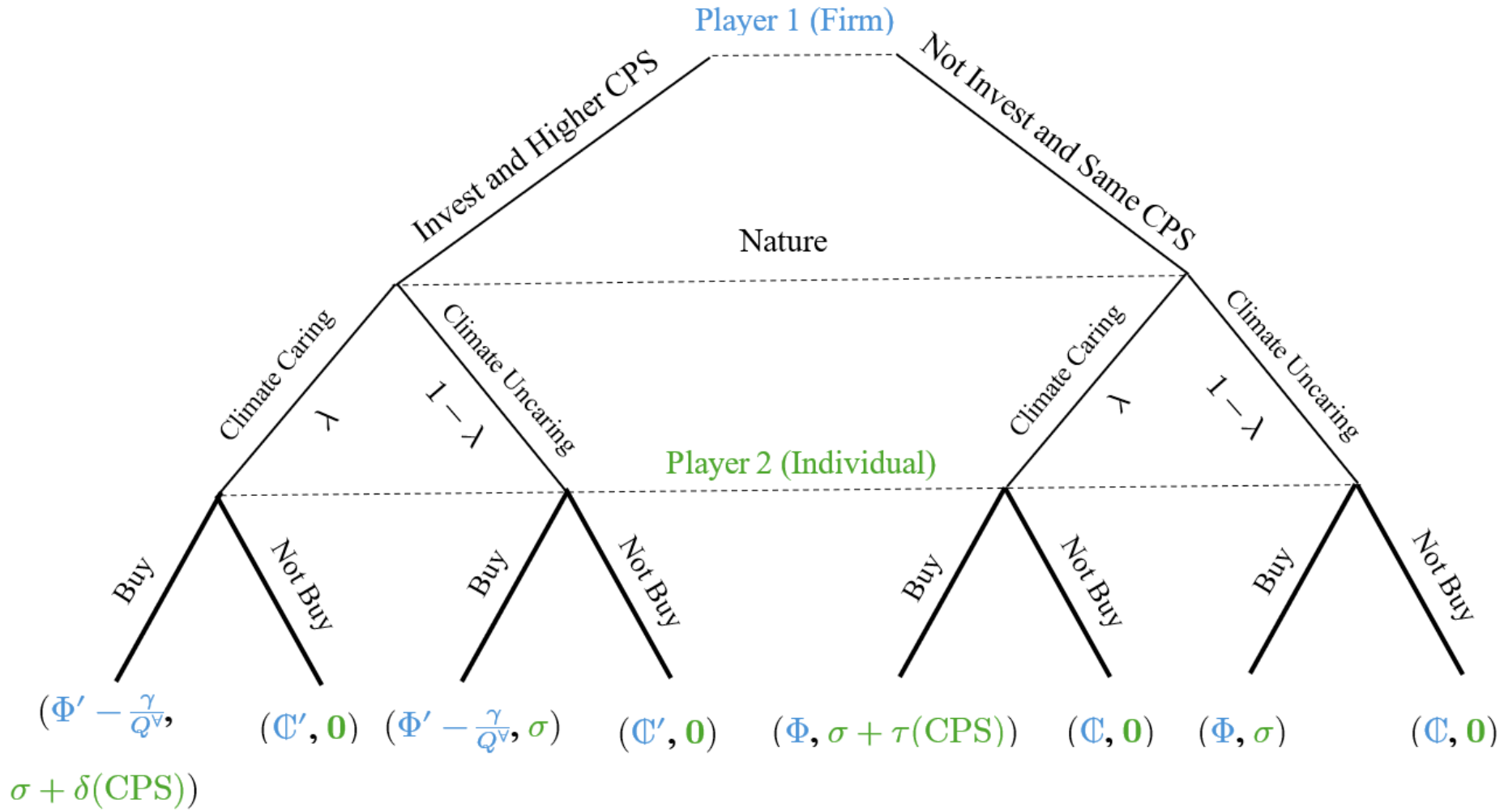

Figure 2: Sequential Game with Climate Performance Score

Now that the climate caring individual has a clear method for comparing, the only addition to the notation, for simplification, is $\tau(CPS)$ which occurs in the form of utility or disutility when the firm has a lower CPS than other firms. In other words,

$$\delta(CPS) > \tau(CPS)$$

is certainly true and $\tau(CPS) \geq 0$ or $\tau(CPS) < 0$ depending on how low the score is. It could be even that

$$\sigma + \tau(CPS) < 0$$

(knowing that the firm has a score of, say, E or F might create more disutility than the utility obtained from consuming the good), which is the new participation constraint for the climate caring individual. Based on this information and using sequential rationality (Osborne and Rubinstein, 1994, p.221; Maschler et al., 2020, p.285), backward induction provides the best strategies for the players.

Holding quantity and thus the base utility fixed, climate caring individuals will buy from the firm with higher CPS if the utility gain exceeds the price difference between the two goods. Otherwise, they will either opt for the lower CPS firm, or will not participate in the transaction, if that brings disutility due to $\sigma + \tau(CPS) < 0$. Yet, climate uncaring individuals will always select the good product by the lower CPS firm as nothing justifies the price increase for them. Table 2 formularises the buying decision for individuals:

Following the same notation as before, Table 3 summarizes the incentive to invest scenarios for the firm:

Table 2: Buying Decision for Individuals (the CPS Setting)

Panel A. Climate Caring Individuals		
Decision	**Condition**	
Buy from firm with higher CPS, if:	$\delta(CPS) - P^1 \geq \tau(CPS) - P^0$ $= \delta(CPS) - \tau(CPS) \geq P^1 - P^0$	(13)
Buy from firm with lower CPS, if: if still $\sigma + \tau(CPS) \geq 0$ and	$\delta(CPS) - P^1 < \tau(CPS) - P^0$ $= \delta(CPS) - \tau(CPS) < P^1 - P^0$	(14)
Do not participate in the transaction, if:	$\sigma + \tau(CPS) < 0$	(15)
Panel B. Climate Uncaring Individuals		
Always buy from firm with lower CPS, as:	$P^1 > P^0$	(16)

Table 3: The Climate Abatement Investment Decision for the Firm (with CPS)

Scenario	**Investment Condition**
Given that climate caring individuals buy from the higher CPS firm; invest in	$\lambda\left(\Phi' - \frac{\gamma(CPS')}{Q^{\forall}}\right) + (1-\lambda)\mathbb{C}' \geq \lambda\cdot\mathbb{C} + (1-\lambda)\Phi$ (17)
climate abatement and get a higher CPS, if	$\Leftrightarrow \lambda\left(\Phi' - \mathbb{C} - \frac{\gamma(CPS')}{Q^{\forall}}\right) \geq (1-\lambda)(\Phi - \mathbb{C}')$
Given that climate caring individuals buy from the lower CPS firm; invest in	$\lambda\cdot\mathbb{C}' + (1-\lambda)\cdot\mathbb{C}' \geq \lambda\cdot\Phi + (1-\lambda)\cdot\Phi$ (18)
climate abatement and get a higher CPS, if	$\Leftrightarrow \mathbb{C}' \geq \Phi$ (which is impossible)
Given that climate caring individuals don't participate;	$\lambda\cdot\mathbb{C}' + (1-\lambda)\cdot\mathbb{C}' \geq \lambda\cdot\mathbb{C} + (1-\lambda)\cdot\Phi$ (19)
invest in climate abatement and get a higher CPS, if	$\Leftrightarrow \mathbb{C}' - \Phi \geq \lambda\cdot(\mathbb{C} - \Phi)$ (which is impossible)

No firm with a profit maximization goal would make the investment in the last two scenarios, as they lead to a net loss. Correspondingly, the decision of the firm becomes a question of satisfying which population: Get a higher CPS and offer the good to climate caring individuals and lose the uncaring population, or only target the climate uncaring population while missing out the caring individuals.

All the results in Table 2 and the first result in Table 3 are Sub-Game Perfect Nash Equilibrium as the players cannot deviate from their strategies in that particular sub-game because their expected pay-off would be smaller (Maschler et al., 2020, p.282).

Apart from the implications of CPS on investors' decision making (and therefore more contingencies which are out of the scope of this paper), Equation (17) demonstrates that ultimately the climate abatement investment incentive comes down to (i) the new profit by targeting climate caring individuals due to their adjusted willingness to pay for a product produced by a firm with higher CPS, (ii) the investment costs per product, and (iii) the proportion of climate caring individuals in a given market. Only empirical investigations can provide some evidence on these variables. The rest of the paper, therefore, analysis the first question and tries to answer whether and in which conditions individuals exert a higher willingness to pay, given a CPS difference between firms.

3 RESEARCH DESIGN AND DATA

A central problem of whether a policy design like CPS would incentivize firms to invest in climate abatement is to find out if individuals exert a higher willingness to pay (WTP) when they are faced with the buying decision of two identical products produced by two different firms having different CPS. As a randomized controlled trial was not possible, the second-best options in the form of contingent valuation and revealed preference studies were employed, which are widely used in many areas including the valuation of public goods as well as consumer products (Vossler et al., 2012, p.145). Further details about the studies and their respective caveats will be explained in their sub-sections below.

3.1 Contingent Valuation Studies

3.1.1 Methodology

For this survey-based method, participants from the United States, South Africa, and India were recruited, with the idea that fighting against climate change requires global policies implementable in different settings (Barrett, 2021, p.1) and to see whether individuals living in countries with different development levels show similar responses to the treatment. In terms of the fundamental information provided to the treatment group participants, they were given two sets of information first of which was about CPS (a refined version of Section 1.3), while the other one provided the hypothetical scenario that there are only two firms producing the good, that one of the firms charges the average market price (detailed below), that the products are identical in every aspect, and that they should think of themselves as repeat buyers regularly making this purchasing decision (the nature of the products are compatible with that, as explained below). The control groups had only the second set of information, without any referrals to CPS.

One last information provided to the treatment group participants was the federal/country level regulation outlining CPS and giving discretion to states/provinces on whether to mandate CPS or keep it as voluntary. As argued in the law and economics literature, the best policy intervention is "the one that least interferes the market but that is still effective in solving the prevailing market failure" (Philipsen, 2018, pp.542-543). Although the fully mandatory version of a CPS regulation would intervene the market much less than the other policies, perhaps leaving it voluntary would distort the market to a lesser degree and still could lead to similar behavioural responses (if any). Accordingly, the study had two main treatment arms, the mandatory setting and the voluntary setting, as explained more in detail below.

Undoubtedly, a crucial aspect of these studies is to select a good for which the participants will make a hypothetical purchasing decision. Inspired from R. T. Jensen and Miller, 2008, I used one staple food for each country with the reasoning that the cross-price elasticity of demand for the same staple food produced by two different firms would be very

high, if not infinite, without brand or reputation having any effects. Therefore, if indeed the treatment were to be effective in this context, it would be reasonable to expect it to be effective for all sorts of goods. Correspondingly, based on evidence, potatoes for the U.S. study (U.S. Department of Agriculture, 2019), maize meal for the South Africa study (South Africa Department of Agriculture, Land Reform and Rural Development, 2022) and masoor dal (red lentils) for the India study (Singh and Singh, 2014) were selected as the staple foods. All groups were given the information that the products are absolutely identical in every aspect including their taste, usage, calories, bio-scores, nutritional scores, and how they were planted and raised (or processed). Different representations of the goods were presented to make the interpretation that there are two different firms in question easier. This situation necessitated the establishment of a control group to, on average, eliminate the effects of the different representations.

Using staple foods has the advantage of setting the null hypothesis at zero more confidently as we would not expect a higher WTP for a staple while there is another identical option, only produced by a different firm. By the same token, using staple foods probably yields conservative estimates relative to the effects of the treatment on other goods.

Hence, participants in the treatment group compared two identical products with slightly different pictures and exerted their willingness to pay for one of them (for the product on the left) given the CPS difference between the firms and given the fact that the other firm (on the right) was charging the country-wide average market price. The May 2024 all-U.S. average market price data for potatoes was obtained from the Consumer Price Index Average Price Data (U.S. Bureau of Labor Statistics, 2024), the South Africa average market price for maize meal from the March 2024 Essential Food Price Monitoring Report (Competition Commission South Africa, 2024), and the June 2024 all-India average market price data for red lentils from the Daily Retail and Wholesale Prices (India Department of Consumer Affairs, 2024). While the other goods had precise price data, the maize meal average price in February 2024 was indicated to be between 30-35 South African Rand (closer to 35) for 2.5 kilograms of maize meal. After doing a market research including 66

products, the average price was set at 34.58 Rand considering also that yearly inflation of 5.1% in South Africa (Statistics South Africa, 2024).

One issue with using a country-wide average price in the U.S. and India studies was the fear that participants who reside in states where the market price for that commodity is substantially higher or lower than the average might exert a WTP higher or lower independent from the treatment. I deal with this by controlling for the residency state of the participant whenever the model predicts a significant effect of this variable, as explained in the Estimation Strategy section of this paper. Another task was to determine the units of product the participants would be most familiar to buy and therefore provide more reliable answers. Based on market reports and market research, 3 pounds of potatoes, 2.5 kilograms of maize meal, and 1 kilogram of red lentils were determined as the units. Example representations provided in Appendix 7.2 give a clearer picture on what participants experienced.

In terms of the treatment arms, as CPS is thought to be a measure between A and F, there are 30 combinations 15 of which assigns a higher score for the firm on the left in our hypothetical scenario. As depicted in Figure 3, 9 of these combinations were selected in a way that would allow to detect trends in exerted WTP. 2 of the treatment arms were mirrored (orange boxes) so that the firm on the left had a lower score. The reason for this twist was first to see whether participants were answering randomly and secondly to understand to what degree they would "punish" a firm if the competitor has already a higher score and charges the average market price. As we go vertically between treatment arms in Figure 3, we would not expect to see a difference in mean WTP as we are moving within the same level, whereas the expectation for moving horizontally eastward and diagonally northern eastward is an increase in mean WTP.

Moreover, the voluntary disclosure setting offered 12 combinations. 3 out of 6 where the firm on the left was voluntary disclosing their score were selected. Although it is difficult to guess how this uncertainty of one firm not having a score would be evaluated by individuals, the expectation is to find a lower mean WTP as we move downwards in Figure 4.

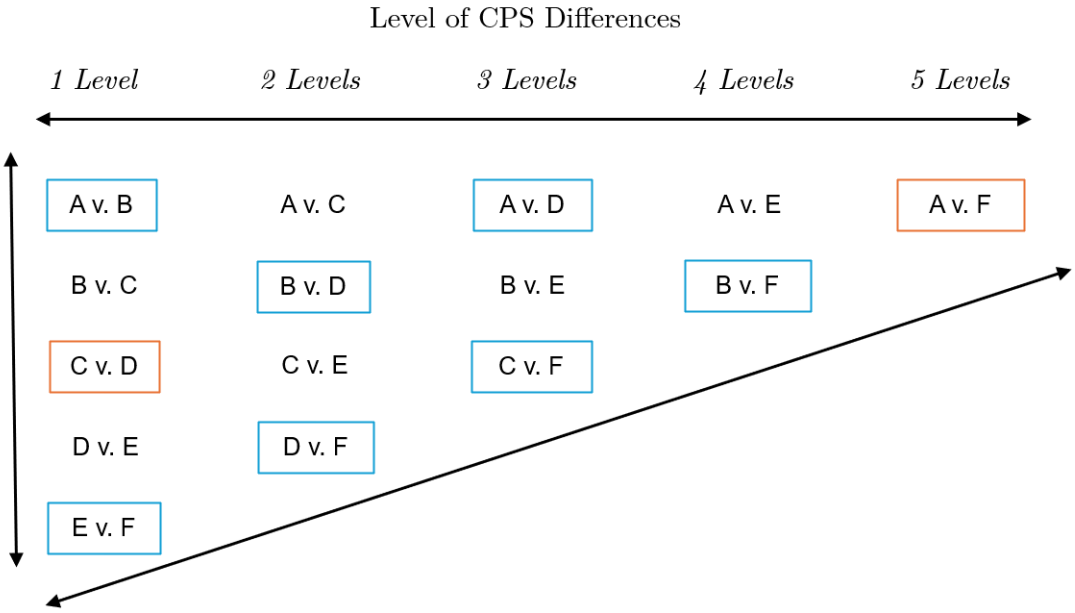

Figure 3: Treatment Arms for Mandatory Disclosure

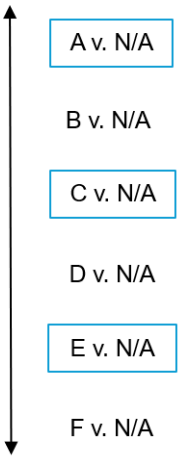

Figure 4: Treatment Arms for Mandatory Disclosure

In terms of the flow and some technical aspects of the survey, the participants first completed the informed consent part and then were asked a binary question about whether they are committed to providing reliable data for the study, as one research showed that a commitment question increases data quality (Geisen, 2022). The two (one, if assigned to control) information sets followed suit and they were accompanied by 6 (3, if control) True/False questions to ensure that the information was read and digested. Then the randomizer feature of Qualtrics stepped in, randomizing participants to one of the 12 treatment arms.

The first question answered by the participants was what their WTP preference would be after reminding them that the products are identical in any characteristic imaginable and that the only difference is in between the companies' CPS. This question had three options: shortly, (i) same amount, (ii) slightly more / more, and (iii) slightly less / less. Although some research indicate that they may lead respondents to avoid exerting the necessary cognitive effort to make a choice (Krosnick et al., 2002, p.376), the option "same amount" [substituting the "no answer" option recommended by the NOAA Panel (Arrow et al., 1993)], allowed respondents to show that they value the CPS difference with a true zero (Johnston et al., 2017, p.350; Boyle, 2017, p.108).

A very important decision at this point was to decide on which WTP elicitation method to use, among the methods widely used in contingent valuation studies. An extensive literature review showed that there is consensus on only one aspect in this research field: none of the formats is unequivocally better than others (Bateman et al., 2002; Voltaire et al., 2019, p.3770). However, given the empirical research indicating that;

- even though the dichotomous choice method ("DC") might be the most incentive compatible format and yield the most reliable demand revelation (Vossler and McKee, 2006, p.137),
- DC renders substantially larger estimates than the payment card method ("PC") (Champ and Bishop, 2006, pp.169-172; Ready et al., 2001),
- DC suffers from the starting point bias (Boyle et al., 1985, p.192),

- DC suffers from yea-saying (Babcock et al., 2020, p.2077),
- DC is susceptible to anchoring effects while PC is not (Frör, 2008, p.579),
- open-ended questions ("OE") usually provide either unrealistically high or zero WTP responses (Johnston et al., 2017, p.346),
- and at the end all formats can elicit statistically indistinguishable WTP distributions (Vossler and Zawojska, 2020) when done correctly,

I used a modified version of the PC method where participants were able to select their WTP in a continuous spectrum, rather than selecting an option among pre-determined amounts, thanks to the slider option in Qualtrics. It should also be noted that this literature has developed around environmental economics and the valuation of non-market public goods, whereas this study is about market goods, which is why PC was most likely the most compatible method in any case. Apart from this, the main reason for selecting PC was to get as conservative numbers as possible, given the limitations of the study outlined below and given that the other methods lead to much larger estimates. Moreover, another modification was to use the two-way payment ladder approach where the participants are selecting the first amount that they would definitely pay and the first amount that they would definitely not pay. In addition to several advantages, this approach has been shown to have more convergent validity than the classical formats (Voltaire et al., 2019, pp.3782-3783). However, similar to the classical formats (Boyle, 2017, p.106), this approach also assumes that the true WTP is at the middle between the two chosen amounts. However, I did not use this assumption to avoid overestimating the results. Instead, I followed the design used by Soeteman et al., 2017, where participants were given an open-ended follow-up question to insert their maximum WTP given the range they have chosen. This two-step PC-OE approach has the potential advantage of leading to more thought-through answers with a richer data set (Soeteman et al., 2017, p.744).

All said, PC suffers from range bias which simply means that the higher the payment card range, the higher the mean estimations become, which was evidenced by Whynes

et al., 2004, Dubourg et al., 1997, Soeteman et al., 2017, Shackley and Dixon, 2014, and Covey et al., 2007. Nonetheless, to limit the range bias as much as possible and obtain conservative estimates, the WTP ranges were limited to plus minus one third of the average market price. So, the ranges were USD[1.93, (2.89), 3.85], SAR[23.05, (34.58), 46.11], and IR[62.77, (94.16), 125.55] for the U.S., South Africa, and India studies respectively.

One shortcoming of contingent valuation that can never be overcome is the hypothetical bias, stemming from participants not really spending the amount they exert as their WTP but simply answering a hypothetical scenario. A recent meta study by Foster and Burrows, 2017 analysing 77 papers finds the median bias ratio (the ratio of the mean WTP drawn from the hypothetical treatment to the mean WTP drawn from the real treatment) as 1.39, and the mean bias ratio as 2.33 (p.271). However, controlling for other factors, their regression results find no statistically significant bias when the subject good is a familiar good for the participants (pp.280-285). This finding gives more hope for the results of this study, as staple foods are arguably the most familiar good that participants can evaluate.

Coming back to the survey flow, after the WTP part participants answered 4 questions about their climate perception on a scale of 0-10: (i) their climate change awareness, (ii) whether climate change has any physical, mental, financial (or alike) effects on their life, (iii) their perception on how companies (not only in their country but globally) do in fighting climate change, and (iv) their general perception on how countries (not only their country but globally) do in fighting climate change. The survey ended with demographics questions. Similar to the average price data, the demographics questions such as highest degree of education, household income, or household expenditure on groceries were adjusted for each country based on the local context and based on the data obtained from governmental authorities and research institutions. Lastly, in the U.S. and South Africa studies all participants were screened to be the primary grocery shopper of their household, based on the crowdsourcing platform's database. This pre-screener was added to increase the reliability of the data. Moreover, apart from residence, country of birth and nationality pre-screeners were also added to ensure that the staple food idea works as best as possible.

3.1.2 Data and Descriptive Statistics

Study participants were recruited mainly from Prolific, due to the high-quality data provided by the participants in this platform compared to other research platforms, as argued by Peer et al., 2022, and due to the ethical compensation scheme, which arguably incentivizes the participants more for data reliability. After dropping participants who had less than 66.6% correctness in the True/False questions, the total number of observations is 1,646: 637 coming from the U.S. study, 597 from the South Africa study, and 412 from the India study. 203 observations in the India study are obtained, however, from Amazon Mechanical Turk due to limited participation on Prolific.

A critical factor that has to be considered when there is more than one treatment arm is the allocation ratio between the treatment arms and the control group. Statistically, the highest power is achieved when treatment and control groups are equally allocated (Glennerster and Takavarasha, 2013, p.264). However, this might not be optimal in certain circumstances one of which is when the control group plays a particularly important role due to being used in all pairwise comparisons (Glennerster and Takavarasha, 2013, pp.280-281). Hence, following A. V. Banerjee, Duflo, et al., 2010, Buchmann et al., 2021, and A. V. Banerjee, Banerji, et al., 2010, a higher proportion was given to the control groups compared to each treatment arm. When deciding on the number of treatment arms, there was an inevitable trade-off between having more precise estimations and answering more interesting questions (Glennerster and Takavarasha, 2013, p.291), and I tried my best to balance this trade-off given the limited resources.

Table 4 provides the descriptive statistics for the studies. Using descriptive statistics is particularly important in this context not only because the samples might not be representative of the population but also because the randomization is quasi-randomization at best due to the nature of the recruitment. Following the best practices, all studies were divided to multiple phases, published at different days so as to increase the representativeness.

Panel A reports the results for the U.S. sample and participants are not statistically distinguishable in almost all characteristics, except that there are 13.6 percentage points more people in the control group from the white ethnicity. Participants in the control group

identify themselves as more aware about climate change (statistically significant difference at the 5% level) and think that companies and countries are doing worse in fighting climate change compared to the treatment group. These, however, are not expected to affect the results substantially, if anything, they may lead to underestimation because hypothetically we would expect a positive correlation between climate change awareness and WTP given the research design, but the treatment participants identify themselves as less climate change aware. Similarly, we would expect a negative correlation, if any, between perception about companies in terms of fighting climate change and WTP, but the treatment participants trust companies more.

Panel B reports the results for the South Africa sample. The groups are statistically identical in every characteristic, except that control group participants are 8 percentage points more likely to be unemployed, a difference significant only at 10% level.

Panel C reports the results for the India sample. Opposite to the previous two samples, the groups are statistically distinguishable in almost every characteristic. Undoubtedly, this raises scrutiny about the internal validity of this part of the study. The researcher hopes to partially mitigate this by controlling for the observable characteristics, but this situation will prevent some statistical assumptions [such as the expected value of the error term equalling to zero mean independently from other variables (Wooldridge, 2020, p.42)] to be employed due to the unmeasured and unknown factors affecting the error term (Naci, 2017, p.3).

3.2 The Revealed Preference Study

3.2.1 Methodology

Due to the limitations of contingent valuation, I conducted a revealed preference study using the Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964), where an individual states her bid for an item, followed by the generation of a random price. When the random price is greater than her bid, she cannot purchase the product, whereas if the random price is less than or equal to her bid, she purchases the product but pays the random price drawn rather than the stated bid. It has been argued that BDM is incentive

Table 4: Descriptive Statistics for the Contingent Valuation Studies

	Control	Any Treatment	Difference
	(1)	(2)	(3)
Panel A. United States Study			
Age	39.92	40.60	−0.68
	[12.84]	[12.57]	(1.49)
Gender	0.51	0.50	0.002
	[0.50]	[0.50]	(0.06)
Black Ethnicity	0.06	0.13	−0.07**
	[0.24]	[0.34]	(0.03)
White Ethnicity	0.84	0.70	0.14***
	[0.37]	[0.46]	(0.05)
Education	2.94	3.01	−0.07
	[1.72]	[1.72]	(0.20)
	(College Degree)	(College Degree)	
Full-Time Job	0.52	0.57	−0.05
	[0.50]	[0.50]	(0.06)
Part-Time Job	0.20	0.17	0.03
	[0.40]	[0.37]	(0.05)
Student	0.13	0.11	0.02
	[0.34]	[0.32]	(0.04)
Household Size	2.86	2.78	0.08
	[1.58]	[1.49]	(0.18)
Annual Household Income	6.01	6.44	−0.43
	[3.85]	[3.71]	(0.45)
	(≈$60,100)	(≈$64,400)	
Weekly Groceries	1.72	1.77	−0.05
	[1.05]	[1.06]	(0.12)
Weekly Groceries Expenditure	3.60	3.68	−0.08
	[2.13]	[2.28]	(0.25)
	(≈$115)	(≈$116)	
Living in Northeast	8.24%	15.11%	
Living in Midwest	20.00%	21.27%	
Living in South	49.41%	45.15%	
Living in West	22.35%	18.47%	
Climate Change Awareness	7.13	6.60	0.53**
	[2.26]	[2.36]	(0.27)
Affected by Climate Change	6.27	6.19	0.08
	[2.84]	[2.74]	(0.33)
Perception on Companies	3.17	3.67	−0.49**
	[2]	[2.14]	(0.24)
Perception on Countries	3.56	3.99	−0.43*
	[2.13]	[2.05]	(0.25)
Number of Observations	85	552	

Table 4 Continued

	Control	Any Treatment	Difference
	(1)	(2)	(3)
Panel B. South Africa Study			
Age	30.15	29.37	0.78
	[8.61]	[7.72]	(1.02)
Gender	0.55	0.54	0.01
	[0.50]	[0.50]	(0.06)
Black Ethnicity	0.84	0.80	0.03
	[0.37]	[0.40]	(0.05)
White Ethnicity	0.08	0.09	−0.01
	[0.27]	[0.28]	(0.03)
Mixed Ethnicity	0.05	0.06	−0.01
	[0.22]	[0.23]	(0.03)
Education	5.88	5.85	0.03
	[1.01]	[1.06]	(0.12)
	(TVET or other College)	(TVET or other College)	
Full-Time Job	0.63	0.67	−0.04
	[0.49]	[0.47]	(0.06)
Part-Time Job	0.15	0.18	−0.03
	[0.36]	[0.38]	(0.04)
Unemployed	0.19	0.11	0.08*
	[0.39]	[0.31]	(0.05)
Household Size	4.46	4.45	0.016
	[1.75]	[1.89]	(0.21)
Monthly Household Income	6.66	7.22	−0.56
	[3.81]	[3.64]	(0.45)
	(≈ SAR20,000)	(≈ SAR22,000)	
Weekly Groceries	1.78	1.85	−0.07
	[1.09]	[1.16]	(0.13)
Weekly Groceries Expenditure	3.71	3.79	−0.08
	[2.93]	[2.98]	(0.35)
	(≈ SAR950)	(≈ SAR970)	
Climate Change Awareness	7.64	7.67	−0.03
	[1.86]	[1.93]	(0.22)
Affected by Climate Change	7.04	7.30	−0.26
	[2.51]	[2.43]	(0.3)
Perception on Companies	5.15	5.31	−0.16
	[2.31]	[2.43]	(0.28)
Perception on Countries	5.45	5.58	−0.12
	[2.41]	[2.3]	(0.29)
Number of Observations	80	517	

Table 4 Continued

	Dependent variable:		
	Control	Any Treatment	Difference
	(1)	(2)	(3)
Panel C. India Study			
Age	28.60	32.83	-4.23^{***}
	[9.63]	[9.35]	(1.39)
Gender	0.29	0.49	-0.19^{***}
	[0.46]	[0.50]	(0.07)
Education	6.28	6.58	-0.30^{**}
	[1.03]	[0.89]	(0.14)
	(Undergraduate degree)	(Undergraduate degree)	
Full-Time Job	0.33	0.62	-0.29^{***}
	[0.47]	[0.49]	(0.07)
Part-Time Job	0.31	0.14	0.17^{**}
	[0.47]	[0.35]	(0.07)
Unemployed	0.24	0.12	0.12^{*}
	[0.43]	[0.33]	(0.06)
Household Size	4.60	4.11	0.48^{**}
	[1.71]	[1.33]	(0.24)
Monthly Household Income	8.23	6.70	1.53^{***}
	[3.97]	[4.36]	(0.57)
	($\approx$ IR52,000)	($\approx$ IR38,000)	
Weekly Groceries	2.60	2.72	-0.12
	[1.45]	[1.43]	(0.21)
Weekly Groceries Expenditure	2.67	2.84	-0.17
	[2.62]	[2.54]	(0.37)
	($\approx$ IR2,700)	($\approx$ IR2,850)	
Climate Change Awareness	8.10	7.79	0.31
	[1.54]	[1.68]	(0.22)
Affected by Climate Change	7.65	7.75	-0.10
	[1.94]	[1.86]	(0.28)
Perception on Companies	4.81	5.98	-1.17^{***}
	[2.59]	[2.62]	(0.37)
Perception on Countries	5.33	6.36	-1.04^{***}
	[2.39]	[2.40]	(0.34)
Number of Observations	55	357	

Notes: Gender is a binary variable where Female=1 and Male=0. Ethnicity and employment related variables are also binary equalling 1 if the participant belongs to that group. Annual/monthly household income is before taxes. Climate awareness and perception related variables are on a scale of 0-10, where 10 is the highest score. Figures are rounded to two decimal places wherever applicable. Column (1) shows the average for the observations assigned to the control group, while Column (2) does that for the observations assigned to any treatment arm. Column (3) compares the averages by conducting two-sample mean comparison tests with unequal variances. Standard deviations are reported in square brackets in Columns (1)-(2) and robust standard errors in brackets in Column (3). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

compatible, meaning that the subject's dominant strategy is to bid her true maximum WTP, as the subject's stated WTP affects only whether she purchases the item and not the price she pays (Berry et al., 2020, p.1438; Burchardi et al., 2021, p.1).

In the setting of this study, I approached students at Erasmus University Rotterdam with a tray which had two concealed packages of stroopwafels, a Dutch sweet frequently consumed by people living in the Netherlands. Even though stroopwafels would not be considered as staple food by no means, it was used not only because presumably residents of Netherlands would be sensitive to the prices charged by two different firms producing stroopwafels but also because I would not be able to sell a staple food to students. After their acceptance to participate, students were given the same information as above (CPS as a perfect indicator, no political economy concerns, identical products, repeat buyer etc). However, before they stated their WTP for a single stroopwafle, they were told that a random number between 0 and 1 (up to four decimal places) will be generated and the generated number would determine the result as prescribed in the previous paragraph. The reason for the range [0,1] was the country-wide average price charged for a single, factory-made stroopwafel (20 cents) which was also presented as the price charged by the firm on the right either having a lower or no score. Example representations are provided in Appendix 7.2. Figures 5 and 6 show the selected treatment arms for each setting.

After the WTP elicitation stage, participants were given a questionnaire about their demographic information.

There are several major shortcomings of the study, starting with the unclarity on whether participants really felt that they would have to pay for the product (which would undermine the whole purpose). Moreover, social desirability bias (answering in a way that will be favoured by others) is an issue in this context as evidenced by Dizon-Ross and Jayachandran, 2022 (p.554). It is more so for this study, as most of the interviews were conducted with groups of students (usually 2-3 people). In addition, it is not clear whether international students would know about the product and be as price sensitive as Dutch students, which is partially accounted for in the estimation strategy.

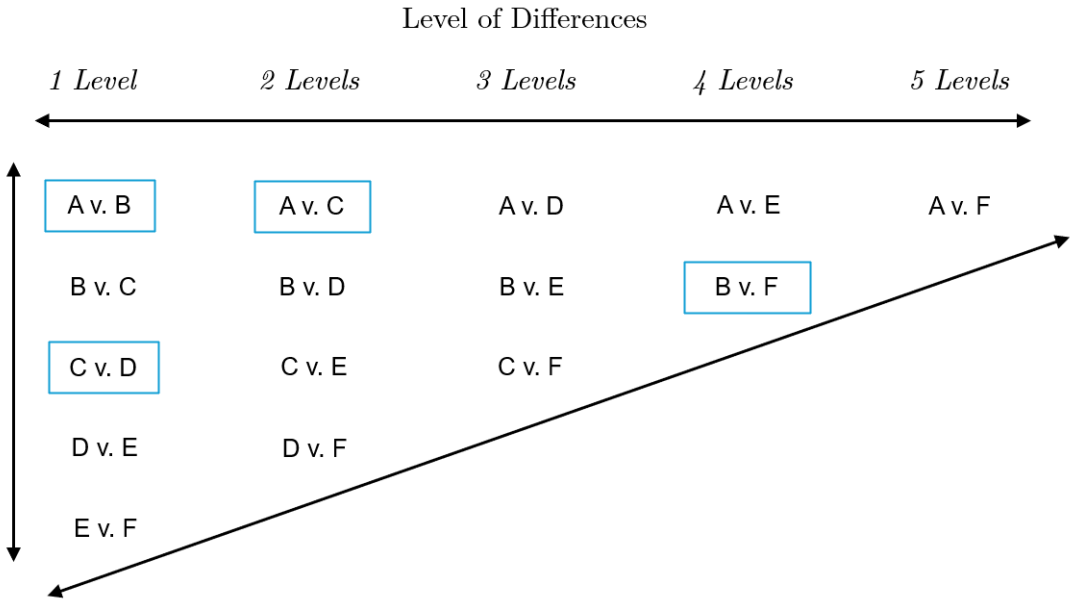

Figure 5: Treatment Arms for Mandatory Disclosure

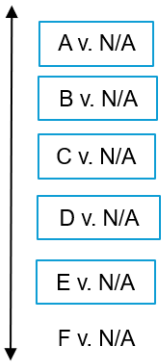

Figure 6: Treatment Arms for Voluntary Disclosure

3.2.2 Data and Descriptive Statistics

In total, 302 students participated in the study, 30 of which were randomly selected to the control group. Table 5 reports the descriptive statistics. The groups only differ on their perception about how countries do in terms of fighting climate change, but this is statistically significant only at the 10% level.

Table 5: Descriptive Statistics for the Revealed Preference Study

	Control (1)	Any Treatment (2)	Difference (3)
Age	21.1	21.16	-0.06
	[2.93]	[3.4]	(0.57)
Gender	0.57	0.62	-0.05
	[0.50]	[0.49]	(0.01)
Dutch Dummy	0.38	0.27	0.1
	[0.49]	[0.45]	(0.1)
Share of Undergraduates	0.79	0.79	0.01
	[0.41]	[0.41]	(0.08)
Monthly Allowance	1192.86	1252.11	−59.26
	[640.56]	[976.82]	(155.54)
Climate Change Awareness	6.07	6.38	−0.31
	[2.4]	[1.75]	(0.46)
Perception on Companies	4.31	4.56	−0.25
	[1.49]	[1.63]	(0.29)
Perception on Countries	4.22	4.81	−0.58*
	[1.82]	[1.67]	(0.35)
Number of Observations	30	272	

Notes: Gender is a binary variable where Female=1 and Male=0. Dutch Dummy is also a binary variable taking the value of 1 if the participants is Dutch. Figures are rounded to two decimal places wherever applicable. Column (1) shows the average for the observations assigned to the control group, while Column (2) does that for the observations assigned to any treatment arm. Column (3) compares the averages by conducting two-sample mean comparison tests with unequal variances. Standard deviations are reported in square brackets in Columns (1)-(2) and robust standard errors in brackets in Column (3). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

4 ESTIMATION STRATEGY AND RESULTS

4.1 Contingent Valuation Studies

Using ordinary least squares (OLS) and least absolute shrinkage and selection operator (LASSO) as the methods, I first estimate the impact of the treatment with the following empirical specification:

$$WTP_i = \alpha + \beta TREAT_{ij} + \delta X_i + \varepsilon_i \tag{21}$$

where WTP measures the willingness-to-pay of individual i, $TREAT$ is a binary variable equal to zero if the individual was assigned to the control group and one if to the treatment group and j indicates the treatment arm. X is a vector for demographic control variables, and ε is the error term, which is mean independently expected to be zero (Wooldridge, 2020, p.42), given the quasi-random assignment (except the India study). A two-tailed hypothesis testing, i.e. H_0: $\beta = 0$ and H_A: $\beta \neq 0$, was opted for, as this is the first paper testing the average treatment effect for such a policy intervention and as different treatment arms could lead to different results.

Even though one would not expect any multicollinearity between the explanatory variable and the control variables due to the quasi-random assignment and hence can be more liberal in the specifications (Wooldridge, 2020, p.201), an iterative process was employed for selecting the right control variables to keep the statistical precision intact, based on (i) the bivariate relationships between the dependent variable and the control variables (joint F-test for the categorical variables), (ii) the forward and backward specification process where the p-values and joint F-test p-values were assessed with adding or removing variables, and (iii) the development of the adjusted R^2 with shorter and longer specifications [without putting too much emphasis on it (Wooldridge, 2020, p.200)]. This iterative process was done for each country and setting, and the model was adjusted accordingly.

Consequently, age, the square root of age (as it fits the data better), gender, education,

employment status, annual/monthly household income, weekly groceries expenditure, a variable called *ClimateIndex* which is an equally weighted average of the four-climate awareness/perception related questions, and the region/state the participant resides were added whenever they seemed to have a significant effect on the model. One might argue that there is multi-collinearity between annual/monthly household income and weekly groceries expenditure, hence both should not be included in the same specification. However, as mentioned, these are control variables, and their multicollinearity with the single independent variable is not worrying (Allison, 2012), which is why both of them were kept. In any case, a LASSO regression, which penalizes adding irrelevant variables and takes only the most appropriate control variables, was run to test whether there is overspecification.

Tables 10, 11, and 12, all under Appendix 7.3 to keep this section concise, report the average treatment effect on nominal WTP results for the U.S. study, South Africa study, and India study respectively. The models predict that in most of the treatment arms where the evaluation is made as higher CPS versus lower CPS, there is indeed a relationship between being assigned to the treatment group and exerting a higher WTP, holding other factors fixed. The opposite seems also be true for the mirrored treatment arms, i.e. D v. C and F v. A, but the results are more mixed in terms of both statistical and economical significance. However, the hypothesis that a higher CPS level difference (moving horizontally eastward and diagonally northern eastward in Figure 3) leading to a higher average WTP seem not necessarily correct as sometimes the coefficient for A v. D (a three-level difference) is bigger than the coefficient for B v. F (a four-level difference). Maybe relatedly, in all studies the A v. B treatment arm always yields a higher coefficient (statistically significant at 1% level) than E v. F which statistically does not always provide coefficients different than zero. The same situation can be observed for the two-level difference (B v. D and D v. F) as well as the three-level difference treatment arms (A v. D and C v. F). Therefore, my hypothesis that moving vertically in Figure 3 within the same CPS level difference would not lead to different results can be rejected, and having a certain level of CPS (say, A rather than C) seem to matter even if we compare the results

within the same CPS level difference. Lastly, the voluntary settings also offer a lot of insight and the clearest pattern because as expected, in all countries we see a decreasing average effect of the treatment, both statistically and economically, the more we go down the ladder from A v. N/A to F v. N/A. Further elaborations will be presented below with a model allowing cross-country comparisons. Although the coefficients and standard errors sometimes differ, the results are mostly consistent between the OLS estimation and LASSO.

In addition to the main specification, Equation (22) regresses the lower bound of the WTP (the first amount that the participant would definitely pay) on the explanatory variable to find more conservative estimates which may or not be closer to the true value:

$$LowerBoundWTP_i = \alpha + \beta TREAT_{ij} + \delta X_i + \varepsilon_i \tag{22}$$

where the same notation and techniques are followed. Tables 13, 14, and 15 in Appendix 7.3 report the results in the same order. Naturally, the economic significance decreases but the statistical significance is kept in all setting and in almost all treatment arms, and therefore the interpretations are identical to the above. In other words, even the most conservative estimations predict a significant average treatment effect, controlling for factors that may affect the results.

As the prices and therefore WTPs are different in all the settings, I estimate the natural logarithm of WTP to make the comparisons between the settings in percentage terms. Furthermore, the log-level model has the advantage of reducing the impact of the outliers on the dependant variable and mitigating heteroskedasticity or skewness (Wooldridge, 2020, p.187):

$$\log(WTP_i) = \alpha + \beta TREAT_{ij} + \delta X_i + \varepsilon_i \tag{23}$$

$$\log(LowerBoundWTP_i) = \alpha + \beta TREAT_{ij} + \delta X_i + \varepsilon_i \tag{24}$$

where the same notation and techniques are followed. 16, 17, and 18 report the results

for Equation (23), and 19, 20, and 21 for Equation (24), which are all in Appendix 7.3. The semi-elasticity corrections (Kennedy, 1981, p.801; Wooldridge, 2020, p.227) are not calculated because the estimated coefficients are relatively small, hence the correction does not yield substantial changes.

Estimating the natural logarithm of WTP with both OLS and LASSO, Table 6 summarizes all the results. A visual interpreter might want to skip to Figure 7 and Figure 8[2] which neatly demonstrate the table with the point estimates accompanied by their 95% confidence interval.

Table 6: Average Treatment Effect on Log(WTP): All Studies

	Dependent variable: Log(WTP)					
	OLS - U.S. (1)	OLS - S.Africa (2)	OLS - India (3)	LASSO - U.S. (4)	LASSO - S.Africa (5)	LASSO - India (6)
Panel A. The Mandatory Setting						
Panel A1. One-Level CPS Difference						
A v. B	10.01%***	5.81%***	6.49%**	7.64%***	6.63%***	4.83%**
	(0.017)	(0.017)	(0.025)	(0.017)	(0.016)	(0.019)
D v. C (Mirrored)	−7.39%***	−4.57%*	−0.03%	−7.05%***	−4.35%*	−0.02%
	(0.021)	(0.024)	(0.030)	(0.020)	(0.024)	(0.030)
E v. F	7.35%***	2.17%	5.72%***	7.39%***	2.22%	4.56%**
	(0.021)	(0.022)	(0.020)	(0.016)	(0.018)	(0.020)
Panel A2. Two-Level CPS Difference						
B v. D	8.91%***	8.59%***	7.04%**	10.19%***	8.40%***	6.28%**
	(0.021)	(0.018)	(0.030)	(0.020)	(0.017)	(0.020)
D v. F	7.50%***	7.06%***	2.72%	6.70%***	6.16%***	4.22%**
	(0.017)	(0.017)	(0.020)	(0.015)	(0.017)	(0.015)
Panel A3. Three-Level CPS Difference						
A v. D	15.51%***	4.32%***	11.16%***	14.59%***	5.81%***	9.76%***
	(0.022)	(0.020)	(0.010)	(0.018)	(0.020)	(0.020)
C v. F	14.05%***	9.01%***	5.27%**	13.04%***	7.14%***	4.57%
	(0.018)	(0.019)	(0.030)	(0.017)	(0.020)	(0.030)
Panel A4. Four-Level CPS Difference						
B v. F	12.43%***	7.35%***	7.01%***	12.85%***	7.11%***	6.10%***
	(0.023)	(0.022)	(0.020)	(0.019)	(0.019)	(0.020)
Panel A5. Five-Level CPS Difference						
F v. A (Mirrored)	−13.27%***	−8.72%***	−3.73%	−14.05%***	−7.83%***	−3.24%*
	(0.023)	(0.020)	(0.020)	(0.022)	(0.019)	(0.020)
Panel B. The Voluntary Setting						
A v. N/A	12.78%***	5.38%**	8.22%***	12.24%***	4.95%**	7.72%***
	(0.018)	(0.022)	(0.020)	(0.018)	(0.018)	(0.020)
C v. N/A	4.85%***	2.36%*	6.29%*	4.53%***	3.56%***	3.59%
	(0.015)	(0.014)	(0.030)	(0.013)	(0.012)	(0.030)
E v. N/A	0.26%	−1.53%	−0.03%	0.04%	−1.82%	0.05%
	(0.016)	(0.014)	(0.020)	(0.016)	(0.014)	(0.020)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2 [Range]		[23.52%, 66.43%]		Wald χ^2 Test [Range]	[0, 66.89]	
Number of Observations [Range]		[74, 132]			[74, 132]	

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Columns (1)-(3) report the OLS regression results for the U.S., South Africa and India studies respectively, while Columns (4)-(6) report the LASSO regression results in the same order. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

[2]The tables were created in Python, though I got some help from Gemini (credit is due). This was the only involvement in any shape, format or form of an external factor in this paper.

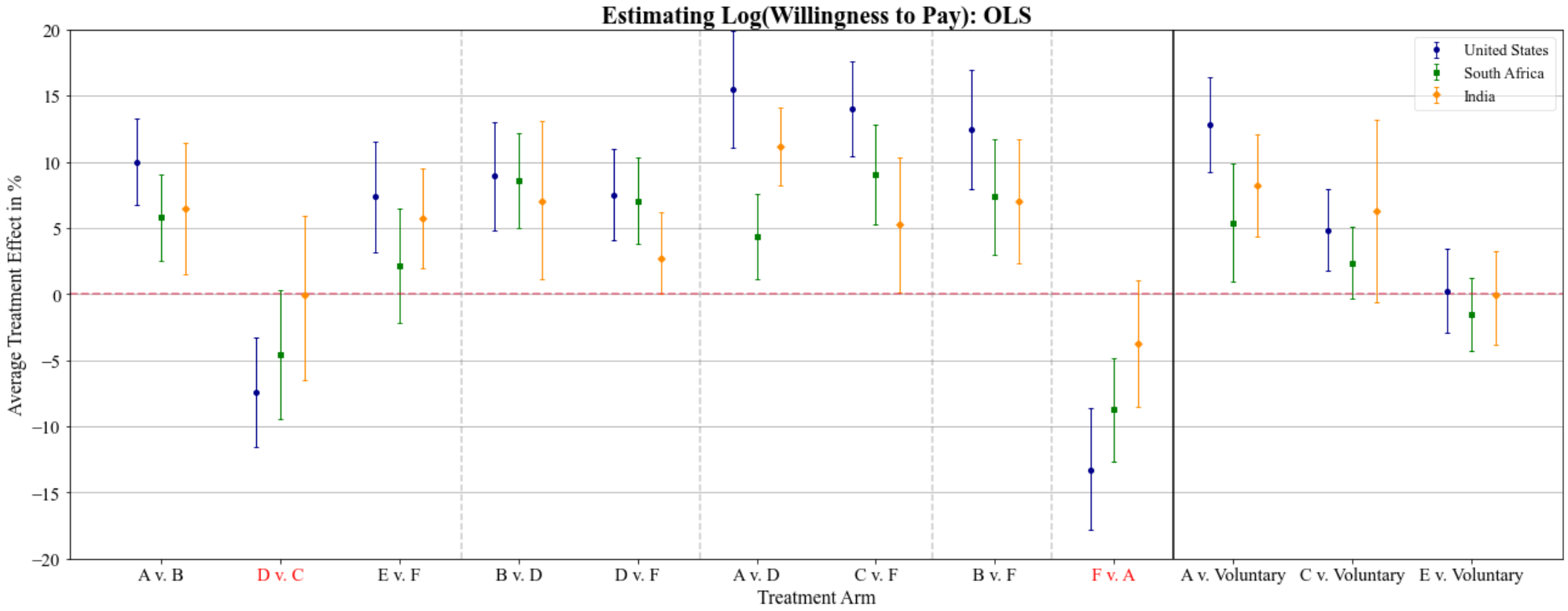
Estimating Log(Willingness to Pay): OLS
Average Treatment Effect in %
20
15
10
5
0
−5
−10
−15
−20
United States
South Africa
India
A v. B
D v. C
E v. F
B v. D
D v. F
A v. D
C v. F
B v. F
F v. A
A v. Voluntary
C v. Voluntary
E v. Voluntary
Treatment Arm

Figure 7

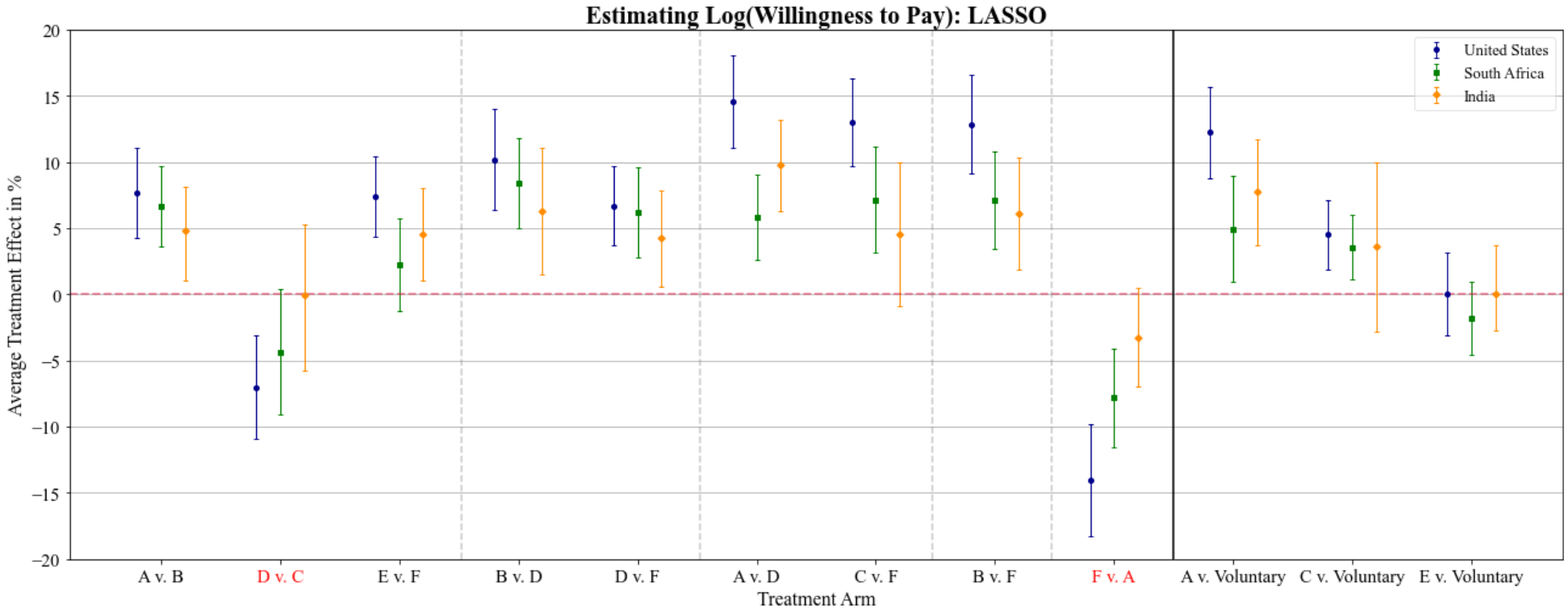
Estimating Log(Willingness to Pay): LASSO
Average Treatment Effect in %
20
15
10
5
0
−5
−10
−15
−20
United States
South Africa
India
A v. B
D v. C
E v. F
B v. D
D v. F
A v. D
C v. F
B v. F
F v. A
A v. Voluntary
C v. Voluntary
E v. Voluntary
Treatment Arm

Figure 8

In all countries, when individuals compare firms with scores A and B, they exert a higher WTP between 5-10%. The rate is lower for E v. F and not statistically different from 0 in South Africa, while only the U.S. participants "punish" the firm having a lower score when the CPS is difference is one level (D v. C). On the other hand, when the firm has a substantially lower score (F v. A) the punishment gets bigger (8-14%), as one would predict, excluding the Indian participants who still statistically exert the same WTP which might be valid but also be a bad sign in terms of the internal validity of that study given the other results.

In all other mandatory setting conditions (CPS difference two to four) the data finds a 5-15% higher WTP across all studies, usually in an increasing order as the CPS level difference gets bigger, and these are almost always statistically significant at the 5% significance level at the least. As mentioned before, an interesting finding is within level differences. For instance, looking at the two-level difference (B v. D and D v. F) results with the LASSO estimation, individuals exert a 2-3.5 percentage points (47 to 52%) higher WTP for the when they are assigned to the B v. D condition. The same can be said about the three-level difference scenario, apart from South Africa, where the LASSO estimates for India even transform from having no statistically significant effect in the C v. F condition to 10% higher WTP result statistically significant at the 1% level in the A v. D condition. These results suggest that, by simply eliminating the information asymmetry between the parties, a CPS policy might induce a cut-throat competition to become more and more environmentally friendly, and thereby decrease the negative externalities imposed by firm across all countries and all markets.

In the voluntary settings, on the other hand, across all countries a statistically and economically higher WTP is detected when one firm has CPS of A and the other decided not to obtain a score, in the range of 5-13%. But this quickly fades away as the obtained score gets lower across all countries. Interestingly, individuals do not directly judge the non-obtained score to be an F because while U.S. participants exert a 14% higher WTP in the C v. F condition, this is only 5% in the C v. N/A condition, an almost 200% difference. In other countries, individuals show a statistically higher WTP only at the

10% level in the C v. N/A condition, different from the C v. F condition, and to a much economically lesser degree. Moreover, the result found for E v. F (5-7% higher WTP, statistically significant at the 1% level in U.S. and India) completely vanishes in the E v. N/A arm. These findings suggest that a voluntary compliance regulation would not be as effective as mandatory compliance, and hence may not render the desired outcomes.

Furthermore, Table 22 under Appendix 7.3 reports the accumulated results for the natural logarithm of the lower bound WTP estimations. As mentioned, the statistical significance is usually kept even with these most conservative estimates. If a behavioural response is observed even in these scenarios, incentivizing firms for climate abatement might be realized in a much wider scale.

Although the results indicate that different WTP estimates emerge even when the comparison is made within the same CPS level differences, I lastly pool the observations when the WTP was elicited for the same CPS level difference to find within-level averages with more statistical precision. Table 7 reports and Figure 9 and Figure 10 demonstrate.

Table 7: Within Level Average Treatment Effect on Log(WTP): All Studies

	Dependent variable: Log(WTP)					
	OLS - U.S. (1)	OLS - S.Africa (2)	OLS - India (3)	LASSO - U.S. (4)	LASSO - S.Africa (5)	LASSO - India (6)
One-Level CPS Difference	7.97%***	4.39%***	6.52%***	8.04%***	4.44%***	5.09%***
	(0.015)	(0.014)	(0.016)	(0.013)	(0.013)	(0.014)
Two-Level CPS Difference	8.26%***	8.45%***	4.94%***	8.32%***	7.12%***	5.25%***
	(0.014)	(0.014)	(0.018)	(0.014)	(0.024)	(0.016)
Three-Level CPS Difference	14.65%***	6.78%***	7.36%***	14.02%***	6.44%***	7.05%***
	(0.016)	(0.015)	(0.018)	(0.014)	(0.014)	(0.016)
Four-Level CPS Difference (only B v. F)	12.43%***	7.35%***	7.01%***	12.85%***	7.11%***	6.10%***
	(0.023)	(0.022)	(0.020)	(0.019)	(0.019)	(0.020)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	[26.24%, 46.45%]			Wald χ^2 Test [Range]	[11.44, 37.78]	
Number of Observations [Range]	[117, 177]				[117, 177]	

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Columns (1)-(3) report the OLS regression results for the U.S., South Africa and India studies respectively, while Columns (4)-(6) report the LASSO regression results in the same order. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

According to the results, we would, on average, expect 4-8% higher WTP when the CPS difference is one, 5-8.5% when the difference is two, 7-14% when three, and 6-13% when the level difference is four. The results do not always follow a logical order given the previous trend but one reason for that might be the limited sample sizes. Furthermore, finding variations between individuals of different countries is expected given the difference in living standards.

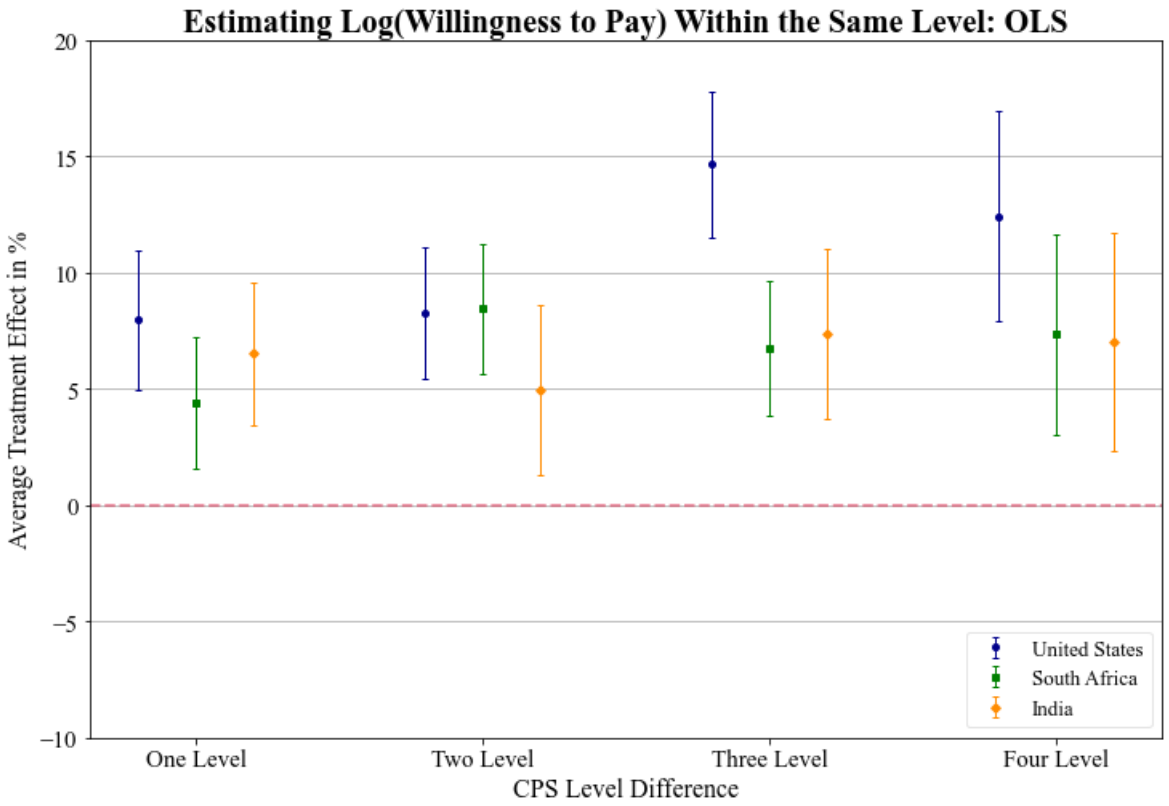

Figure 9

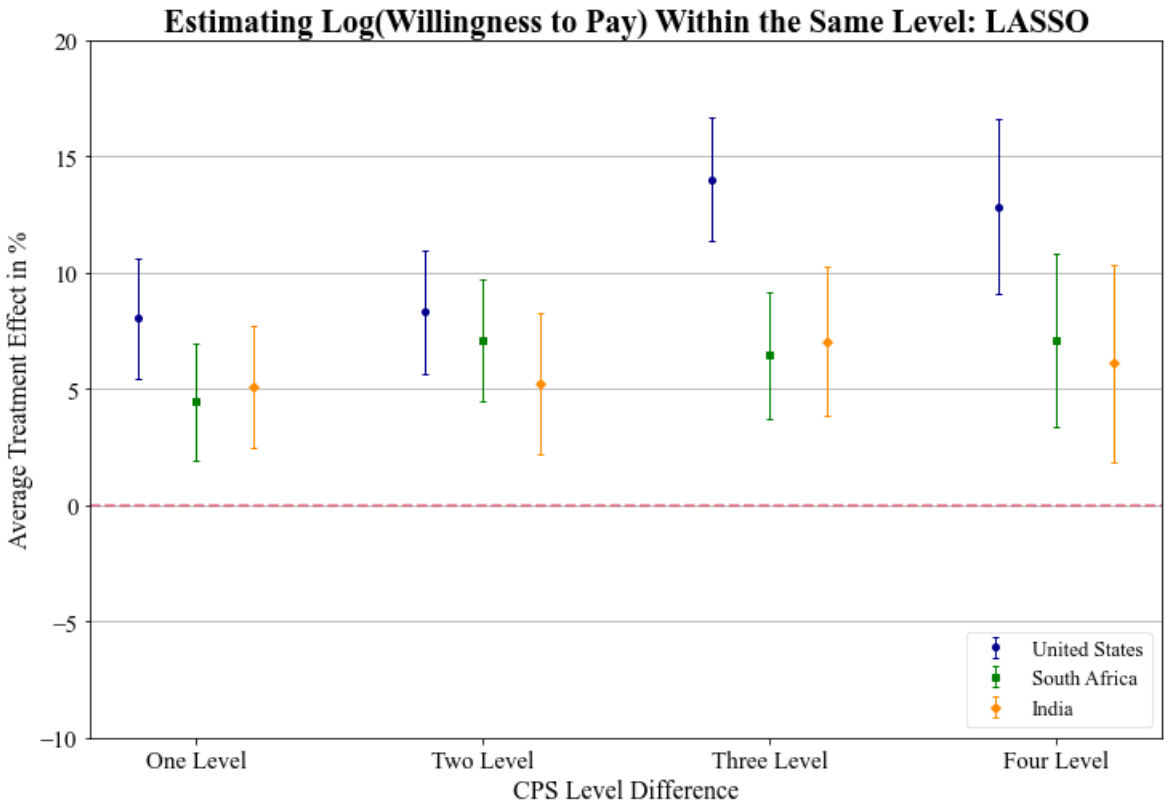

Figure 10

Although one might interpret the results as economically significant enough, it must also be kept in mind that the research design has evolved around staple foods where the participants were told that the goods were identical in any characteristics so that cross-price elasticity for the same staple produced by two different firms could be assumed as infinite, and that the treatment can be the only factor if any difference were to be found. In a real-life environment where products are already differentiated and where they are consumed for pleasure rather than necessities, one might expect a bigger difference in WTP, conditioned on the CPS level differences. At the end, one might as well pay 40 cents (or $400) more for a chocolate bar (watch) produced by a firm with CPS of B, where the average price is $3 ($2000) and the average CPS is E (thus a 13.3% or 20% higher WTP), given the extra utility of knowing and being sure that the firm is more climate friendly than others.

4.2 The Revealed Preference Study

Notwithstanding its shortcomings outlined above, the revealed preference study conducted among students at Erasmus University Rotterdam validates the last argument. Using Equation (23) and accounting for the participants age, gender, nationality (whether they are Dutch or not), monthly allowance, and $ClimateIndex$ score, Table 8 reports the regression results and Figure 11 visualizes them. One thing to note here is that I combined the limited A v. B observations with A v. C observations, and C v. N/A with D v. N/A because the coefficients were close, and I wanted to increase statistical precision. Nevertheless, the coefficient for A v. C is slightly deflated and D v. N/A is slightly inflated.

After applying the semi-elasticity corrections, the results indicate that individuals are willing to pay between 40 to 72% more (which translates to 8 to 14 cents), for a single stroopwaffle in the mandatory setting, depending on the CPS difference, and holding other factors fixed. As depicted in Figure 11, this time we can detect a clear pattern: The higher the CPS difference, the higher the WTP estimation becomes.

Moreover, the findings of the voluntary setting assert that a firm can charge around 73% more per unit if it has the highest score where the competitors did not get a score.

The findings in this setting are also consistent with the previous results as we see a clear decrease in WTP the lower the obtained score gets. Similarly, in the E v. N/A condition participants statistically neither reward nor punish the firm which made the effort to obtain their CPS. Hence, this results also favour a mandatory policy design, rather than voluntary compliance. Nevertheless, the large confidence intervals should be noted, although all estimations are statistically significant at the 1% level, except for the null result for E v. N/A.

Table 8: Average Treatment Effect on Log(WTP): The Revealed Preference Study

	OLS		LASSO	
	(1)	Semi-Elasticity Corrections	(2)	Semi-Elasticity Corrections
Panel A. The Mandatory Setting				
C v. D	34.06%***	40.58%	32.58%***	38.51%
	(0.09)		(0.07)	
A v. C	45.87%***	58.20%	44.58%***	56.17%
	(0.06)		(0.09)	
B v. F	54.03%***	71.65%	55.43%***	74.07%
	(0.08)		(0.08)	
Panel B. The Voluntary Setting				
A v. N/A	54.85%***	73.07%	54.88%***	73.12%
	(0.09)		(0.09)	
B v. N/A	34.02%***	40.52%	34.77%***	41.58%
	(0.07)		(0.06)	
D v. N/A	22.81%***	25.62%	16.45%***	17.88%
	(0.07)		(0.06)	
E v. N/A	−4.52%	−4.42%	−5.66%	−5.50%
	(0.07)		(0.06)	
Demographic Controls	Yes		Yes	
Adjusted R^2 [Range] /	[16.46%, 56.07%]	Wald χ^2 Test [Range]	[0.81, 59.83]	
Number of Observations [Range]	[50, 79]		[50, 79]	

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results, accompanied with their respective semi-elasticity corrections. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

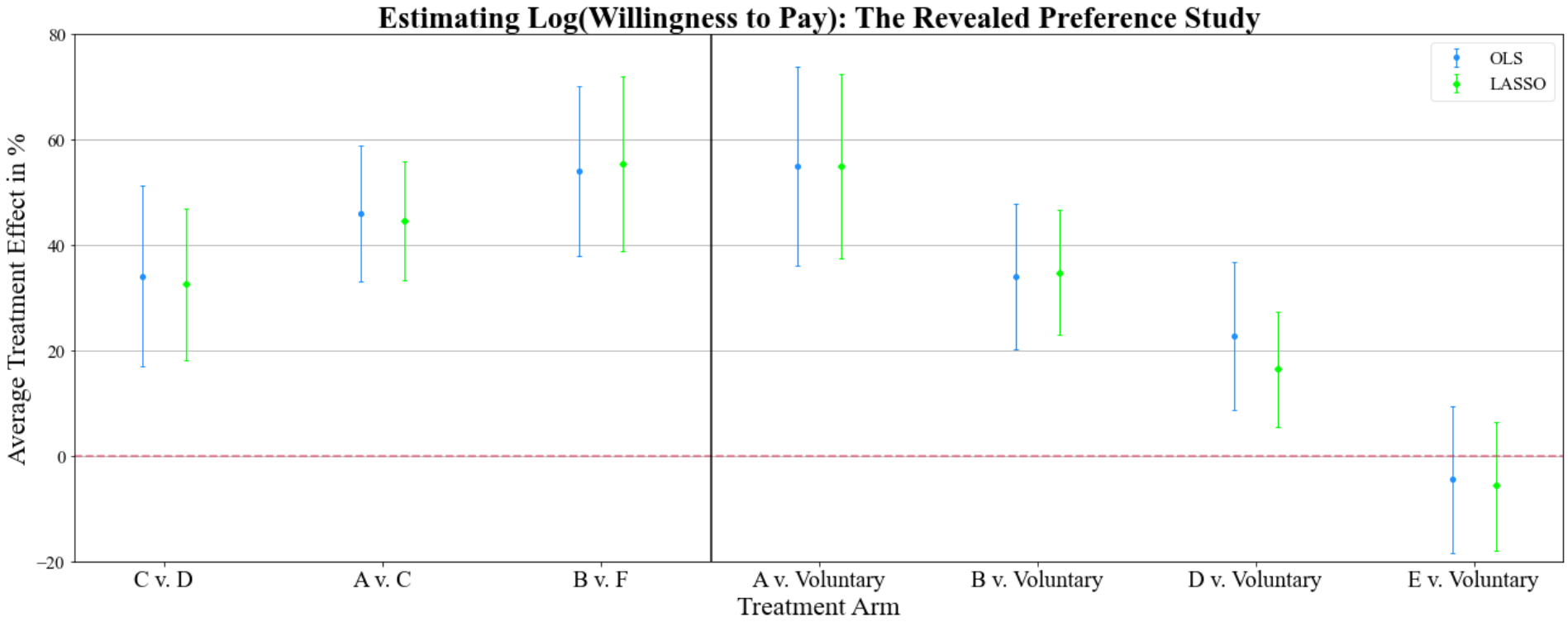
Estimating Log(Willingness to Pay): The Revealed Preference Study
Average Treatment Effect in %
80
60
40
20
0
−20
OLS
LASSO
C v. D
A v. C
B v. F
A v. Voluntary
B v. Voluntary
D v. Voluntary
E v. Voluntary
Treatment Arm

Figure 11

5 DISCUSSION AND CONCLUSION

Testing several research designs in four countries, this paper provides the first preliminary (though noisy) empirical evidence that, by providing clear and easily interpretable information, a soft policy intervention directly eliminating the information asymmetry between the firms and the individuals in terms of the firm's climate impact might lead to a behavioural response where individuals exert a higher WTP for the product to get an extra utility while maximizing their environmental-impact-adjusted indirect utility function. Although wider and more robust research are needed to validate this argument, this policy design, especially when it is mandatory for all firms, has the potential to work in different countries, albeit in varying scale and effectiveness.

After dealing with the information asymmetry problem, the question becomes whether the intervention would incentivize firms to invest in climate abatement and reduce their adverse impact on the environment. Answering this question is still in the realm of theoretical frameworks, and as shown in Equation (17), is highly contingent on the proportion of climate caring individuals as well as the expected profit change for the firm after the climate abatement investment, which has to dominate the investment cost and the loss incurred from losing the demand from climate uncaring individuals. Therefore, one cannot answer the question at this point in time nor can make an educated guess as a policy design close to CPS does not exist. Nonetheless, the findings suggest that a firm can charge 6-10% more per unit for a good no one would pay even a marginally higher amount, by having only a one level higher CPS from its competitors. As this rate is increasing up to 15% with the increase in the CPS level difference, CPS might have the potential to incentivize firm to invest in climate abatement. The results are much higher (up to 70%) when the nature of the good slightly transforms from staple to a normal consumption good.

Considering evidence indicating substantial effects for having a score of A or B, a race to the top in which all firms compete for getting higher and higher CPS might emerge, as theoretically the firm would only worry about the investment without incurring any

costs for conveying the information or signalling. In return, all countries and their markets might reduce their negative externalities on the environment. Adjusting the old saying a little bit: "One policy intervention, two market failures!"

Nevertheless, the CPS scheme is a very abstract idea, and it is not even certain whether it would be possible to engineer a perfect indicator like that. Furthermore, CPS undoubtedly would have some implications, the most evident one a potential increase in all prices, to the detriment of the most vulnerable the most, who are already suffering from global warming the most as well. Another concern would be the situation for the small and medium enterprises who might not be able to undertake the requirements, and consequently exit the markets. But one has to think what if engineering CPS is just a matter of time and research, what if the regulator is benevolent enough [an analogous of the benevolent car expert who solves Akerlof, 1970's lemon problem by eliminating the information asymmetry free of charge] to subsidize the initial steps of acquiring CPS and some of the investments, rather than spending millions on policies that are destined to not work (such as CSR), and what if the solutions to some of the problems arising from such a policy can be as simple as a universal ultra basic income. Given the scientific consensus on temperature anomalies (National Aeronautics and Space Administration, 2024) and the scientific literature about the danger imposed by climate change, or, if there is scepticism about science and everything is about more economic growth, the projections suggesting that a 3.7Celsius of warming could reduce global GDP by 7-12% by 2099 (Nath et al., 2024, p.4) some trade-offs might actually be desirable in a world where there is no free lunch.

At least for the developed world, which are by all means [currently, cumulatively (historically), consumption-wise etc.] the main scapegoats of this adverse situation (Chancel and Piketty, 2015), maybe the solution lies in producing and consuming less and by learning how to be "*content not only with cleaner cars but also with smaller cars, or no cars at all*" (A. V. Banerjee and Duflo, 2019, p.217). The novice researcher hopes to shed some light, most likely very limited, to all these questions in future research.

[*P.S. This paper was shortened, and some extra data analysis were left out due to the word limit. The full version of the paper will be available soon.*]

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7 APPENDICES

7.1 Appendix 1 - Proof of Equations

Table 9: Proof of Table 1

For Environmentally Friendly Firms

$$(\lambda) * (\Phi' - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) + (1-\lambda) * (\Phi' - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}})$$
$$\geq (\lambda) * \Phi + (1-\lambda) * \Phi$$
$$= (\Phi^{'} - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) \geq \Phi$$
$$= \Phi' - \Phi \geq \frac{\gamma}{Q^{\forall}} + \frac{\theta - \varepsilon_2}{Q^{\forall}}$$

if climate caring individuals participate, i.e.

$$\left(\sigma + \delta - (\beta - \varepsilon_1)\right) * \mu \geq 0$$

$$(\lambda) * \mathbb{C}' + (1-\lambda) * (\Phi' - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) \geq (\lambda) * \mathbb{C} + (1-\lambda) * \Phi$$
$$= (1-\lambda) * (\Phi^{'} - \Phi - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) \geq (\lambda) * \left(\mathbb{C} - \mathbb{C}^{'}\right)$$

if climate caring individuals do not participate, i.e.

$$\left(\sigma + \delta - (\beta - \varepsilon_1)\right) * \mu < 0$$

For Environmentally Unfriendly Firms

$$(\lambda) * (\Phi' - \frac{(\gamma * \varsigma)}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) + (1-\lambda) * (\Phi' - \frac{(\gamma * \varsigma)}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}})$$
$$\geq (\lambda) * \Phi + (1-\lambda) * \Phi$$
$$= (\Phi^{'} - \frac{(\gamma * \varsigma)}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) > \Phi$$
$$= \Phi' - \Phi > \frac{(\gamma * \varsigma)}{Q^{\forall}} + \frac{\theta - \varepsilon_2}{Q^{\forall}}$$

if climate caring individuals participate, i.e.

$$\left(\sigma + \delta - (\beta - \varepsilon_1)\right) * \mu \geq 0$$

$$(\lambda) * \mathbb{C}'' + (1-\lambda) * (\Phi' - \frac{(\gamma * \varsigma)}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}})$$
$$\geq (\lambda) * \mathbb{C} + (1-\lambda) * \Phi$$
$$= (1-\lambda) * (\Phi^{'} - \Phi - \frac{\gamma}{Q^{\forall}} - \frac{\theta - \varepsilon_2}{Q^{\forall}}) > (\lambda) * \left(\mathbb{C} - \mathbb{C}^{''}\right)$$

if climate caring individuals do not participate, i.e.

$$\left(\sigma + \delta - (\beta - \varepsilon_1)\right) * \mu < 0$$

7.2 Appendix 2 - Research Design Examples

COMPANY 1

CLIMATE PERFORMANCE SCORE: **A**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **D**

PRICE: $2.89 / per 3 lb

COMPANY 1

CLIMATE PERFORMANCE SCORE: **F**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **A**

PRICE: $2.89 / per 3 lb

COMPANY 1

CLIMATE PERFORMANCE SCORE: C

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: N/A

PRICE: $2.89 / per 3 lb

COMPANY 1

CLIMATE PERFORMANCE SCORE: B

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: F

PRICE: R34.58 / per 2.5 kg

COMPANY 1

CLIMATE PERFORMANCE SCORE: **A**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **B**

PRICE: R34.58 / per 2.5 kg

COMPANY 1

CLIMATE PERFORMANCE SCORE: **B**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **F**

PRICE: ₹94.16 / per 1 kg

COMPANY 1

CLIMATE PERFORMANCE SCORE: **A**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **N/A**

PRICE: ₹94.16 / per 1 kg

COMPANY 1

CLIMATE PERFORMANCE SCORE: **E**

WTP:

COMPANY 2

CLIMATE PERFORMANCE SCORE: **N/A**

PRICE: ₹94.16 / per 1 kg

COMPANY [X]

CLIMATE PERFORMANCE SCORE: A

PRICE:

COMPANY [Y]

CLIMATE PERFORMANCE SCORE: C

PRICE: 0.20

What would be your willingness to pay for the product on the left?

Please note that the products are identical in every measure including their weights, calories, and nutri-scores.

COMPANY [X]

CLIMATE PERFORMANCE SCORE: B

PRICE:

COMPANY [Y]

CLIMATE PERFORMANCE SCORE: D

PRICE: 0.20

What would be your willingness to pay for the product on the left?

Please note that the products are identical in every measure including their weights, calories, and nutri-scores.

7.3 Appendix 3 - Auxiliary Regression Tables

Table 10: Average Treatment Effect on WTP: The U.S. Study

	Dependent variable: WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	0.31^{***}	0.24^{***}
	(0.05)	(0.05)
D v. C (Mirrored)	-0.19^{***}	-0.18^{***}
	(0.05)	(0.05)
E v. F	0.22^{***}	0.22^{***}
	(0.06)	(0.05)
Panel A2. Two-Level CPS Difference		
B v. D	0.28^{***}	0.32^{***}
	(0.06)	(0.06)
D v. F	0.23^{***}	0.20^{***}
	(0.05)	(0.05)
Panel A3. Three-Level CPS Difference		
A v. D	0.48^{***}	0.46^{***}
	(0.07)	(0.06)
C v. F	0.43^{***}	0.40^{***}
	(0.06)	(0.05)
Panel A4. Four-Level CPS Difference		
B v. F	0.39^{***}	0.40^{***}
	(0.07)	(0.06)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	-0.34^{***}	-0.36^{***}
	(0.06)	(0.05)
Panel B. The Voluntary Setting		
A v. N/A	0.39^{***}	0.38^{***}
	(0.05)	(0.06)
C v. N/A	0.14^{***}	0.13^{***}
	(0.04)	(0.04)
E v. N/A	0.01	0.005
	(0.05)	(0.05)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[25.05%, 59.68%]	[0.01, 66.55]
Number of Observations [Range]	[126, 132]	[126, 132]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: $^{*}10\%$, $^{**}5\%$, $^{***}1\%$.

Table 11: Average Treatment Effect on WTP: The South Africa Study

	Dependent variable: WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	2.16***	2.46***
	(0.58)	(0.56)
D v. C (Mirrored)	−1.28	−1.20
	(0.84)	(0.82)
E v. F	0.90	0.89
	(0.76)	(0.62)
Panel A2. Two-Level CPS Difference		
B v. D	3.23***	3.16***
	(0.66)	(0.65)
D v. F	2.63***	2.32***
	(0.60)	(0.64)
Panel A3. Three-Level CPS Difference		
A v. D	1.61***	2.15***
	(0.59)	(0.60)
C v. F	3.42***	2.75***
	(0.74)	(0.72)
Panel A4. Four-Level CPS Difference		
B v. F	2.83***	2.72***
	(0.80)	(0.70)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−2.76***	−2.44***
	(0.62)	(0.60)
Panel B. The Voluntary Setting		
A v. N/A	2.13***	1.94***
	(0.79)	(0.75)
C v. N/A	0.82*	1.27***
	(0.49)	(0.45)
E v. N/A	−0.47	−0.60
	(0.46)	(0.47)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[23.81%, 46.29%]	[1.61, 23.68]
Number of Observations [Range]	[119, 126]	[119, 126]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 12: Average Treatment Effect on WTP: India Study

	Dependent variable: WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	6.78***	4.61**
	(2.53)	(1.86)
D v. C (Mirrored)	0.94	0.21
	(2.90)	(2.72)
E v. F	5.94***	4.73***
	(1.90)	(1.75)
Panel A2. Two-Level CPS Difference		
B v. D	7.46***	6.66***
	(2.78)	(2.29)
D v. F	2.74*	4.37**
	(1.64)	(1.82)
Panel A3. Three-Level CPS Difference		
A v. D	11.27***	9.89***
	(1.52)	(1.82)
C v. F	5.81**	5.25**
	(2.44)	(2.62)
Panel A4. Four-Level CPS Difference		
B v. F	7.20***	6.34***
	(2.29)	(2.10)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−3.07	−2.74
	(2.19)	(1.71)
Panel B. The Voluntary Setting		
A v. N/A	8.27***	7.89***
	(1.93)	(1.97)
C v. N/A	7.13**	4.30
	(3.39)	(0.04)
E v. N/A	0.002	0.75
	(1.66)	(1.52)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[30.86%, 66.95%]	[0.01, 29.53]
Number of Observations [Range]	[74, 89]	[74, 89]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 13: Average Treatment Effect on Lower Bound WTP: The U.S. Study

	Dependent variable: Lower Bound WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	0.23^{***}	0.17^{**}
	(0.05)	(0.05)
D v. C (Mirrored)	-0.27^{***}	-0.27^{***}
	(0.06)	(0.06)
E v. F	0.19^{***}	0.18^{***}
	(0.05)	(0.04)
Panel A2. Two-Level CPS Difference		
B v. D	0.18^{***}	0.20^{***}
	(0.05)	(0.05)
D v. F	0.18^{***}	0.16^{***}
	(0.04)	(0.04)
Panel A3. Three-Level CPS Difference		
A v. D	0.38^{***}	0.36^{***}
	(0.06)	(0.05)
C v. F	0.33^{***}	0.29^{***}
	(0.05)	(0.05)
Panel A4. Four-Level CPS Difference		
B v. F	0.31^{***}	0.31^{***}
	(0.07)	(0.06)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	-0.48^{***}	-0.48^{***}
	(0.06)	(0.06)
Panel B. The Voluntary Setting		
A v. N/A	0.29^{***}	0.28^{***}
	(0.05)	(0.05)
C v. N/A	0.13^{***}	0.10^{**}
	(0.04)	(0.04)
E v. N/A	-0.04	-0.04
	(0.05)	(0.05)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[28.08%, 58.81%]	[0.82, 64.56]
Number of Observations [Range]	[126, 132]	[126, 132]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: $^{*}10\%$, $^{**}5\%$, $^{***}1\%$.

Table 14: Average Treatment Effect on Lower Bound WTP: The South Africa Study

	Dependent variable: Lower Bound WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	1.94***	2.12***
	(0.58)	(0.52)
D v. C (Mirrored)	−2.15**	−1.88**
	(0.86)	(0.81)
E v. F	0.78	0.87
	(0.78)	(0.61)
Panel A2. Two-Level CPS Difference		
B v. D	2.85***	2.83***
	(0.65)	(0.64)
D v. F	2.23***	1.80***
	(0.57)	(0.58)
Panel A3. Three-Level CPS Difference		
A v. D	1.17**	1.58***
	(0.56)	(0.55)
C v. F	2.67***	2.20***
	(0.64)	(0.70)
Panel A4. Four-Level CPS Difference		
B v. F	2.34***	2.26***
	(0.80)	(0.69)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−3.42***	−2.98***
	(0.70)	(0.70)
Panel B. The Voluntary Setting		
A v. N/A	1.57**	1.56**
	(0.76)	(0.65)
C v. N/A	0.75	1.16**
	(0.57)	(0.47)
E v. N/A	−0.93	−0.88
	(0.61)	(0.64)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[24.44%, 46.11%]	[1.90, 19.66]
Number of Observations [Range]	[119, 126]	[119, 126]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 15: Average Treatment Effect on Lower Bound WTP: India Study

	Dependent variable: Lower Bound WTP	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	5.01**	3.16**
	(2.03)	(1.52)
D v. C (Mirrored)	0.80	0.079
	(3.01)	(2.81)
E v. F	5.34***	4.10**
	(1.89)	(1.66)
Panel A2. Two-Level CPS Difference		
B v. D	4.33	4.25**
	(2.74)	(2.11)
D v. F	2.65*	4.51**
	(1.54)	(1.75)
Panel A3. Three-Level CPS Difference		
A v. D	8.92***	7.80***
	(1.70)	(1.89)
C v. F	4.24*	3.74
	(2.15)	(2.57)
Panel A4. Four-Level CPS Difference		
B v. F	6.21***	5.34***
	(2.27)	(2.02)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−3.54	−3.49*
	(2.47)	(1.84)
Panel B. The Voluntary Setting		
A v. N/A	6.29***	5.47***
	(1.65)	(1.72)
C v. N/A	6.11*	3.50
	(3.39)	(3.09)
E v. N/A	−0.61	−0.21
	(1.60)	(1.56)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[31.40%, 58.78%]	[0, 17.06]
Number of Observations [Range]	[74, 89]	[74, 89]

Notes: Figures rounded to two decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Robust standard errors in brackets. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 16: Average Treatment Effect on Log(WTP): The U.S. Study

	Dependent variable: Log(WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	10.01%***	7.64%***
	(0.017)	(0.017)
D v. C (Mirrored)	−7.39%***	−7.05%***
	(0.021)	(0.020)
E v. F	7.35%***	7.39%***
	(0.021)	(0.016)
Panel A2. Two-Level CPS Difference		
B v. D	8.91%***	10.19%***
	(0.021)	(0.020)
D v. F	7.50%***	6.70%***
	(0.017)	(0.015)
Panel A3. Three-Level CPS Difference		
A v. D	15.51%***	14.59%***
	(0.022)	(0.018)
C v. F	14.05%***	13.04%***
	(0.018)	(0.017)
Panel A4. Four-Level CPS Difference		
B v. F	12.43%***	12.85%***
	(0.023)	(0.019)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−13.27%***	−14.05%***
	(0.023)	(0.022)
Panel B. The Voluntary Setting		
A v. N/A	12.78%***	12.24%***
	(0.018)	(0.018)
C v. N/A	4.85%***	4.53%***
	(0.015)	(0.013)
E v. N/A	0.26%	0.04%
	(0.016)	(0.016)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[25.66%, 57.53%]	[0, 66.89]
Number of Observations [Range]	[126, 132]	[126, 132]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 17: Average Treatment Effect on Log(WTP): The South Africa Study

	Dependent variable: Log(WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	5.81%***	6.63%***
	(0.017)	(0.016)
D v. C (Mirrored)	−4.57%*	−4.35%*
	(0.024)	(0.024)
E v. F	2.17%	2.22%
	(0.022)	(0.018)
Panel A2. Two-Level CPS Difference		
B v. D	8.59%***	8.40%***
	(0.018)	(0.017)
D v. F	7.06%***	6.16%***
	(0.017)	(0.017)
Panel A3. Three-Level CPS Difference		
A v. D	4.32%***	5.81%***
	(0.020)	(0.020)
C v. F	9.01%***	7.14%***
	(0.019)	(0.020)
Panel A4. Four-Level CPS Difference		
B v. F	7.35%***	7.11%***
	(0.022)	(0.019)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−8.72%***	−7.83%***
	(0.020)	(0.019)
Panel B. The Voluntary Setting		
A v. N/A	5.38%**	4.95%**
	(0.022)	(0.018)
C v. N/A	2.36%*	3.56%***
	(0.014)	(0.012)
E v. N/A	−1.53%	−1.82%
	(0.014)	(0.014)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[23.52%, 45.64%]	[1.56, 23.09]
Number of Observations [Range]	[119, 126]	[119, 126]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 18: Average Treatment Effect on Log(WTP): India Study

	Dependent variable: Log(WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
Panel A1. One-Level CPS Difference		
A v. B	6.49%**	4.83%**
	(0.025)	(0.019)
D v. C (Mirrored)	−0.03%	−0.02%
	(0.030)	(0.030)
E v. F	5.72%***	4.56%**
	(0.020)	(0.020)
Panel A2. Two-Level CPS Difference		
B v. D	7.04%**	6.28%**
	(0.030)	(0.020)
D v. F	2.72%	4.22%**
	(0.020)	(0.015)
Panel A3. Three-Level CPS Difference		
A v. D	11.16%***	9.76%***
	(0.010)	(0.020)
C v. F	5.27%**	4.57%
	(0.030)	(0.030)
Panel A4. Four-Level CPS Difference		
B v. F	7.01%***	6.10%***
	(0.020)	(0.020)
Panel A5. Five-Level CPS Difference		
F v. A (Mirrored)	−3.73%	−3.24%*
	(0.020)	(0.020)
Panel B. The Voluntary Setting		
A v. N/A	8.22%***	7.72%***
	(0.020)	(0.020)
C v. N/A	6.29%*	3.59%
	(0.030)	(0.030)
E v. N/A	−0.03%	0.05%
	(0.020)	(0.020)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[31.20%, 66.43%]	[0.01, 30.63]
Number of Observations [Range]	[74, 89]	[74, 89]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 19: Average Treatment Effect on Log(LowerBound WTP): The U.S. Study

	Dependent variable: Log(LowerBound WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
	Panel A1. One-Level CPS Difference	
A v. B	7.59%***	5.41%**
	(0.017)	(0.017)
D v. C (Mirrored)	−10.74%***	−10.50%***
	(0.024)	(0.022)
E v. F	6.56%***	6.35%***
	(0.019)	(0.013)
	Panel A2. Two-Level CPS Difference	
B v. D	5.95%***	6.86%***
	(0.019)	(0.017)
D v. F	6.43%***	5.73%***
	(0.015)	(0.013)
	Panel A3. Three-Level CPS Difference	
A v. D	12.85%***	11.80%***
	(0.021)	(0.017)
C v. F	10.93%***	9.57%***
	(0.019)	(0.018)
	Panel A4. Four-Level CPS Difference	
B v. F	10.24%***	10.12%***
	(0.025)	(0.021)
	Panel A5. Five-Level CPS Difference	
F v. A (Mirrored)	−19.34%***	−19.39%***
	(0.026)	(0.025)
Panel B. The Voluntary Setting		
A v. N/A	10.00%***	9.53%***
	(0.018)	(0.015)
C v. N/A	4.61%***	3.42%**
	(0.016)	(0.014)
E v. N/A	−1.64%	−1.66%
	(0.018)	(0.018)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[27.04%, 58.18%]	[0.82, 60.37]
Number of Observations [Range]	[126, 132]	[126, 132]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 20: Average Treatment Effect on Log(LowerBound WTP): The South Africa Study

	Dependent variable: Log(LowerBound WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
	Panel A1. One-Level CPS Difference	
A v. B	5.47%***	6.08%***
	(0.017)	(0.015)
D v. C (Mirrored)	−7.20%***	−6.37%**
	(0.027)	(0.025)
E v. F	2.00%	2.44%
	(0.024)	(0.019)
	Panel A2. Two-Level CPS Difference	
B v. D	7.87%***	7.82%***
	(0.019)	(0.018)
D v. F	6.39%***	5.11%***
	(0.017)	(0.017)
	Panel A3. Three-Level CPS Difference	
A v. D	3.38%**	4.59%***
	(0.020)	(0.020)
C v. F	7.27%***	5.97%***
	(0.019)	(0.021)
	Panel A4. Four-Level CPS Difference	
B v. F	6.29%***	6.18%***
	(0.023)	(0.020)
	Panel A5. Five-Level CPS Difference	
F v. A (Mirrored)	−11.12%***	−9.73%***
	(0.023)	(0.023)
Panel B. The Voluntary Setting		
A v. N/A	4.14%*	4.35%**
	(0.023)	(0.015)
C v. N/A	2.32%	3.49%**
	(0.017)	(0.014)
E v. N/A	−3.04%	−2.82%
	(0.020)	(0.021)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[24.02%, 44.13%]	[1.70, 18.13]
Number of Observations [Range]	[119, 126]	[119, 126]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 21: Average Treatment Effect on Log(LowerBound WTP): India Study

	Dependent variable: Log(LowerBound WTP)	
	OLS	LASSO
	(1)	(2)
Panel A. The Mandatory Setting		
	Panel A1. One-Level CPS Difference	
A v. B	4.98%**	3.20%**
	(0.021)	(0.017)
D v. C (Mirrored)	0.48%	−0.42%
	(0.032)	(0.030)
E v. F	5.21%***	4.03%**
	(0.020)	(0.020)
	Panel A2. Two-Level CPS Difference	
B v. D	3.94%	3.97%*
	(0.030)	(0.020)
D v. F	2.74%	4.49%**
	(0.020)	(0.017)
	Panel A3. Three-Level CPS Difference	
A v. D	9.02%***	7.73%***
	(0.020)	(0.020)
C v. F	3.74%	3.05%
	(0.021)	(0.021)
	Panel A4. Four-Level CPS Difference	
B v. F	6.11%**	5.21%**
	(0.020)	(0.020)
	Panel A5. Five-Level CPS Difference	
F v. A (Mirrored)	−4.32%	−4.16%**
	(0.030)	(0.020)
Panel B. The Voluntary Setting		
A v. N/A	6.43%***	5.49%***
	(0.020)	(0.020)
C v. N/A	5.41%	2.80%
	(0.040)	(0.014)
E v. N/A	−0.10%	−0.01%
	(0.020)	(0.020)
Demographic Controls	Yes	Yes
Adjusted R^2 / Wald χ^2 Test [Range]	[31.10%, 57.63%]	[0, 16.84]
Number of Observations [Range]	[74, 89]	[74, 89]

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Column (1) reports the OLS regression results, while Column (2) reports the LASSO regression results. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, ***1%.

Table 22: Average Treatment Effect on Log(LowerBound WTP): All Studies

	Dependent variable: Log(LowerBound WTP)					
	OLS - U.S.	OLS - S.Africa	OLS - India	LASSO - U.S.	LASSO - S.Africa	LASSO - India
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. The Mandatory Setting						
			Panel A1. One-Level CPS Difference			
A v. B	7.50%***	5.47%***	4.98%**	5.41%**	6.08%***	3.20%**
	(0.017)	(0.017)	(0.021)	(0.017)	(0.015)	(0.017)
D v. C (Mirrored)	−10.74%***	−7.20%***	0.48%	−10.50%***	−6.37%**	−0.42%
	(0.024)	(0.027)	(0.032)	(0.022)	(0.025)	(0.030)
E v. F	6.56%***	2.00%	5.21%***	6.35%***	2.44%	4.03%**
	(0.019)	(0.024)	(0.020)	(0.013)	(0.019)	(0.020)
			Panel A2. Two-Level CPS Difference			
B v. D	5.95%***	7.87%***	3.94%	6.86%***	7.82%***	3.97%*
	(0.019)	(0.019)	(0.030)	(0.017)	(0.018)	(0.020)
D v. F	6.43%***	7.50%***	2.74%	5.73%***	5.11%***	4.49%**
	(0.015)	(0.017)	(0.020)	(0.013)	(0.017)	(0.017)
			Panel A3. Three-Level CPS Difference			
A v. D	12.85%***	3.38%**	9.02%***	11.80%***	4.59%***	7.73%***
	(0.021)	(0.020)	(0.020)	(0.017)	(0.020)	(0.020)
C v. F	10.93%***	7.27%***	3.74%	9.57%***	5.97%***	3.05%
	(0.019)	(0.019)	(0.021)	(0.018)	(0.021)	(0.021)
			Panel A4. Four-Level CPS Difference			
B v. F	10.24%***	6.29%***	6.11%**	10.12%***	6.18%***	5.21%**
	(0.025)	(0.023)	(0.020)	(0.021)	(0.020)	(0.020)
			Panel A5. Five-Level CPS Difference			
F v. A (Mirrored)	−19.34%***	−11.12%***	−4.32%	−19.39%***	−9.73%***	−4.16%**
	(0.026)	(0.023)	(0.030)	(0.025)	(0.023)	(0.020)
Panel B. The Voluntary Setting						
A v. N/A	10.00%***	4.14%*	6.43%***	9.53%***	4.35%**	5.49%***
	(0.018)	(0.023)	(0.020)	(0.015)	(0.015)	(0.020)
C v. N/A	4.61%***	2.32%	5.41%	3.42%**	3.49%**	2.80%
	(0.016)	(0.017)	(0.040)	(0.014)	(0.014)	(0.014)
E v. N/A	−1.64%	−3.04%	−0.10%	−1.66%	−2.82%	−0.01%
	(0.018)	(0.020)	(0.020)	(0.018)	(0.021)	(0.020)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2 [Range]		[27.04%, 58.18%]		Wald χ^2 Test [Range]	[0.82, 60.37]	
Number of Observations [Range]		[74, 132]			[74, 132]	

Notes: Coefficients rounded to two and robust standard errors to three decimal places wherever applicable. Columns (1)-(3) report the OLS regression results for the U.S., South Africa and India studies respectively, while Columns (4)-(6) report the LASSO regression results in the same order. Coefficients that are significantly different from zero are