

Environmental costs in second-hand clothing purchase:

A voucher choice experiment

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Abstract

The economic and psychological literature on second-hand consumption has identified specific drivers and barriers affecting consumer adoption. A key barrier is consumers' limited awareness of the environmental costs associated with producing new goods. While awareness is typically raised through labels reporting CO2 emissions, consumers report difficulty interpreting such information, prompting consideration of alternative metrics that may be more tangible and relatable. In this paper, we investigate how the framing of environmental production costs—expressed in terms of CO2 emissions, electric energy consumption, or water usage—affects intention to engage in second-hand clothing consumption. We measure behavioral responses using both stated changes in purchasing intention and incentivized choices via multiple price lists, eliciting willingness to accept (WTA) and willingness to pay (WTP) for vouchers redeemable at first-hand versus second-hand clothing stores. Our experimental design was administered to a large, nationally representative sample of the Italian population ($N = 10,496$). Our findings reveal several key patterns. First, information provision is effective in promoting second-hand adoption across all environmental metrics, but water-based information generates the strongest effects on both stated intentions (43.03% report increased likelihood) and voucher choices, followed by energy (35.27%) and CO2 (30.73%). Second, treatment effects exhibit meaningful heterogeneity: individuals who initially underestimate environmental benefits respond most strongly to information provision, while those with accurate prior beliefs show greater resistance to updating. Third, pro-environmental behavior amplifies responsiveness across all treatments, while need for uniqueness operates as a significant barrier, with high-NFU individuals requiring approximately €2 additional compensation

to switch to second-hand options. Fourth, while the type of environmental metric matters substantially, local environmental conditions—including water scarcity, energy poverty, and air quality—do not systematically moderate treatment effects, suggesting that information operates through relatively context-independent cognitive pathways. Finally, we document important methodological insights: imposing second-hand as a status quo in WTP elicitation creates an artificial reference point that attenuates treatment effects, and store proximity influences baseline preferences but does not moderate information provision effects. These findings suggest that shifting from CO₂-based to water- or energy-based environmental messaging could enhance the effectiveness of sustainability communication, though reaching consumers beyond those already pro-environmentally inclined remains a challenge.

Keywords: Second-hand consumption · Information provision · Environmental cost · Voucher choice · Framed field experiment

JEL Codes: D12 · D83 · D91 · Q50

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1 Introduction

The fashion industry is one of the biggest markets worldwide. Two independent research reports by Euromonitor and McKinsey estimate that in 2021 it generated globally between \$1.7 trillion and \$2.5 trillion in revenues.¹ This economic turnover comes however with massive production costs, especially for the environment. Niinimäki et al. (2020) estimate that the fashion industry is responsible for 8-10% of global CO₂ emissions (equivalent to 4-5 billion tonnes annually), and is also a major water user (79 trillion liters per year) and polluter (around 20% of industrial water); besides, 92 million tonnes of textile waste each year come from production residues and unsold apparel.

An effective way to ameliorate the environmental harm of fashion is to resort to second-hand clothing acquisition. It is estimated that the purchase of 100 second-hand clothes can be associated with a reduction in the purchase of 60-85 new clothes, and that such substitution reduces by 45% the toxic emissions caused by polyester cotton clothes and by 14% the impact on global warming of producing cotton T-shirts (Farrant et al., 2010). Fortunately, the market for second-hand apparel is substantial and continuing to grow. In 2023, it was estimated to be worth \$197 billion globally, with \$43 billion in the United States alone.² Even though the adoption of second-hand clothing

¹ <https://fashionunited.com/statistics/global-fashion-industry-statistics>

² <https://www.retaildive.com/news/resale-secondhand-apparel-market-growth-projections/711476/>

seems to be particularly widespread in the US, it is less so in other parts of the world. For instance, surveys among Chinese consumers reveal that around 9% had ever purchased second-hand clothing (Xu et al., 2014) and that only 10% of Chinese consumers were willing to purchase it in the future (Liang and Xu, 2018). The rate of adoption for second-hand clothes is generally low, also in the European Union: only 22% of post-consumer textile waste is collected separately for recycling or reuse, while the rest is often burned or deposited in landfills (European Commission Press Release, 2023). It is thus relevant from a policymaking viewpoint to investigate what factors underlie demand for second-hand clothing, so that barriers to it can be adequately addressed.

While the literature has identified most of these barriers (Hur, 2020; Silva et al., 2021; Schiaroli et al., 2024), it has devoted little attention to consumers’ awareness of the environmental costs associated with the production of new clothes and how much these can be reduced when switching to second-hand ones. It is known that consumers’ awareness fosters intention to engage in second-hand fashion (Borusiak et al., 2020; Pretner et al., 2021; Negash and Akhbar, 2024). Still, awareness of the sustainability impact of clothing remains generally low among consumers (Goworek et al., 2012; Harris et al., 2016). This calls for the need to educate and inform consumers (Jimenez-Fernandez et al., 2023).

Contributions in the literature have mostly focused on the provision of information on CO₂ emissions, typically through carbon labels (e.g., Guenther et al., 2012; Hartikainen et al., 2014). These contributions highlight that consumers report difficulties in understanding this information (Guenther et al., 2012; Hartikainen et al., 2014) and thus that it may be necessary to study how it can be expressed in metrics that can be more easily digested (Larrick et al., 2015; Pace et al., 2025). Among others, Camilleri et al. (2019) convert CO₂ emissions into equivalent light-bulb energy usage and show that it is more effective in stimulating sustainable behavior as it is more familiar to consumers. Relatedly, Attari (2014) shows that, while people underestimate both water and energy use for most appliances and situations, their perception of water usage is higher than that of energy. Taken together, these findings suggest that information on CO₂ can be made more relatable and comprehensible by converting it into equivalent metrics for energy or water.

In this paper, we study how the respondent’s intention to purchase second-hand clothing and their willingness to pay/accept for it varies depending on the metric used to express the environmental net cost of consuming an extra item of first-hand clothing. We set up a framed field experiment involving two tasks. In the first task, we ask respondents to guess the environmental benefit of not producing a new piece of clothing, in a quiz fashion, and then we inform them about the correct figure. As anticipated, depending on treatment, the cost is expressed as kilograms of CO₂ emitted (Kg CO₂e), kilowatt-hour of electric energy (kWh), or liters of water (ℓ); we provide an anchor to ensure the measurement can be more relatable for the respondents. Respondents in the control skip this task. We also ask them whether, based on the information received, they would be less, equally, or more likely to purchase second-hand in their daily lives. The second task involves a voucher choice: we ask participants to answer a multiple price list involving pairs of vouchers of different amounts, where one can be redeemed in a traditional clothing retailer and the other in a second-hand shop, and to choose a voucher from each pair. This last task allows us to elicit one’s willingness to accept (WTA) to switch consumption from first-hand to second-hand clothing and, likewise, one’s willingness to pay (WTP) to do the opposite.

We find that, regardless of the environmental metric used, information provision increases stated

intention to adopt second-hand clothing and affects voucher choices in the expected direction. More central to our research question, and consistent with our hypotheses, providing information in terms of water or energy usage generates stronger effects than CO₂-based information across multiple outcome measures. Water-based information yields the largest treatment effects, with 43.03% of respondents reporting increased intention to purchase second-hand (compared to 35.27% for energy and 30.73% for CO₂), and reducing willingness to accept compensation to forgo second-hand options by approximately €4 relative to the control group. Energy information shows intermediate effectiveness, while CO₂ emerges as the weakest treatment despite being the most commonly used metric in sustainability communication.

We also document important heterogeneity in treatment effects. Information provision is most effective among individuals who initially underestimate environmental benefits—a group comprising approximately 62% of our sample—while those with accurate prior beliefs show substantial resistance to updating their stated intentions. Pro-environmental behavior strongly amplifies responsiveness across all treatments and outcomes, while need for uniqueness operates as a significant barrier, with high-NFU individuals requiring over €2 in additional compensation to switch from first-hand to second-hand vouchers. Interestingly, respondents exhibit consistent preferences across different status quo framings at the point of equal valuation, suggesting that our results are not driven by simple framing effects or endowment biases, though the magnitude of WTA and WTP varies meaningfully with which option is designated as the baseline.

Contrary to our expectations, we find limited evidence that local environmental conditions moderate treatment effects. Despite linking respondents to administrative data on water scarcity, energy poverty, and air quality at provincial or regional levels, treatment effectiveness does not vary systematically with locally-salient environmental challenges. Similarly, store proximity influences baseline voucher preferences but does not moderate information provision effects, indicating that treatment operates primarily through belief updating and preference shifts rather than through perceived feasibility of action. These null moderations suggest that environmental information may influence behavior through relatively context-independent cognitive processes—such as surprise at the magnitude of environmental impacts—rather than through resonance with lived environmental problems.

The paper proceeds as follows: [Section 2](#) reviews the relevant literature on second-hand consumption, environmental information provision, and belief updating. [Section 3](#) details our experimental procedure, sampling strategy, and measurement approach. [Section 4](#) presents our main findings on stated intentions and voucher choices. [Section 5](#) examines heterogeneity by prior beliefs and individual characteristics, and explores potential moderators including geographic location, store proximity, and local environmental conditions. [Section 6](#) discusses theoretical contributions, policy implications, limitations, and directions for future research.

2 Literature Review

This paper differentiates itself from other contributions on at least three fronts. First, we address the niche focusing on conscious consumption choices, particularly in the textile industry, by adopting a voucher choice task. We are aware of just a few papers relying on voucher choices, such as

in meat consumption (Pizzo et al., 2024), yet none do it for clothing.

Second, rather than simply providing information about environmental costs, we test an information provision task in the form of a quiz, intending to enhance the respondent’s curiosity, engagement, and deliberation. This could be a fine addition to the repertoire of tasks adopted in information provision experiments.

Finally, by varying the types of environmental costs presented, we contribute to the discussion on how to design effective information campaigns and labels that encourage consumers to adopt greener consumption patterns by raising awareness of sustainability issues.

This paper starts precisely from the evidence that consumers who are aware of the environmental consequences of their actions are more likely to engage in sustainable behavior. For example, awareness of how second-hand consumption can slow down the depletion of natural resources fosters intention to purchase second-hand goods (Harris et al., 2016). This also holds for clothing demand (Papadopoulou et al., 2022; Jimenez-Fernandez et al., 2023; Negash and Akhbar, 2024), in the sense that respondents who report higher environmental awareness are also more likely to engage in second-hand fashion (Negash and Akhbar, 2024).

Nonetheless, research also reveals limited awareness of the sustainability impact of clothing among consumers (Goworek et al., 2012; Harris et al., 2016). This means that consumers may need to be educated or informed about the environmental impact of sustainable products for them to appreciate their benefits and exhibit a stronger intention to purchase them (Jimenez-Fernandez et al., 2023; Pretner et al., 2021). In effect, providing information on environmental benefits increases attitudes towards products with strong positive environmental attributes (Cerri et al., 2018). When these benefits are quantified, consumers state more positive attitudes towards sustainable products (Borin et al., 2011) and higher willingness to pay for them (Pretner et al., 2021).

For example, Pretner et al. (2021) conducted a survey experiment involving around 1,000 consumers from Italy (249) and the US (742). Participants were offered the purchase of a second-hand hoodie. Specifically, they were asked how much they would pay for a second-hand hoodie that costs \$40 new (€40 in the Italian sample). In one treatment, participants were told that “choosing the second-hand hoodie prevents 20 pounds of carbon dioxide (CO₂) from being released into the environment”. In another treatment, they were told that this information came from an independent certifier. The experiment shows that, as compared to the baseline average WTP found in the Italian (€18) and US (\$11.90) samples, WTP increases when information on CO₂ emission is provided (€19.60 and \$15.30) and increases further when the information is said to have been verified by an independent certifier (€21.20 and \$17.20).

While providing information on CO₂ emissions may increase awareness and intention to engage in second-hand fashion, such an increase appears to be modest (e.g., €19.60 vs. €18). One reason might be that consumers show difficulty in understanding this information (Dreist et al., 2024; Pace et al., 2025). For instance, many consumers report difficulties when trying to read carbon labels (Guenther et al., 2012; Hartikainen et al., 2014). The lack of clarity may hinder the effectiveness of the information received. A way that has been suggested to enhance information clarity is to convert metrics that are found to be confusing into more comprehensible and relatable ones (Camilleri and Larrick, 2014; Larrick et al., 2015; Ungemach et al., 2018). In effect, contributions such as Camilleri et al. (2019) and Lohmann et al. (2022) show that carbon labels work in directing

food choices toward sustainable options, but these labels complement the information on CO2 with a traffic light system (Camilleri et al., 2019; Lohmann et al., 2022) or equivalence with other, more-relatable measures, such minutes of usage of a light bulb (Camilleri et al., 2019). In the search for alternative metrics to convey environmental impact, it is interesting that the finding by Attari (2014) concerning consumers’ perception of water usage. Her survey finds that US consumers are more accurate in estimating water consumption associated with various appliances and situations than electric energy.

Building on these findings, in our experiment, we provide information on the environmental damage that can be avoided by purchasing a second-hand piece of clothing rather than a new one by expressing the damage avoided in terms of a different environmental externality (CO2, energy, or water) and converting the metric for each externality into a familiar, but closely related, equivalent metric. Specifically, we link the magnitude of the damage to habitual or familiar actions that make the perception of the damage more concrete, to enhance information clarity.

Our approach reflects two key considerations regarding information design. First, while CO2 emissions are frequently used as the primary or sole indicator of environmental impact in sustainability communication, clothing production generates multiple distinct environmental externalities—as discussed in the Section 1—including energy consumption and water usage. Over-reliance on CO2 as a catch-all environmental metric may conflate conceptually distinct impacts and create confusion among respondents about what specific environmental harm they are evaluating. Second, the relationship between CO2 emissions and energy consumption, while related, is not identical: not all CO2 emissions stem from energy use (e.g., direct emissions from industrial processes), and not all energy production generates CO2 (e.g., renewable sources). Using the same conversion metric (light-bulb usage) for both would further blur this distinction and risk compounding respondent confusion.

For these reasons, in the case of CO2, we restated kilograms of CO2 emitted when producing a new piece of clothing as those emitted by an equivalent number of kilometers driven by a medium-sized gasoline car. We chose not to adopt the light-bulb conversion used in Camilleri et al. (2019), as we believe driving is more closely related to pollution production in respondents’ perception (see also Pace et al., 2025 for a similar conversion rationale). To convert the metrics used when giving information on energy and water saving, we respectively adopted the equivalence of kWh to hours of usage of a 100-watt light-bulb and liters to minutes of open kitchen sink, since both are actions that individuals can easily relate to and experience in their daily lives (details of the implementation are discussed in Section 3.2).

3 Experimental Design

The information provision and voucher choice tasks were embedded in a larger survey. The study received a Certificate of Good Standing by the Board for Ethical Questions in Science of the University of Innsbruck and was pre-registered on Open Science Framework (OSF).³ The full text of the belief elicitation and voucher choice tasks is available in Section A.1 and Section A.2, respectively, and together with the full texts of the other study tasks in the public Open Science

³ OSF repository can be accessed here: osf.io/wzcqh

Framework (OSF) repository for the project.

3.1 Procedure

The survey was implemented and administered by the survey company Scenari S.r.l. from October to December 2024 to members of their panel of respondents, using the computer-assisted web interviewing (CAWI) methodology. Respondents were recruited using a stratified sampling approach to ensure that the sample ($N = 10,496$) was representative at the province level with respect to major sociodemographic variables. In our sample, representativeness holds for age ($m = 49.67$, $sd = 15.62$), gender (percentage of men: 48.83%; percentage of women: 51.17%), education (percentage of people holding a university degree or above: 36.81%) and residence area (Northwest: 26.93%; Northeast: 19.76%; Center: 19.63%; South: 22.77%; Islands: 10.91%); see [Table 17](#) in [Appendix B](#) for the full descriptive statistics.

During the completion of the survey, respondents were unaware of the upcoming questions and could not go back to amend their answers. As a reward for completion, respondents were paid €4. Additionally, participants could win a voucher of a certain amount (€0–20) based on their choices in the voucher task and a random draw (see details in [Section 3.3](#)).

3.2 Information provision task

Information provision experiments are designed to study the impact of information on people’s beliefs and on the behavioral responses that may be determined by the updating of those beliefs. In a typical information provision experiment, it is customary to elicit the participant’s prior belief, so that one can later analyze whether belief updating took place and in which direction, and also assess whether treatment effects are larger for respondents for whom the distance between the prior belief and the information received was larger ([Haaland et al., 2023](#)). Accordingly, respondents in our experiment were first presented with a task designed to elicit their beliefs about the environmental benefit associated with purchasing a second-hand piece of clothing, and then were informed of the benefit as estimated in the technical literature.

The information provision task involved guessing how much it is possible to save when adopting second-hand clothing in terms of either CO₂, energy, or water. To aid respondents in this task, we supplemented the information with equivalences using measures that we felt could be more familiar and relatable to respondents. Specifically, to put kg CO₂e in perspective, we told respondents that 1 kg CO₂e equates to 6 kilometers driven by a medium-sized gasoline car. In the energy treatment, respondents were informed that 1 kWh equates to 10 hours of use of a 100-watt bulb. In the water treatment, they learned that 10 liters equate to 2 minutes of an open kitchen sink. After eliciting their belief, respondents were shown the true values in a separate screen: 4 kg CO₂e, 17 kWh, and 340 liters, respectively. Following the information provision, respondents were asked to state whether they were now less, equally, or more inclined to buy second-hand clothing in their daily life. It has to be noted that in the task, we did not elicit the respondent’s posterior or updated beliefs. This is because the updating boiled down to a simple figure, and we conjectured that the respondents would have been able to input the exact true figure because they had just seen it. Besides, a potential downside of measuring both prior and posterior beliefs is that the latter may

be biased by consistency or experimenter demand effects (Haaland et al., 2023).

The values and equivalences adopted in the task were based on estimates from various scientific and institutional sources.⁴ An example of the task for the CO₂e treatment, translated from Italian, is provided in Figure 1 (see Section A.1 for the task in the other two treatments).

Figure 1: Illustration of the belief elicitation task

Before answering the next question, we want to inform you of the value of one kilogram of carbon dioxide emitted (CO₂e).

1 kg CO₂e = 6 km driven by a medium-sized gasoline car

When you buy a second-hand piece of clothing, do you know how much CO₂ emissions you can avoid by not producing a new one?
 About: ____ kg CO₂e

The exact estimate is about 4 kg CO₂e, or about 24 km traveled by a medium-sized gasoline car.

Based on this information, are you more or less inclined to buy second-hand in your daily life?

☐ I am more inclined
☐ I am equally inclined
☐ I am less inclined

3.3 Voucher choice task

After the information provision task, respondents are presented with a multiple price list (MPL) in which they can express their preferences among pairs of vouchers of different amounts to be redeemed in either a first-hand or a second-hand store. Figure 2 offers an illustration of how the MPL has been presented to participants (see Section A.2 for the experimental screens of the voucher task).

We have decided to present two different versions of this MPL, reflecting different reference points for respondents. In the former version, with Voucher A being the first-hand voucher, the task elicits the respondent’s willingness to accept (hereinafter WTA) compensation to switch from a first-hand voucher to a second-hand voucher. In the latter version, with Voucher A being the second-hand voucher, the task elicits the respondent’s willingness to pay (hereinafter WTP) to upgrade from a second-hand voucher to a first-hand voucher.⁵ The conceptual distinction between these two measures stems from the nature of the status quo: WTA captures the compensation

⁴ We based the equivalence 1 kg CO₂e = 6 km traveled by an average car on that provided by a 2023 UK governmental report (<https://www.gov.uk/government/publications/greenhouse-gas-reporting-conversion-factors-2023>). This estimate is roughly consistent with figures provided by other sources, such as CO₂everything (around 5 km; <https://www.co2everything.com/co2e-of/cotton>). For the equivalences related to energy and water, we referred to those provided by WRECC.com (<https://www.wrecc.com/what-uses-watts-in-your-home/>) and the UK Government (<https://assets.publishing.service.gov.uk/media/5a75bd8940f0b67b3d5c8da0/gbs-taps-automatic-sprays-showers-urinal-2015.pdf>), respectively. For the amount of resources (CO₂, electricity, and water) saved when adopting second-hand clothing, we referred to estimates indicated in a report by ThredUp (https://cf-assets-tup.thredup.com/resale_report/2023/thredUP_2023_Resale_Report_FINAL.pdf).

⁵ For a more in-depth discussion of the differences between WTA and WTP, see Bateman et al. (2002).

required when moving from a default option perceived as superior (first-hand) to an alternative perceived as inferior (second-hand), while WTP captures the premium one is willing to pay for the reverse transition. This framing allows us to present our results in a clearer and more interpretable manner, as each measure naturally aligns with the perceived direction of the trade-off from the respondent’s perspective. The first version of the task is both the most intuitive for the interpretation of results, as it sets the first-hand store as the respondents’ status quo, and the most widespread setting in similar experiments (Allcott and Taubinsky, 2015). We chose to administer also the version with second-hand as status quo to provide a more complete estimation of the treatment effects and potentially provide methodological insights on this type of policy evaluation (e.g., consistency in treatment effects across the two MPLs or lack thereof, and the relative magnitude of WTA versus WTP responses).

The selection of the two stores was made with considerable care to ensure comparability and ecological validity. We chose stores with comparable price levels and similar territorial diffusion across Italy, ensuring that both are widely present and accessible to respondents. This comparability is particularly important in our setting, given that participants are geographically distributed across the entire country. The role of store proximity and accessibility in moderating treatment effects will be discussed in the Section 5.

Figure 2: Illustration of the MPL for the voucher task

Voucher A	Voucher B	Your choice	
€ 10	€ 0	☐ Voucher A	☐ Voucher B
€ 10	€ 5	☐ Voucher A	☐ Voucher B
€ 10	€ 10	☐ Voucher A	☐ Voucher B
€ 10	€ 15	☐ Voucher A	☐ Voucher B
€ 10	€ 20	☐ Voucher A	☐ Voucher B

In a different screen, participants can choose to opt out of the lottery. We included this possibility to check whether the information on the environmental cost might encourage individuals to reduce their consumption *tout court* rather than favoring second-hand.

3.4 Registered hypotheses

Our paper aims to answer the following research questions: *i)* Does information provision on environmental costs foster intention to buy second-hand goods over new ones? *ii)* Does information provision on environmental costs foster willingness to accept for second-hand over first-hand, and willingness to pay for first-hand over second-hand? *iii)* Does each of the environmental costs referenced have a different impact on stated intention and WTA/WTP as compared to the others?

Following our research questions, we first hypothesize that, regardless of the externality under consideration (CO2, energy, water), the impact of information provision on the change in the stated relative intention to purchase second-hand clothing is higher the higher the difference between the respondent’s initial belief about the environmental cost, measured in the quiz, and the revealed true value. Accordingly, our hypothesis is:

H.1 The further from the true value is the respondent’s belief...

H.1.a in positive (overestimation of the true value), the lower the intention to purchase second-hand clothing.

H.1.b in negative (underestimation of the true value), the higher the intention to purchase second-hand clothing.

Secondly, we hypothesize that the effectiveness of information provision on the intention to buy second-hand clothing is higher when it refers to water rather than energy and to energy rather than CO2 emissions, since the three environmental externalities differ in the degree to which the respondent can “relate” to them.

H.2 The frequency of respondents declaring a higher intention to purchase second-hand goods is...

H.2.a higher in Water than Energy treatments

H.2.b higher in Energy than CO2 treatments

H.2.c higher in Water than CO2 treatments

We also hypothesize that WTA (WTP) differentials between first-hand (second-hand) and second-hand (first-hand) clothing will be sensitive to the type of environmental externality and the distance between the respondent’s guess in the quiz and the true value revealed with the information provision. Specifically, based on our hypotheses *H.1* and *H.2*, we hypothesize the following:

H.3 We expect the WTA differential (with first-hand as baseline) to be lower (respondents accept a lower value of the voucher to switch to second-hand)...

H.3.a the more relatable is the environmental cost.

H.3.b the higher the underestimation of the true value.

H.4 We expect the WTP differential (with second-hand as baseline) to be higher (respondents accept a higher value of the voucher to switch to first-hand)...

H.4.a the more relatable is the environmental cost.

H.4.b the higher the underestimation of the true value.

4 Main Results

4.1 Intention Analysis

A substantial proportion of respondents (53.84%) declared that they would be equally likely as before receiving the information to adopt second-hand clothing in the future, followed by a proportion of participants stating they were more likely to do so (36.34%). Only a minority (9.82%) stated a lower propensity after the belief updating.

To test Hypothesis *H.1*, we coded respondents’ initial beliefs (guesses) as correct, overestimated, or underestimated by creating a tolerance interval of $\pm 15\%$ around the true value. This classification allowed us to identify which respondents had overestimated and which had underestimated

the true environmental benefit of purchasing second-hand clothing. We then employed chi-squared tests to examine whether the distribution of stated intentions (i.e., equally likely, more likely, or less likely to adopt second-hand) differed significantly between those who overestimated versus those who did not, and between those who underestimated versus those who did not. The results of these tests are reported in Table 1; furthermore, Figure 15 in Appendix B shows this trend.

Table 1: Intention Distribution by Belief Accuracy

Intention	Belief Accuracy		
	Underestimate	Accurate	Overestimate
Less likely	551 (11.26%)	62 (6.52%)	159 (7.89%)
Equally likely	2,441 (49.88%)	624 (65.62%)	1,167 (57.92%)
More likely	1,902 (38.86%)	265 (27.87%)	689 (34.19%)
Total	4,894 (100%)	951 (100%)	2,015 (100%)

Note: Cross-tabulation of stated intention changes by belief accuracy category. Belief accuracy determined using $\pm 15\%$ tolerance interval around true values: Underestimate = belief $<$ (true value $- 0.15 \times$ true value); Accurate = belief within $\pm 15\%$ of true value; Overestimate = belief $>$ (true value $+ 0.15 \times$ true value). Percentages represent row proportions within each belief accuracy category.

The findings reveal meaningful heterogeneity in how respondents react to information depending on their prior beliefs (Pearson $\chi^2 = 103.41$, $p < 0.001$). Information provision appears most effective among those who initially underestimated the environmental benefit: 38.86% of underestimators reported being more inclined to adopt second-hand clothing, compared to 34.19% of overestimators. Conversely, respondents whose initial beliefs were accurate proved to be the most resistant to changing their stated intentions, with 65.62% reporting no change in their propensity to purchase second-hand. Notably, relatively few respondents across all groups became less inclined to adopt second-hand following the information provision (ranging from 6% to 11%), suggesting that providing environmental information rarely produces counterproductive effects. From a policy perspective, these results highlight that underestimators represent both the largest segment of our sample (62%) and the group most responsive to information provision, suggesting considerable potential for interventions targeting this population.

Result 1 (H.1): *Information provision has heterogeneous effects depending on prior beliefs, with underestimators showing significantly greater responsiveness than overestimators or those with accurate initial beliefs.*

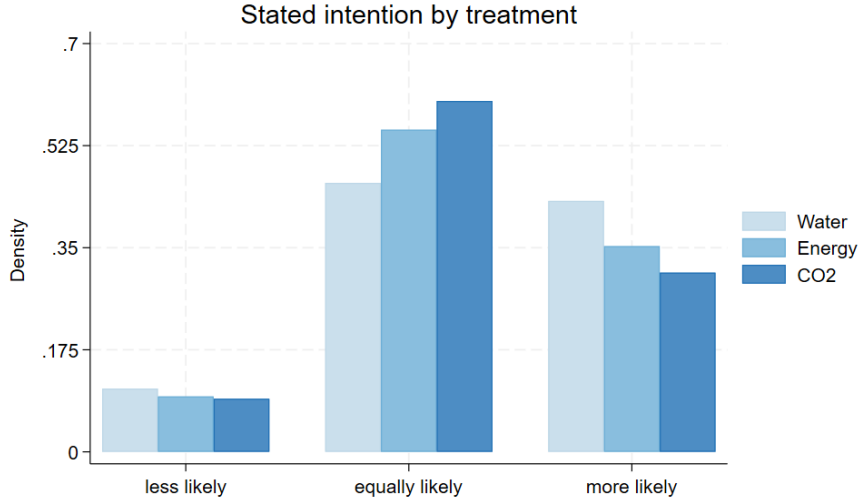
Moving on to testing Hypothesis H.2, we examine whether the type of environmental externality presented influences respondents’ stated intentions to adopt second-hand clothing. Our hypothesis predicted that information framed in terms of water and energy would be more effective than CO2-based information in increasing adoption intentions. As illustrated in Figure 3, the patterns identified earlier exhibit meaningful variation across the three treatment conditions, consistent with our directional prediction. In the CO2 treatment, 60.16% of respondents reported being equally likely, 30.73% more likely, and 9.11% less likely to adopt second-hand clothing following the information provision. In the energy treatment, the corresponding figures were 55.23%, 35.27%, and 9.50%, respectively. The water treatment yielded the most favorable distribution, with 46.12% remaining equally likely, 43.03% becoming more likely, and 10.85% becoming less likely to purchase

second-hand items.

A chi-squared test confirms that these differences across treatments are statistically significant (Pearson $\chi^2 = 109.56$, $p < 0.001$; see Table 18 in the Appendix Appendix B for detailed results). The distribution reveals a clear gradient in effectiveness, with water-related information generating the most positive response, followed by energy, and then CO2, precisely as hypothesized. While the proportion of respondents becoming less inclined remains fairly stable across treatments (around 9-11%), the key difference lies in the shift from “equally likely” to “more likely” categories, with water information proving most effective in moving respondents toward increased adoption intentions.

Result 2 (H.2): *The type of environmental externality communicated significantly affects stated behavioral intentions. Water-based information is most effective in increasing adoption intentions (43.03% more likely), followed by energy (35.27%), and CO2 (30.73%), supporting the hypothesis that more tangible and relatable environmental metrics generate stronger behavioral responses.*

Figure 3: Stated intention to adopt second-hand clothing across treatments



Note: Histogram showing the distribution of stated intention changes (less likely, equally likely, more likely to adopt second-hand) across treatment conditions (Water, Energy, CO2). Density represents the proportion of respondents in each category within each treatment group.

To corroborate our initial results, we estimate a series of ordered logistic regressions examining how stated intention to purchase second-hand clothing varies with both the type of environmental information provided and the magnitude of belief updating required. Table 2 reports the main specifications of these regressions, while the complete models including sociodemographic controls are presented in Table 25 the Appendix Appendix B.

Model (1) examines the role of belief accuracy across all treatments. The negative and significant coefficient on Delta Belief indicates that stated intention to increase second-hand purchasing decreases as the distance between the respondent’s prior belief and the true value increases, regardless of which environmental resource was targeted. This result provides additional support for Result 1, confirming that respondents with more accurate initial beliefs are less responsive to information provision.

Model (2) incorporates treatment indicators to test whether the type of environmental externality affects stated intentions, using CO2 as the baseline category. Consistent with Result 2, both Water and Energy treatments generate significantly more positive responses than the CO2 treatment. The Water treatment exhibits the strongest effect, followed by the Energy treatment. These findings reinforce our earlier descriptive evidence that more tangible and relatable environmental metrics are more effective in shifting behavioral intentions.

Table 2: Ordered Logistic Regression on Change in Intention to Purchase SH

	(1)	(2)
Delta Belief	-0.004** (0.002)	-0.001 (0.002)
Water		0.371*** (0.050)
Energy		0.135** (0.059)
Cut 1	-2.226*** (0.032)	-2.065*** (0.051)
Cut 2	0.553*** (0.025)	0.726*** (0.041)
Observations	7860	7860

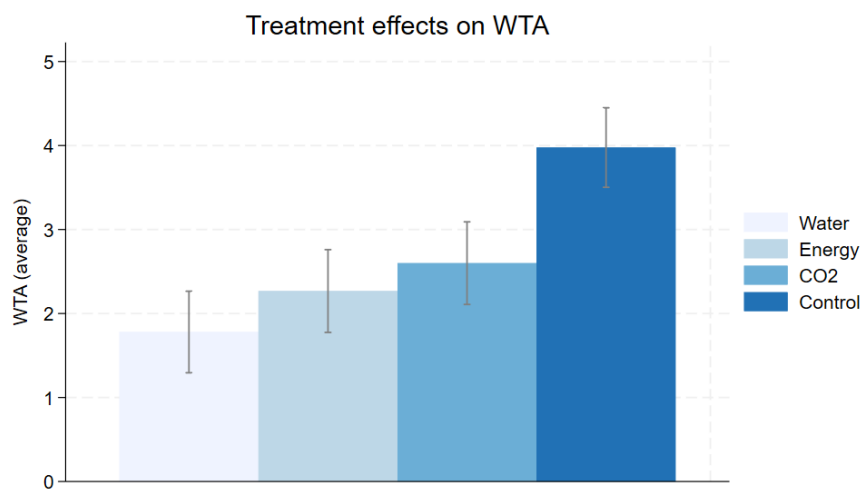
Note: Ordered logit regression estimation with bootstrap standard errors. Dependent variable. *Change in intention to purchase more SH clothes*: categorical variables ranging from -1 to $+1$ (3 categories). Regressors. *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value over the true value. *Water* dummy variables equal 1 if the treatment is Water, 0 otherwise. *Energy* dummy variables equal 1 if the treatment is Energy, 0 otherwise. Baseline *CO2*. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Willingness to Accept and Willingness to Pay Analysis

We now turn to testing Hypotheses *H.3* and *H.4*, which concern how respondents' willingness to accept (WTA) compensation to switch from first-hand to second-hand vouchers, and their willingness to pay (WTP) a premium to switch from second-hand to first-hand vouchers, are affected by the type of environmental information provided and the magnitude of belief updating required. Recall that WTA is elicited using a multiple price list where first-hand represents the status quo (Voucher A), while WTP is elicited with second-hand as the status quo. Both measures are computed as the midpoint of the interval in which the respondent switches from one option to the other, normalized as the difference from the baseline value of €10, resulting in variables ranging from -€12.5 to €12.5.

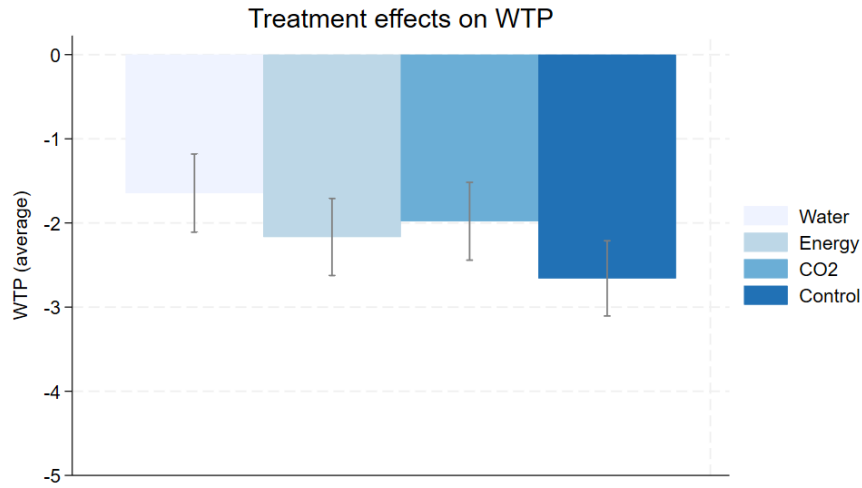
Figure 7a and Figure 7b illustrate how WTA and WTP vary across treatments. We begin by examining simple mean comparisons before moving to multivariate regression analysis to disentangle the effects of treatment assignment and belief accuracy.

Figure 4: Willingness to Accept (WTA) for Second-Hand Vouchers by Treatment



Note: Bar graph showing mean WTA (willingness to accept compensation to switch from first-hand to second-hand vouchers) across treatment conditions. WTA computed as the midpoint of the switching interval in the multiple price list, normalized to baseline €10. Negative values indicate lower WTA (greater willingness to accept second-hand). Error bars represent 95% confidence intervals.

Figure 5: Willingness to Pay (WTP) for First-Hand Vouchers by Treatment



Note: Bar graph showing mean WTP (willingness to pay premium to switch from second-hand to first-hand vouchers) across treatment conditions. WTP computed as the midpoint of the switching interval in the multiple price list, normalized to baseline €10. Positive values indicate higher WTP (greater willingness to pay for first-hand). Error bars represent 95% confidence intervals.

4.2.1 Willingness to Accept: The Effect of Information Provision

We first examine whether providing environmental information reduces respondents' WTA to switch from first-hand to second-hand vouchers. Table 3 presents Bonferroni-adjusted pairwise mean comparisons across treatment groups. All three information treatments (Water, Energy, and CO2) significantly reduce WTA compared to the Control group, with the Water treatment showing the largest effect (-€2.20), followed by Energy (-€1.71) and CO2 (-€1.38). Notably, the differences between the three information treatments are not statistically significant, suggesting that while information provision itself is effective, the relative advantage of more relatable metrics is modest when examining WTA in isolation.

To further investigate the relative importance of information type versus belief accuracy, we estimate a series of Tobit regressions reported in Table 5 (Table 5 reports the main specifications of these regressions, while the complete models including sociodemographic controls are presented in Table 26 the Appendix Appendix B). Column (1) confirms that all three information treatments significantly reduce WTA relative to Control, with Water showing the strongest effect. Column (2) uses CO2 as the baseline and introduces Delta Belief, the standardized distance between the respondent's prior belief and the true value. While Water remains significantly more effective than CO2, the effect of Energy is no longer statistically distinguishable from CO2. Importantly, Delta Belief exhibits a negative and marginally significant effect, indicating that respondents who were further from the true value show somewhat lower WTA reductions. Column (3) introduces controls for stated intention changes and confirms that the primary driver of WTA reduction is whether respondents report being more likely to adopt second-hand clothing following information provision, rather than the magnitude of belief updating per se.

Table 4 examines whether WTA varies by initial belief accuracy. Contrary to what we observed for stated intentions, belief accuracy shows no significant impact on WTA: the differences between underestimators, accurate believers, and overestimators are all statistically indistinguishable. This pattern suggests that while belief accuracy matters for stated behavioral intentions, the more consequential voucher choice task is primarily driven by whether individuals update their intentions at all, regardless of their prior beliefs.

Table 3: WTA: Pairwise Mean Comparisons (Bonferroni Adjusted)

	Control	Water	Energy	CO2
Control	—	-2.20 ***	-1.71 ***	-1.38 ***
Water		—	0.49	0.82
Energy			—	0.33
CO2				—

Note: Significance of coefficients (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These findings provide partial support for our hypotheses:

Result 3 (H.3.a): *Information provision significantly reduces WTA to switch from first-hand to second-hand vouchers across all treatment conditions. Water-based information demonstrates*

Table 4: WTA: Pairwise Mean Comparisons by Belief Accuracy (Bonferroni Adjusted)

	Underestimate	Accurate	Overestimate
Underestimate	—	-0.35	-0.47
Accurate		—	-0.12
Overestimate			—

Note: Significance of coefficients (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the strongest effect, though the advantage over other environmental metrics is more modest than observed for stated intentions, with only Water showing a statistically significant difference from CO2 when controlling for belief updating.

Result 4 (H.3.b): *Contrary to H.3.b, the magnitude of belief underestimation does not significantly affect WTA. Instead, WTA reduction is primarily driven by whether respondents report increased inclination to adopt second-hand clothing, irrespective of their prior belief accuracy.*

Table 5: Tobit regression estimations of WTA for second-hand

	(1)	(2)	(3)
Water	-4.294*** (0.615)	-2.079*** (0.661)	-1.193* (0.657)
Energy	-3.239*** (0.713)	-0.963 (0.788)	-0.737 (0.550)
CO2	-2.531*** (0.721)		
Delta Belief		-0.047* (0.026)	-0.050* (0.026)
More likely (intention)			-7.894*** (0.547)
Less likely (intention)			2.503** (1.110)
Constant	7.635*** (0.469)	5.373*** (0.524)	7.589*** (0.535)
Observations	5241	3930	3930

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control* in column (1), *CO2* in columns (2)-(3). *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value divided by the true value. *More likely (intention)* is a dummy variable equal to 1 if the respondent reported being more likely to purchase second-hand after information provision, 0 otherwise. *Less likely (intention)* is a dummy variable equal to 1 if the respondent reported being less likely to purchase second-hand after information provision, 0 otherwise.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2.2 Willingness to Pay: The Role of Status Quo Framing

We now examine WTP, where second-hand serves as the status quo and respondents indicate how much they would pay to switch to a first-hand voucher. It is important to note that by setting second-hand as the default in this task, we are effectively forcing a status quo that may not align with respondents' natural reference point, potentially attenuating treatment effects relative to what we observed for WTA.

Table 6 presents Bonferroni-adjusted mean comparisons. Only the Water treatment shows a significant increase in WTP relative to Control (difference of €1.01), while Energy and CO2 do not differ significantly from Control. This pattern already suggests more muted effects compared to the WTA analysis.

The Tobit regressions in Table 8 provide further insight (Table 8 reports the main specifications of these regressions, while the complete models including sociodemographic controls are presented in Table 27 the Appendix Appendix B). Column (1) shows that both Water and CO2 increase WTP relative to Control, but Energy does not. Column (2) introduces Delta Belief, which now exhibits a positive and marginally significant effect: respondents who were further from the true value show higher WTP to switch back to first-hand. This contrasts sharply with the WTA results and suggests that when second-hand is imposed as the status quo, those who had underestimated environmental benefits (and thus receive larger corrections) are more inclined to pay to return to first-hand options. Column (3) reveals that neither Water nor Energy significantly differs from CO2 when controlling for stated intentions and belief accuracy. Critically, stated intentions again emerge as the dominant predictor: respondents who report being more likely to adopt second-hand exhibit substantially higher WTP.

Table 7 shows a striking pattern: respondents with accurate initial beliefs exhibit significantly higher WTP than underestimators (difference of €1.34) and overestimators (difference of €0.89). This suggests that when second-hand is imposed as the default, those who already had accurate beliefs about environmental benefits are most willing to pay to revert to first-hand, possibly because they feel less need to be “nudged” away from their preferred choice.

Table 6: WTP: Pairwise Mean Comparisons (Bonferroni Adjusted)

	Control	Water	Energy	CO2
Control	—	1.01 **	0.49	0.68
Water		—	-0.52	-0.33
Energy			—	0.19
CO2				—

Note: Significance of coefficients (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These findings provide limited and qualified support for our hypotheses:

Result 5 (H.4.a): *Information provision has more limited effects on WTP than on WTA, consistent with the observation that imposing second-hand as the status quo creates an artificial reference point. Only Water-based information shows a robust effect relative to Control, and when*

Table 7: WTP: Pairwise Mean Comparisons by Belief Accuracy (Bonferroni Adjusted)

	Underestimate	Accurate	Overestimate
Underestimate	—	1.34 ***	0.89 **
Accurate		—	-0.44
Overestimate			—

Note: Significance of coefficients (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

using CO2 as baseline, no information type demonstrates a significant advantage.

Result 6 (H.4.b): *Contrary to H.4.b, respondents with accurate initial beliefs exhibit the highest WTP to switch from second-hand to first-hand, while underestimators show the lowest WTP. This reversal from the WTA pattern suggests that belief accuracy and status quo framing interact in complex ways: when second-hand is imposed as the default, those who already understood environmental benefits are most resistant to this imposed status quo, while underestimators who receive corrective information are more willing to accept the second-hand option.*

Table 8: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)
Water	1.574*** (0.507)	0.710 (0.586)	-0.057 (0.521)
Energy	0.751 (0.550)	-0.166 (0.617)	-0.598 (0.556)
CO2	1.097** (0.534)		
Delta Belief		0.032* (0.017)	0.034** (0.017)
More likely (intention)			5.830*** (0.441)
Less likely (intention)			-5.401*** (0.901)
Constant	-4.206*** (0.362)	-3.328*** (0.450)	-4.610*** (0.372)
Observations	5255	3930	3930

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control* in column (1), *CO2* in columns (2)-(3). *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value divided by the true value. *More likely (intention)* is a dummy variable equal to 1 if the respondent reported being more likely to purchase second-hand after information provision, 0 otherwise. *Less likely (intention)* is a dummy variable equal to 1 if the respondent reported being less likely to purchase second-hand after information provision, 0 otherwise.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Underlying Mechanisms: Pro-Environmental Behavior and Need for Uniqueness

To better understand the psychological drivers of responses to environmental information, we examine two pre-registered individual-level mechanisms: pro-environmental attitudes and need for uniqueness using validated scales, respectively [Menardo et al. \(2020\)](#) adapted from [Markle \(2013\)](#), and [Ruvio et al. \(2008\)](#) adapted from [Tian et al. \(2001\)](#). Pro-environmental behavior captures respondents’ general orientation toward sustainable consumption practices, which we expect to amplify responsiveness to environmental messaging. Need for uniqueness, by contrast, reflects individuals’ desire to differentiate themselves from others through distinctive consumption choices.

[Table 9](#) reports ordered logistic regressions examining how these mechanisms relate to changes in stated intention to purchase second-hand clothing. Column (1) includes only Delta Belief and the two mechanism variables, pooling across all treatment conditions. As expected, pro-environmental behavior strongly predicts increased intention to adopt second-hand. This finding confirms that environmental messaging resonates most strongly with individuals already predisposed toward sustainable consumption.

Strikingly, need for uniqueness exhibits a negative and highly significant association with increased intention. Individuals who score higher on NFU are significantly less likely to report increased willingness to adopt second-hand clothing after learning about environmental benefits. This counterintuitive result can be understood through two complementary mechanisms. First, high-NFU individuals may paradoxically seek differentiation through conformity to the familiar: while they desire to feel special and unique, they may achieve this psychological goal by doubling down on conventional consumption patterns (first-hand shopping) that they already know and master, rather than venturing into the less familiar territory of second-hand retail. In this interpretation, uniqueness is constructed not through counter-normative behavior but through confident adherence to established consumption modes.

Second, high-NFU individuals may be particularly prone to reactance against perceived experimenter demand. When presented with information explicitly highlighting environmental benefits—which carries an implicit normative message that “you should care about this”—individuals motivated by uniqueness may resist precisely because the desired response seems too obvious or prescribed. This resistance represents a form of asserting autonomy and differentiation: by rejecting the environmentally virtuous option that the experiment appears to endorse, high-NFU respondents signal that they make independent choices unconstrained by external expectations.

Column (2) adds treatment indicators, using CO2 as the baseline. The effects of pro-environmental behavior and need for uniqueness remain virtually unchanged, indicating that these mechanisms operate independently of treatment assignment. Water continues to show the strongest effect relative to CO2, with Energy showing a more modest advantage. Importantly, the stability of mechanism coefficients across specifications suggests that pro-environmental orientation and uniqueness motives moderate overall responsiveness to information rather than selectively enhancing certain types of environmental messaging.

[Table 10](#) extends this analysis to WTA and WTP, allowing us to assess whether these mechanisms influence consequential choices in addition to stated intentions. Focusing first on WTA (columns 1-2), we observe that pro-environmental behavior significantly reduces WTA across

Table 9: Ordered Logistic Regression on Change in Intention to Purchase SH

	(1)	(2)
Delta Belief	-0.007*** (0.002)	-0.005** (0.002)
Water		0.335*** (0.052)
Energy		0.092* (0.053)
Pro-environmental Behavior	0.764*** (0.031)	0.761*** (0.030)
Need for Uniqueness	-0.315*** (0.025)	-0.316*** (0.032)
Cut 1	-0.766*** (0.139)	-0.643*** (0.155)
Cut 2	2.240*** (0.139)	2.373*** (0.159)
Observations	7860	7860

Note: Ordered logit regression estimation with bootstrap standard errors. Dependent variable. *Change in intention to purchase more SH clothes*: categorical variables ranging from -1 to $+1$ (3 categories). Regressors. *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value over the true value. *Water* dummy variables equal 1 if the treatment is Water, 0 otherwise. *Energy* dummy variables equal 1 if the treatment is Energy, 0 otherwise. Baseline *CO2*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

all specifications, meaning pro-environmentally oriented individuals require less compensation to switch from first-hand to second-hand vouchers. This effect persists even when controlling for Delta Belief and stated intention changes, confirming that pro-environmental disposition operates as a direct driver of voucher choices beyond its influence on belief updating or stated preferences.

Conversely, need for uniqueness significantly increases WTA in both specifications. High-NFU individuals demand greater compensation to switch to second-hand vouchers, reinforcing the pattern observed for stated intentions. This substantial effect—representing over €2 in additional compensation required per standard deviation increase in NFU—suggests that resistance to second-hand adoption among high-NFU individuals extends beyond mere stated preferences to consequential financial trade-offs. The persistence of this effect even when controlling for stated intentions (column 2) indicates that uniqueness motives operate both through expressed attitudes and through independent influences on revealed preferences.

Turning to WTP (columns 3-4), the patterns reveal important asymmetries. Pro-environmental behavior strongly increases WTP to switch from second-hand to first-hand. The positive coefficient suggests that pro-environmental individuals who have just been informed about environmental benefits find the imposed second-hand baseline more acceptable and thus require a higher premium to switch away from it. In other words, they are not eager to pay for first-hand; rather, they exhibit higher measured WTP because they are relatively resistant to abandoning the second-hand option when it is presented as the default.

Need for uniqueness shows a positive but substantially weaker association with WTP. High-

NFU individuals are somewhat more willing to pay to switch from second-hand to first-hand, though the effect is much smaller than the €2.17 WTA effect. This asymmetry is consistent with our interpretation: when first-hand is the natural default (WTA), high-NFU individuals strongly resist switching because doing so would mean conforming to the experimenter’s apparent endorsement of second-hand shopping. When second-hand is artificially imposed as the default (WTP), the pressure to conform to this imposed option may actually trigger reactance in the opposite direction, but the effect is muted because the framing itself is transparently artificial, reducing the perceived authenticity of the choice context.

These analyses yield three key insights:

Result 7 (PEB): *Pro-environmental behavior consistently amplifies responsiveness to environmental information across all outcomes. Individuals with stronger pro-environmental orientations report greater increases in stated intentions, exhibit lower WTA to switch to second-hand, and show greater acceptance of second-hand options when imposed as the status quo. This suggests that information campaigns are most effective among populations already predisposed toward sustainable consumption, though it also raises questions about whether messaging strategies can reach beyond the “already converted.” Future research should explore whether alternative framings (e.g., emphasizing economic savings or fashion variety rather than environmental benefits) can engage less pro-environmentally oriented consumers.*

Result 8 (NFU): *Need for uniqueness operates as a substantial barrier to second-hand adoption, with high-NFU individuals showing significantly lower stated intentions and requiring €2.17 more in compensation to switch from first-hand to second-hand vouchers. This effect appears to reflect two psychological processes: (1) a tendency to seek differentiation through confident adherence to familiar consumption patterns (first-hand shopping) rather than through novel alternatives, and (2) reactance against perceived experimenter demand, whereby high-NFU individuals resist the environmentally virtuous option precisely because it appears to be what the experiment implicitly endorses. Marketing strategies targeting high-NFU consumers should avoid explicit environmental or normative appeals, instead emphasizing the autonomy, individuality, and counter-mainstream nature of second-hand consumption (e.g., “don’t follow trends, create your own style”).*

Importantly, neither mechanism systematically interacts with treatment type—both operate similarly across Water, Energy, and CO2 conditions. This suggests that pro-environmental orientation and uniqueness motives influence general receptivity to sustainability information rather than selectively enhancing specific types of environmental framing. The robustness of these mechanism effects across treatment conditions strengthens their interpretation as stable individual differences that moderate information processing, rather than as artifacts of particular experimental manipulations.

5 Additional Results and Robustness Checks

5.1 Study of Beliefs

To extend the analysis on how information provision affects behavioral outcomes, we analyze the distribution and accuracy of respondents’ prior beliefs about the environmental benefits of second-hand clothing. Understanding the structure of these beliefs is crucial for interpreting our treatment

Table 10: Tobit regression estimations of WTA for second-hand

	WTA		WTP	
	(1)	(2)	(3)	(4)
Water	-4.279*** (0.654)	-1.194** (0.590)	1.331*** (0.470)	-0.075 (0.553)
Energy	-3.199*** (0.691)	-0.649 (0.598)	0.583 (0.516)	-0.623 (0.554)
CO2	-2.670*** (0.709)		0.997* (0.542)	
Delta Belief		-0.045* (0.023)		0.028 (0.018)
More likely (intention)		-7.026*** (0.591)		4.928*** (0.416)
Less likely (intention)		2.232** (1.056)		-5.060*** (0.919)
Pro-environmental Behavior	-2.505*** (0.380)	-1.049*** (0.366)	3.455*** (0.266)	2.543*** (0.244)
Need for Uniqueness	2.173*** (0.325)	1.500*** (0.401)	0.369* (0.220)	0.760** (0.301)
Constant	9.685*** (2.129)	6.370*** (1.593)	-16.773*** (1.273)	-15.093*** (1.345)
Observations	5241	3930	5255	3930

Note: Tobit regression estimation with bootstrap standard errors. Dependent variables: Columns (1)-(2): *WTA*, ranging from -€12.5 to €12.5, computed as the midpoint of the switching interval in the MPL, normalized to baseline €10. Negative values indicate lower WTA (greater willingness to accept second-hand). Columns (3)-(4): *WTP*, same construction. Positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control* in column (1)-(3), *CO2* in columns (2)-(4). *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value divided by the true value. *More likely (intention)* is a dummy variable equal to 1 if the respondent reported being more likely to purchase second-hand after information provision, 0 otherwise. *Less likely (intention)* is a dummy variable equal to 1 if the respondent reported being less likely to purchase second-hand after information provision, 0 otherwise. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

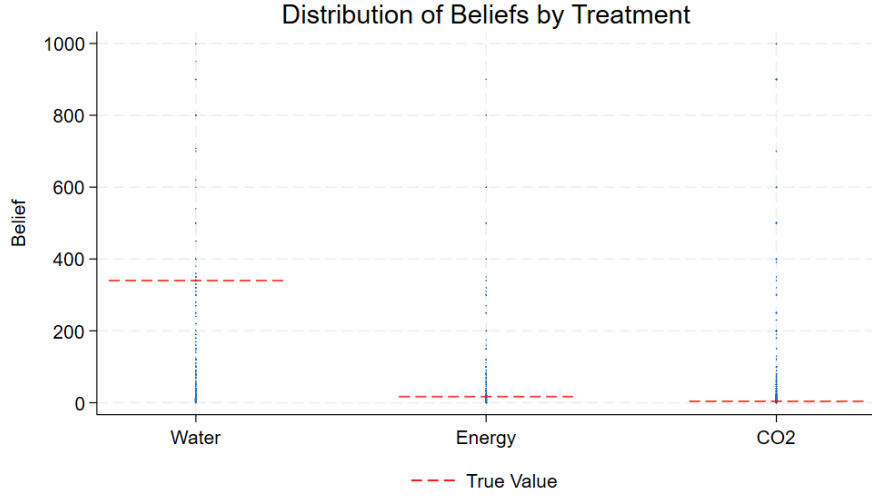
effects, as the magnitude and direction of belief updating may vary substantially across individuals and treatment conditions.

Figure 6 presents scatter plots showing the distribution of respondents' initial beliefs against the true values for each environmental externality. Several patterns emerge from this visualization. First, there is substantial heterogeneity in prior beliefs across all three treatments, with respondents' guesses spanning a wide range around the true values. Second, while a few respondents cluster near the correct values, a notable proportion systematically under- or overestimate the environmental benefits. Third, and most importantly, the scale of the true values varies dramatically across treatments: water savings amount to 340 liters, energy to 17 kWh, and CO2 to 4 kg.

This variation in scale warrants careful consideration. While the water treatment begins with an anchor of 10 liters (compared to 1 unit for energy and CO2), the true value of 340 liters represents

a 34-fold increase from the anchor—effectively equivalent to an anchor-normalized value of 34, which is still substantially larger than the other two treatments. This difference in magnitude may influence both the difficulty of the belief elicitation task and the psychological impact of the information provision, as larger absolute numbers may be perceived as more substantial regardless of their real-world equivalence.

Figure 6: Distribution of Beliefs vs. True Values by Treatment



Note: Scatter plots showing respondents’ initial beliefs about environmental savings against the true values for each treatment. The dashed line represents perfect accuracy (belief = true value). Each point represents an individual respondent. True values: Water = 340 liters, Energy = 17 kWh, CO2 = 4 kg.

Following our pre-registered analysis plan, we identify and examine outliers in the belief data using Tukey’s rule: observations falling below $Q1 - 1.5 \times IQR$ or above $Q3 + 1.5 \times IQR$ are classified as outliers, where $Q1$ and $Q3$ denote the first and third quartiles, and IQR is the interquartile range. Figure 7 displays violin plots both with and without outliers, allowing visual comparison of how outlier removal affects the distribution of beliefs across treatments.

The impact of outlier removal is not uniform across treatments. To examine the distribution of belief accuracy more systematically, Figure 8 presents violin plots of Delta Belief—the standardized distance between respondents’ beliefs and the true values, computed as $(\text{belief} - \text{true value}) / \text{true value}$ —for each treatment. For CO2 and energy, eliminating outliers produces distributions that remain reasonably balanced between under- and overestimation, preserving the essential structure of belief heterogeneity. However, for the water treatment, outlier removal has a more dramatic effect: the resulting distribution consists exclusively of respondents who underestimated the true value. This asymmetry arises because the upper tail of the water belief distribution—those who overestimated water savings—is disproportionately classified as outliers due to the large true value of 340 liters, while the lower tail remains within the non-outlier range.

This differential impact raises an important methodological consideration. While outlier removal is a standard practice and was pre-registered in our analysis plan, its application to the

water treatment fundamentally alters the composition of the sample in that condition, leaving primarily underestimators. Such a transformation could artificially amplify treatment effects for water if underestimators respond more strongly to information provision—a pattern we indeed observe in our main results. To address this concern, we report all main analyses both with and without outlier removal. The primary results presented in the main text include all observations to preserve sample composition and comparability across treatments. However, we provide complete robustness analyses excluding outliers in [Appendix B](#), allowing readers to assess the sensitivity of our findings to this specification choice.

Result 9 (Belief heterogeneity): *Respondents’ prior beliefs exhibit substantial heterogeneity across all treatments, with the majority systematically underestimating environmental benefits. The distribution of belief accuracy varies by treatment scale: while CO₂ (true value = 4 kg) and Energy (true value = 17 kWh) show relatively balanced distributions of under- and overestimation, Water (true value = 340 liters) displays marked underestimation. Outlier removal disproportionately affects the Water treatment, eliminating virtually all overestimators and leaving an exclusively underestimating sample, raising concerns about comparability across treatments in robustness specifications.*

5.2 Default Options Across MPL Frames

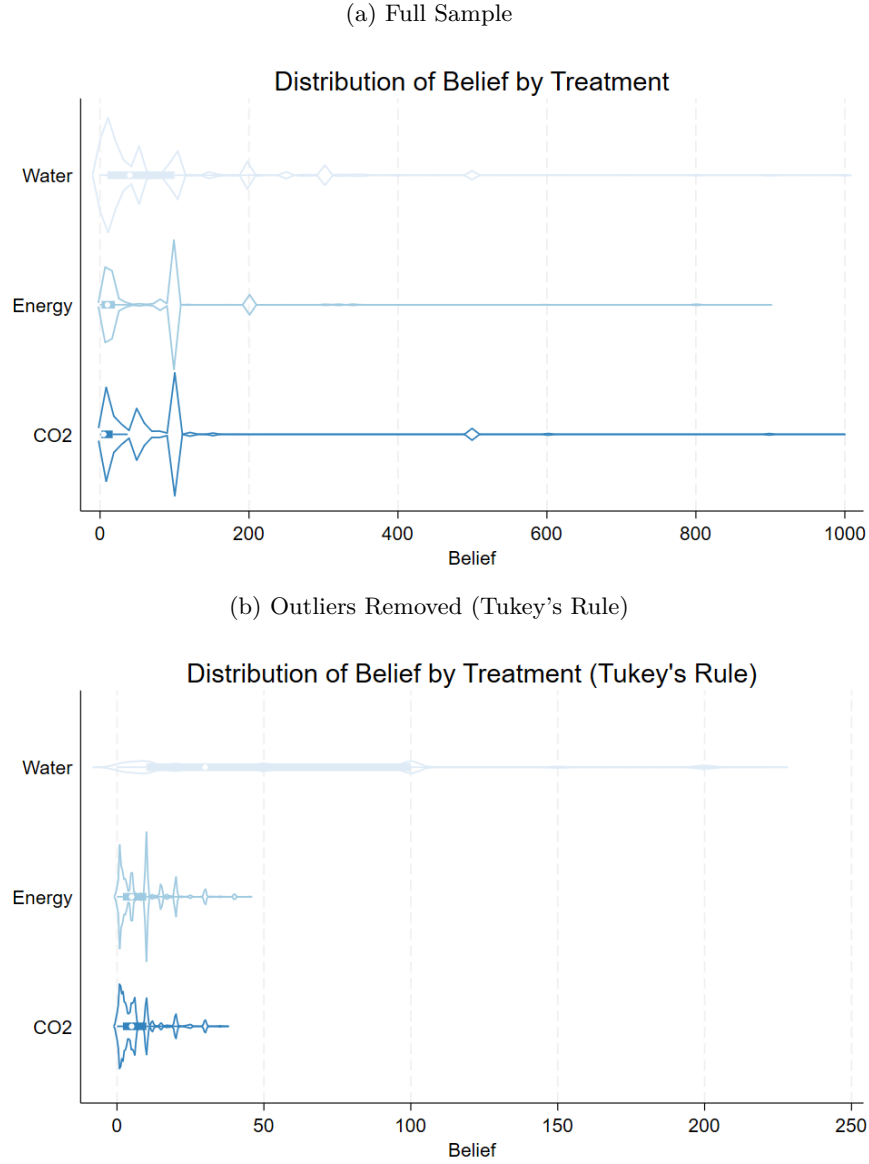
A key feature of our experimental design is the use of two multiple price list (MPL) frames: one with first-hand (FH) as the baseline and another with second-hand (SH) as the baseline. This dual-frame approach allows us to test whether treatment effects are robust to the way choice problems are presented, and whether individuals exhibit consistent preferences when the status quo is manipulated.

To examine preference consistency across frames, we focus on the critical choice where both vouchers are valued equally at €10—the point at which neither option offers a financial advantage and the decision reflects pure preference. For each respondent, we code a binary variable equal to 0 if they choose the first-hand voucher and 1 if they choose the second-hand voucher at this equal-value point, regardless of which option served as the baseline in their assigned MPL. This measure allows us to compare preferences in a financially neutral setting across the two framing conditions.

[Figure 9](#) presents the proportion of respondents choosing second-hand at the €10-€10 point, broken down by MPL baseline (FH vs. SH) and by treatment condition. The results reveal remarkable consistency across framing conditions, with proportion test details reported in [Table 11](#). Pooling across all treatments, 38.5% of respondents with FH as baseline chose second-hand, compared to 37.7% with SH as baseline—a difference of less than one percentage point that is not statistically significant. This suggests that, on average, respondents exhibit stable preferences regardless of which option is framed as the status quo.

Treatment-specific comparisons similarly reveal no significant differences between the two baseline conditions. In the Control condition, 32.1% of respondents with FH baseline chose second-hand compared to 35.0% with SH baseline, a 2.9 percentage point difference that does not reach statistical significance. The Water treatment shows 42.0% versus 39.8%, Energy shows 40.7% versus 37.6%, and CO₂ shows 39.1% versus 38.5%. None of these within-treatment comparisons reach

Figure 7: Distribution of Beliefs: Full Sample and Outliers Removed

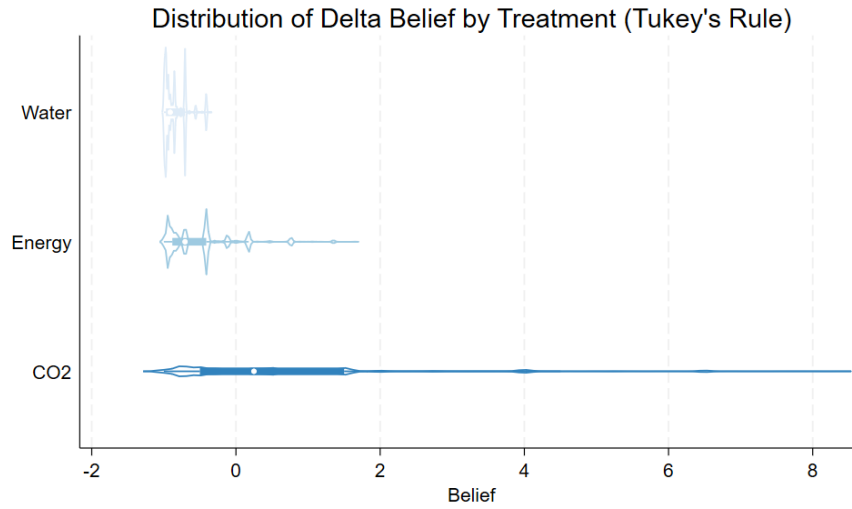


Note: Violin plots showing the distribution of beliefs for each treatment. Panel (a): full sample. Panel (b): after removing outliers using Tukey's rule ($Q1 - 1.5 \times IQR$, $Q3 + 1.5 \times IQR$). Each violin shows the probability density of belief values, with wider sections indicating higher concentration of responses.

conventional levels of statistical significance, indicating that the status quo framing does not systematically alter choice behavior at the point of equal valuation.

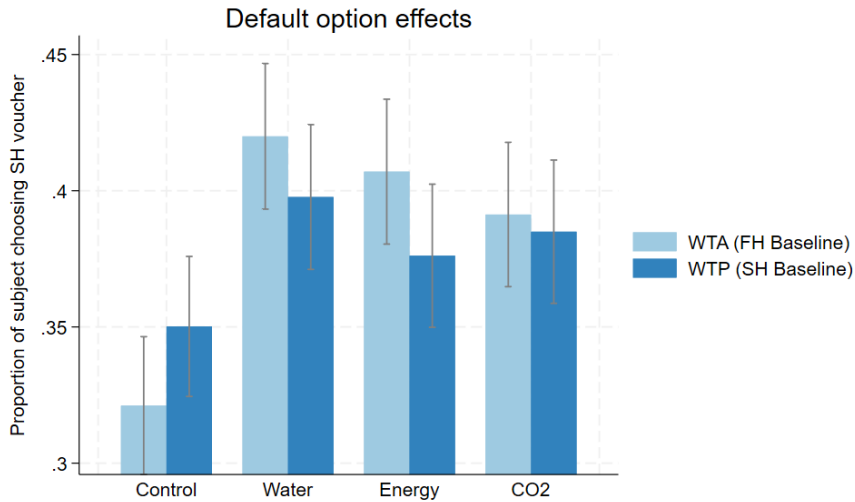
These findings provide reassuring evidence that our main results are not driven by framing effects or status quo bias. Despite the substantial literature documenting endowment effects and reference-dependent preferences, respondents in our experiment appear to exhibit relatively stable preferences when choosing between equally-valued vouchers, regardless of which option is design-

Figure 8: Distribution of Delta Belief: Outliers Removed



Note: Violin plots showing the distribution of Delta Belief (standardized distance between belief and true value) for each treatment after removing outliers using Tukey's rule ($Q1 - 1.5 \times IQR$, $Q3 + 1.5 \times IQR$). Negative values indicate underestimation; positive values indicate overestimation.

Figure 9: Proportion Choosing Second-Hand at Equal Value (€10-€10), by Baseline and Treatment



Note: Bar chart showing the proportion of respondents choosing the second-hand voucher when both options are valued at €10, separately by MPL baseline (FH vs. SH) and treatment condition. Error bars represent 95% confidence intervals.

nated as the baseline. This consistency strengthens the interpretation of our WTA and WTP results: while the magnitude of stated valuations differs across frames (as expected given the WTA-WTP gap), the underlying directional preferences appear robust to manipulations of the status quo.

Table 11: Proportion Tests: Indifference by Voucher Baseline

	WTA (Baseline FH)	WTP (Baseline SH)	Difference	z-stat	p-value
Total	0.385	0.377	-0.008	0.81	0.4175
Control	0.321	0.350	0.029	-1.58	0.1142
Water	0.420	0.398	-0.022	1.16	0.2466
Energy	0.407	0.376	-0.031	1.62	0.1056
CO2	0.391	0.385	-0.006	0.33	0.7404

Note: Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

That said, it is important to note that this consistency test examines only a single point in the choice set—the equal-value condition—and does not capture potential framing effects that may operate when the vouchers differ in value. Our earlier WTA and WTP analyses revealed meaningful differences in treatment effects across the two frames, particularly regarding the role of belief accuracy. The present analysis therefore complements rather than contradicts those findings, suggesting that while respondents exhibit consistent preferences at the point of indifference, the path by which they reach that point (i.e., the trade-offs they are willing to make) can vary with the framing of the choice problem.

Result 10 (Default option effect): *Respondents exhibit consistent preferences across MPL frames at the point of equal valuation (€10-€10), with no significant differences in second-hand choice between FH baseline (38.5%) and SH baseline (37.7%) conditions, either overall, or within any individual treatment. This stability suggests that our main findings are not driven by status quo bias or framing effects at the point of indifference, though the path to that point—as reflected in WTA and WTP magnitudes—varies meaningfully with baseline manipulation.*

5.3 Stores proximity

An important consideration when interpreting our results is the role of store accessibility in moderating treatment effects. While information about environmental benefits may shift preferences toward second-hand clothing, the feasibility of acting on these preferences depends critically on whether suitable retail options are available nearby. In this section, we examine whether the effects we observe vary systematically with respondents’ proximity to the stores featured in our voucher task.

Our dataset includes 2,435 unique postal codes corresponding to respondents’ locations across Italy, and we need to identify the closest FH and SH stores from a total of 308 store locations (covering both chains). Computing the precise driving distance between every respondent location and every store would require 749,980 API requests to Google Maps, at substantial computational cost. To make this process tractable while maintaining accuracy, we implemented a multi-stage filtering procedure.

First, we retrieved geographic coordinates for all store addresses and postal codes using the Google Maps Geocoding API. For postal codes associated with multiple municipalities, we computed the geometric center of the area to establish a single reference point. Second, we used the geodesic distance formula to compute straight-line distances between all respondent locations and stores, accounting for the Earth’s curvature. This allowed us to efficiently identify candidate stores

within a reasonable distance threshold without yet accounting for road networks and actual travel routes.

We examined several distance thresholds to balance comprehensiveness with feasibility. Setting the threshold at 200 km retained sufficient store options for the vast majority of respondents while reducing the number of required API calls to approximately 25% of the initial total. Further refinement to 140 km reduced the computational burden to just 15% of the original requests (114,370 distance calculations) while still ensuring that nearly all respondents—including those in remote locations such as Sardinia—had at least one store of each type within range. The only exception was respondents on the small island of Pantelleria, who were retained in the sample given their negligible number.

Using this filtered set of candidate stores, we then queried the Google Maps Routes API (DistanceMatrix) to obtain precise driving distances accounting for actual road networks. For each respondent, we identified the single closest FH store and the single closest SH store, as well as which of the two was closer overall. This final dataset allows us to test whether proximity to stores amplifies or attenuates the information provision effects documented in the previous sections.

Table 12 reports Tobit regressions examining how store proximity moderates treatment effects on WTA. Column (1) includes only the binary indicator for which store type is closer. The results reveal a striking pattern: respondents for whom the second-hand store is the closest option exhibit significantly higher WTA, meaning they require greater compensation to switch from first-hand to second-hand vouchers. This finding is counterintuitive at first glance—one might expect proximity to second-hand stores to facilitate adoption. However, the result is consistent with convenience reinforcing existing preferences: when second-hand stores are farther away, respondents may view them as less viable alternatives and thus be more willing to accept lower-valued vouchers to “try” second-hand shopping, knowing the barrier to actually redeeming the voucher is higher.

Column (2) replaces the binary proximity indicator with continuous distance measures for both store types. Neither the distance to first-hand stores nor the distance to second-hand stores shows a significant association with WTA. This suggests that absolute accessibility matters less than relative accessibility—it is the comparison between the two store types, rather than their individual distances, that influences willingness to accept second-hand vouchers.

Column (3) includes both the binary proximity indicator and the continuous distance measures simultaneously. The binary indicator remains positive and significant, while the continuous measures remain insignificant. This specification confirms that relative proximity is the primary moderating factor.

Critically, across all three specifications, the treatment effects remain remarkably stable. Water consistently reduces WTA by approximately €4 relative to Control, Energy by approximately €2.50, and CO2 by approximately €2.70. The consistency of these coefficients across specifications indicates that store proximity does not substantially moderate treatment effects—information provision appears equally effective regardless of store accessibility.

Table 13 presents parallel analyses for WTP, where second-hand serves as the baseline. Column (1) shows that the binary proximity indicator has no significant effect on WTP, contrasting sharply with the WTA results. Column (2) confirms that neither continuous distance measure significantly predicts WTP, and Column (3) demonstrates that including all proximity measures simultaneously yields no significant effects.

Table 12: Tobit regression estimations of WTA for second-hand

	(1)	(2)	(3)
Water	-3.996*** (0.707)	-4.004*** (0.729)	-3.993*** (0.852)
Energy	-2.459*** (0.701)	-2.466*** (0.563)	-2.461*** (0.755)
CO2	-2.667*** (0.705)	-2.652*** (0.618)	-2.671*** (0.659)
SH Closest	1.247*** (0.477)		1.415** (0.589)
FH Distance (in km)		-0.018 (0.014)	0.002 (0.014)
SH Distance (in km)		0.007 (0.010)	-0.010 (0.015)
Constant	6.834*** (0.580)	7.694*** (0.554)	6.923*** (0.686)
Observations	5074	5074	5074

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent’s postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent’s postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As with WTA, treatment effects remain highly stable across specifications. Water increases WTP by approximately €1.57 relative to Control, while CO2 increases WTP by approximately €1.10. Energy shows no significant effect relative to Control in any specification.

The absence of proximity effects for WTP is consistent with our earlier interpretation that imposing second-hand as the status quo creates an artificial reference point that attenuates behavioral responses. When respondents are forced to consider switching from second-hand to first-hand, the relative accessibility of stores appears irrelevant—the decision is driven primarily by preferences rather than logistical considerations.

These results yield two key insights:

Result 11 (Store proximity 1): *While relative store proximity affects baseline WTA (with closer second-hand stores paradoxically increasing WTA), it does not moderate treatment effects—information provision remains equally effective regardless of store accessibility. This suggests that our experimental manipulation operates primarily through belief updating and preference shifts rather than through changes in perceived feasibility of action.*

Result 12 (Store proximity 2): *The asymmetry between WTA and WTP results reinforces our earlier conclusion that status quo framing matters: relative proximity only influences choices when first-hand serves as the baseline, consistent with convenience considerations being more salient when evaluating a voluntary switch away from one’s default option.*

Table 13: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)
Water	1.571*** (0.544)	1.569*** (0.548)	1.573*** (0.583)
Energy	0.707 (0.480)	0.700 (0.550)	0.701 (0.584)
CO2	1.103** (0.540)	1.100** (0.550)	1.102** (0.542)
SH Closest	-0.013 (0.372)		0.139 (0.497)
FH Distance (in km)		-0.003 (0.011)	-0.001 (0.013)
SH Distance (in km)		-0.008 (0.008)	-0.010 (0.010)
Constant	-4.075*** (0.376)	-3.860*** (0.499)	-3.936*** (0.437)
Observations	5105	5105	5105

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand).. Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent’s postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent’s postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Geographic Heterogeneity: Regional Variation in Treatment Effects

Our analysis thus far has examined treatment effects pooling across all respondents. However, the effectiveness of different types of environmental information may vary systematically across Italy’s diverse geographic and socio-economic contexts. Regional differences in economic development, urbanization, educational attainment, and exposure to specific environmental challenges may shape how individuals respond to information about water, energy, and CO2 savings.

To explore potential geographic heterogeneity, we replicate our main voucher choice analyses (Table 5 and Table 8) separately for Italy’s five macro-regions: Northwest (Piedmont, Lombardy, Liguria, Valle d’Aosta), Northeast (Veneto, Trentino-Alto Adige, Friuli-Venezia Giulia, Emilia-Romagna), Center (Tuscany, Umbria, Marche, Lazio), South (Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria), and Islands (Sicily, Sardinia). These macro-regional classifications, while broad, capture meaningful variation in climate, infrastructure, economic conditions, and cultural norms. Complete regression results are reported in Appendix B.

Despite the limitations in statistical power, several suggestive patterns emerge from the regional analysis that warrant discussion. For WTA (Table 28), the Water treatment demonstrates remarkable consistency across geographic contexts, showing significant effects in all macro-regions in the baseline specification (columns 1, 3, 5, 7, 9), with coefficients ranging from -€3.28 in the

Northwest to -€5.52 in the Center. This robustness suggests that water-related environmental information resonates broadly across Italy’s diverse regional contexts. In contrast, CO2 emerges as the weakest treatment across most regions, achieving significance only in the Northeast (-€3.35, $p < 0.05$) and Center (-€4.27, $p < 0.01$), while showing no significant effects in the Northwest, South, or Islands. This geographic pattern reinforces our main finding that CO2-based messaging is less effective than more tangible environmental metrics.

The most striking regional specificity appears in the Islands macro-region, where the Energy treatment exhibits an exceptionally strong effect on WTA (-€7.21, $p < 0.01$ in column 9), nearly double the magnitude observed in other regions. Even after controlling for stated intentions and belief updating (column 10), the Energy effect remains substantial and marginally significant. This outsized response may reflect the particular salience of energy issues in Sardinia and Sicily, where geographic isolation amplifies energy costs, islands face unique challenges in energy infrastructure and grid connectivity, and energy poverty rates tend to be higher than in mainland regions. For residents of these areas, information about energy savings from second-hand consumption may connect more directly to lived economic realities and everyday concerns about energy affordability.

The Center region (Lazio, Tuscany, Umbria, Marche) emerges as the most uniformly responsive area, with all three information treatments showing significant effects in the baseline specification (column 5: Water -€5.52, Energy -€2.88, CO2 -€4.27, all $p < 0.05$ or better). This heightened responsiveness may reflect the region’s relatively higher urbanization, educational attainment, and concentration of politically progressive populations. Conversely, the South shows a more selective pattern: while Water maintains strong effects (-€3.83, $p < 0.01$), neither Energy nor CO2 reach significance, and Water is the only treatment to retain marginal significance when controlling for stated intentions (-€2.93, $p < 0.10$). This could reflect the greater immediacy of water-related concerns in Southern Italy, where water scarcity, infrastructure challenges, and seasonal droughts make water conservation a more pressing and familiar issue than abstract metrics like CO2 emissions.

Turning to WTP (Table 29), the regional patterns are strikingly different and generally much weaker, reinforcing our interpretation that imposing second-hand as the status quo creates an artificial framing that attenuates treatment effects. The most notable exception occurs in the South, where Water is the only treatment to show a robust positive effect on WTP (+€3.14, $p < 0.01$ in column 7), even though this effect dissipates when controlling for stated intentions. This Southern specificity for water information appears in both WTA and WTP measures, providing convergent evidence that water-related messaging may be particularly effective in regions where water scarcity is a salient lived experience. Elsewhere, treatment effects on WTP are sparse and inconsistent: CO2 shows significance only in the Northwest (+€2.03, $p < 0.05$), while Energy fails to reach significance in any region. This geographic fragmentation of WTP effects, combined with their generally smaller magnitudes, suggests that the artificial status quo framing may interact with regional contexts in complex and unstable ways.

Several important caveats apply to these regional findings. The sample sizes for individual macro-regions range from 428 to 1,062 observations in the specifications with controls, substantially reducing power to detect effects and inflating standard errors. Regional classifications necessarily obscure enormous within-region heterogeneity. Moreover, we did not pre-register hypotheses about regional moderation, and the large number of comparisons (5 regions $\times$ 3 treatments $\times$ 2 outcomes) raises concerns about multiple testing and the possibility of spurious findings. We

therefore emphasize that these results should be interpreted as hypothesis-generating rather than hypothesis-testing, pointing toward promising directions for future research with larger, regionally-stratified samples designed explicitly to test geographic heterogeneity.

Nevertheless, the consistency of certain patterns—particularly the robustness of Water effects across regions, the exceptional Energy response in the Islands, and the relative weakness of CO₂ messaging outside central Italy—suggests that local environmental conditions and economic contexts may meaningfully shape how individuals respond to different types of environmental information. Future work should investigate whether these regional patterns can be explained by objective indicators (e.g., water scarcity indices, energy poverty rates, air quality measurements) or by subjective perceptions and cultural norms around environmental issues. Such research could inform geographically-targeted information campaigns that match environmental messaging to locally salient concerns.

5.5 Local Environmental Conditions as Treatment Moderators

While the preceding regional analysis provides a coarse-grained assessment of geographic heterogeneity, it does not directly test whether specific, measurable environmental conditions moderate treatment effects. In this section, we adopt a complementary approach by linking individual-level responses to continuous measures of local environmental challenges and pro-environmental political attitudes. This strategy allows us to examine whether treatment effects vary systematically with the intensity of locally-experienced environmental problems, providing a more direct test of the hypothesis that information resonates most strongly when it connects to salient, lived experiences.

We leverage the geographic granularity of our data—which includes respondents’ region, province, and postal code—to link survey responses with administrative data from Italian national and regional statistical agencies. This approach offers several advantages over the macro-regional analysis. First, it exploits continuous variation in environmental conditions rather than imposing arbitrary geographic boundaries, increasing precision and power. Second, it tests specific, theoretically-motivated mechanisms linking local context to information effectiveness, rather than relying on broad regional labels as proxies for unmeasured contextual factors. Third, it allows us to examine whether different types of environmental information are differentially effective depending on locally-salient challenges.

We construct four categories of moderating variables based on publicly available administrative data from Italian national and regional statistical agencies:

Proxy for exposure to pro-environmental attitudes. We measure the prevalence of environmental concern in respondents’ local communities using the percentage of votes cast for the green party (Alleanza Verdi e Sinistra) in the 2024 European Parliament elections, aggregated at the provincial level. Data are sourced from Eligendo, the Italian Ministry of Interior’s official electoral database. This variable captures the degree to which respondents are embedded in communities where environmental issues are politically salient, which may amplify the effectiveness of environmental information provision across all treatment conditions.

Factors enhancing Water information treatment. We construct two variables intended to capture the local salience of water-related environmental issues. First, we measure flooding risk using the percentage of provincial land area classified as subject to High Probability Hazard of Flooding

in 2020, as mapped by ISPRA (Istituto Superiore per la Protezione e la Ricerca Ambientale), Italy’s national environmental protection agency. Second, we measure water scarcity using the number of days in 2021 during which domestic water usage was subject to partial suspension at the provincial level, as reported by ISTAT (Istituto Nazionale di Statistica). Both variables proxy for direct experience with water-related environmental challenges, which may heighten responsiveness to information about water savings from second-hand clothing.

Factors enhancing Energy information treatment. We measure exposure to energy affordability challenges using the Energy Poverty Index (Indice di Povertà Energetica) for 2020, constructed by ISTAT in collaboration with OIPE (Osservatorio Italiano sulla Povertà Energetica) and available at the regional level. This composite index captures the prevalence of households unable to adequately heat their homes or pay energy bills, reflecting both economic vulnerability and heightened awareness of energy consumption. Respondents in regions with higher energy poverty may be more attentive to information about energy savings.

Factors enhancing CO2 information treatment. We measure air quality using average annual concentrations of four key pollutants in 2021 at the provincial level: nitrogen dioxide (NO₂), ozone (O₃), particulate matter PM10, and particulate matter PM2.5. Data are sourced from ISPRA’s national air quality monitoring network. These pollutants are closely associated with fossil fuel combustion and industrial activity, making them plausible proxies for local exposure to CO2-generating processes. Respondents in provinces with poorer air quality may find CO2-related information more personally relevant.

Table 14 reports descriptive statistics for these variables across our sample, revealing substantial geographic variation. For instance, green voting ranges from 2.58% to 15.18% across provinces, flooding risk from 0 to 23.91 days, and the Energy Poverty Index from 3.95 to 15.69 across regions.

Table 14: Descriptive Statistics: Environmental Context Variables

Variable	N	Mean	SD	Min	Max
Green voting (% , provincial)	10,496	6.02	1.81	2.58	15.18
Flooding risk (% land area, provincial)	10,496	5.50	5.07	0	23.91
Water scarcity (dummy, provincial)	10,425	.13	.34	0	1
Energy Poverty Index (regional)	10,496	7.2	3.84	3.95	15.69
NO ₂ concentration ($\mu\text{g}/\text{m}^3$, provincial)	10,466	23.52	4.41	10	35.6
O ₃ concentration ($\mu\text{g}/\text{m}^3$, provincial)	10,214	59.43	7.81	44	95
PM10 concentration ($\mu\text{g}/\text{m}^3$, provincial)	10,466	18.79	5.14	4	33.31
PM2.5 concentration ($\mu\text{g}/\text{m}^3$, provincial)	10,280	14.49	4.20	7	25

Note: Descriptive statistics for environmental context variables linked to respondents based on their province or region of residence. Green voting measured in 2024 EU elections; flooding risk and air quality measured in 2020-2021; water scarcity measured in 2021; energy poverty measured in 2020.

The results provide limited evidence that local environmental conditions moderate treatment effects in the hypothesized directions. Table 15 and Table 16 report specifications testing whether pro-environmental political attitudes (column 1), water-related challenges (column 2), energy poverty (column 3), and air quality (column 4) amplify the effectiveness of corresponding environmental information treatments.

Green party vote share shows no significant main effect on WTA or WTP, nor do any of

the interaction terms with treatment indicators reach significance. This null result suggests that living in a politically pro-environmental province does not systematically amplify responsiveness to environmental information, contrary to the hypothesis that community-level environmental norms would enhance treatment effects. One interpretation is that individual-level pro-environmental attitudes (Result 7) matter more than aggregate political context, or that provincial-level vote shares are too coarse to capture meaningful variation in local environmental culture.

Neither flooding risk nor water scarcity shows the expected positive interaction with the Water treatment. The flooding risk interaction is essentially zero for both WTA and WTP. Interestingly, provinces with water usage restrictions show lower baseline WTA (coefficient = -1.381, $p < 0.10$) and WTP (coefficient = -2.094, $p < 0.01$), suggesting these areas have systematically different preferences. However, the Water treatment interaction with water scarcity, while positive, does not reach conventional significance for WTA and is only marginally significant for WTP (coefficient = 2.734, $p < 0.10$). This marginal WTP interaction suggests that in water-scarce provinces, the Water treatment may slightly amplify willingness to pay for first-hand options when second-hand is imposed as the status quo—a pattern difficult to interpret substantively and possibly reflecting statistical noise given the multiple tests conducted.

The Energy Poverty Index shows no significant main effect or interaction with the Energy treatment for either WTA or WTP. The interaction term for WTA is negative and not significant, providing no support for the hypothesis that energy-poor regions would be more responsive to energy savings information. This null result may reflect the fact that energy poverty is measured at the regional level, which may be too aggregated to capture meaningful variation in energy affordability concerns that would make energy information more salient.

For the air quality specifications (column 4) the only marginally significant interaction is $\text{CO}_2 \times \text{O}_3$ for WTA (coefficient = 0.173, $p < 0.10$), but this positive coefficient suggests that higher ozone concentrations make the CO_2 treatment less effective (higher WTA), opposite the hypothesized direction. For WTP, none of the air quality interactions approach significance.

Contrary to our expectations, treatment effects do not vary systematically with local environmental conditions in ways that would suggest information resonates more strongly when connected to lived environmental challenges. Several explanations are plausible. First, the measures of local environmental conditions, while objectively meaningful, may not map onto subjective salience—individuals may not be aware of or attentive to provincial-level statistics on flooding risk or air quality. Second, the geographic aggregation levels (provincial for most variables, regional for energy poverty) may obscure finer-grained variation that matters for individual responses. Third, treatment effects may be sufficiently strong and uniform that local contextual moderation is simply not detectable with our sample size and measurement precision. Finally, environmental information may operate through cognitive and affective pathways (belief updating, emotional responses to environmental harm) that are relatively independent of whether the specific externality is locally salient.

Result 13: *Local environmental conditions—including pro-environmental political attitudes, water scarcity, energy poverty, and air quality—do not systematically moderate treatment effects on voucher choices. With the marginal exception of a weak water scarcity $\times$ Water treatment interaction for WTP ($p < 0.10$), none of the hypothesized moderations reach statistical significance. This pattern suggests that the effectiveness of different environmental framings is not strongly contingent*

on whether respondents face corresponding local environmental challenges, raising questions about the mechanisms through which environmental information influences behavior. Information provision may operate through relatively context-independent cognitive processes (e.g., surprise at the magnitude of environmental impacts) rather than through resonance with locally-salient problems.

Table 15: Tobit regression estimations of WTA for first-hand

	(1)	(2)	(3)	(4)
Water	-6.433** (2.549)	-2.607*** (0.926)		
Energy	-3.800 (2.540)		0.948 (1.244)	
CO2	-2.192 (2.540)			-9.560 (7.831)
Green vote share	-0.012 (0.331)			
Water $\times$ Green vote share	0.355 (0.401)			
Energy $\times$ Green vote share	0.093 (0.405)			
CO2 $\times$ Green vote share	-0.056 (0.412)			
HPH		0.054 (0.058)		
Water $\times$ HPH		0.012 (0.097)		
Days of partial suspension (dummy)		-1.381 (0.855)		
Water $\times$ Days of partial suspension (dummy)		2.100 (1.832)		
Modified LIHC			0.059 (0.068)	
Energy $\times$ Modified LIHC			-0.261* (0.143)	
NO2 concentration				0.206** (0.083)
CO2 $\times$ NO2 concentration				-0.086 (0.143)
PM10 concentration				0.064 (0.131)
CO2 $\times$ PM10 concentration				-0.199 (0.239)
PM2.5 concentration				-0.342*** (0.132)
CO2 $\times$ PM2.5 concentration				0.397* (0.233)
O3 concentration				-0.066 (0.052)
CO2 $\times$ O3 concentration				0.173** (0.086)
Constant	7.709*** (1.953)	5.592*** (0.522)	4.932*** (0.522)	8.558* (4.562)
Observations	5241	5208	5241	5000

Note on Table 15: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Green vote share*: percentage of votes for green parties in 2024 EU elections, standardized (provincial level). *HPH*: percentage of land area with High Probability Hazard of flooding in 2020, standardized (provincial level). *Days of partial suspension (dummy)*: dummy variable equal to 1 if province experienced water usage restrictions in 2021, 0 otherwise. *Modified LIHC*: Energy Poverty Index for 2020, standardized (regional level). *NO2 concentration*, *O3 concentration*, *PM10 concentration*, *PM2.5 concentration*: average annual pollutant concentrations in 2021, all standardized (provincial level). Interaction terms test whether treatment effects vary with local environmental conditions.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)	(4)
Water	3.691** (1.688)	0.396 (0.722)		
Energy	-0.083 (1.802)		0.913 (0.874)	
CO2	1.422 (1.763)			-2.228 (5.497)
Green vote share	0.363 (0.223)			
Water $\times$ Green vote share	-0.358 (0.278)			
Energy $\times$ Green vote share	0.133 (0.281)			
CO2 $\times$ Green vote share	-0.062 (0.281)			
HPH		-0.034 (0.051)		
Water $\times$ HPH		0.022 (0.080)		
Days of partial suspension (dummy)		-2.094*** (0.582)		
Water $\times$ Days of partial suspension (dummy)		2.734** (1.087)		
Modified LIHC			-0.045 (0.068)	
Energy $\times$ Modified LIHC			-0.147 (0.121)	
NO2 Concentration				-0.054 (0.050)
CO2 $\times$ NO2 Concentration				0.014 (0.105)
PM10 Concentration				-0.123 (0.093)
CO2 $\times$ PM10 Concentration				-0.130 (0.204)
PM2.5 concentration				0.089 (0.086)
CO2 $\times$ PM2.5 Concentration				0.203 (0.187)
O3 Concentration				-0.026 (0.037)
CO2 $\times$ O3 Concentration				0.038 (0.067)
Constant	-6.353*** (1.337)	-3.107*** (0.406)	-2.993*** (0.510)	0.723 (3.103)
Observations	5255	5217	5255	5028

Note on Table 16: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors: *Water*, *Energy*, and *CO2* are

dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. *Baseline: Control*. *Green vote share*: percentage of votes for green parties in 2024 EU elections, standardized (provincial level). *HPH*: percentage of land area with High Probability Hazard of flooding in 2020, standardized (provincial level). *Days of partial suspension (dummy)*: dummy variable equal to 1 if province experienced water usage restrictions in 2021, 0 otherwise. *Modified LIHC*: Energy Poverty Index for 2020, standardized (regional level). *NO2 concentration*, *O3 concentration*, *PM10 concentration*, *PM2.5 concentration*: average annual pollutant concentrations in 2021, all standardized (provincial level). Interaction terms test whether treatment effects vary with local environmental conditions.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Discussion and conclusion

This paper investigates how the framing of environmental information—expressed in terms of CO2 emissions, energy consumption, or water usage—affects individuals’ willingness to adopt second-hand clothing. Using a large, nationally representative sample of Italian consumers ($N = 10,496$), we find robust evidence that the choice of environmental metric matters substantially: water-based information generates the strongest effects on both stated intentions and incentivized voucher choices, followed by energy, with CO2—the dominant metric in current sustainability communication—emerging as the least effective frame. Beyond this core finding, our results reveal important heterogeneity in treatment effects based on prior beliefs and individual characteristics, document the sensitivity of elicited preferences to status quo framing, and provide surprising null results regarding the role of local environmental conditions in moderating information effectiveness.

6.1 Summary of Main Findings

Our experimental design allows us to decompose treatment effects across multiple dimensions. First, regarding the primary research question, we find clear evidence that environmental information framed in terms of water savings outperforms both energy and CO2 metrics. Water-based information increases stated intention to purchase second-hand clothing among 43.03% of respondents (compared to 35.27% for energy and 30.73% for CO2), and reduces willingness to accept compensation to forgo second-hand vouchers by approximately €4 relative to control. Energy information shows intermediate effectiveness, while CO2 consistently produces the weakest effects despite its ubiquity in environmental labeling and sustainability campaigns. These patterns are statistically significant and robust across multiple outcome measures, supporting the hypothesis that more tangible and experientially relatable environmental metrics generate stronger behavioral responses than abstract measures like CO2 emissions.

Second, we document meaningful heterogeneity in treatment effects based on prior beliefs. Information provision is most effective among individuals who initially underestimate environmental benefits—a group comprising approximately 62% of our sample. These underestimators show significantly higher rates of increased stated intention (38.86%) compared to overestimators (34.19%) or those with accurate initial beliefs (who exhibit substantial resistance to updating). This pattern suggests that information campaigns have the greatest potential impact among populations

with systematically biased priors, though it also raises questions about diminishing returns as environmental awareness increases over time.

Third, individual psychological characteristics play a critical role in moderating responsiveness. Pro-environmental behavior strongly amplifies treatment effects across all outcomes: a one-standard-deviation increase in pro-environmental orientation is associated with a €2.51 reduction in WTA and substantially higher odds of reporting increased intention to adopt second-hand. Conversely, need for uniqueness operates as a significant barrier, with high-NFU individuals requiring approximately €2.17 more in compensation to switch to second-hand vouchers. We interpret this counterintuitive pattern as reflecting two processes: high-NFU individuals may seek differentiation through confident adherence to familiar consumption patterns rather than through novel alternatives, and they may exhibit reactance against perceived experimenter demand by resisting the environmentally virtuous option that the information provision implicitly endorses.

Fourth, we find important methodological insights regarding preference elicitation. Respondents exhibit consistent choices across different status quo framings at the point of equal valuation (€10-€10), with no significant differences between first-hand and second-hand baseline conditions. However, the magnitude of stated valuations varies dramatically with framing: imposing second-hand as the status quo in WTP elicitation substantially attenuates treatment effects relative to the WTA frame where first-hand serves as the baseline. This asymmetry suggests that artificial status quo manipulations create reference points that may not reflect genuine preferences, raising concerns about the validity of WTP measures in contexts where the baseline option is not naturally experienced as the default.

Finally, we document surprising null results regarding contextual moderation. Despite linking respondents to detailed administrative data on local environmental conditions—including water scarcity, energy poverty, air quality, and pro-environmental political attitudes—we find no systematic evidence that treatment effects are stronger when information aligns with locally-salient environmental challenges. Similarly, store proximity influences baseline preferences but does not moderate information provision effects. These patterns suggest that environmental information operates through relatively context-independent cognitive mechanisms, such as surprise at the magnitude of environmental impacts, rather than through resonance with lived environmental problems or logistical feasibility considerations.

6.2 Theoretical Contributions

Our findings contribute to several literatures. First, we extend research on environmental information provision and belief updating by demonstrating that the effectiveness of sustainability communication depends critically on how environmental externalities are framed. While prior work has focused primarily on whether information provision works (Allcott and Taubinsky, 2015; ?), we show that what type of information is provided matters substantially. The superiority of water and energy metrics over CO2 aligns with psychological research emphasizing the importance of concreteness and experiential grounding in risk communication: individuals process information more effectively when it connects to familiar, tangible experiences rather than abstract quantities. Our results suggest that the widespread reliance on CO2 metrics in environmental labeling may reflect scientific convention and policy coordination rather than psychological effectiveness.

Second, we contribute to understanding heterogeneity in information treatment effects. The finding that underestimators respond most strongly to information provision is consistent with Bayesian updating models in which larger surprises generate stronger revisions. However, the resistance to updating among those with accurate priors—despite receiving confirmatory information—suggests that confirmation does not necessarily strengthen commitment to existing beliefs in this domain. This pattern contrasts with political contexts where confirmation often reinforces partisan attitudes, potentially reflecting domain differences in motivated reasoning or the relatively low stakes and weak identity associations of clothing consumption.

Third, our results speak to debates about reference dependence and the WTA-WTP gap. The substantial asymmetry we observe between WTA and WTP measures—with treatment effects much stronger for WTA—reinforces concerns that elicitation methods imposing artificial status quos may not reveal genuine preferences. More broadly, the finding that stated intentions align more closely with WTA than WTP when second-hand is artificially imposed as baseline suggests that respondents recognize and resist experimentally manipulated reference points, contrary to some models of endowment effects that predict strong attachment to any designated status quo.

Fourth, the null results on contextual moderation challenge intuitive expectations about when environmental information resonates. The absence of enhanced effects in water-scarce provinces for water information, or in energy-poor regions for energy information, suggests that the salience mechanisms linking information to behavior may operate differently than often assumed. Rather than connecting abstract information to concrete local problems, individuals may respond to the magnitude and surprise value of environmental impacts independent of personal experience. This finding has important implications for theories of information processing and behavioral change, suggesting limits to context-dependent persuasion models.

6.3 Policy Implications

Our findings offer actionable guidance for policymakers and organizations seeking to promote sustainable consumption through information provision. Most directly, the results suggest that shifting from CO₂-based to water- or energy-based environmental messaging could substantially enhance the effectiveness of sustainability communication campaigns. Given that water information generates effects approximately 40% larger than CO₂ (comparing 43% vs. 31% increased stated intention), this represents a meaningful improvement achievable through simple reframing of existing information without requiring additional resources or infrastructure changes.

The superiority of water and energy metrics is particularly relevant for environmental labeling policies. Current regulations in many jurisdictions mandate CO₂ disclosure for consumer products, reflecting policy coordination around climate targets. However, if the goal is to maximize behavioral change—rather than simply providing standardized metrics for carbon accounting—our results suggest that complementing or replacing CO₂ labels with water or energy information could prove more effective. This recommendation applies especially to sectors like fashion and textiles where water and energy consumption are substantial and may be more intuitively meaningful to consumers than greenhouse gas emissions.

That said, an important caveat emerges from our finding that pro-environmental behavior strongly amplifies treatment effects while need for uniqueness operates as a barrier. Information

provision appears most effective among populations already predisposed toward sustainable consumption, raising the challenge of reaching beyond the “already converted.” This pattern suggests diminishing marginal returns to information campaigns as environmental awareness increases, and points to the need for complementary strategies targeting less environmentally engaged consumers. For high-NFU individuals who resist environmental messaging, alternative framings emphasizing individuality, style, and counter-mainstream identity—rather than environmental virtue—may prove more persuasive.

The null results on local environmental condition moderation offer both encouraging and cautionary insights for policy. On one hand, the finding that treatment effects do not vary systematically with local context suggests that standardized national or regional information campaigns can be effective across diverse geographic areas without requiring costly customization. Water-based messaging, for instance, appears equally effective in water-scarce and water-abundant regions, simplifying campaign design and implementation. On the other hand, the absence of enhanced effects when information aligns with locally-salient problems suggests that naive appeals to personal experience (“your region faces water shortages”) may not amplify impact as expected, requiring policymakers to invest in message testing rather than assuming that context-matching will automatically enhance effectiveness.

Finally, our methodological findings regarding WTA/WTP framing have implications for policy evaluation. Researchers and policymakers increasingly use stated preference methods to estimate demand for pro-environmental products or willingness to pay for sustainability attributes. Our results demonstrate that estimates are highly sensitive to which option is framed as the status quo, with artificial baseline manipulations substantially attenuating measured effects. This suggests that WTP measures imposing counter-factual defaults (e.g., asking willingness to pay for conventional products when sustainable alternatives are framed as baseline) may underestimate true preferences, potentially leading to overly pessimistic assessments of demand for sustainable options and underinvestment in green alternatives.

6.4 Limitations and Future Research

Several limitations of our study merit discussion. First, while our sample is large and nationally representative in terms of demographics, it consists of individuals who voluntarily participate in online surveys, potentially selecting for higher education, digital literacy, and perhaps environmental awareness. Although we control for observable demographics, unobserved selection on motivation or attentiveness could affect generalizability. Replication with alternative sampling frames—including in-person recruitment or behavioral observations in retail settings—would strengthen confidence in external validity.

Second, our outcome measures combine stated intentions and incentivized voucher choices, but we do not observe actual second-hand purchasing behavior. While the voucher task is consequential in that respondents make real choices with financial implications, redemption rates and follow-through remain unobserved. It is possible that information provision affects stated preferences and hypothetical choices more strongly than actual consumption, particularly if contextual factors at the point of purchase (e.g., product availability, quality concerns, social signaling) override intentions formed during the experiment. Field experiments tracking actual purchases would

provide valuable evidence on whether information effects persist in naturalistic settings.

Third, our measures of local environmental conditions, while based on official administrative data, are aggregated at provincial or regional levels and may not capture individual-level variation in environmental exposure or awareness. Respondents within the same province face heterogeneous conditions depending on neighborhood, occupation, housing quality, and daily routines. More granular measures—potentially using individual-level survey questions about personal environmental experiences—might reveal contextual moderations that are obscured by geographic aggregation. Additionally, objective indicators like water scarcity days or air quality measurements may not align with subjective perceptions of environmental salience, which could be the more relevant moderator.

Fourth, our experiment provides one-shot information provision, whereas real-world sustainability campaigns involve repeated exposure, social reinforcement, and normative messaging that may compound effects over time. It remains unclear whether the superiority of water and energy information would persist or amplify with repeated exposure, or whether habituation would reduce effectiveness. Longitudinal designs with multiple information interventions would shed light on dynamic treatment effects and optimal messaging frequency.

Fifth, our focus on second-hand clothing in the Italian context limits generalizability to other product categories, geographies, and cultural contexts. The relative effectiveness of water versus energy versus CO2 information may depend on category-specific production processes, cultural attitudes toward used goods, and baseline environmental awareness. Cross-national comparisons and extensions to durable goods, food, or transportation would clarify the boundary conditions of our findings.

Future research should address several open questions. First, understanding why water and energy information outperform CO2 requires process-tracing methods to identify the psychological mechanisms at work. Do these metrics generate stronger emotional responses? Are they easier to visualize or connect to personal experience? Do they reduce cognitive load in processing environmental information? Experimental manipulations isolating these channels—for instance, using think-aloud protocols, physiological measures, or mediation analyses—would illuminate the pathways through which metric choice affects behavior.

Second, the challenge of reaching non-environmental consumers warrants targeted investigation. Our finding that high-NFU individuals resist environmental messaging suggests that one-size-fits-all campaigns may fail to engage important population segments. Developing and testing alternative message frames emphasizing different values—autonomy, status, economic savings, aesthetic appeal—could identify effective strategies for broadening the reach of sustainability communication beyond the already-committed.

Third, the null moderations by local environmental conditions raise questions about when and whether context-matching enhances persuasion. Rather than assuming that water messages work better in water-scarce areas, future research should systematically vary the degree of alignment between message content and local salience, potentially finding thresholds or specific conditions under which context-dependent effects emerge. It may be that only extreme or highly publicized local problems (e.g., acute droughts receiving media coverage) activate the relevance mechanisms we hypothesized.

Fourth, extending our design to actual purchasing behavior through field experiments would

address the most significant limitation of laboratory-style studies. Partnering with retailers to randomize information provision at point of sale, or conducting natural experiments around policy changes in environmental labeling, would provide stronger evidence on real-world effectiveness and identify contextual factors that facilitate or impede translation from intentions to actions.

Finally, our findings on WTA/WTP asymmetries suggest broader questions about preference elicitation in pro-environmental domains. If imposing sustainable options as defaults substantially alters stated valuations, this has implications not only for measurement but for choice architecture interventions. Future work should examine whether making sustainable options the actual default in retail settings—through product placement, pre-selected options in online shopping, or subscription models defaulting to sustainable variants—can overcome the barriers we identify, and whether such defaults face consumer resistance similar to what we observe for high-NFU individuals in our experiment.

6.5 Conclusion

This paper demonstrates that the effectiveness of environmental information provision depends critically on how environmental costs are framed. Water-based information outperforms energy, which in turn outperforms CO₂—the current standard in sustainability communication—across multiple behavioral measures in a large, representative sample. These effects are strongest among individuals who underestimate environmental impacts and those already predisposed toward pro-environmental behavior, while need for uniqueness operates as a significant barrier. Importantly, treatment effects do not vary with local environmental conditions or store proximity, suggesting that information influences behavior through relatively context-independent cognitive processes rather than through resonance with lived problems or logistical considerations.

For policymakers and practitioners, our results offer clear guidance: shifting from CO₂-based to water- or energy-based environmental messaging can substantially enhance the effectiveness of sustainability campaigns promoting second-hand consumption. However, the challenge of reaching consumers beyond those already environmentally engaged remains unresolved, pointing to the need for diversified messaging strategies that appeal to different psychological motivations. More broadly, our findings underscore the importance of psychological considerations in environmental policy design: even when information is accurate and relevant, how it is framed can determine whether it changes behavior.

A Experimental Screens

A.1 Information provision task

Figure 10: Illustration of the quiz task in CO2 treatment

Before answering the next question, we want to inform you of the value of one kilogram of carbon dioxide emitted (CO₂e). 1 kg CO₂e = 6 km driven by a medium-sized gasoline car When you buy a second-hand garment, do you know how much CO₂ emission you can avoid by not producing a new one? About: ____ kg CO₂e
The exact estimate is about 4 kg CO₂e, or about 24 km traveled by a medium-sized gasoline car. Based on this information, are you more or less inclined to buy second-hand in your daily life? ☐ I am more inclined ☐ I am equally inclined ☐ I am less inclined

Figure 11: Illustration of the quiz task in Energy treatment

Before answering the next question, we want to inform you of the value of a kilowatt-hour (kWh) of electricity. 1 kWh = 10 hours of use of a 100-watt bulb When you buy a second-hand garment, do you know how much energy waste you can avoid by not producing a new one? About: ____ kWh
The exact estimate is about 17kWh, or about 170 hours of use of a 100-watt bulb. Based on this information, are you more or less inclined to buy second-hand in your daily life? ☐ I am more inclined ☐ I am equally inclined ☐ I am less inclined

Figure 12: Illustration of the quiz task in Water treatment

Before answering the next question, we want to inform you of the value of ten liters (10ℓ) of water.

$10\ell = 2$ minutes of open kitchen sink

When you buy a second-hand garment, do you know how much water waste you can avoid by not producing a new one?

About: _____ ℓ

The exact estimate is about 340 liters of water or about 17 minutes of running tap.

Based on this information, are you more or less inclined to buy second-hand in your daily life?

- ☐ I am more inclined
- ☐ I am equally inclined
- ☐ I am less inclined

A.2 Voucher choice task

Figure 13: Illustration of the voucher task (WTA)

Below, we present you with five pairs of vouchers of different amount for purchases at two chain stores, the “Mercatino” and “H&M.” The first is a chain of second-hand stores; with the selected voucher, you can buy only clothing. The second is a chain of clothing stores. We ask you to indicate, for each pair, which voucher you would prefer to receive. At the end of the data collection, we will randomly draw 500 participants; for each participant drawn, one of the five pairs will be randomly chosen, and he/she will be emailed the voucher selected in that pair.

Voucher A	Voucher B	Your choice	
			
€ 10	€ 0	☐ Voucher A	☐ Voucher B
€ 10	€ 5	☐ Voucher A	☐ Voucher B
€ 10	€ 10	☐ Voucher A	☐ Voucher B
€ 10	€ 15	☐ Voucher A	☐ Voucher B
€ 10	€ 20	☐ Voucher A	☐ Voucher B

Figure 14: Illustration of the voucher task (WTP)

Below, we present you with five pairs of vouchers of different amount for purchases at two chain stores, the “Mercatino” and “H&M.” The first is a chain of second-hand stores; with the selected voucher, you can buy only clothing. The second is a chain of clothing stores. We ask you to indicate, for each pair, which voucher you would prefer to receive. At the end of the data collection, we will randomly draw 500 participants; for each participant drawn, one of the five pairs will be randomly chosen, and he/she will be emailed the voucher selected in that pair.

Voucher A	Voucher B	Your choice	
			
€ 10	€ 0	☐ Voucher A	☐ Voucher B
€ 10	€ 5	☐ Voucher A	☐ Voucher B
€ 10	€ 10	☐ Voucher A	☐ Voucher B
€ 10	€ 15	☐ Voucher A	☐ Voucher B
€ 10	€ 20	☐ Voucher A	☐ Voucher B

B Additional Tables and Figures

B.1 Balance Table

Table 17: Balance Table: Descriptive Statistics by Treatment and MPL Baseline

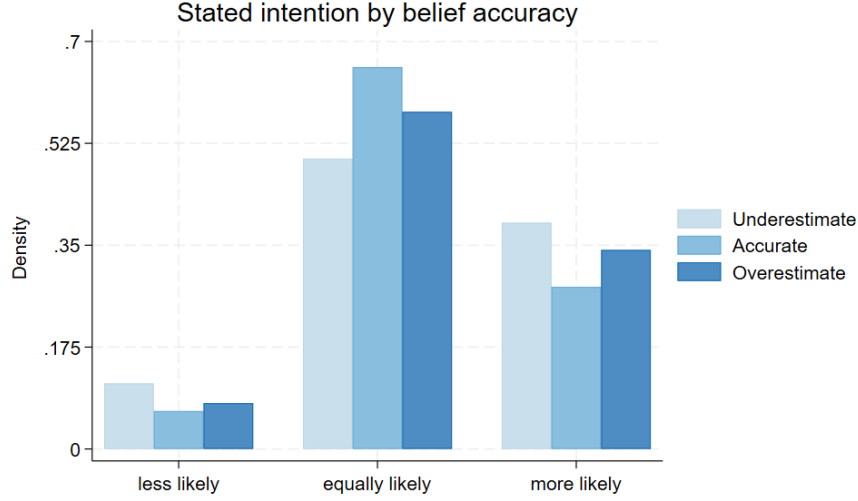
	WTA				WTP				Total
	CO2	Energy	Water	Control	CO2	Energy	Water	Control	
Macroregion									
North West	27.87%	25.91%	27.29%	29.06%	25.97%	27.29%	26.21%	25.89%	26.93%
North East	19.98%	19.82%	18.67%	18.23%	20.96%	20.64%	19.46%	20.30%	19.76%
Center	18.38%	20.05%	21.27%	19.07%	20.05%	18.27%	20.69%	19.25%	19.63%
South	22.51%	23.32%	21.27%	22.96%	21.64%	23.01%	23.14%	24.30%	22.77%
Islands	11.26%	10.90%	11.51%	10.68%	11.39%	10.78%	10.50%	10.26%	10.91%
Age class									
18-25	8.81%	9.38%	8.77%	9.69%	9.57%	8.33%	9.35%	11.40%	9.41%
26-45	30.63%	27.90%	28.81%	28.91%	30.90%	29.89%	28.35%	27.47%	29.11%
46-65	42.88%	40.85%	42.76%	39.82%	40.55%	41.74%	37.39%	40.38%	40.80%
65+	17.69%	21.88%	19.66%	21.59%	18.98%	20.03%	24.90%	20.75%	20.68%
Gender									
Women	50.92%	51.68%	53.73%	47.98%	49.96%	54.20%	50.80%	50.11%	51.17%
Men	49.08%	48.32%	46.27%	52.02%	50.04%	45.80%	49.20%	49.89%	48.83%
Monthly income									
Prefer not to answer	18.22%	17.61%	17.99%	15.56%	18.07%	19.57%	17.01%	17.81%	17.73%
< €1,000	4.21%	6.02%	4.73%	4.81%	3.95%	3.67%	3.98%	4.68%	4.51%
€1,000 - €1,499	11.87%	10.29%	10.82%	11.37%	9.49%	10.17%	11.03%	11.40%	10.80%
€1,500 - €1,999	16.46%	16.01%	16.54%	17.54%	16.02%	16.44%	16.09%	16.00%	16.39%
€2,000 - €2,999	20.83%	24.16%	23.40%	22.96%	24.37%	22.40%	23.75%	22.34%	23.03%
€3,000 - €4,999	18.38%	17.15%	18.22%	18.46%	19.59%	18.81%	18.77%	19.17%	18.57%
€5,000 - €9,999	6.05%	5.79%	5.34%	5.87%	5.24%	4.66%	6.59%	5.51%	5.63%
> €10,000	3.98%	2.97%	2.97%	3.43%	3.26%	4.28%	2.76%	3.09%	3.34%
Education									
Middle School	7.43%	10.14%	8.31%	8.77%	7.37%	7.57%	9.12%	10.57%	8.66%
High School	57.66%	55.41%	53.20%	55.23%	53.38%	54.97%	52.87%	53.58%	54.54%
University	30.86%	29.42%	33.23%	30.82%	33.94%	31.88%	32.64%	31.17%	31.75%
PhD or equivalent	4.06%	5.03%	5.26%	5.19%	5.32%	5.58%	5.36%	4.68%	5.06%
Job occupation									
Worker (blue collar)	7.35%	8.92%	7.24%	7.17%	6.91%	7.34%	7.66%	7.32%	7.49%
Employed	43.26%	36.59%	42.00%	42.64%	42.37%	42.28%	37.55%	38.42%	40.63%

Self-employed	11.64%	11.20%	11.81%	10.22%	12.45%	10.70%	12.41%	13.06%	11.69%
Student	5.36%	6.40%	6.17%	5.64%	6.38%	5.89%	5.59%	7.55%	6.13%
Homemaker	9.42%	9.30%	7.55%	8.54%	8.43%	10.09%	7.89%	9.66%	8.86%
Retired	15.47%	19.21%	17.76%	18.92%	16.40%	16.97%	21.61%	18.26%	18.07%
Looking for a job	5.36%	7.39%	5.64%	5.57%	5.62%	4.36%	5.21%	3.92%	5.38%
Other	2.14%	0.99%	1.83%	1.30%	1.44%	2.37%	2.07%	1.81%	1.74%
Living in a ...									
urban environment	76.65%	76.60%	76.07%	75.51%	75.47%	77.45%	75.40%	75.62%	76.10%
rural environment	23.35%	23.40%	23.93%	24.49%	24.53%	22.55%	24.60%	24.38%	23.90%
Ppl in residence									
< 10,000	17.23%	18.98%	16.23%	17.47%	17.46%	18.96%	16.86%	18.34%	17.69%
10,000 - 50,000	27.57%	28.73%	28.43%	28.99%	28.09%	26.83%	29.04%	27.02%	28.09%
50,000 - 250,000	35.60%	34.38%	34.07%	34.55%	35.69%	33.33%	34.48%	33.36%	34.43%
> 250,000	19.60%	17.91%	21.27%	18.99%	18.75%	20.87%	19.62%	21.28%	19.79%
Households size									
People in hh (avg)	2.94	2.82	2.84	2.86	2.84	2.91	2.80	2.78	2.85
Minors in hh (avg)	0.47	0.42	0.43	0.45	0.46	0.44	0.43	0.42	0.44

Note: Descriptive statistics by treatment assignment and MPL baseline (WTA with first-hand baseline; WTP with second-hand baseline). Columns report percentages for categorical variables and means for continuous variables. Sample includes all respondents who completed the respective voucher task ($N_{WTA} = 5,241$; $N_{WTP} = 5,255$).

B.2 Intention by belief accuracy

Figure 15: Intention by belief accuracy



Note: Histogram showing the distribution of stated intention changes (less likely, equally likely, more likely to adopt second-hand) across belief accuracy categories (Underestimate, Accurate, Overestimate). Belief accuracy determined using $\pm 15\%$ tolerance interval around true values. Density represents the proportion of respondents in each intention category within each belief accuracy group.

B.3 Intention by treatment

Table 18: Intention Distribution by Treatment

Intention	Treatment		
	Water	Energy	CO2
Less likely	284 (10.85%)	249 (9.50%)	239 (9.11%)
Equally likely	1207 (46.12%)	1447 (55.23%)	1578 (60.16%)
More likely	1126 (43.03%)	924 (35.27%)	806 (30.73%)
Total	2617 (100%)	2620 (100%)	2623 (100%)

Note: Cross-tabulation of stated intention changes (less likely, equally likely, more likely to adopt second-hand) across treatment conditions (Water, Energy, CO2). Percentages represent row proportions within each treatment condition.

B.4 Regression analysis: Belief outliers

All analyses reported in this section exclude outliers in belief values, identified using Tukey's rule (observations falling below $Q1 - 1.5 \times IQR$ or above $Q3 + 1.5 \times IQR$).

Table 19: Ordered Logistic Regression on Change in Intention to Purchase SH

	(1)	(2)	(3)
Delta Belief	-0.025 (0.020)	0.050** (0.023)	0.023 (0.019)
Water		0.476*** (0.080)	0.383*** (0.064)
Energy		0.186*** (0.070)	0.133** (0.060)
Pro-environmental Behavior			0.766*** (0.031)
Need for Uniqueness			-0.303*** (0.029)
Cut 1	-2.167*** (0.037)	-1.974*** (0.055)	-0.529*** (0.151)
Cut 2	0.553*** (0.025)	0.763*** (0.060)	2.430*** (0.157)
Observations	6698	6698	6698

Note: Ordered logit regression estimation with bootstrap standard errors. Dependent variable. *Change in intention to purchase more SH clothes*: categorical variables ranging from -1 to $+1$ (3 categories). Regressors. *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value over the true value. *Water* dummy variables equal 1 if the treatment is Water, 0 otherwise. *Energy* dummy variables equal 1 if the treatment is Energy, 0 otherwise. Baseline *CO2*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 20: Tobit regression estimations of WTA for second-hand

	(1)	(2)	(3)	(4)	(5)	(6)
Water	-4.215*** (0.586)	-4.170*** (0.771)	-3.791*** (0.672)	-1.774* (0.977)	-1.646** (0.762)	-0.986 (0.849)
Energy	-3.198*** (0.709)	-3.323*** (0.784)	-2.292*** (0.699)	-1.404* (0.798)	-1.299* (0.685)	0.081 (0.823)
CO2	-2.187*** (0.676)	-2.442*** (0.791)	-2.606*** (0.637)			
Pro-environmental Behavior		-2.558*** (0.303)	-3.026*** (0.382)		-1.070** (0.428)	-1.736*** (0.389)
Need for Uniqueness		2.246*** (0.288)	2.118*** (0.432)		1.543*** (0.398)	1.309*** (0.378)
SH Closest			1.399*** (0.489)			1.056* (0.623)
FH Distance (km)			0.003 (0.016)			-0.013 (0.014)
SH Distance (km)			-0.011 (0.013)			-0.002 (0.012)
Delta Belief				-0.521* (0.287)	-0.456** (0.230)	-0.325 (0.263)
More likely (intention)				-8.338*** (0.685)	-7.442*** (0.529)	-7.221*** (0.640)
Less likely (intention)				2.086** (0.989)	1.841 (1.310)	4.259*** (1.253)
Constant	7.667*** (0.549)	9.678*** (1.571)	10.906*** (2.122)	8.187*** (0.665)	6.828*** (2.037)	8.755*** (2.083)
Observations	4670	4670	4517	3359	3359	3231

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 21: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)	(4)	(5)	(6)
Water	1.538*** (0.573)	1.310*** (0.505)	1.320** (0.610)	-0.291 (0.631)	-0.268 (0.503)	-0.427 (0.587)
Energy	0.960 (0.599)	0.761 (0.573)	0.756 (0.587)	-0.576 (0.620)	-0.593 (0.504)	-0.690 (0.473)
CO2	1.325** (0.608)	1.168** (0.552)	1.234** (0.521)			
Pro-environmental Behavior		3.446*** (0.314)	3.288*** (0.311)		2.546*** (0.336)	2.529*** (0.322)
Need for Uniqueness		0.426* (0.228)	0.352 (0.279)		0.812*** (0.294)	0.788*** (0.264)
SH Closest			0.078 (0.457)			0.014 (0.487)
FH Distance (km)			-0.005 (0.016)			-0.003 (0.013)
SH Distance (km)			-0.003 (0.011)			-0.001 (0.012)
Delta Belief				0.034* (0.019)	0.027 (0.019)	0.028* (0.016)
More likely (intention)				5.836*** (0.376)	4.934*** (0.414)	5.016*** (0.501)
Less likely (intention)				-5.875*** (0.814)	-5.517*** (0.975)	-5.755*** (1.080)
Constant	-4.313*** (0.432)	-16.971*** (1.440)	-16.056*** (1.644)	-4.445*** (0.477)	-15.093*** (1.769)	-14.796*** (1.436)
Observations	4898	4898	4757	3644	3644	3539

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.5 Regression analysis: Time outliers

All analyses reported in this section exclude outliers in response times, identified using Tukey's rule (observations falling below $Q1 - 1.5 \times IQR$ or above $Q3 + 1.5 \times IQR$).

Table 22: Ordered Logistic Regression on Change in Intention to Purchase SH

	(1)	(2)	(3)
Delta Belief	-0.003*	-0.001	-0.004***
	(0.002)	(0.002)	(0.001)
Water		0.362***	0.330***
		(0.055)	(0.062)
Energy		0.136**	0.089*
		(0.054)	(0.052)
Pro-environmental Behavior			0.768***
			(0.035)
Need for Uniqueness			-0.315***
			(0.030)
Cut 1	-2.225***	-2.066***	-0.628***
	(0.040)	(0.053)	(0.153)
Cut 2	0.572***	0.743***	2.412***
	(0.026)	(0.044)	(0.153)
Observations	7283	7283	7283

Note: Ordered logit regression estimation with bootstrap standard errors. Dependent variable. *Change in intention to purchase more SH clothes*: categorical variables ranging from -1 to $+1$ (3 categories). Regressors. *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value over the true value. *Water* dummy variables equal 1 if the treatment is Water, 0 otherwise. *Energy* dummy variables equal 1 if the treatment is Energy, 0 otherwise. Baseline *CO2*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 23: Tobit regression estimations of WTA for second-hand

	(1)	(2)	(3)	(4)	(5)	(6)
Water	-4.297*** (0.594)	-4.335*** (0.693)	-3.982*** (0.554)	-1.333 (0.897)	-1.328** (0.633)	-0.911 (0.650)
Energy	-3.128*** (0.696)	-3.103*** (0.604)	-2.152*** (0.634)	-0.693 (0.907)	-0.590 (0.792)	0.508 (0.668)
CO2	-2.393*** (0.729)	-2.587*** (0.660)	-2.714*** (0.697)			
Pro-environmental Behavior		-2.551*** (0.294)	-3.040*** (0.395)		-1.053*** (0.384)	-1.696*** (0.347)
Need for Uniqueness		2.081*** (0.309)	1.983*** (0.335)		1.479*** (0.342)	1.291*** (0.365)
SH Closest			1.482** (0.592)			1.124* (0.635)
FH Distance (km)			-0.002 (0.017)			-0.017 (0.017)
SH Distance (km)			-0.009 (0.013)			-0.002 (0.016)
Delta Belief				-0.047* (0.024)	-0.042 (0.026)	-0.039 (0.024)
More likely (intention)				-7.863*** (0.478)	-6.989*** (0.581)	-6.800*** (0.570)
Less likely (intention)				2.097** (1.057)	1.836 (1.183)	4.063*** (1.044)
Constant	7.468*** (0.489)	9.959*** (1.543)	11.155*** (1.992)	7.544*** (0.626)	6.381*** (1.594)	8.258*** (1.672)
Observations	4867	4867	4710	3639	3639	3505

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 24: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)	(4)	(5)	(6)
Water	1.538*** (0.577)	1.310** (0.566)	1.320*** (0.471)	-0.291 (0.554)	-0.268 (0.670)	-0.427 (0.519)
Energy	0.960* (0.574)	0.761 (0.615)	0.756 (0.500)	-0.576 (0.644)	-0.593 (0.582)	-0.690 (0.484)
CO2	1.325** (0.592)	1.168** (0.536)	1.234** (0.505)			
Pro-environmental Behavior		3.446*** (0.281)	3.288*** (0.227)		2.546*** (0.365)	2.529*** (0.330)
Need for Uniqueness		0.426 (0.261)	0.352 (0.293)		0.812** (0.335)	0.788*** (0.284)
SH Closest			0.078 (0.499)			0.014 (0.573)
FH Distance (km)			-0.005 (0.016)			-0.003 (0.016)
SH Distance (km)			-0.003 (0.012)			-0.001 (0.012)
Delta Belief				0.034** (0.015)	0.027* (0.014)	0.028* (0.015)
More likely (intention)				5.836*** (0.440)	4.934*** (0.452)	5.016*** (0.447)
Less likely (intention)				-5.875*** (0.769)	-5.517*** (0.896)	-5.755*** (0.871)
Constant	-4.313*** (0.407)	-16.971*** (1.432)	-16.056*** (1.163)	-4.445*** (0.466)	-15.093*** (1.911)	-14.796*** (1.678)
Observations	4898	4898	4757	3644	3644	3539

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand).. Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.6 Regression analysis: Complete specifications

All analyses reported in this section include the full sample with complete model specifications controlling for sociodemographic variables, individual characteristics, and geographic fixed effects, as detailed in the regression tables.

Note on Table 25: Ordered logit regression estimation with bootstrap standard errors. Dependent variable. *Change in intention to purchase more SH clothes*: categorical variables ranging from -1 to $+1$ (3 categories). Regressors. *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value over the true value. *Water* dummy variables equal 1 if the treatment is Water, 0 otherwise. *Energy* dummy variables equal 1 if the treatment is Energy, 0 otherwise. Baseline *CO2*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *Age*: continuous variable in years. *Male*: dummy equal to 1 for male respondents, 0 otherwise. *Income*: categorical variable with baseline €2,000-€3,000. *Job*: categorical variable with baseline "Employee (white collar)". *Education*: categorical variable with baseline "High School". *Rural context*: dummy equal to 1 if respondent lives in rural area, 0 otherwise. *Population*: categorical variable for municipality size, with baseline 50,000-250,000. *People in the HH* and *Minor in the HH*: number of household members and minors, respectively.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note on Table 26: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between $-\text{€}12.5$ and $\text{€}12.5$, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest* is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type. *Age*: continuous variable in years. *Male*: dummy equal to 1 for male respondents, 0 otherwise. *Income*: categorical variable with baseline €2,000-€3,000. *Job*: categorical variable with baseline "Employee (white collar)". *Education*: categorical variable with baseline "High School". *Rural context*: dummy equal to 1 if respondent lives in rural area, 0 otherwise. *Population*: categorical variable for municipality size, with baseline 50,000-250,000. *People in the HH* and *Minor in the HH*: number of household members and minors, respectively.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note on Table 27: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTP*, variable ranging between $-\text{€}12.5$ and $\text{€}12.5$, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control*. *Pro-environmental Behavior* and *Need for Uniqueness*: standardized scales. *SH Closest*

is a dummy variable equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code, 0 otherwise. *FH Distance (km)* and *SH Distance (km)* are continuous variables measuring driving distance from the respondent's postal code to the closest store of each type. *Age*: continuous variable in years. *Male*: dummy equal to 1 for male respondents, 0 otherwise. *Income*: categorical variable with baseline €2,000-€3,000. *Job*: categorical variable with baseline "Employee (white collar)". *Education*: categorical variable with baseline "High School". *Rural context*: dummy equal to 1 if respondent lives in rural area, 0 otherwise. *Population*: categorical variable for municipality size, with baseline 50,000-250,000. *People in the HH* and *Minor in the HH*: number of household members and minors, respectively.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 25: Ordered Logistic Regression on Change in Intention to Purchase SH

	(1)
Delta Belief	-0.005*** (0.002)
Water	0.366*** (0.053)
Energy	0.120** (0.059)
Pro-environmental Behavior	0.793*** (0.040)
Need for Uniqueness	-0.247*** (0.030)
Age	-0.010*** (0.003)
Male	-0.042 (0.041)
Income (no answer)	-0.441*** (0.082)
Income (<€1,000)	-0.571*** (0.117)
Income (€1,000 - €1,500)	-0.171* (0.100)
Income (€1,500 - €2,000)	-0.120 (0.082)
Income (€3,000 - €5,000)	-0.079 (0.065)
Income (€5,000 - €10,000)	-0.373*** (0.121)
Income (>€10,000)	0.131 (0.173)
Job: Worker (blue collar)	-0.098 (0.079)
Job: Self-employed	-0.048 (0.088)
Job: Student	-0.223** (0.104)
Job: Homemaker	-0.092 (0.086)
Job: Retired	-0.227*** (0.086)
Job: Looking for a job	-0.251** (0.112)
Job: Other	-0.050 (0.187)
Education: Middle School (or lower)	-0.269*** (0.085)
Education: University	-0.005 (0.045)
Education: PhD	-0.287** (0.137)
Rural context	0.072 (0.076)
Population (< 10,000)	-0.042 (0.092)
Population (10,000 - 50,000)	-0.062 (0.062)
Population (> 250,000)	-0.059 (0.067)
People in the HH	0.023 (0.017)
Minors in the HH	0.105*** (0.036)
Cut 1	-1.098*** (0.192)
Cut 2	1.985*** (0.189)
Observations	7860

Table 26: Tobit regression estimations of WTA for second-hand

	(1)	(2)	(3)	(4)
Water	-3.953*** (0.608)	-3.976*** (0.670)	-0.759 (0.659)	-0.794 (0.714)
Energy	-2.311*** (0.670)	-2.283*** (0.548)	0.420 (0.612)	0.506 (0.595)
CO2	-2.820*** (0.722)	-2.809*** (0.725)		
Delta Belief			-0.042** (0.019)	-0.039* (0.023)
More likely (intention)			-6.871*** (0.615)	-6.681*** (0.588)
Less likely (intention)			4.384*** (0.970)	4.338*** (1.167)
Need for Uniqueness	2.078*** (0.313)	1.828*** (0.314)	1.314*** (0.334)	1.210*** (0.378)
Pro-environmental Behavior	-2.967*** (0.378)	-3.416*** (0.322)	-1.665*** (0.376)	-2.086*** (0.472)
SH Closest	1.475*** (0.569)	1.497*** (0.476)	1.192 (0.746)	1.199 (0.759)
FH Distance (km)	0.004 (0.020)	-0.000 (0.015)	-0.008 (0.023)	-0.009 (0.022)
SH Distance (km)	-0.008 (0.015)	-0.004 (0.013)	0.000 (0.016)	0.002 (0.017)
Age		0.075*** (0.025)		0.054** (0.026)
Male		-1.741*** (0.522)		-2.080*** (0.528)
Income (no answer)		0.742 (0.876)		0.242 (0.847)
Income (<€1,000)		1.530 (1.139)		0.418 (1.172)
Income (€1,000 - €1,500)		0.050 (0.796)		-0.656 (0.936)
Income (€1,500 - €2,000)		-0.032 (0.800)		-0.080 (0.884)
Income (€3,000 - €5,000)		0.722 (0.814)		0.185 (0.850)
Income (€5,000 - €10,000)		1.537 (1.067)		1.647 (1.258)
Income (> €10,000)		0.105 (1.417)		-0.384 (1.540)
Job: Worker (blue collar)		-1.947** (0.984)		-2.532*** (0.878)
Job: Self-employed		0.494 (0.889)		0.429 (0.826)
Job: Student		-1.753* (1.061)		-2.414** (1.179)
Job: Homemaker		-1.493** (0.706)		-1.982 (1.229)
Job: Retired		-0.882 (0.927)		-1.156 (0.894)
Job: Looking for a job		-1.609 (1.011)		-1.452 (1.108)
Job: Other		-1.609 (2.187)		-0.807 (2.134)
Education: Middle School (or lower)		0.643 (0.909)		0.180 (1.235)
Education: University		-0.145 (0.508)		0.024 (0.531)
Education: PhD		-1.625 (1.362)		-3.107** (1.449)
Rural context		-0.866 (0.781)		-0.390 (0.727)
Population (< 10,000)		1.593 (0.975)		0.866 (0.985)
Population (10,000 - 50,000)		1.437** (0.632)		1.377** (0.694)
Population (> 250,000)		0.965 (0.668)		0.702 (0.667)
People in the HH		0.009 (0.179)		-0.059 (0.279)
Minor in the HH		-0.250 (0.278)		-0.142 (0.341)
Constant	10.692*** (1.761)	9.735*** (2.135)	7.931*** (1.537)	8.354*** (2.706)
Observations	5074	5074	3788	3788

Table 27: Tobit regression estimations of WTP for first-hand

	(1)	(2)	(3)	(4)
Water	1.316*** (0.444)	1.415*** (0.502)	-0.211 (0.496)	-0.229 (0.426)
Energy	0.547 (0.480)	0.677 (0.520)	-0.706 (0.446)	-0.670 (0.424)
CO2	1.034** (0.471)	1.077** (0.489)		
Delta Belief			0.029** (0.015)	0.027* (0.015)
More likely (intention)			4.997*** (0.491)	5.025*** (0.444)
Less likely (intention)			-5.278*** (1.061)	-5.258*** (0.768)
Need for Uniqueness	0.314 (0.243)	0.442* (0.246)	0.754*** (0.258)	0.726** (0.295)
Pro-environmental Behavior	3.326*** (0.258)	3.518*** (0.281)	2.547*** (0.341)	2.633*** (0.314)
SH Closest	0.060 (0.438)	0.152 (0.496)	0.077 (0.434)	0.221 (0.477)
FH Distance (km)	-0.001 (0.014)	0.002 (0.013)	0.001 (0.014)	0.008 (0.015)
SH Distance (km)	-0.007 (0.010)	-0.009 (0.012)	-0.004 (0.010)	-0.006 (0.010)
Age		-0.079*** (0.019)		-0.057*** (0.021)
Male		0.290 (0.370)		0.185 (0.421)
Income (no answer)		-2.418*** (0.635)		-1.880*** (0.711)
Income (<€1,000)		-0.807 (0.952)		1.313 (1.395)
Income (€1,000 - €1,500)		-1.622** (0.734)		-1.462* (0.811)
Income (€1,500 - €2,000)		-0.212 (0.590)		-0.180 (0.585)
Income (€3,000 - €5,000)		-0.318 (0.593)		-0.193 (0.695)
Income (€5,000 - €10,000)		-0.431 (0.999)		0.267 (0.883)
Income (> €10,000)		-0.033 (1.282)		0.775 (1.371)
Job: Worker (blue collar)		1.049 (0.842)		0.855 (0.747)
Job: Self-employed		1.233* (0.678)		1.639** (0.654)
Job: Student		1.202 (0.872)		0.964 (0.865)
Job: Homemaker		-0.171 (0.783)		0.206 (0.853)
Job: Retired		0.822 (0.891)		1.591** (0.720)
Job: Looking for a job		-1.481 (1.004)		-1.953** (0.993)
Job: Other		4.151*** (1.449)		2.656 (1.741)
Education Middle School (or lower)		-0.073 (0.686)		-0.723 (0.751)
Education University		-0.426 (0.393)		-0.617 (0.516)
Education PhD		-0.851 (0.771)		-0.878 (0.806)
Rural context		0.261 (0.558)		0.456 (0.599)
Population (< 10,000)		-0.147 (0.690)		-0.878 (0.787)
Population (10,000 - 50,000)		-0.906** (0.408)		-1.375** (0.603)
Population (> 250,000)		0.087 (0.605)		0.107 (0.680)
People in the HH		-0.179 (0.127)		-0.101 (0.162)
Minors in the HH		-0.794*** (0.279)		-1.086*** (0.388)
Constant	-15.947*** (1.407)	-11.758*** (1.587)	-14.953*** (1.589)	-11.229*** (1.993)
Observations	5105	5105	3817	3817

B.7 WTA and WTP by macroregion

Table 28: Tobit regression estimations of WTA for second-hand

	North West		North East		Center		South		Islands	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Water	-3.279** (1.281)	-1.061 (1.290)	-4.697*** (1.810)	-0.602 (1.463)	-5.517*** (1.455)	-0.083 (1.280)	-3.829*** (1.452)	-2.928* (1.699)	-4.718** (2.122)	-0.679 (2.318)
Energy	-2.917*** (1.118)	-1.282 (1.464)	-3.089** (1.433)	-0.072 (1.441)	-2.882* (1.490)	1.796 (1.333)	-2.252 (1.488)	-1.092 (1.404)	-7.212*** (2.446)	-4.151* (2.208)
CO2	-1.779 (1.343)		-3.345** (1.506)		-4.266*** (1.516)		-0.910 (1.591)		-3.275 (2.308)	
Delta Belief		-0.105 (0.064)		-0.054 (0.047)		-0.021 (0.088)		-0.023 (0.047)		-0.082 (0.077)
More likely (intention)		-9.291*** (1.023)		-7.426*** (1.188)		-7.051*** (1.219)		-7.536*** (1.215)		-8.035*** (2.341)
Less likely (intention)		3.416* (1.928)		0.920 (2.224)		5.903** (2.771)		6.271*** (2.005)		-6.230* (3.564)
Constant	7.797*** (0.888)	8.658*** (1.176)	7.317*** (1.288)	6.519*** (1.268)	7.442*** (1.068)	4.989*** (1.146)	6.881*** (0.989)	8.240*** (1.279)	9.811*** (1.620)	10.144*** (2.056)
Observations	1443	1062	1005	766	1032	780	1180	879	581	441

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower WTA (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control* in column (1), *CO2* in columns (2)-(3). *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value divided by the true value. *More likely (intention)* is a dummy variable equal to 1 if the respondent reported being more likely to purchase second-hand after information provision, 0 otherwise. *Less likely (intention)* is a dummy variable equal to 1 if the respondent reported being less likely to purchase second-hand after information provision, 0 otherwise.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 29: Tobit regression estimations of WTP for first-hand

	North West		North East		Center		South		Islands	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Water	1.578 (0.970)	-0.990 (1.049)	0.168 (1.129)	-1.046 (1.429)	1.271 (1.403)	0.067 (1.037)	3.139*** (1.039)	1.514 (1.279)	1.290 (1.865)	0.937 (1.558)
Energy	1.442 (1.018)	-0.727 (1.039)	-0.194 (1.201)	-1.280 (1.259)	0.718 (1.323)	0.278 (1.138)	1.172 (1.221)	-0.456 (1.453)	-0.098 (2.110)	-0.558 (1.615)
CO2	2.030** (0.967)		0.688 (1.113)		0.232 (1.311)		1.361 (1.102)		0.361 (1.758)	
Delta Belief		0.023 (0.018)		0.037 (0.067)		0.018 (0.026)		0.065 (0.064)		0.074 (0.054)
More likely (intention)		6.460*** (0.806)		6.552*** (0.917)		5.137*** (0.953)		6.300*** (0.929)		3.238** (1.519)
Less likely (intention)		-3.585** (1.616)		-10.594*** (2.854)		-6.718*** (2.025)		-5.329*** (1.718)		-0.218 (2.695)
Constant	-4.514*** (0.660)	-4.161*** (0.737)	-3.586*** (0.794)	-4.101*** (0.900)	-3.024*** (0.832)	-3.937*** (0.895)	-5.416*** (0.725)	-6.215*** (1.010)	-4.175*** (1.368)	-5.084*** (1.532)
Observations	1384	1041	1069	800	1028	773	1210	888	564	428

Note: Tobit regression estimation with bootstrap standard errors. Dependent variable: *WTA*, variable ranging between -€12.5 and €12.5, computed as the midpoint of the interval in which the respondent shifts from one option to the other in the multiple price list, normalized as the difference from the baseline value (i.e., minus €10). Negative values indicate lower *WTA* (greater willingness to accept second-hand). Regressors: *Water*, *Energy*, and *CO2* are dummy variables equal to 1 if the respective treatment was administered, 0 otherwise. Baseline: *Control* in column (1), *CO2* in columns (2)-(3). *Delta Belief* denotes the standardized distance between the belief and the true value, computed as the difference between the belief and the true value divided by the true value. *More likely (intention)* is a dummy variable equal to 1 if the respondent reported being more likely to purchase second-hand after information provision, 0 otherwise. *Less likely (intention)* is a dummy variable equal to 1 if the respondent reported being less likely to purchase second-hand after information provision, 0 otherwise.

Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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