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Experimental Evidence on Circular Economy

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Abstract

When it comes to making pro-environmental choices, consumers often lack accurate information about the consequences of what they buy, or hold perceptions of themselves and others that systematically distort their decisions. This dissertation investigates these two barriers, informational and perceptual, within the framework of the circular economy, focusing on how consumers can be encouraged to prevent waste and to reuse existing goods rather than buy new ones. Across three chapters, it does so through experiments conducted both in the field and with representative samples of the population, allowing the resulting evidence to speak to real-world policy design rather than only to laboratory behavior.

The first chapter turns to waste prevention, testing whether EU-style date labeling (best-before versus use-by) shapes how consumers value perishable food as it approaches its labeled expiry date, using an in-store field experiment. Consumers respond strongly to the date itself, but not to the type of label attached to it. Educating consumers about the meaning of the two labels has an asymmetric effect: it helps meet the health and safety objective of discouraging consumption of potentially unsafe food, but does little to reduce the waste of food that remains perfectly usable.

The second and third chapters turn to reuse, studying what drives the adoption of second-hand goods through two linked designs fielded on the same nationally representative Italian sample. The second chapter asks how self-image (private self-evaluation), social image (others' view), and second-order self-image (beliefs about others' private views) shape adoption across four dimensions: financial, social approval, environmental, and hygiene. Respondents evaluate a randomized vignette varying product, condition, and price, report their adoption probability, and answer balanced 4-point image items. Contrary to the prevailing literature on image, self-image is consistently a stronger predictor of adoption intentions than both social image and second-order self-image.

The third chapter asks whether informing consumers about the environmental cost of a new garment, expressed in terms of CO2 emissions, electricity use, or water use, shifts both stated intentions and incentivized choices toward second-hand alternatives. The specific metric used matters: water-based information produces the largest effect on both intentions and voucher choices, with results consistent across WTA and WTP framings, though effect sizes vary with the designated baseline. Respondents' own pro-environmental orientation and need for uniqueness are independently associated with intentions and choices, in opposite directions, while local environmental conditions and store proximity do not systematically moderate the results – suggesting that relatable, well-chosen metrics such as water can nudge second-hand adoption fairly uniformly across contexts.

Together, the three chapters show that expanding the circular economy is less a matter of providing more information than of providing the right information, matched to the specific informational or perceptual barrier that stands between favorable attitudes and sustainable choices.

Contents

Introduction

Waste generated at the point of final consumption is, in economic terms, a puzzle: it represents value that was created, purchased, and then destroyed before it was fully used. This challenge is increasingly framed within the paradigm of the *circular economy*, which aims to decouple economic growth from resource depletion by keeping products, materials, and resources in use for as long as possible (Ekins et al., 2020). Unlike the traditional linear model of production and consumption, a circular economy emphasizes strategies such as reuse, repair, remanufacturing, and recycling to minimize waste and reduce the extraction of virgin resources (Congiu et al., 2025). The European Union has placed the circular economy at the centre of its policy agenda, notably through the Waste Framework Directive, which establishes a waste management hierarchy that prioritises prevention and reuse over recycling and disposal.[1]

Two categories of consumer goods illustrate the scale of this puzzle particularly starkly, on a European level: food and textile waste[2]. In the EU, over 58 million tonnes of food waste are generated annually, around 129 kg per inhabitant, with an associated market value estimated at €132 billion (European Commission, 2023; Eurostat, 2025). This occurs while almost 42 million people in the EU cannot afford a quality meal every second day (Eurostat, 2023). Households generate more than half of this total (53%, or 69 kg per inhabitant), with the remainder arising upstream in the supply chain (Eurostat, 2025).

The clothing sector tells a parallel story, on a European scale. In 2020, the EU-27 generated an estimated 6.95 million tonnes of textile waste, around 16 kg per per-

[1] See https://environment.ec.europa.eu/topics/waste-and-recycling/waste-framework-directive_en.

[2] These sectors are so central to the EU policy agenda that the 2023 revision of the Waste Framework Directive, a proposal for a Directive of the European Parliament and of the Council amending Directive 2008/98/EC on waste, 2023/0234(COD) was built around exactly these two sectors. The proposal is available at: https://www.europarl.europa.eu/RegData/etudes/BRIE/2023/753168/EPRS_BRI(2023)753168_EN.pdf

son, of which only 4.4 kg per person was collected separately for reuse and recycling, while 11.6 kg per person ended up in mixed household waste destined largely for incineration or landfill (Deckers et al., 2024). Eighty-two percent of this total was post-consumer waste, generated directly by households rather than by manufacturing or unsold stock. While mandatory separate collection of textiles is already in place in more than half of the EU-27 Member States, this mostly captures reusable garments rather than waste destined for recycling, and without a corresponding scale-up in sorting and recycling capacity, a large share of what is collected separately risks ending up incinerated, landfilled, or exported outside the EU regardless. The European Environment Agency itself points to the harmonization of definitions and mandatory reporting on used and waste textiles as a precondition for setting meaningful future targets and monitoring the sector's progress toward circularity.

These figures matter for economics for three distinct reasons. First, they represent a resource-allocation failure with a clear efficiency cost: goods embodying real production, labor, and environmental inputs are discarded while still capable of delivering utility. Second, unlike many environmental externalities, waste at the consumption stage is proximately driven by individual choice, what to buy, what to keep, what to discard, ...which makes it, at least in principle, sensible to demand-side policy instruments such as labeling, information campaigns, and framing interventions, rather than only supply-side regulation, though recent work cautions against treating consumers as an undifferentiated category in circular-economy research (Deutz, 2025). Third, and most importantly for this dissertation, the same behavioral regularities that make waste persist despite widely shared pro-environmental attitudes – inattention, social-image concerns, and the gap between stated preferences and incentivized choices – recur across otherwise very different product categories. This suggests that circular consumption is not merely a food problem or a fashion problem, but an instance of a more general behavioral phenomenon: consumers are frequently willing, in principle, to make more sustainable choices, yet fail to do so in practice because of how information is presented and how choices are perceived, by themselves and by others. Understanding these two channels, namely information and perception, and how they causally shape consumption decisions is the task this dissertation sets for itself.

Throughout this dissertation, I use *informational barriers* to refer to frictions in how consumers acquire, interpret, or act on objective facts about a good or its external effects: for instance, when a label is misread, when its meaning is unclear, or when an accurate figure (e.g., an environmental cost expressed in CO2, energy, or water) is more or less actionable depending on how it is framed, even though the underlying fact does not change. I use *perceptual barriers*, by contrast, to refer to frictions rooted not in the information itself but in how consumers believe a choice reflects on who they are, or on how they are seen by others, including systematic misperceptions of what others privately think or do. This broad distinction echoes recent conceptual reviews of the sustainable-consumption literature, which similarly separate market, informational, and psychological obstacles to sustainable choice (Andrade & Vieites, 2025), and is closely related to typologies of behavioral barriers to circularity more specifically (Congiu et al., 2025). The distinction is analytically useful because the two barriers call for different remedies, better-designed information versus corrected beliefs about oneself or others, even though, as Chapter 3 shows, they can also operate jointly within a single decision.

A large body of research documents that consumers hold broadly favorable attitudes toward waste reduction and sustainable consumption, yet these attitudes translate only weakly and unevenly into behavior (Dablander et al., 2025) – the so-called attitude-behavior gap. Two complementary explanations for this gap organize the literature this dissertation builds on.

The first explanation centers on *informational barriers*. A substantial literature has documented that consumers misunderstand or misuse the information provided to them at the point of purchase: date labels are frequently confused with safety indicators regardless of their intended meaning (Neff et al., 2019; Wilson et al., 2017), and self-reported survey evidence has long suggested that unclear labeling contributes to unnecessary discarding (Collart & Interis, 2018; Van der Werf et al., 2018). A parallel literature on environmental information provision shows that consumers routinely misperceive the environmental impact of the goods they buy and that different metrics are not processed equivalently, even when they describe the same underlying externality (Camilleri & Larrick, 2014; Camilleri et al., 2019). What is largely missing from both literatures is *causal* evidence, obtained outside

the laboratory, on how specific features of information affect revealed, incentivized valuation rather than stated attitudes or self-reported behavior (see Haaland et al., 2023, for a discussion of the general challenges of causal identification in information-provision experiments).

The second explanation centers on *perceptual barriers*. Consumption decisions, particularly for visible or reputationally-charged categories such as second-hand goods, are shaped not only by what a good costs or provides, but by what owning or displaying it communicates to oneself (self-image) and to others (social image) (Bénabou & Tirole, 2006; Bursztyn & Jensen, 2017). A growing literature documents that individuals frequently hold inaccurate beliefs about how favorably others regard sustainable choices, a phenomenon known as pluralistic ignorance, with consequences for both individual behavior and collective norm change (Andre et al., 2024). Yet how self-image and social image jointly and separately shape adoption of circular consumption goods and whether correcting misperceived norms is an effective lever remains comparatively underexplored, especially outside laboratory or small-sample vignette settings.

Both strands, finally, converge on a further open question that recent reviews of behavioral interventions in the circular economy have flagged explicitly: educating or informing consumers is often treated in policy design as a self-evidently beneficial, uniform intervention, when in fact its effects may be small, absent, or, as this dissertation will show, asymmetric across different types of consumer response. The causal mechanisms that determine when information and perception correction actually change sustainable consumption behavior, as opposed to merely appearing to in stated-preference data, are not yet well understood.

This dissertation addresses this gap directly. In doing so, it moves from studying *what* information does (Chapter 1: how label type affects valuation) to *why* perceptions matter (Chapter 2: how image dimensions affects adoption) to *how* information should be framed (Chapter 3: how environmental metric affects behavioral response).

Building on these two open strands, this dissertation sets out to close the gap between stated pro-environmental attitudes and observed consumption behaviour by identifying, causally, when and how information and perception actually move

choices. It provides causal evidence combining incentivized experimental methods with in-store and large-scale representative samples to shed light on the mechanisms through which informational and perceptual barriers operate.

The three chapters share a common focus on the circular economy broadly defined: reducing waste by extending the useful life of products, whether by encouraging the consumption of food that is still safe to eat (Chapter 1) or by promoting the adoption of second-hand goods (Chapter 2 and Chapter 3). Understanding what holds consumers back from these circular behaviours is a question with both theoretical and practical relevance for policymakers seeking to accelerate the circular transition (Congiu et al., 2025; Gilal et al., 2024). In each case, the dissertation identifies a specific barrier (i.e., confusion about date labels, misperceived social norms, or abstract environmental metrics), and provides experimental evidence on its causal relevance.

Its contribution is threefold. First, it provides causal, rather than correlational or self-reported, evidence on questions that the existing literature has largely addressed through surveys or laboratory studies with convenience samples: whether date-mark type and consumer education affect real willingness to pay for perishable food (Chapter 1), whether self-image and social image differentially predict second-hand adoption (Chapter 2), and whether the choice of environmental metric changes both stated intentions and incentivized willingness to pay or accept for second-hand clothing (Chapter 3). Second, it moves beyond testing whether information or perception correction has *an* effect, to testing which specific form of information or perceptual framing is effective, and for whom: a comparative approach that speaks directly to the design choices policymakers actually face (which label, which metric, which message) rather than to the binary question of whether to intervene at all. Third, methodologically, it demonstrates that causal identification on these questions is feasible outside the laboratory, using in-store multiple price list experiments and large, nationally representative samples, thereby extending the external validity of the evidence base available to researchers and policymakers.

One further point of coherence, methodological rather than substantive, is worth making explicit before turning to each chapter individually. Chapter 1 and Chapter 3 both elicit valuation through a multiple price list format (Andersen et al.,

2006, 2007), expressing treatment effects in a common, monetary metric that allows willingness to pay and willingness to accept to be compared directly across studies. Chapter 2, by contrast, relies on a large-scale vignette experiment, a design better suited to disentangling the multiple, potentially correlated image dimensions at its core than an incentivized choice task would allow. The progression, from a single in-store sample to two large, nationally representative surveys, also lets the dissertation move from establishing a mechanism in a controlled setting to testing whether it holds, and is policy-relevant, at scale.

In the rest of this Introduction, each chapter is addressed individually. This includes the chapter's research question, empirical strategy, and main findings.

The first chapter, *"True to type? EU-style date marking and the valuation of perishable food"*, a collaboration with Alessio D'Amato, Timo Goeschl, and Mariangela Zoli, examines the impact of EU-style date marking (best before and use by) on consumers' valuation of perishable food around the expiry date. Perishable goods account for a significant proportion of goods traded in the economy, particularly in the food sector. In order to help consumers make informed decisions, most countries have introduced date marking as a specific form of product labelling. In the European Union (EU), date marking is mandatory for all packaged food products and comes in two forms. One type, 'best before' (BB), indicates the date after which the item will no longer be of its best quality, though it will remain safe to consume for some time afterwards. The other type, 'use by' (UB), shows the calendar date until which the food is safe to consume; after this date, it should be considered unsafe.

We conducted a series of experiments in a grocery store in Germany. This included a preparatory survey ($n = 100$), which informed the selection of an item used in two subsequent experiments, as well as a follow-up experiment ($n = 160$) concerning consumers' knowledge of date marks. In our main experiment ($n = 200$), participants were given €2 and a six-egg carton due to expire that day. The experiment was randomized with four treatment groups. There were two treatment dimensions: date mark type ('best before' vs. 'use by') and information provided ('none' vs. 'brief education and a quiz'). The main task involved three multiple price lists (MPLs): participants could either keep the current carton or exchange it for one with a different expiry date (in seven days, tomorrow or yesterday). One

choice was paid out at random. The follow-up buy-back MPL task involved choosing whether to keep or return the current carton in exchange for payments (including zero or negative values), in order to detect negative willingness to pay (WTP).

The results show that, although consumers react to the date indicated on the label, the type of date marking has no statistically significant effect on willingness to pay for perishable foods, regardless of the level of information provided. However, the date label does have a statistically significant impact on intertemporal differences in willingness to pay (WTP), albeit only among educated consumers who value perishable products beyond their expiry date. These consumers have a greater WTP differential for products labelled 'use by' than for products labelled 'best before'.

The results suggest that educating consumers about the meaning of labels has asymmetric effects. Education about the 'use-by' label reduced consumers' valuation. Education about the 'best before' label did not. Education could help to achieve the health and safety objective of preventing the consumption of potentially unsafe food, while doing little to reduce food waste.

The second and third chapters stem from a research project in collaboration with Luca Congiu and Mariangela Zoli, and are closely linked projects that investigate drivers of second-hand adoption using complementary methods. The study is implemented on a large, geographically diverse Italian panel ($n = 10,496$) via CAWI, with representativeness targets at the province level. The former disentangles self-image, social image, and second-order beliefs as determinants of second-hand adoption across financial, social, environmental, and hygiene dimensions. Both chapters leverage experimental tasks to produce policy-relevant evidence on what moves second-hand uptake and why. The latter studies how framing environmental costs (CO2, energy, water) shifts stated intentions and incentivized choices between first-hand and second-hand clothing.

The second chapter, *"Image Considerations in Second-Hand Adoption: A Vignette Experiment"*, isolates the role of image considerations by eliciting self-image (private self-evaluation), social image (how others would see me), and second-order self-image (what I think others privately feel) across four dimensions—financial, social approval, environmental consciousness, and hygiene/disgust. The vignette presents a randomized hypothetical choice over a second-hand product (T-shirt,

closet, refrigerator, or smartphone) with randomized condition (excellent, good, acceptable) and price (free vs. affordable), followed by: a 0–100 stated probability of adoption; image statements on an even 4-point scale to avoid indecision; and an incentivized coordination task eliciting beliefs about the modal self-image responses in the respondent's macro-region (with a lottery paying €1 per matched item to a subset of participants).

Two sets of results emerge. First, vignette attributes operate as expected: adoption probability falls sharply when moving from free to low price and when worsening product condition; these effects are sizable and robust in fractional probit models controlling for product type and demographics. The jump from acceptable to better conditions raises adoption, and receiving the product for free substantially increases uptake relative to an affordable price. Second, image considerations are powerful predictors of adoption, with a key asymmetry: self-image coefficients are systematically larger than social-image coefficients across financial, social approval, environmental, and hygiene dimensions, indicating that how individuals see themselves when adopting second-hand matters more than how they think others would see them. The environmental dimension shows some salience under social and second-order perspectives, suggesting a role for public signaling, but self-image remains the dominant driver overall.

A third contribution documents pluralistic ignorance. Respondents systematically underestimate how positively others view second-hand adoption within their macro-region: large shares mispredict the modal self-image in financial, social, environmental, and hygiene dimensions, and these mismatches are statistically significant. Moreover, misalignment matters behaviorally: when a respondent's self-image is below their second-order self-image, the probability of adopting the second-hand product decreases; when self-image exceeds second-order beliefs, adoption tends to increase (more robustly for social approval and hygiene). These findings confirm that underestimating others' private evaluations depresses second-hand uptake and that correcting such misperceptions could meaningfully raise adoption.

The third chapter, *"Environmental costs in second-hand clothing purchase: A voucher choice experiment"*, tests whether providing information on environmental costs—expressed as CO2 emissions, electric energy, or water usage—promotes

second-hand clothing adoption and how the metric chosen shapes effectiveness. The design combines a quiz-style information provision task, which reveals the correct environmental cost with relatable equivalences (e.g., CO2 to km driven; kWh to light-bulb hours; liters to minutes of running water), with an incentivized multiple price list over vouchers redeemable at a first-hand vs. a second-hand store. By varying which voucher is fixed across rows, we elicit willingness to accept (WTA) to switch from first-hand to second-hand and willingness to pay (WTP) to upgrade from second-hand to first-hand, thus capturing preferences around different baselines. One pair is paid out for randomly selected participants, and an opt-out screen allows respondents to forgo the lottery, permitting us to observe shifts in extensive-margin consumption as well.

The main findings show that the choice of environmental metric matters substantially: water-based information generates the strongest effects on both stated intentions and voucher choices. Respondents' voucher choices are consistent across WTA and WTP task versions (i.e., not driven by framing), although magnitudes differ with the designated baseline. Respondents' own pro-environmental orientation and need for uniqueness are independently associated with stated intentions and incentivized choices: those higher in pro-environmental orientation report greater willingness to switch to second-hand, while those with a stronger need for uniqueness require more compensation to do so. Contrary to expectations, local environmental conditions and store proximity do not systematically moderate information effectiveness, although proximity shapes baseline preferences. Exploratory regional analyses underscore the robustness of water effects but are power-limited and hypothesis-generating. Overall, presenting environmental costs with relatable metrics, especially water, meaningfully nudges consumers toward second-hand adoption.

Taken together, these three chapters point to a consistent lesson across two very different circular-economy problems, food waste and second-hand adoption: what moves consumer behavior is rarely the presence of information as such, but its specific form and target. By form, we mean that changing how the same fact is presented, a date label versus an environmental metric, can be inert in one setting and decisive in another; by target, we mean that when a message is directed at how consumers see themselves, as opposed to how they believe others see them, it tends

to carry more weight. Read together, the chapters suggest that policies aimed at expanding circular consumption are more likely to succeed when they are calibrated to the specific informational or perceptual barrier they target, a point we return to, and generalize, in the concluding chapter of this dissertation.

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Chapter 1

True to type? EU-style date marking and the valuation of perishable food

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Abstract

Date marking is intended to help consumers make informed food safety and quality choices when confronted with perishable food products. We provide causal in-store evidence on how EU-style date marking (best before and use by) influences consumers' valuation of perishable food around the expiry date. In a preparatory survey ($n = 100$), we first identify perishable food items amenable to experimental manipulation. A modified multiple price list (MPL) experiment ($n = 200$) then tests shoppers' valuation of perishable food with expiry dates in the future and the past. We vary date mark type (use-by versus best-before) and information status (with and without education) while preventing free disposal censoring. We find that expiry dates affect consumer valuation. Variation in date mark type has little practical relevance. Educating consumers about the meaning of date mark types reduces willingness to pay for potentially unsafe food, but does not increase it for more durable items. An attentiveness experiment ($n = 160$) finds that inattention and consumers' native understanding of current date marks can explain the evidence from the modified MPL experiment. Jointly, these results help explaining existing observational evidence and assessing the prospects of consumer education campaigns.

Key words: Date marking · food waste · consumer valuation · information-based policies · multiple price list experiment · in-store experiment

JEL codes: D12 · C93 · Q53 · Q18

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1.1 Introduction

Perishable goods make up a substantial share of final goods traded in the economy, particularly in the food sector. There, consumers use physical properties of food items, such as appearance or smell, as visual and olfactory cues for inferring whether a food item can be consumed, could be stored for later consumption, or should be discarded (van Boxstael et al., 2014). Packaging of food restricts consumers' ability to use such sensory cues. To help consumers make informed decisions when food is packaged, most countries have introduced date marking as a specific form of product labeling. The European Union (EU), the world's largest integrated market with over 450 million consumers, is no exception. There, date marking is attached to all packaged food products and takes one of two types. One type, *best before* (BB) is attached to relatively durable food items. It shows the calendar date with the intention to communicate the time horizon over which the item maintains its highest quality and after which it remains safe to consume for some time.[1] The other type, *use by* (UB) is attached to food items that spoil quickly. It shows the calendar date with the intention to communicate the date until which the food item is safe to consume and after which it should be considered unsafe.[2] The presence of two date mark types, as observed in the EU, illustrates a wider pattern in date marking schemes, namely an attempt to serve both resource efficiency and food safety objectives. When safe, food of acceptable quality should be consumed, thus minimizing waste (*BB* label). If unsafe, food should not be consumed, but discarded (*UB* label). Yet, both date marks are typically referred to as the *expiry date* of the product, despite the different messages that the date mark intends to convey.

In this paper, we provide *causal* experimental evidence on how date marks such as those used in the EU influence consumers' valuation of perishable food around the expiry date. In its focus on valuation, the paper follows a recent literature (see review below) that deliberately studies observable choice behavior under varying incentives (Collart & Interis, 2018; Sapci & Sapci, 2020; Wilson et al., 2017). This literature relates consumers' valuations of food items, derived from observed choice, to food consumption and waste behavior that is itself unobservable by the researcher.

[1] The legal definition in the European Union is that the best-before date is "the date until which the food retains its specific properties" (EU Regulation No. 1169/2011, p. 26)

[2] After that date, "food shall be deemed to be unsafe" (EU Regulation 1169/2011, p. 35).

Its approach complements a rich body of earlier research built on an alternative methodology, namely surveys of consumers who self-report the food consumption and waste behavior that the researcher cannot observe directly (Garrone et al., 2014; Van der Werf et al., 2018). This earlier literature has provided the basis for concluding that date marks appear to struggle to induce intended consumer behavior, leading to health risks if products with UB labels are consumed after the expiration date[3] and to unnecessary food waste if consumers discard products with BB labels at the expiry date despite being safe to consume.[4] The benefit of studying observable choice behavior for our purposes is that it can provide causal evidence on how variations in the date marking of a food item drive consumers' *valuation* of the product and whether educating consumers about date mark meaning has an impact. Understanding the date mark – valuation link is critical for understanding the observational evidence and for assessing the assumptions underlying initiatives aimed at improving consumers' native knowledge through education. This is where our study comes in.

Our experimental study finds that without further consumer education, a change in the expiry *date* drives a change in consumers' native valuation of a food item at and around that date. The average consumer, in other words, takes perishability into account when they assess the value of a packaged food product, also in the absence of education. A change in the date mark *type*, however, does not affect consumers' native valuation: The BB and the UB label induce statistically indistinguishable valuation profiles of food items around the expiry date. Educating consumers at the point of sale about the date mark has an asymmetric effect: Consumers' valuation only responds to the safety aspects implicit in the UB label type, leading to a significant drop in the value of a perished item. Educating consumers does not increase their valuation of BB-labeled products around the expiry date, however, thus frustrating improvements in resource efficiency through education. A follow-up experiment traces these causal patterns to consumers' cognitive inattention to label type at the point of sale and asymmetric native knowledge.

Experiments that systematically vary date marks in the field are rare. The re-

[3]The World Health Organization estimates that a significant share of the 600 million food illness episodes can be linked back to pathogens (such as Salmonella and Listeria) present in packaged foods (Havelaar et al., 2015).

[4]In the EU, 10% of the 88 million tons of food waste have been linked directly to date marking, with an estimated cost of €14 billion (https://ec.europa.eu/food/safety/food_waste/eu_actions/date_marking_en). In the US, more than 80 percent of consumers report that they discard food before the expiry date (Leib et al., 2016).

sulting lack of causal evidence is the result of particular empirical challenges that researchers encounter in a food labeling context as soon as they move beyond surveys and university laboratories. Ideally, the researcher would be able to manipulate subjects' beliefs and knowledge about and awareness of existing information-based policies. In practice, subjects may hold strong beliefs or possess hard knowledge about the existing policy such that the researcher's experimental treatment fails to induce a different set of beliefs or knowledge. The researcher would also want to manipulate the type of date marks and the calendar dates stamped on otherwise identical food products. Such external manipulation is important in order to disentangle factors associated with specific foods from the role of the date mark. In practice, unless expertly done, subjects may recognize the mismatch between product type and date label type or detect the manipulation. The researcher will also want to create food management decision contexts that allow issues of acquiring, keeping, and consuming or discarding food items to affect consumers' valuation in a consequential way.[5] Any manipulation, finally, needs to adhere strictly to research ethics and food safety: Consumers may not be needlessly deceived or exposed to additional food risks as a result of experimental manipulation. This is particularly true if the research involves food items at or beyond their labeled expiry date.

Our experimental approach addresses the empirical challenges to causal inference through a sequence of three steps. First, on subjects' knowledge and beliefs, our experiment is informed by a preparatory survey ($n = 100$) in which subjects disclose their beliefs about date marking of different grocery products and their confidence that their beliefs are accurate. We find a widespread lack of confidence and considerable heterogeneity in the accuracy of shoppers' beliefs about the applicable date mark across grocery items. For some items, shoppers are no better than a coin toss in guessing the correct date mark, have low confidence in their guess, and only a quarter or less of subjects express confidence in their guess. This informs our second step, a modified in-store multiple price list (MPL) experiment. There, we employ a food item (eggs) for which beliefs on date marking turn out to be demonstrably weak. The choice of product, time, and country setting (eggs, 2019, Germany) is

[5] Typical food items used in experiments, such as chocolate bars (Davis and Millner (2005)), lend themselves to impulse consumption and are typically consumed well within the product lifetime. The issue of discarding food therefore fails to arise.

conducive to date mark manipulation that is credible and non-detectable: In contrast to other countries, date-marking of individual eggs was unfamiliar to German consumers in 2019. The date-mark were instead attached to the outside of egg cartons and could be replaced by the experimenter with acceptable effort, skill, and suitable equipment for producing industry-grade date mark labels. Third, storage, consumption and discarding are relevant features of consumers valuing eggs: Eggs are not consumed on the spot, but are perishable[6] and easily disposed of. Finally, the setting is conducive to maintaining ethics and food safety: Eggs are, by law, date-marked *best-before*. By forward-dating expiry dates on egg cartons and by affixing a *use-by* date mark, subjects face the same or lower risk in all experimental conditions than they do in the marketplace.

We recruit 200 grocery shoppers in a suburban grocery store for the MPL experiment by inviting them individually to participate in an initial survey about food shopping habits in exchange for a combination of monetary (€2) and in-kind (a box of six organic eggs) rewards. After receiving the reward, subjects are randomly assigned to one of four treatment conditions. One condition varies the date mark type (*BB* or *UB*). The other varies the information status of the subjects, either relying on subjects' *native* knowledge or *educating* subjects prior to taking consequential choices. In this two-by-two design, all subjects then make MPL choices over cartons of eggs with expiry dates manipulated by the experimenter. The modification of the standard MPL experiment consists of adding a specific terminal buy-back mechanism at the end of the experiment. This methodological contribution of the paper allows the researcher to overcome free-disposal censoring in cases in which theory supports both positive and negative valuations for a product and overcomes a possible upwards bias. This terminal buy-back mechanism has applications beyond food (waste) to areas such as products that are potentially noisome or dangerous to all or a subgroup of consumers, for example allergenic or repugnant products. The modified MPL experiment delivers for each of the four treatment conditions willingness-to-pay (WTP) data of 50 individuals for eggs with an expiry date seven days after, one day after, on the day, and the day before the subject participates.

The final step in our approach is an experiment that tests whether the treatment

[6] In addition, eggs' shelf life cannot be extended through freezing.

effects that we establish in the modified MPL experiment are consistent with the nature of consumers' information about date marks. We recruit 160 shoppers in two treatment conditions that provide incentives to subjects to reveal their native knowledge about date marks and the attention that they pay to the information that policy-makers attach to packaged food products.

Individually and jointly, the three research steps provide answers for the empirical challenges that beset field experimental research on date marking. They allow the researcher to safely manipulate the relevant date mark features while holding the product constant and therefore draw causal conclusions about how variations in the date mark affect consumers' valuation. This furthers our understanding of how consumers value perishable goods with different expiry dates and different date mark types. Expiry dates clearly impact on consumers' valuation of perishable goods and are therefore a promising target of future policy changes. Date mark types, on the other hand, exhibit inattention problems in the absence of educational campaigns and can give rise to asymmetric effects of consumer education. This should be of interest to both governments and retailers interested in balancing food safety and food waste.

1.2 Related literature

Date marking has attracted considerable interest among researchers in OECD countries, in particular in the context of understanding food waste (EU-Council, 2016; Hall et al., 2009). This has led to a number of surveys in which consumers repeatedly report that they are aware, understand, and act upon date marks on food product (e.g. Eurobarometer, 2015) while researchers consistently find the opposite (Neff et al., 2015, 2019). Evidence suggests that the intended effect of date-marking policies may be limited or can even backfire: Thompson et al. (2018) find in an online survey of 548 Scottish consumers that consumers report no difference in willingness to consume 'expired' food items between use-by and best-before labels. Wilson et al. (2018) show that consumers make unwarranted inferences about food items based on date labels beyond safety and quality, and Roe et al. (2018) find in a lab survey that date marking itself leads to increased discarding of otherwise unobjectionable food

items.[7] Jointly, these and other results from surveys and studies with consumers cast doubt on the usefulness of date marking perishable food for guiding consumer's purchase, consumption, and discarding decisions. European evidence supports this conclusion: Around one quarter of respondents from countries in the EU misinterpreted the *BB* label in a survey as implying that the food is no longer safe after the corresponding date. Such misinterpretation are likely to result in underestimating the true value of food products. At the same time, 28% of respondents exhibit an inaccurate understanding of *UB* date marks (Eurobarometer, 2015). If this leads to households overestimating the true value of unsafe food after its expiry date, there could also be an over-consumption of certain food items and too little discarding.

While survey evidence, such as cited above, is informative, it cannot shed light on the causal relationship between date marks and consumers' valuation of food items. There is a small number of experimental studies that provide causal evidence. Collart and Interis (2018) conduct a laboratory experiment ($n = 150$) with general population subjects that can use a $35 endowment to acquire food items of varying shelf life and different perishability, including expired items, in an on-screen choice experiment. Their treatment conditions vary information and expiry dates, but not the label type. Education on date mark meaning does not impact on consumer choice in their experiment, while information relating product choice to food waste and its environmental consequences does. Closer in spirit to our study is a paper by Wilson et al. (2017). They conduct a laboratory experiment ($n = 200$) with non-student subjects under four different date labels conditions ("Best by", "Fresh by", "Use by" or "Sell by"), remaining shelf life, package size, and three types of food. An auction is used to elicit both participants' willingness to pay and expected amount of food waste in order to calculate a measure of willingness to waste (WTW), on the basis of the idea that date labels may have an impact on WTW by affecting both WTP and the expected amount of waste. More specifically, the closer the expiry date, the lower the WTP and the larger the expected waste, leading to a larger WTW. The auction results suggest that WTW is larger for labels that imply, in the consumers' perception, a safety concern, namely *UB*. In our paper, we also study the WTP for products of different labels and expiry dates. Differently from Wilson et

[7] In sessions with 88 consumers that could inspect and smell opened milk containers, subjects reported a stronger intention to discard the milk if the container had a date mark compared to containers that did not bear a date mark

al. (2017), however, we do not aim at establishing any explicit relationship between consumers' evaluation and waste choices. This clearly does not exclude that there is an an intuitive mapping between such valuation and food waste. Low consumer's valuation of an item close to the due date may lead it not to be sold and thus to spoil. Even when bought, misconception of the label may reduce the consumer's perceived value of the food item which is then more likely to be wasted. Also related is an experimental auction study conducted in a lab setting using student subjects ($n = 159$) by Sapci and Sapci (2020). Like our study, the experiment elicits willingness to pay for food items under two date marks popular in the US context, "sell by" and "expires on". Other differences are that consumer education is always provided, procedures involve multiple sequential auctions using pen-and-paper and within-subject comparisons, and valuation of products is not examined close to the expiry date. They find that consumers are willing to pay more when the same product is labeled with the "expires on" date mark type and interpret this as a willingness to avoid the ambiguity implicit in the "sell by" label.

We contribute to previous studies in three ways: First, we move the experiment to an in-store setting that gets the experimenter closer to the average consumer. Second, our design involves directly manipulating date marks on products handled by consumers and tested for being amenable to manipulation, thus establishing a clean treatment effect. Third, we adopt a modified MPL experiment as the elicitation mechanism, overcoming some of the challenges implicit in negatively valued consumption goods.

1.3 Theoretical considerations and design

The objective of our experimental design is to establish how date mark type and the information level causally affect consumers' WTP for perishable food with different expiry dates, including the possibility of expired food and negative WTP. The guidance provided by the theoretical literature for design and hypothesis development is surprisingly limited.[8] In a much-cited empirical paper, Tsiros and Heilman (2005) provide a review of the literature up to 2005 and conclude that while there

[8] One exception is an early paper on optimal household inventory management with perishable products (Reinhardt et al., 1973). The focus of the paper's deterministic model is on the implications of product lifetime for shopping frequency, however, rather than the valuation questions explored here.

is a considerable theoretical literature on supply-side aspects of perishability in the retail sector, the demand side is largely unexplored in applied economic theory. As a result, the authors qualitatively sketch the outlines of a theoretical framework that, fifteen years later, remains little improved upon.[9] We build on these outlines to inform our experimental design and the formulation of four testable hypotheses on date mark type, information, and expiry date.

Perishability is synonymous with a fixed time horizon over which a good provides positive benefits. Arguing intuitively, Tsiros and Heilman (2005) posit that the instantaneous consumption value (the expected immediate satisfaction derived from consuming the product) is decreasing over time as the good degrades. A slightly richer characterization of the consumer's problem is to consider that the owner of a perishable item faces a trade off between the instantaneous consumption value[10] and the option value, i.e. the value of postponing consumption to a later point. For most food items, both values are directly connected with the expiry date. Distance to the expiry date, i.e. remaining shelf life, weakly increases the perceived quality of the food item and hence the expected consumption value. Likewise, remaining shelf life increases the option value because there is more time left for postponing consumption. Perfectly intuitively, therefore, WTP tends to decrease as a perishable product approaches its expiry date.[11] The evolution of the WTP is captured in its day-to-day changes, or *intertemporal* differentials, and these changes tend to increase in absolute terms as the good approaches (or exceeds) the end of its shelf-life.

Both date mark types target the formation and evolution of consumption and option values, but intend to affect them differently: If effective, a *best-before* mark induces a roughly constant expected consumption value up until the expiry date and a slow decline thereafter, thus giving rise to a positive option value beyond the expiry date and a smooth increase in the intertemporal differentials. A *use-by* date mark, on the other hand, is intended to induce an expected consumption value that is positive up until the expiry date and negative thereafter. If effective, the

[9] There is a handful of papers that test psychological theories in the context of perishable food, such as the theory of planned behavior (Siddique, 2012) or value-belief-norm theory (Farr-Wharton et al., 2014).

[10] On contrast to Tsiros and Heilman (2005), one need not require the instantaneous consumption value to decrease strictly with time. It can remain constant for long times, peak at certain times (for example for festive events with traditional food items) or change through time with changes in preferences (e.g. appetite) and external conditions (e.g. weather).

[11] Hypothesis 1 in Tsiros and Heilman (2005) states much the same, without taking the option value explicitly into account, however.

option value therefore collapses to zero at the expiry date, forcing a jump in the intertemporal WTP differential.

The intended difference between the two date mark types on the formation and evolution of WTP for perishable food constitutes the primary empirical target of the experiment. On theoretical grounds, it is smallest for distant expiry dates and largest at and just beyond the expiry date. Conditions for detecting a treatment effect of date mark type will therefore be favorable when the otherwise identical food items on offer are date-marked close to their expiry date. The secondary target of the experiment is the effect of consumer education. We reason that compared to consumers with native knowledge about date marking, consumers with additional education present more favorable targets for detecting the effects of different expiry dates and date mark types. This reflects our reading of the prior evidence, cited earlier, that members of the public frequently overlook or misinterpret the date mark.

In order to detect an effect of date mark type on WTP, we use a modified multiple price list experiment in which otherwise identical food items differ, within subjects, by expiry dates and, across subjects, by date mark type. Among the experimental methods commonly used to elicit individual valuations, we opt for an MPL approach. The reasons are that MPL experiments are easy to implement, induce truthful revelation by participants (Andersen et al., 2006, 2007), and have demonstrated feasibility and fidelity even in demanding field settings (Berry et al., 2020; Burchardi et al., 2021). As such, they appear apt for an application in an in-store setting with consumers as participants.

In an MPL experiment, participants are asked a sequence of "yes/no" decisions for a set of discrete prices: Would the participant want to pay for a certain outcome (such as receiving a good) should that price be drawn? Vis-á-vis the classic BDM, this implies a loss of point identification, giving only interval identification, but it is arguably more transparent and easier to understand: subjects take a sequence of consequential binary decisions with ordered bid prices such that the researcher can identify, with some precision, the switching point between the two choices. The researcher then selects one of the paired options at random, and the choice is implemented. As the selection is randomly drawn and participants are aware that

their responses do not affect which of the binary decisions will be chosen, they are stimulated to answer truthfully in order to get their preferred choice. One, often unspoken, assumption in standard MPL experiments is that the choice outcomes have non-negative value to the subject. In the context of perishable food, this assumption needs not hold, in particular not in the context of food items close to or beyond the expiry date: Subjects may well prefer not to own food that is spoiled or about to spoil. There are two avenues open to the researcher: One is to ensure compliance with an experimental protocol in which food items chosen must eventually and verifiably be consumed by the subject. This is the solution used in the seminal paper by Coursey et al. (1987), in which experimental subjects who win an auction have to hold an unpleasant tasting substance in their mouths for a period of time. Similarly, in the papers by Shogren et al. (1994) and Hayes et al. (1995) subjects have to consume potentially unsafe meat sandwiches in order to collect their participation reward. Even if this approach survived a current ethics review, its feasibility is established only for lab experiments involving student subjects. The other avenue is to augment the design. Here, a simple extension of the MPL into negative price terrain will not suffice due to free disposal censoring. Free disposal means that experimental subjects can always dispose of products by throwing them away at no cost to themselves. This option means that any positive payment in exchange for receiving the unwanted product will be acceptable to subjects. This induces censoring at zero payments: Subjects who prefer to dispose of the good rather than consume it will not be induced to express truthfully their negative WTP even if the MPL includes negative prices. The presence of free disposal censoring can be detected, however, through a simple procedural change: The experimenter offers to buy the item back after the MPL experiment. This terminal buy-back mechanism provides subjects with zero or negative WTP a clear incentive to exchange the unwanted good for a positive reward.

Among the two approaches, our design opts for the terminal buy-back mechanism for three main reasons. One, in an in-store setting with general population subjects, strict protocol compliance is untested and suffers both from questionable ethicality and tenuous enforceability. Two, explaining such a protocol is also likely to have a chilling effect on recruitment of subjects. Three, the buy-back mechanism allows the

option value of food products to establish itself: When protocol compliance forces subjects to consume the item on site (to prevent disposal), the option value could not enter WTP and its estimates would understate true WTP for acquiring the product. These three reasons underpin our choice of a modified MPL experiment for eliciting WTP in an in-store experiment for perishable food items.

1.4 Procedures

1.4.1 Preparatory survey

Strong beliefs among consumers, whether correct or incorrect, about which foods bear which date mark and high confidence in their beliefs are an obstacle to a successful experimental manipulation of date mark labels. Worse, subjects may be suspicious of labels affixed to the food item that conflict with their highly confident beliefs. In surveys, German subjects claim that they always (51%), often (25%) or at least sometimes (9%) check the expiry label on a product and understand its meaning (Eurobarometer, 2015). This makes German consumers potentially not amenable to experimental manipulation of date mark labels.

To test the viability of an in-store experiments in such a context, we conducted a preparatory survey with general population subjects ($n = 100$) at a grocery store in Germany in order to elicit beliefs, knowledge, and confidence among members of the target population about current date marking practices for a range of food items.[12] This survey was carried out from Sept. 4^{th} to Sept. 6^{th}, 2019 during morning, afternoon, and evening shopping hours. Subjects were invited to participate in a survey for a small symbolic reward unrelated to their answers. They were handed a tablet computer and were presented, for a selection of seven commonly purchased food products, with a choice of which of the two labels, BB or UB, they believed to be associated with that product. For each choice, subjects were asked to report their confidence in their choice on an 11-point Likert scale from 0 (not at all sure) to 10 (absolutely sure).[13] The survey was not incentivized, reflecting the unsettled debate about incentives in confidence tasks (Lebreton et al., 2018).

[12] See Appendix B, Figure A1, for a flowchart of the survey.

[13] Other researchers choose a format in which subjects rate their confidence on a scale between 50 % (no confidence=random choice) and 100 % (absolute confidence) (Lebreton et al., 2018; Murad et al., 2016). While appropriate for highly educated student subjects, members of the general population with little familiarity of a quantitative

Table 1.1: Preparatory survey - summary statistics

Food product	Share of correct answers	Median confidence level	Confidence level > 7
Eggs	0.50	5	0.25
Table salt	0.66	6	0.33
Fresh fish	0.64	6	0.31
Sliced bread	0.65	6	0.27
Minced meat	0.66	7	0.42
Fruit juice	0.72	7	0.40
Yogurt	0.90	8	0.51

Note: Share of correct answers as to label type, median confidence level and share of strong beliefs, by food item

Table 1.1 reports the results of the survey by food category (first column), ranked by the share of correct answers (second column). For each category, it also reports the median confidence level among subjects (third column) and the share of subjects with a strong (> 7 on the Likert scale) belief (fourth column). The share of correct answers ranges from no better than flipping a coin (eggs) to 90 % (yogurt). Median confidence across subjects closely tracks the share of correct answers across food items and so does the share of subjects with high confidence in their choice.[14] Among the food categories, eggs stand out as a food item that is a particularly suitable target for an experimental manipulation of date mark labels: Our sample of German shoppers had low median confidence in their guess of the correct date mark type and only a quarter of shoppers held a strong belief in their choice. This is consistent with the fact that only half the subjects picked BB as the correct date label type. These findings provide the basis on which eggs were chosen as the product whose date mark label would be experimentally manipulated in an in-store setting.

1.4.2 In-store experiment

The in-store experiment was run in a sub-urban grocery store of a medium-sized German city between Sept. 20^{th} and Sept. 21^{st}, 2019 during morning, afternoon, and evening shopping hours. This store belongs to the same chain of supermarkets as the store in which the preparatory survey was carried out, thus attracting the same customer segment. However, it is located 5km away in a different part of the

approach are in our opinion likely to find this format challenging.

[14] At the individual level, there is only a weak statistical association (Kendall's τ) between the likelihood of correctly answering the question on the label type and the confidence that the subject expresses about her choice.

city, reducing overlap between the participant pools of the preliminary survey and the in-store experiment.

To recruit subjects, we invited shoppers walking through the store lobby to participate in a survey in exchange for a reward. Participants were assigned to one of four treatment groups through a randomized assignment protocol.[15] Irrespective of assignment, they completed a survey on grocery shopping behavior and demographics on a tablet computer. The survey was uniform for all treatments and did not refer to date marks or the perishability of food items.[16] As compensation for their time, participants received two rewards at the end of the survey: One reward was €2.00 in coins.[17] The other reward was a carton of six organic eggs date-marked to industry standards with an expiry date always coinciding with the day of the experiment.

The treatment arms separated with the handing-over of the reward: Two of the four treatment groups, labeled *BB*, underwent the MPL experiment handling egg cartons exclusively bearing date marks that read *best before DD.MM.YY*. The other two groups, labeled *UB*, underwent the MPL experiment handling egg cartons exclusively bearing date marks that read *use by DD.MM.YY*. This variation in date mark *type* constitutes the first of two treatment dimensions of the MPL, as summarized in table 1.2. The second dimension varied the information status of the shoppers. Two of the four groups, labeled *n* for *native*, proceeded to the MPL experiment without receiving additional information about the meaning of date marks attached to food items. The other two treatment groups, labeled *e* for *education*, underwent a three-step procedure to enhance their knowledge about date mark labeling: After being reminded or educated about the meanings of *BB* and *UB* labels through a tablet screen, they had to successfully complete a two-question, four-options multiple-choice quiz on date mark interpretation and had to correctly identify which of the two date mark label was present on the carton they had received. Only then were subjects in the *e*-treatments able to proceed to the MPL experiment.

All four treatment groups were administered the same MPL experiment, which consisted of a sequence of three screens. On each screen, the consumers made seven

[15] See Appendix B, Figure A2, for a flowchart of the MPL experiment.
[16] See Appendix A for details on the survey questions.
[17] Consumers received one €1-coin plus one €0.50, two €0.20 and one €0.10 coin such that every subsequent transaction in the MPL could be immediately implemented.

Table 1.2: Treatments, by treatment condition

	Information Status	
Label Type	Best-before — native	Best-before — educated
	Use-by — native	Use-by — educated

consecutive choices between a 'keep option' and an 'exchange option'. Choosing the former meant retaining the carton received as a reward and dated-marked with an expiry date on the day of the experiment. Choosing the latter meant exchanging it, for a financial transaction, against another, otherwise identical carton with a different expiry date. Table 1.3 shows the entries of the price list, with the keep option on the left and the exchange option on the right, ordered by size of payment. The upper bound of €1.00 was determined by reference to the fact that the product on offer could be purchased in the same store for a retail price between €2.19 to €2.69 with an expiry date typically 20 days ahead. The lower bound of receiving money in order to accept a carton of eggs with a longer shelf life acted as a basic check for the presence of status quo bias (Samuelson & Zeckhauser, 1988) and endowment effects (Kahneman et al., 1991). The number of days before the expiry date, x, moved from $x = 7$ on the first to $x = 1$ (written as 'tomorrow' in the MPL) on the second screen. For $x = -1$ (written as 'yesterday') on the third screen, the exchange option lists *receive* instead of *pay* and vice versa to account for the inversion of the remaining lifetime of the product.

Table 1.3: Price list, basic format

Keep option	**Exchange option**
Carton dated today	Carton dated in x days and pay €1.00
Carton dated today	Carton dated in x days and pay €0.80
Carton dated today	Carton dated in x days and pay €0.60
Carton dated today	Carton dated in x days and pay €0.40
Carton dated today	Carton dated in x days and pay €0.20
Carton dated today	Carton dated in x days and pay nothing
Carton dated today	Carton dated in x days and receive €0.20

To maintain incentive compatibility, one of the $7 \times 3 = 21$ choices were randomly implemented for each subject. Upon conclusion of the MPL, each subject therefore either held a pack of eggs expiring on the date of the experiment and €2.00 in coins; or a pack with a different expiration date and a cash balance in accordance with the implemented choice. Then, subjects proceeded to the last stage of the experiment, the buy-back task. This task was essentially another MPL experiment in which

subjects had to select, within a list of six possible alternatives, whether to keep the pack of eggs or return it to the staff in exchange for a payment (see Table 1.4). Note that the range includes a zero payment and a negative payment. This ensures that the experimenter can observe whether participants have a negative WTP for the food item currently in their possession and interpret the evidence from the preceding MPL experiment accordingly without concerns about free disposal censoring.[18]

Table 1.4: Buy-back task

Keep option	**Exchange option**
Keep carton	Return carton and receive €3.50
Keep carton	Return carton and receive €2.50
Keep carton	Return carton and receive €1.50
Keep carton	Return carton and receive €0.50
Keep carton	Return carton and receive nothing
Keep carton	Return carton and pay €0.50

As in other MPL experiments, incentive compatibility is maintained by one of the choices being randomly selected for implementation. The experiment concluded with a brief questionnaire on demographic characteristics.

On the basis of the described experiment we have constructed a set of outcome variables: the willingness to pay (WTP) and the intertemporal WTP differentials. WTP is derived from the second buy-back MPL (as shown in Table 1.4) with the aim to calculate the consumer valuation for an eggs carton expiring today (i.e. on the day of the experiment); the WTP is therefore constructed on the basis of preferences revealed in the buy-back MPL by consumers that, after the implementation of the first MPL experiment, were left with a carton expiring on the day of the experiment. The WTP differentials are instead derived from the first MPL experiment, on the basis of the three series of choices made by consumers in the three consecutive screens (as shown in Table 1.3); this provided us with three different WTP differentials between an eggs carton expiring today and the ones expiring respectively in a week, tomorrow and yesterday.

Since the in-store experiments expose subjects to food items whose date marks have been manipulated by the experimenter, the researchers involved discussed two concerns. One was whether the procedures exposed subjects to increased health risk.

[18]To detect free disposal despite the buy-back mechanism, we also monitored waste bins in the proximity of the store during the duration of the experiments. There was no evidence of cartons of eggs being disposed of close-by. Whether participants took eggs home to dispose of them is something that our design does not allow us to observe.

We concluded that this was not the case: The sourcing of the eggs used was organized such that each and every manipulated date mark bore an expiry date *earlier* than that originally on the packaging.[19] The other concern was the use of deception in the in-store experiment. Deception can be harmful to subjects. Deception can also undermine the trust that the general public has in empirical research. The most severe forms of deception, in declining severity, are those that cause physical or physiological trauma, withholding of promised payments, leading subjects to purchase mislabeled products, and providing subjects with false information about their own or other subjects' performance (Cason & Wu, 2019; Colson et al., 2015; Rousu et al., 2015). Our procedures ensure that physical or physiological trauma to subjects can be excluded by design. The products used in the in-store experiment are not mislabeled in the sense that they are of worse quality than announced. All subjects receive products in the in-store experiment that are of equal or better quality than what the date mark conveys.

We conducted the experiment with a sample size of $n = 200$, evenly distributed across the four treatment cells based on a randomized assignment protocol. The size of the sample was determined on the basis of a power analysis performed to detect statistically significant differences between the current average price of an eggs pack (€2.40) and a discounted price, ranging from 25% to 50% discount. These price differentials have been chosen by considering the discount policies that grocery stores sometimes adopt to promote the purchase of perishable products approaching their expiry date. Using a price differential up to €0.60 (25% discount), as well as conventional levels of significance ($\alpha = 0.05$) and power ($\beta = 0.8$) the most pessimistic n per treatment cell is 45. We took a conservative stance and recruited 50 subjects per cell.

[19] It is also important to note that other than in the US, eggs are not sold refrigerated in the EU. European Commission Regulation No. 589/2008 prohibits this practice, reflecting diverging views on different risks posed by egg-borne food diseases on both sides of the Atlantic.

1.5 Hypotheses and Results

1.5.1 Hypotheses

The experiment is designed to detect two treatment effects. The primary effect is the impact of date mark type (*BB* vs. *UB*) on consumers' consequential decisions about perishable food.

Hypothesis 1 *Willingness to pay for perishable food is higher for products labeled best-before than for products labeled use-by.*

Theory predicts that the strongest support for hypothesis 1 will arise for expiry dates on the day of the experiment (see section 1.3). The reliance of hypothesis 1 on a comparison of WTP across date mark types re-emphasizes the usefulness of a buy-back experiment that generates WTP *level* data.

The same theoretical considerations predict that the date mark type will affect how consumers' valuation varies around the expiry date. The *UB* date mark, being associated with greater perishability than the *BB* mark, is predicted to lead to larger day-to-day WTP differentials around the expiry date: Consumers are expected to discount food more heavily as the items approach or exceed the end of their posted shelf-life. These *intertemporal* WTP differentials are recovered from subjects' choices in the MPL experiment and form the basis of hypothesis 2.

Hypothesis 2 *Intertemporal WTP differences for perishable food are lower for products labeled best-before than for products labeled use-by.*

Both hypothesis 1 and hypothesis 2 therefore make statements about the impact of the date mark on WTP and measure these impacts around the expiry date. The difference is that hypothesis 1 draws on WTP for products expiring exactly on the day of the experiment while hypothesis 2 examines the changes around the expiry date, both before and after. Differently put, hypothesis 1 tests for differences in levels while hypothesis 2 tests for differences in slopes.

The secondary effect we expect to detect through the experiment is the impact of educating (or reminding) consumers about the meaning of date marks before they take their choices in the MPL experiment. In light of prior evidence (see section 1.2), we expect a significant share of consumers to be confused or mistaken about

date mark meaning in the baseline condition (n). Educated consumers understand that products labeled *UB* are more perishable. Since they are expected to constitute a greater share of consumers in treatment condition (e), average WTP is predicted to be lower compared to the native treatment condition (n) for *UB* products and vice versa for *BB* products.

Following the structure of hypotheses 1 and 2, we examine this prediction both in terms of its effects on the WTP level and the intertemporal WTP differentials. For the WTP level, this leads to hypothesis 3.

Hypothesis 3 *Compared to native consumers, educated consumers have a higher WTP for perishable food labeled best-before and a lower WTP for perishable food labeled use-by.*

Again, theoretical considerations favor detection around the expiry date. Consumers' understanding of how the passing of the expiry date affects food safety and quality is likely to have the greatest impact on product valuation. The same effect is expected to establish itself in intertemporal WTP differentials.

Hypothesis 4 *Compared to native consumers, educated consumers exhibit greater intertemporal WTP differences for perishable food.*

A final consideration for guiding the data analysis is that the two-by-two design allows the experiment to also speak to the combination of the primary effect of label type and the secondary effect of education. The effects are likely to be mutually supportive, giving rise to a positive interaction effect.

1.5.2 Main Results

Our procedures recruited 50 subjects for each of the four treatment cell in a balanced design. All 200 subjects completed the modified MPL experiment, including the terminal buy-back mechanism. There were no drop-outs. Filters for eliminating subjects who declared themselves to be following a vegan diet or to have someone with an egg allergy in their household were present, but no subject triggered these filters in our sample.

Data from MPL experiments with general population subjects tends to be noisy and deviations from basic axioms of choice are common. A potential source of

inconsistency comes from multiple switching behaviors, i.e. subjects can switch back and forth from one option to the other of each binary decision when they move down the price list. Differently from subjects that have a single switching point, or, in other terms, that maintain the same option up to their WTP and then switch to the other option, for multiple switchers their choices do not allow the researcher to infer their true WTP. Our in-store experiment is no exception with respect to potentially inconsistent preferences. For example, between 4% and 7% of subjects switch more than once in one of the ordered bid price lists, 15% in at least one of them. These shares are in line with or below those reported in other MPL experiments with general population subjects (Gaudecker et al., 2011). More importantly, the frequency of deviations is statistically unrelated either to treatment assignment or to expiry dates. We err on the conservative side and exclude subjects from the subsequent analysis of MPL data if they switched more than once on at least one screen, leaving us with complete MPL observations from 170 subjects. The MPL data used for hypothesis testing includes, however, the sizeable share of non-switching subjects. While subjects that have a single switching point always constitute the largest share, subjects exhibit considerable status quo bias.

For the terminal buy-back mechanism, from where we draw the data for testing hypotheses 1 and 3, we obtain 123 observations from subjects that were assigned an egg carton with an expiry date on the day of the experiment by the randomized incentive mechanism.[20] If we restrict the sample in the most conservative way by excluding all those subjects that switched multiple times at least once, this leaves us with exactly 100 subjects. These form the basis of the analysis below. Among the 100 subjects, 15 have a strictly non-positive WTP for a carton, highlighting the potential scale of free disposal censoring in the absence of a terminal buy-back mechanism. Unaccounted for, this censoring would bias WTP estimates upwards.[21]

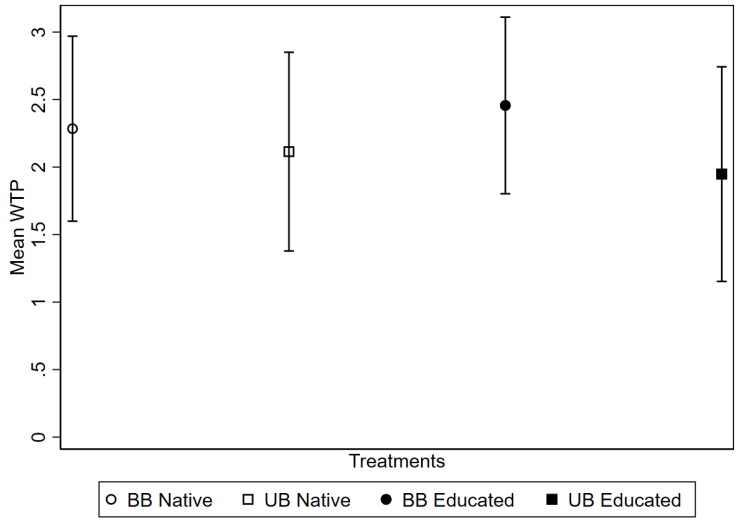
Figures 1.1 and 1.2 summarize and display the key statistics that form the basis of hypothesis testing. Figure 1.1 reports, for each treatment condition, participants' average WTP for an egg carton with an expiry date on the day of the experiment. WTP measurements for this day are most favorable for detecting the treatment

[20] The remaining 77 were randomly assigned cartons with a different expiry date.

[21] In the present data, failing to take non-positive WTP into account would have led to a positive bias of around 6%. This estimate comes from re-coding the MPL data as if the buy-back mechanism had not included zero or negative payments and re-computing the WTP.

effects of both date mark type and education. A first observation is that the average WTP between €1.95 and €2.46 reported by subjects are reasonable in the context of an in-store retail price for the same carton of eggs between €2.19 and €2.69. While our analysis exclusively focuses on the treatment effects, these levels indicate that subjects were, on average, taking meaningful choices in the MPL experiment. A second observation is that the standard errors, represented by the error bars around the average WTP, are relatively large: Across subjects, there is considerable variation in WTP, which reaffirms earlier findings that general population subjects tend to return noisy WTP data in MPL experiments.

Figure 1.1: Average WTP



Note: Average WTP in € for an egg carton with expiry date on day of experiment, by treatment condition.

Figure 1.2 displays, grouped by expiry day, the mean intertemporal WTP differential in all four treatment conditions. The WTP differentials are premiums, or discounts, relative to a carton of eggs expiring on the day of the experiment. The four leftmost estimates are the premiums for eggs with an expiry date one week after the experiment in the four treatment conditions, starting with *BB* and *UB* with native consumer information, followed by *BB* and *UB* after consumer education. In the middle are the premium estimates for eggs with an expiry date one day after the experiment, by treatment as above. The four rightmost estimates show the discounts (negative premiums) for eggs whose expiry date was one day before the experiment, again by treatment.

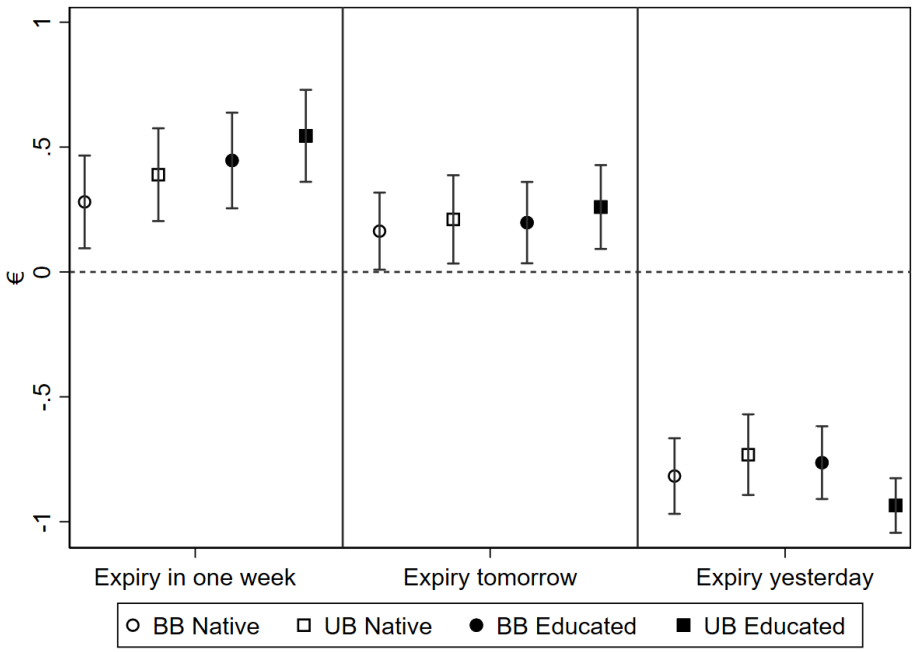
Like the level estimates, the intertemporal WTP differentials pass a visual plausibility test and accord with the basic economic intuition of section 1.3: For eggs with an expiry date one week in the future, the premium of around €0.42 is equivalent to around 20% of the average purchase price of the egg carton. With an expiry date one day in the future, the WTP premium relative to eggs expiring today is about €0.21 or 8% of the purchase price. When the expiry date is one day in the past of the experiment, then participants demand a discount of around €0.79 or 34% off the price. The systematic decrease in the WTP differentials as the expiry date approaches and passes is not only in line with theory, it is also empirically robust: The ordering of the estimated WTP differentials, pooled by expiry date, is statistically highly significant ($p < 0.001$ for each pairwise comparisons, one-sided t-test). At the same time, figure 1.2 shows that within each expiry date, the estimated WTP differentials associated with each of the four treatment variations cluster close together.

The first formal test is of Hypothesis 1, which posits that a *BB* label induces higher WTP than a *UB* label. Result 1 reports on testing this hypothesis 1 using the WTP data from the buy-back experiment as reported in figure 1.1.

Result 1 *The date mark type has no statistically significant effect on willingness to pay for perishable food, irrespective of information status: Willingness to pay for egg cartons date-marked* ***best-before*** *and willingness to pay for egg cartons date-marked* ***use-by*** *are statistically indistinguishable, both for native information and after consumer education.*

For both conditions of the information status, a comparison of the mean WTP indicates a negative treatment effect of selling the same perishable good under a *UB* date mark rather than a *BB* date mark. This is in line with the prediction of hypothesis 1. With native information (condition n), the difference between means of 2.28 (BB) and 2.11 (UB) is not significant, however ($p = 0.68$, M.W. Rank Sum Test). With educated consumers, the difference between means of 2.46 (BB) and 1.95 (UB) is larger, but still not significant ($p = 0.46$, M.W. Rank Sum Test). This indicates that given the heterogeneity among shoppers, the information-based policy induces some differences in consumer valuation of food, but not at a significant level. This is true even on the date of expiry, that is, in circumstances in which the

Figure 1.2: WTP premium



Note: WTP premium (discount) relative to carton with expiry date on day of experiment, in €, grouped by expiry date (in one week, on the next day, on the day before) and by treatment conditions. The average premium for an expiry date a week (a day) later is €0.42 (€0.21), equivalent to 20% (8%) of purchase price. The average discount for an expiry date one day before is €0.79, equivalent to 34% of purchase price. Variations in label type (best before or use by) and in information status (native or educated) have no significant impact on the WTP premium. The exception is a negative impact on WTP for expired food items labeled *UB* of educating consumers about label meaning.

information conveyed by the date mark type reaches its maximum consequentiality.

Hypothesis 2 postulated that a *UB* label induces higher intertemporal WTP differences than a *BB* label. The test of hypothesis 2 is performed on the basis of subjects' choices in the MPL experiment and reported in figure 1.2.

Result 2 *There is a statistically significant impact of the date label on the intertemporal differences in WTP, but only for educated consumers valuing perishable products beyond the expiry date: Educated consumers have a greater WTP differential for products labeled* ***use-by*** *than for products labeled* ***best-before****.*

When consumers are educated, the difference between the mean WTP differential is only significant beyond the expiry date (-0.74 (BB) vs. -0.91 (UB), $p = 0.06$, M.W. Rank Sum Test). At all other expiry dates, the difference is not significant ($p > 0.37$ across all other dates, M.W. Rank Sum Test).

Hypothesis 3 concerns the effect of education returns on WTP. To test the hypothesis, we compare WTP level data (Figure 1.1) separately for each date mark type in order to detect a treatment effect of education.

Result 3 *Education has no statistically significant effect on WTP: WTP of native and educated consumers are statistically indistinguishable, both for perishable food labeled* ***best-before*** *and for perishable food labeled* ***use by****.*

Under a *BB* label, mean WTP increases from €2.28 for consumers drawing on their native information to €2.46 for consumers that have undergone education about label meanings. Under a *UB* label, mean WTP decreases from €2.11 under native information to €1.95 following education. These effects are in the direction predicted by hypothesis 3, but do not reach conventional levels of statistical significance ($p = 0.89$ and $p = 0.78$ for *BB* and *UB*, respectively, M.W. Rank Sum Test). Given the heterogeneity among shoppers, education induces the desired impact on valuation of perishable food, but even on the date of expiry not at a significant level.

Result 4 *Intertemporal WTP differences for perishable food differ between educated consumers and consumers drawing on native knowledge, but only for expired food products labeled* ***use-by****.*

When goods are labeled *UB* and consumers trade off an expiry date on the day of the experiment with an expiry date on the date before the experiment, the difference between the mean intertemporal WTP differential of native and educated consumers is significant (-0.67 (n) vs. -0.91 (e), $p = 0.03$, M.W. Rank Sum Test). For good labeled *BB* and at all other expiry dates, the difference is not significant ($p > 0.31$ across all other dates, M.W. Rank Sum Test).

1.5.3 Econometric evidence

We complete the analysis of the experiment by drawing on the additional data about subjects' demographic and shopping characteristics that the survey at the outset of the experiment collected. This econometric approach additionally serves as a robustness check on our main results and allows us to formally test for interaction effects.

Tables 1.5, 1.6 and 1.7 report on the econometric results. Table 1.5 explains WTP for goods with an expiry date on the day of the experiments as a function of the treatment conditions and subjects' demographic characteristics, moving from very parsimonious (only treatment conditions) to increasingly richer specifications. We use two estimation methods: A basic OLS regression on the mid-points of the buy-back intervals and a tobit estimation. The baseline across all methods and specifications is the treatment condition *BB/native*.

Table 1.5: Treatment effects on WTP

	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
UB Label	-0.330	-0.170	0.0221	-0.418	-0.333	0.0376
	(-0.95)	(-0.35)	(0.04)	(-0.70)	(-0.39)	(0.05)
Education	0.00833	0.172	0.323	0.0515	0.138	0.436
	(0.02)	(0.37)	(0.62)	(0.09)	(0.17)	(0.54)
UB Label × Education		-0.339	-0.644		-0.178	-0.893
		(-0.49)	(-0.81)		(-0.15)	(-0.71)
Constant	2.357***	2.284***	2.179***	2.980***	2.941***	2.745***
	(8.04)	(6.81)	(3.45)	(5.57)	(4.75)	(2.89)
Socio-dem. controls	No	No	Yes	No	No	Yes
N	100	100	100	100	100	100

Note: Dependent variable: WTP at expiry date. Baseline: Date mark *BB* at *n*ative information, no socio-demographic controls. Standard errors clustered at individual level. $^{*}p > 0.1$, $^{**}p > 0.05$, $^{***}p > 0.01$

Irrespective of method and specifications, the coefficient of the *UB* treatment does not attain statistical significance. Once socio-demographic controls are included (specifications (3) and (6)), the quantitative relevance of the *UB* mark decreases by an order of magnitude. The coefficient of the *e*ducation treatment has the predicted positive effect on WTP and becomes quantitatively more relevant by including socio-demographic controls, but not reaching statistical significance. Quantitatively, when sociodemographic controls are included, a tobit estimation, capable of accounting for the censored nature of our data, predicts an increase of €0.44 in WTP for goods labeled *BB* following education. In the same specification, we also recover a negative interaction effect between a *UB* label and education. The magnitude of the interaction effect more than offsets the education effect on WTP under a *BB* label alone. This provides indicative evidence that jointly, education and the *UB* mark

decrease WTP, thus supporting the non-parametric tests of hypotheses 1 and 3.

Table 1.6 explains the intertemporal WTP differentials measured in the experiment as a function of remaining shelf-life, treatment condition, and subjects' characteristics, again moving from very parsimonious to increasingly richer specifications. We again use two estimation methods: A basic OLS regression on the mid-points of the MPL intervals, and a tobit estimation. The baseline across all methods and specifications is the treatment condition *BB/native* with a remaining shelf-life of one week and no socio-demographic controls.

Table 1.6: Treatment effects on WTP differentials

	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
Expires tomorrow	-0.212***	-0.212***	-0.212***	-0.294***	-0.294***	-0.291***
	(-6.10)	(-6.10)	(-6.01)	(-5.83)	(-5.83)	(-5.81)
Expired yesterday	-1.205***	-1.205***	-1.205***	-1.761***	-1.762***	-1.748***
	(-21.04)	(-21.02)	(-20.71)	(-14.21)	(-14.21)	(-14.33)
UB label	0.0552	0.112	0.105	0.0663	0.171	0.163
	(0.99)	(1.42)	(1.39)	(0.78)	(1.44)	(1.45)
Education	0.0268	0.0813	0.0964	0.0361	0.138	0.160
	(0.48)	(1.07)	(1.29)	(0.42)	(1.18)	(1.44)
UB x Education		-0.110	-0.108		-0.206	-0.195
		(-0.99)	(-1.02)		(-1.22)	(-1.23)
Constant	0.378***	0.350***	0.367***	0.524***	0.472***	0.505***
	(6.57)	(5.65)	(3.75)	(5.83)	(4.93)	(3.37)
Socio-dem. controls	No	No	Yes	No	No	Yes
N	510	510	510	510	510	510

Note: Dependent variable: WTP differential. Baseline: Treatment condition *BB/native*, comparison with a remaining shelf-life of one week and no socio-demographic controls. Standard errors clustered at individual level. $*p > 0.1$, $**p > 0.05$, $***p > 0.01$

Across specifications, the coefficient estimates for the pooled data show that remaining shelf life consistently matters for WTP. Reducing remaining shelf life from one week to one day decreases WTP by €0.21 to €0.29. Expired food items are valued between €1.21 and €1.76 less. These effects are all statistically significant at the 1% level. But neither a *UB* date mark nor education have a statistically significant impact on the WTP differential. Likewise, the interaction effect between the *UB* and *e*ducation treatments is still negative, as observed in the non-parametric tests,

but does not attain statistical significance, even when socio-demographic controls are included. This provides further evidence for the findings from testing hypotheses 2 and 4 that label type and education have little detectable impact on the valuation of food items.

Table 1.7: Treatment effects on WTP differentials for expired eggs

	OLS			Tobit		
	(1)	(2)	(3)	(4)	(5)	(6)
UB label	-0.0185	0.144	0.150	-0.0632	0.334	0.367
	(-0.26)	(1.31)	(1.46)	(-0.35)	(1.32)	(1.61)
Education	-0.0831	0.0734	0.0806	-0.151	0.237	0.273
	(-1.16)	(0.72)	(0.80)	(-0.84)	(0.96)	(1.24)
UB x Education		-0.317**	-0.271*		-0.798**	-0.660*
		(-2.23)	(-1.88)		(-2.28)	(-1.96)
Constant	-0.734***	-0.814***	-0.930***	-1.231***	-1.424***	-1.478***
	(-11.58)	(-11.14)	(-7.75)	(-7.13)	(-7.01)	(-4.98)
Socio-dem. controls	No	No	Yes	No	No	Yes
N	170	170	170	170	170	170

Note: Dependent variable: WTP differential between eggs expiring on day of experiment and expired eggs. Baseline: Date mark *BB* with *n*ative information, no socio-demographic controls. Standard errors clustered at individual level. *$p > 0.1$, **$p > 0.05$, ***$p > 0.01$

Restricting the pooled sample to observations where the intertemporal comparison only concerns food items just before and after expiry, table 1.7 reports again no significant treatment effects of the UB label or education, but a significant and negative treatment effect for their interaction: For consumers educated about the meaning of date marks, WTP for food labeled UB is significantly lower. Adding socio-demographic controls attenuates the statistical significance of this result somewhat, but the results of the parametric analysis remain consistent with the non-parametric results. Taken together, they suggest that educating consumers about the meaning of date marks makes their valuation of food more responsive to the safety message of *use-by* labels. This provides a pathway through which education could lead to more unsafe food being discarded. At the same time, education does not make consumers' valuation more responsive to the quality message of *best-before* labels and therefore does not provide a pathway toward less food being discarded that is still of good quality.

1.6 Discussion and further experimental evidence

Results 1 through 4 and the econometric evidence demonstrate the consumers pay attention to the calendar *date* on the date mark. They raise doubts, however, about the ability of information-based policies to impact consumers' valuation of packaged food products through variations in the date mark *type*. While the general effects on the formation and evolution of WTP go in the desired direction, the treatment effects affect an insufficient share of subjects in their respective groups consistently enough to result in impacts of statistical or economic significance. Our sample size was chosen to detect effects comparable to price promotions conventionally employed by retailers to sell off items close to their expiry date. As the results make clear, the effect of variations in the date mark type on consumers' valuation is not as large as price promotions in the real world.

The results also indicate that educating consumers can produce asymmetric effects. The consistently negative impact on WTP levels of the interaction effect of labeling food with a *UB* date mark and educating consumers provides some evidence that educating consumers decreases consumers' valuation of unsafe food products. This makes it more likely that health-related discarding of food will increase. There is no evidence, however, that an education campaign can increase the valuation of *BB*-labeled products. This makes less likely that education about date mark types has the potential to decrease food waste.

To look for a possible explanation behind the results of the in-store experiment, we designed and conducted a follow-up experiment. The experiment builds on the hypothesis that information-based policies using date marks fail because shoppers pay little attention to the information conveyed through this vehicle. By design, it allows us to test of whether shoppers notice differences in date mark type (*BB* versus *UB*) and expiry dates in a choice-relevant setting. We summarize its design and results before using its evidence to re-evaluate our main results.

The experiment was conducted in the same in-store setting as the MPL experiment.[22] Shoppers passing through the store lobby were approached and asked whether they would participate in a tablet-based survey, with a carton of free-range eggs as a reward. Filter questions eliminated shoppers that do not purchase eggs

[22] See the appendix for the flowchart of this experiment.

for dietary reasons. Shoppers completed a survey on shopping habits and food preferences before being offered their reward. Experimental treatments began at this step in the procedures and differed with respect to the rewards on offer. Every participant was offered two cartons of free-range eggs, one of which they could pick as their reward. In one treatment condition, labeled *mark*, the two cartons differed by date mark (*BB* versus *UB*) and by brand (*A* vs. *B*)[23], randomly mixed. In the other treatment condition, labeled *date*, the two cartons differed by expiry date (*day of the experiment* vs. *day after the experiment*) and by brand (as above), randomly mixed. The associations between the two brands and the treatment dimensions were additionally randomized, resulting in two *combinations*, 1 and 2. Before selecting their preferred carton, the survey instrument asked shoppers about the criteria that they use when buying eggs in a supermarket and asked them to apply the same criteria to their choice in the experiment. After choosing, participants proceeded to a structured interview about the reasons for their choice. The structure was sequential: The interview progressed from open-ended to increasingly targeted questions about possible differences in the date mark attached to the two cartons and stopped as soon as a subject correctly pointed out the difference. This allowed subjects to be classified in one of four groups: Those who were attentive to the date mark and for whom it was choice relevant; those who were attentive, but for whom it was choice-irrelevant; those who could be guided to be attentive and were therefore theoretically reachable by a date mark; and finally those who remained inattentive to date marks even after guidance by the experimenter.[24] The interview concluded with participants being given the option of revising their choice.

A sample of 160 individuals was assigned to the two treatments (*mark* versus *date*) and, within each treatment, to one of the two brand-treatment combinations (1 or 2). The assignment was based on a randomized assignment protocol that produced a balanced assignment of 80 in each treatment and 40 for each brand-treatment combination. Table 1.8 displays the results of the experiment.

The experimental evidence points to a number of observations that help interpret

[23] We selected two brands of free-range eggs, REWE and Heitlinger, that retailed in the same price band in order to minimize strong brand-specific differences.

[24] Specifically, subjects who mentioned the difference between the date marks on the cartons without prompting by the experimenter were classified as 'choice relevant' if they mentioned the difference as the reason for their choice and 'choice irrelevant' if not. Subjects who recognized the difference after being guided to the date mark were classified as 'reachable', that is attentive after additional effort by the experimenter. All other subjects who concluded the guidance without noting the difference between the date marks were classified as 'inattentive' to date mark labels.

Table 1.8: Follow-up experiment - summary statistics

	Mark		Date		
	Comb. 1	Comb. 2	Comb. 1	Comb. 2	Total
Number of subjects	40	40	40	40	160
Choice relevant	0	0	6	3	9
Choice irrelevant	0	0	6	2	8
Reachable	0	2	10	12	24
Inattentive	40	38	18	23	119
UB≠BB (share)	0.85	0.73	0.73	0.8	0.78
UB correct meaning (share)	0.55	0.43	0.6	0.38	0.49
BB correct meaning (share)	0.95	0.83	0.9	0.8	0.87
Choice revised	2	1	0	3	6

the results of the MPL experiment. First, as expected, brand-treatment combinations do not matter (combination 1 vs. combination 2 for mark and date treatments, $p > 0.81$, chi-square test). Second, pooling the data across brand-treatment combinations, shoppers are much more inattentive about the date mark type than about the expiry date (78 versus 41, $p < 0.01$, chi-square test). Third, irrespective of treatment, there is a significant difference between the *use-by* and *best-before* label: While 78% of subjects agree that the two labels have different and distinct meanings, only 49% correctly interpret the former as opposed to 87% for the latter ($p < 0.01$, chi-squared test).[25]

The evidence from the follow-up experiment suggests that some explanations for the results of the modified MPL experiment are more plausible than others. The ineffectiveness of date mark type manipulations on their own (result 1) is likely a result of consumers' inattentiveness to what is a small visual change in the date mark. Choosing more salient visual cues could be a remedy for this inattentiveness. The information about expiry dates, which is also visually more prominent, has a larger impact and is therefore choice relevant, coherently with the intertemporal WTP differentials (result 2). The most important insight from the follow-up experiment is about result 3: Contrary to the assumptions behind some food waste policies (EU-Council, 2016), consumers are already relatively well informed about the meaning of *best-before* date marks. As a result, it is unsurprising that the education treatment causes no measurable change in willingness to pay in the BB condition. Education, however, does impact on WTP in the UB condition, since a substantial share of

[25] More evidence for misinterpretation of the UB date mark comes from those individuals who accepted to revise their choice: Without exception, these consumers opted for a carton labeled UB in exchange for a carton labeled BB.

shoppers misinterpret the *use-by* date mark.

1.7 Policy Implications

Our four core findings have immediate policy implications, both individually and jointly. Our first finding, namely that consumers typically care about and are attentive to expiry dates, is important: Expiry dates matter to consumers, and food labeling policies should reflect that. These policies also benefit from the experimental affirmation that a link exists between expiry date and food valuation. Date marks therefore constitute a valid entry point for policy intervention since consumers' choices demonstrably respond to variations in expiry dates. Any reform in date marking regulations should therefore be cognizant of expiry dates affecting how consumers value food items.

Policy-makers in the EU are especially concerned with the consequences of date mark *misinterpretation* on food waste production (EU-Council, 2016). Accordingly, the European Union's "Farm to Fork Strategy", part of the European Green Deal, foresees the development of new rules on date marking by the end of 2022 to reduce risks of misunderstanding and misuse of date labelling. Identifying new ways of presenting date labels is one key task included in the strategy.[26] This task should take account of our second finding, that inattention to the date mark *type* is widespread. Inattention and misinterpretation, while often leading to similar problems in choice outcomes, are different phenomena with different root causes. Any future evaluation of how date labels are presented should assess not only whether consumers correctly interpret them once made aware of their presence. It should also assess the extent to which the new design can overcome consumers' lack of attention to label *type* at the point of sale. Only those date label designs should be considered in the final round that demonstrably support consumers' attention to the presence of the date label and its type.

Policies aimed at consumer education about food labeling attempt to correct existing misinterpretations. Our third finding both supports and challenges such policies. We found that educating consumers about label meaning has asymmetric

[26]This task is to involve consultations with the relevant stakeholders, an impact assessment procedure and a consumer research: https://ec.europa.eu/food/safety/food-waste/eu-actions-against-food-waste/date-marking-and-food-waste_en.

effects: Education about the *use-by* label affected consumers' valuation, reducing it. Education about the *best-before* label did not. Policy-makers' expectations about the effects of consumer education should include the possibility of such unanticipated outcomes. Education may further health and safety objectives of preventing the consumption of potentially unsafe food while achieving little in the way of reducing food waste. This policy objective likely requires different routes of intervention.[27]

1.8 Concluding Remarks

Regulations that aim to inform consumers on important features and consequences of their purchase and management decisions about perishable goods are timely. Given the wider impacts of producing, harvesting, and delivering food to consumers' homes, helping consumers to avoid food waste while ensuring public health deserves policy-makers' attention. Date-marks are a plausible approach to providing this help, and initiatives to educate consumers about how to act based on date-marks merit careful assessment.

One challenge for coming to a better assessment of date-marking policies is the nature of the evidence base from which researchers and policy-makers can argue. Previous research has made substantial contributions towards building this evidence base through large-scale surveys, vignette experiments, and laboratory studies. Due to a number of empirical challenges, causal evidence has been more difficult to obtain in order to speak to the question of how date marks types and education causally affect consumers' valuation of perishable food. Our present paper is an attempt to provide such causal evidence for the academic and policy discussion.

In our mind, the evidence presented in this paper makes two contributions. One is the key finding that date mark types, as used at present, are not impactful for consumers' valuation of perishable food products and that educating consumers about their meaning is conducive to the health and safety objectives implicit in date marking food, but not to the objective of preventing food waste. Our follow-up experiment suggests that the explanation for this phenomenon lies in the existing

[27] Along these lines, our paper can also indirectly contribute to the literature on the design of policies to affect consumers' behaviour in the direction of reducing food waste. See, among others, Hamilton and Richards (2019) and Yu and Jaenicke (2021)

asymmetry in consumers' understanding of the two date marks currently in use.

The other contribution is the nature of the evidence that our paper presents. The evidence emerges from the combination of three interlinked building blocks. The first is a preliminary survey, which informs the choice of a perishable food item that lends itself to the experimental manipulation of date marks without compromising research ethics or the safety of participants. With the help of this choice, we believe that we overcome a core challenge for causal identification, namely varying the date mark without varying the underlying product. The second building block is a modified multiple price list experiment that exchanges the laboratory setting and its student subjects for an in-store setting with members of the general population. These procedural choices require an experimental interface that is accessible for a wide range of shoppers coming to the store. It also requires a new design approach in the form of a buy-back mechanism since we cannot rely on protocol compliance to overcome free disposal censoring. The third building block is a follow-up experiment that pursues the hypothesis that the treatment effects plausibly result from the nature of demand-side information. Jointly, these building blocks demonstrate that it is feasible to overcome at least some of the empirical challenges of generating causal evidence to understand information-based policies. Moreover, they demonstrate that the policy proposals are unlikely to accomplish their desired objectives. And they provide at least one explanation why these proposals are likely to underperform, thus informing the development of policy alternatives.

The present paper provides conceptual and methodological points of departure for future research. Much of this paper hinges on the question of how well its evidence generalizes to other food categories. Future work will need to explore this as well as the question of how this approach could be extended to other categories of perishable products. This exploration should also consider the non-food domain, where issues of waste and (planned) obsolescence raise similar issues. While we believe that the in-store experiment takes important steps towards enhanced external validity of the findings, our attempts to conduct this research as a fully-fledged natural field experiment were unsuccessful. We remain hopeful that this avenue can be embarked upon in the future, while respecting the requirements of safety and ethics. Finally, the paper is limited to testing how current date marking practices

perform. Clearly, we cannot rule out, and would in fact strongly suspect, that there exist alternative information-based policies that can make a significant contribution towards increasing consumers' valuation of perishable, but safe foods and reduce food waste. Identifying these alternatives remains a research priority.

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Chapter 2

Image Considerations in Second-Hand Adoption: A Vignette Experiment

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Abstract

Despite growing recognition that image considerations underlie the adoption of second-hand goods, research in this domain has largely overlooked the possibility that different image dimensions operate through distinct channels. Using a vignette experiment on a nationally representative Italian sample ($N = 10,496$), this paper disentangles the effects of three image perspectives – self-image, social image, and second-order self-image (beliefs about others' self-image) – across four main dimensions of second-hand adoption: financial, social, environmental, and hygiene. Contrary to findings in other behavioral domains, self-image emerges as the stronger predictor of intention to adopt second-hand, with the largest gaps over social image for hygiene and social-approval dimensions and the smallest – though still significant – for environmental considerations. Within self-image, hygiene and financial considerations are the strongest correlates of adoption, while environmental consciousness is the weakest; yet it becomes relevant under other-oriented perspectives (social and second-order), suggesting it operates more as a social signal than a private value. Participants also systematically underestimate how positively others view themselves when engaging in second-hand adoption, and this underestimation depresses adoption intentions more than overestimation boosts them – a pattern consistent with pluralistic ignorance that may act as a self-reinforcing barrier to adoption.

Key words: Second-hand adoption · Self-image · Social image · Second-order beliefs · Vignette experiment

JEL codes: D12 · D83 · D91 · Q50

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2.1 Introduction

The past decades have witnessed a significant increase in the demand for a broad variety of pre-owned goods, ranging from electronic devices and appliances to furniture, clothing, and even sports equipment (Gilal et al., 2024). Second-hand adoption, that is, the acquisition of pre-owned goods through specific channels of exchange (Guiot & Roux, 2010), is considered a fundamental practice for achieving the transition to a circular economy (Abdul-Rahman & Wright, 2014; Ekins et al., 2020). As a form of product reuse, aimed at extending the life of goods and reducing the demand for new production, second-hand adoption also plays a fundamental role in the waste management hierarchy established by the Waste Framework Directive.[1] To put its relevance in perspective, consider its impact on the fashion market: it is estimated that purchasing 100 second-hand clothing items can displace between 60 and 85 new purchases (Farrant et al., 2010) and reduce in turn the impact on climate change by up to 42% and on water scarcity by 35–53% (Klooster et al., 2024).

Despite its beneficial effect for the environment, engagement in second-hand adoption, at least in Europe, remains quite low. For instance, the survey by Pérez-Belis et al., 2017 involving a representative sample of Spanish consumers highlights that less than 1% of respondents had ever purchased small used appliances. Understanding what holds consumers back – and what draws them in – remains an open question with both theoretical and practical relevance, especially for policymakers (Congiu et al., 2025; Gilal et al., 2024).

The investigation of the factors underlying demand for second-hand goods has recently drawn attention to potential image concerns arising from their adoption (Congiu et al., 2025; Gilal et al., 2024; Hur, 2020; Silva et al., 2021). Consumers care about what buying second-hand says about them, that is, how they are judged or evaluated for it. Such considerations may entail both barriers and motivators. On the one hand, second-hand goods have historically been associated with financial hardship, inferior quality, or contamination from prior use – factors that can make adoption image-threatening. On the other hand, in an economy of tightening budgets and growing sustainability demands, buying second-hand may also enhance one's image, among others, as an environmentally conscious and financially savvy

[1] https://environment.ec.europa.eu/topics/waste-and-recycling/waste-framework-directive_en

consumer. These observations hint at three important gaps in the investigation of image considerations in the second-hand literature.

First, as just argued, whether image considerations act as barriers or motivators depends on the "valence" of the associations they activate, that is, whether these associations are positive or negative. The literature on second-hand adoption has identified four core evaluative dimensions along which image-relevant associations may form: financial, environmental, social, and hygiene-related (see, e.g., the systematic review by Gilal et al., 2024). Each dimension admits both positive and negative readings: adopting second-hand may be evaluated as financially wise or financially desperate, environmentally conscious or wasteful, embarrassing or socially commendable, and disgusting or hygienic. Ultimately, second-hand adoption may depend on a balance between image-depressing and image-enhancing associations across these different evaluative dimensions, and this calls for measures sensitive to valence, not merely to whether image concerns are present.

Second, image concerns need not stem from a single source of evaluation. Psychological works on the self have long emphasized that individuals evaluate their actions from multiple perspectives, including how they see themselves and how they believe they are seen by others (Baumeister, 1982; Chen & Critcher, 2025). Accordingly, social psychology and behavioral economics distinguish between self-image and social image concerns (Bénabou & Tirole, 2006; Gausel & Leach, 2011; Manstead & Semin, 1981). Self-image refers to how a person perceives and evaluates themselves, that is, whether one's choices feel consistent with one's own values and moral standards. Social image, by contrast, refers to how a person is perceived and evaluated by others, that is, the impression one makes in public and the signals one sends to friends, colleagues, or strangers. These evaluations need not necessarily coincide. A consumer might feel privately proud of engaging in second-hand while worrying about how it looks to others; or might enjoy the social signal of sustainability while feeling, privately, no particular attachment to it. Research in other behavioral domains – such as on dishonesty (Bašić & Quercia, 2022; Fries et al., 2021), prosocial behavior (Filiz-Ozbay & Ozbay, 2014; Friedrichsen & Engelmann, 2018; Lacetera & Macis, 2010) and social comparison (Bottan et al., 2025; Eriksson et al., 2017; Petrishcheva et al., 2023) – has shown that self-image and social image can differ in

the intensity of their behavioral effects. Yet in the second-hand literature, the two have not been properly disentangled, and social image has received by far greater attention. Disentangling their effects is relevant not only from a scientific standpoint but also for designing policies targeting different image motives.

Third, individuals may also hold beliefs about how other people privately evaluate themselves (Bradford et al., 2015; Chen & Critcher, 2025). These beliefs may serve as a signal of how accepted, valued, or desirable a behavior is within a social environment and may therefore affect individual behavior. Since private evaluations are generally unobservable, beliefs about them are necessarily inferential and may depart from actual evaluations, generating instances of "pluralistic ignorance" (e.g., Andre et al., 2024; Barigozzi & Montinari, 2025; Bursztyn et al., 2020). This possibility may be especially plausible in the context of second-hand adoption, given its long-standing association with financial hardship and contamination. Consumers may privately evaluate second-hand adoption more favorably than they believe others do, causing them to underestimate how positively the behavior is privately evaluated by others and potentially reducing their willingness to adopt it. Whether such a misperception characterizes second-hand adoption remains largely unknown. To study these beliefs, we introduce the notion of second-order self-image, namely individuals' beliefs about how others evaluate themselves when engaging in a given behavior.

This paper addresses these literature gaps. It investigates image considerations in second-hand adoption by (i) distinguishing between image perspectives, namely self-image, social image, and second-order self-image; (ii) parsing each into dimensions linked to core determinants of second-hand behavior (financial, environmental, social and hygienic); and (iii) using semantic differentials to assess the valence of each image dimension. Our central question is to study the differential effects of image perspectives and their relations, as declined in four evaluative dimensions, on the decision to adopt second-hand goods.

For this purpose, we set up a vignette-based survey experiment. Participants are presented with a hypothetical choice scenario involving a second-hand product and are asked to state their intention to adopt it. Then, for each of the four image dimensions, the respondent is presented with a pair of opposite statements (one

positive and one negative) and asked to indicate the degree of agreement with one over the other. This task is proposed to each respondent from both self-image and social-image perspectives. In the same setup, we also elicit second-order beliefs about others' self-image (second-order self-image). This allows us to examine whether such perceived private opinions affect individual intention to adopt second-hand, building on extensive evidence that social norms, of which these beliefs are a core component, drive behavior across a wide range of domains (Anderson & Dunning, 2014; Cialdini & Trost, 1998). It also serves to test whether consumers systematically misperceive others' private evaluations about second-hand and to assess whether such misperception affects adoption intentions.

The experiment was administered by a survey company in October–December 2024 to a large representative sample of the Italian population ($N = 10,496$). Among the three perspectives, self-image dimensions turn out to be the strongest predictors of second-hand adoption, followed by social image and second-order self-image. This suggests that, while all perspectives are relevant for second-hand adoption, personal evaluations drive behavior more than perceived judgments from others, in contrast with the literature on the relationship between social image and self-image concerns. Interestingly, the environmental dimension of self-image does not significantly influence second-hand adoption, indicating that sustainability motivations may be instead more about public signaling than private concern. When self-image evaluations exceed social and second-order self-image, adoption increases; the reverse pattern decreases it, and more strongly so. Finally, we find that consumers perceive that second-hand choices enhance their social image less than their self-image, on all the four dimensions, but also rate themselves as more favorable toward second-hand products than they believe others are, indicating the presence of pluralistic ignorance.

The remainder of this paper is structured as follows. Section 2.2 discusses the image dimensions and perspectives studied in the experiment. Section 2.3 illustrates the experimental procedure and design. The main results and exploratory analyses are presented in Section 2.4 and Section 2.5, while Section 2.6 discusses their implications and concludes.

2.2 Literature review

2.2.1 Image dimensions in second-hand adoption

Image is not a unitary construct but rather comprises multiple evaluative dimensions. Following established practice in research on the self and self-evaluation (Marsh, 1990; Shavelson et al., 1976), this paper adopts a domain-specific approach, parsing image into dimensions that correspond to the main determinants identified in the second-hand literature: financial, environmental, social, and hygiene considerations (see, e.g., the systematic literature review by Gilal et al., 2024).

Financial considerations entail one's image as financially distressed versus financially savvy and value-conscious (Gopalakrishnan & Matthews, 2018; Hur, 2020; Williams & Paddock, 2003). Consumers may refrain from purchasing second-hand clothing because it would make them feel poor and as if they could not provide for themselves and their families, but also engage in it for the satisfaction of acquiring quality products for a fraction of the price (Hur, 2020).

Social considerations reflect one's image as socially approved versus disapproved by one's reference group. Shopping at second-hand stores may evoke embarrassment and fear of stigma (Laitala & Klepp, 2018; Silva et al., 2021), even though this varies across cultures (Xu et al., 2014) and generations, with younger cohorts exhibiting lower social image concerns (Liang & Xu, 2018; Valor et al., 2022). In some contexts, second-hand practices may even trigger pride and perceived social approval (Terzioğlu, 2021). More generally, second-hand consumption may carry symbolic meanings that contribute to social approval and disapproval, as it can be associated with traits such as social responsibility, creativity, and fashion awareness (Cervellon et al., 2012).

Environmental considerations relate to whether second-hand consumption enhances one's image as environmentally responsible. Environmentally conscious consumers are more likely to engage in second-hand practices (Cervellon et al., 2012; Parguel et al., 2017), and adopters report feeling they are "doing their part" for the environment, although some remain uncertain whether others appreciate this contribution (Hur, 2020).

Finally, hygiene considerations involve one's image as clean and disgust-free versus

contaminated. Wearing or using items previously owned by strangers may trigger feelings of disgust, as second-hand products can be perceived as carriers of the previous owner's residue (whether physical or symbolic; see e.g. Rozin et al., 1986), ultimately threatening consumers' sense of cleanliness. This is particularly relevant for products in close contact with the body (Bezançon et al., 2019; Hazée & Van Vaerenbergh, 2021). Hygiene evaluations therefore concern not only perceptions of product quality, but also consumers' sense of personal cleanliness and bodily integrity.

As will be discussed in the next sections, insofar as one strives to preserve one's image, one is expected to be more likely to adopt second-hand the more positive the evaluations are on these four dimensions, that is, whether one feels financially savvy rather than distressed, environmentally conscious rather than wasteful, socially approved rather than disapproved, and not disgusted by second-hand adoption.

2.2.2 Image perspectives

One's image can be conceptualized from diverse social perspectives (Baumeister, 1982; Chen & Critcher, 2025; Manstead & Semin, 1981). In this paper, we focus on three of them: self-image, social image, and second-order self-image. These perspectives are linked by a common concern with evaluation, but differ in the target of that evaluation. The evaluative target may be oneself (self-image), oneself as viewed by others (social image), or other individuals as viewed by themselves (second-order self-image). Empirical evidence suggests that individuals can make valid distinctions among these different evaluative perspectives, from private and public self-focused evaluations (Fenigstein et al., 1975) to beliefs about how they are perceived by others (Carlson et al., 2011).

While the three perspectives are conceptually related, they may influence behavior through distinct motivational channels. Self-image acts as an internal source of motivation. Individuals derive psychological benefits from behaving consistently with moral, social, or personal standards, and incur psychological costs when they fail to do so (Falk, 2021; Gausel & Leach, 2011; Ploner & Regner, 2013; Regner, 2018). Experimental studies show that individuals often refrain from behaving selfishly or dishonestly even when their actions cannot be observed by others, indicating that

the maintenance of a positive self-image can serve as a powerful source of motivation (e.g., Falk, 2021; Ploner & Regner, 2013). Related evidence suggests that similar self-evaluative considerations extend to consumption and market settings, where individuals derive utility from behaving consistently with the type of person they wish to be (Gneezy et al., 2012). Qualitative evidence indicates that such mechanisms may also be relevant for second-hand adoption (Hur, 2020).

Social image operates as an external source of motivation, making behavior responsive to anticipated judgments from others. Individuals derive psychological benefits (costs) from conveying a favorable (unfavorable) impression of themselves, that is, from being perceived as possessing desirable rather than undesirable characteristics (Andreoni & Bernheim, 2009; Bénabou & Tirole, 2006; Bursztyn & Jensen, 2017). Since social evaluations may affect reputation, status, and social inclusion, individuals often modify their behavior in response to actual or anticipated observation by others. A large experimental literature (for a review, see Bursztyn & Jensen, 2017) documents social-image effects in domains such as honesty (Bašić & Quercia, 2022; Fries et al., 2021), social comparison (Bottan et al., 2025; Eriksson et al., 2017; Petrishcheva et al., 2023), charitable giving (Adloff & Pondorfer, 2026; Lacetera & Macis, 2010) and cooperation (Filiz-Ozbay & Ozbay, 2014; Grimalda et al., 2016).

The third perspective, second-order self-image, captures beliefs about how other individuals evaluate themselves. Whereas self-image focuses on one's own private evaluations and social image on the evaluations one expects from others, this perspective involves beliefs about the private evaluations held by others. In this sense, it occupies an intermediate position between self-focused and other-focused forms of evaluation: individuals continue to reason about others, but the object of inference is not how others evaluate them, rather how others evaluate themselves. This perspective is rooted in a broader literature on social cognition showing that people routinely make attributions to others about mental states, including beliefs, intentions, emotions, values, and norms, and use such information to form mental models of social actors (Bradford et al., 2015; Happé, 2003).

The image perspectives discussed above complement the image dimensions introduced in Section 2.2.1. The latter identify the content of image evaluations (financial, social, environmental, and hygienic), while the former identify the view-

point from which those evaluations are formed. Together, they define the image framework adopted in our analysis, summarized in Table 2.1.

Table 2.1: Image perspectives and dimensions

Image Dimension	**Self-Image** *(How I see myself)*	**Social Image** *(How others see me)*	**Second-Order Self-Image** *(How others see themselves)*
Financial	Do I feel financially savvy?	Do others see me as financially savvy?	Do others feel financially savvy?
Social	Do I feel socially approved?	Do others approve of my choice?	Do others feel socially approved?
Environmental	Do I feel environmentally conscious?	Do others see me as environmentally conscious?	Do others feel environmentally conscious?
Hygienic	Do I feel disgusted by the product?	Do others think I am disgusted?	Do others feel disgusted?

As anticipated in Section 2.1, while image evaluations associated with second-hand goods have been documented across financial, environmental, social, and hygienic dimensions (Gilal et al., 2024; Hur, 2020; Silva et al., 2021), these concerns have largely been examined through the lens of social image. Existing evidence suggests that consumers often perceive second-hand adoption, particularly in the context of clothing, as potentially detrimental to the image they project to others. Recurring concerns include social embarrassment, low social acceptance, associations with lower socioeconomic status, contamination, and fears that second-hand products may signal financial hardship or undesirable personal characteristics (Cervellon et al., 2012; Congiu et al., 2025; Guiot & Roux, 2010; Hur, 2020; Laitala & Klepp, 2018; Silva et al., 2021). Much less is known, however, about how consumers privately evaluate second-hand adoption or about how they perceive the private evaluations held by others. Consequently, existing evidence provides only a partial picture of image concerns in second-hand markets, leaving largely unexplored how self-image and second-order self-image influence second-hand adoption. At any rate, one's willingness to engage in second-hand adoption is expected to be higher the more positive are one's self-image, social image, and second-order self-image.

2.2.3 Relationships between perspectives

We are not aware of studies systematically investigating the relationships among self-image, social image, and second-order beliefs in second-hand adoption. Exper-

imental studies in other social domains offer some tentative predictions about how the three image perspectives may relate to one another in second-hand adoption.

First, research outside the context of second-hand adoption, including studies of prosocial behavior, honesty, ethical consumption, and social influence, suggests that social image concerns often loom larger than self-image concerns (e.g., Bašić & Quercia, 2022; Bottan et al., 2025; Braghieri, 2024; Ewers & Zimmermann, 2015; Filiz-Ozbay & Ozbay, 2014; Friedrichsen & Engelmann, 2018; Lacetera & Macis, 2010). This asymmetry arises because social evaluation carries tangible consequences, such as reputational damage and social exclusion, whereas private self-evaluation, while still psychologically meaningful, lacks such external stakes (Manstead & Semin, 1981). Individuals therefore strive more to preserve their social image than self-image. For instance, Rind and Benjamin, 1994 and Ewers and Zimmermann, 2015 find stronger compliance effects when social image is at stake, while Bašić and Quercia, 2022 show that lying behavior decreases more when observers' presence is made salient (social image) than when participants' attention is directed toward themselves (self-image). However, this pattern is not universal: Teyssier et al., 2015 and Friedrichsen and Engelmann, 2018 found no statistically significant differences in willingness to pay for fair-trade products between treatments making social- versus self-image salient.

As argued above, the existing literature on second-hand has predominantly focused on social image concerns (e.g., Laitala & Klepp, 2018; Silva et al., 2021) and has overlooked how individuals privately evaluate themselves. If second-hand is perceived to pose image concerns, and if it is more relevant to preserve one's social image than self-image, then we should expect individuals to evaluate themselves more negatively on social image dimensions than self-image ones, and to be more sensitive to changes in social image than self-image.

Second, the literature suggests that one's private evaluations often diverge from those attributed to others, giving rise to what has been termed "pluralistic ignorance" (Bursztyn et al., 2020). A recurrent situation is that of an individual who believes that most others do not support a progressive value while he privately does, and suppresses public expression of that value in response. For example, Bursztyn et al., 2020 show that although 87% of Saudi men privately support women working

outside the home, only 25% believe most others share this view, and this belief sustains a social norm not aligned with prevalent private values. Similarly, Barigozzi and Montinari, 2025 find that personal values on gender issues are systematically more progressive than those attributed to others, while Andre et al., 2024 report that U.S. citizens underestimate both the adoption rate (67%) and normative support (76%) for climate change mitigation practices.

The literature is silent on whether pluralistic ignorance occurs for second-hand adoption. However, insofar as individuals hold positive private attitudes toward second-hand (high self-image) but believe that others view it negatively (low second-order self-image), they might be less likely to engage in this behavior themselves.

The next section illustrates the design of our experiment and discusses how it elicits the valence of the four determinants across the three image perspectives and tests for the conjectured relationships.

2.3 Experiment

The vignette experiment was embedded in a larger study. The entire study received approval from the Board for Ethical Questions in Science of the University of Innsbruck and was pre-registered on Open Science Framework (OSF).[2] Among others, the study included general questions about past adoption of second-hand products and intention to adopt them again in the future, as well as scales eliciting personal traits such as one's need for uniqueness (Tian et al., 2001) and pro-environmental behavior (Menardo et al., 2020). The full text of the vignette experiment is available in Section B.2 and, together with the complete material, in the public OSF repository of the project.

2.3.1 Procedure

The survey was implemented and administered by the survey company Scenari from October to December 2024 to a panel of Italian 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.

[2] https://osf.io/wzcqh/

In our sample, representativeness holds for age (mean: 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 macro-region of residence (North-west: 26.93%; North-East: 19.76%; Center: 19.63%; South: 22.77%; Islands: 10.91%).

During the completion of the survey, respondents were unaware of the upcoming questions and could not go back to amend their answers. Respondents were paid €4 for filling in the survey, and could earn up to €4 for matching modal answers in the second-order self-image elicitation task (see Section 2.3.3.2 for details on this task and its incentive scheme).[3]

2.3.2 Vignette

In vignette studies, the respondent is asked to provide judgments or make choices on the basis of a short description of a hypothetical scenario (a "vignette") involving a person, a situation, or an event; the description contains aspects and elements that the researcher expects to be important or decisive when making judgments or choices (Jasso, 2006; Stantcheva, 2023). The methodology of vignettes has become increasingly popular (Ambuehl & Ockenfels, 2017; Ambuehl et al., 2015; Beegle et al., 2012; Folke & Rickne, 2022; Fromell et al., 2021), and employed in diverse domains, such as the investigation of image concerns in social contexts (Manstead & Semin, 1981; Unzueta et al., 2010) and perception of social norms (Barigozzi & Montinari, 2025; Fromell et al., 2021). To the best of our knowledge, no contribution in the literature has applied the vignette methodology to the analysis of second-hand consumption.

Our vignette describes a hypothetical choice scenario in which the respondent is considering the purchase of a second-hand product with a given condition and price. The product featured in the vignette is randomly selected from four types: T-shirt, closet, refrigerator, or smartphone. Each product type is associated with a single predefined model, which does not change across vignettes for the same product (attributes and levels summarized in Table 2.2).

[3]The survey agency employs a point-based reward system where 50 points can be exchanged for a €10 Amazon gift card. Respondents were rewarded with 20 points for the completion of the survey and 5 points for each correct answer in the second-order self-image task.

Table 2.2: Attributes and levels adopted in the vignette

Attribute	Levels
Products and models characteristics	**T-shirt:** a light blue, 100% cotton T-shirt, in your size **Refrigerator:** an LG with 384ℓ capacity, measures 59 $\times$ 203 $\times$ 68 cm, and energy class A++ **Closet:** a wooden closet, with dimensions 226 $\times$ 212 $\times$ 56 cm, 3 compartments (90, 45, 90 cm), two double doors and one single door **Smartphone:** an Honor 90, Android system, with 6.7" display, 256GB physical storage and 8GB RAM, 200MP back camera and 50MP front camera
Conditions	Excellent conditions Good conditions Acceptable conditions
Price level	Free of charge At an affordable price

Each respondent is presented with one and only one vignette. An example, translated from Italian, is provided in the following (randomized elements indicated in italics; see Section B.2 for the other versions of the vignette):

> Imagine you need to buy a *T-shirt*. You find the model you are interested in, *a light blue, 100% cotton T-shirt, in your size*, second-hand. The product is in *acceptable condition*, has been sanitized, and you can pick it up *at an affordable price*.

We chose to randomize the product described because the literature suggests a heterogeneous impact of the determinants of second-hand adoption across products and thus we conjectured that image concerns could differ accordingly. For instance, hygiene considerations are found to be relevant both for clothing and furniture (Bezançon et al., 2019; Roux & Korchia, 2006; Yan et al., 2015), which enter in close contact with one's body, but are less pronounced for furniture than for clothing (Bezançon et al., 2019).

To select these products, we opted for the most widely researched categories in the second-hand consumption literature (see, e.g., Gilal et al., 2024). The product model and its characteristics were specified to enhance the realism of the choice and encourage the respondents to imagine themselves making a real purchase decision.

The full randomization with 4 products (T-shirt, closet, refrigerator, smartphone) $\times$ 3 condition levels (excellent, good, acceptable) $\times$ 2 price levels (affordable price,

free of charge) yields a total of 24 possible versions of the vignette. On average, 437 participants (4.16% of the sample) were presented with the same version. The distribution of demographic characteristics – including age, gender, education, income, employment status, and macro-region – was balanced across products, conditions and price levels, as shown in the balance tables reported in Section B.1.1.

2.3.3 Statements

First, the respondent is asked to state the probability, from 0 to 100, that they decide to adopt the item described in the vignette ("Referring to the situation described, what is the probability that you decide to take the product?"). Then, the respondent is presented with four Likert items evaluating the valence of financial, environmental, social, and hygiene considerations. The items are proposed under the three social perspectives of self-image, second-order self-image, and social image, in this order.

2.3.3.1 Eliciting self-image

Starting with self-image, the respondent is asked to complete the sentence "*If I decided to take the product described...*" by indicating the best fitting statement from a pair of opposite ones (e.g., "*A = I would feel that I am in financial distress*" versus "*B = I would feel that I was using money responsibly*"). Respondents could indicate a complete agreement with the statement A, a stronger agreement with statement A than B, a stronger agreement with B than A, and a complete agreement with B (4-point scale).[4] The statements pertaining to self-image are reported in the following.

"If I decided to take the product described..."

[4]The use of bipolar semantic pairs (e.g., *pleasant – unpleasant*, *good – bad*, or even *supports the family – does not support the family*, etc.), also termed "semantic differential", is common practice in studies on image concerns and social emotions such as shame and guilt. Recently, Chun et al., 2023 and Negash and Akhbar, 2024 have used bipolar semantic pairs to evaluate consumer orientation toward second-hand products and have done it by presenting opposite pairs as extremes of a 4-point scale, similarly to our implementation. For other contributions adopting semantic differentials, see Leduc, 1976; Manstead and Semin, 1981; Reynolds, 2002. For a general overview of semantic differentials, see Fishbein and Ajzen (2011, Ch. 3) and references therein, especially works by Osgood and associates, and the more recent work by Stantcheva, 2023.

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel I am in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

For all these pairs, we opted for an even-numbered scale to rule out situations of indecisiveness (i.e., answers at the middle point) and force respondents to lean more towards one statement than the other (for a discussion on this practice, see Stantcheva, 2023).

2.3.3.2 Eliciting second-order self-image

The elicitation of second-order self-image was based on the matching coordination game proposed by Krupka and Weber, 2013 and widely adopted in the elicitation of second-order beliefs (e.g., Adloff & Pondorfer, 2026; Barigozzi & Montinari, 2025; Gächter et al., 2025; König-Kersting, 2024; Krupka et al., 2022). The respondent is presented with the same battery of Likert items employed to elicit self-image and asked to indicate the answers most commonly given in a representative sample from the same geographical "macro-region" of the respondent.

To clarify to respondents what we meant by "macro-region" and what were the regions belonging to each macro-region, we adopted the EU Nomenclature of Territorial Units for Statistics (NUTS), which subdivides Italy in five sets of regions, and proceeded to list them in the question text.[5] Reference groups were set at the

[5]Accordingly, we distinguished in North-West (Liguria, Lombardia, Piemonte, Valle d'Aosta), North-East (Emilia-Romagna, Friuli-Venezia Giulia, Provincia Autonoma di Trento, Provincia Autonoma di Bolzano, Veneto), Center (Lazio, Marche, Toscana, Umbria), South (Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia), and Islands (Sardegna, Sicilia).

macro-region level since a more granular subdivision, such as that at the regional or province level, would have compromised the power of our analyses, while less granular subdivisions would have obscured relevant cultural and geographical differences in social norms.

Respondents were also told that they would be compensated with €1 each time their answer coincided with the modal response in the reference group (and thus up to a total of €4). The incentivization of the task had the purpose of ensuring an honest and reliable elicitation of second-order self-image, that is, free from response biases such as experimenter demand effects, social desirable responding, and focal point effects (Fallucchi & Nosenzo, 2022; Fallucchi et al., 2026). In fact, the possibility of obtaining a monetary reward offers an incentive for respondents to try their best to match modal answers in the sample of reference, thereby increasing the accuracy and reliability of their stated beliefs. The reward was given to a subset of 400 participants randomly drawn after the data collection process ended; respondents were informed of the drawing mechanism when undertaking the task. Paying a subset of respondents has been shown to be as effective as paying the full sample (Charness et al., 2016) and is a common mechanism in experimental studies on social norms (Barigozzi & Montinari, 2025; Burks & Krupka, 2012; Fromell et al., 2021).

As anticipated, the elicitation of second-order beliefs also serves to investigate the potential misperception of the prevalent self-image, in line with pluralistic ignorance. This can be done by comparing the responses given on second-order self-image with the modal responses for self-image within the reference group.

2.3.3.3 Eliciting perceived social image

Finally, respondents were presented with a battery similar to that to elicit self-image but framed in terms of how others would see the respondent if he decided to take the product described in the vignette. That is:

"If I decided to take the product described, others would think that..."

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel that I was in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

In addition to eliciting social-image determinants, this task enables us to investigate the relationship between self and social image, and whether it affects second-hand adoption.

2.3.4 Registered Hypotheses

We formulate the following hypotheses. First, we hypothesize that the probability to adopt the product described in the vignette is influenced by its condition and price levels, in the following ways:

H.1 The stated intention to adopt the SH product is higher...

H.1.a the better the conditions are.

H.1.b if the product is free.

Based on the literature reviewed above (Section 2.2), we hypothesized that willingness to engage in second-hand would be higher the more positive image is, regardless of the perspective under consideration. More specifically, on the impact of self-image, social image, and second-order self-image on second-hand adoption, we hypothesize what follows:

H.2 The stated intention to adopt the SH product is higher the stronger the respondent's belief that, by adopting the SH product...

H.2.a s/he is financially savvy.

H.2.b s/he receives social approval.

H.2.c s/he is environmentally conscious.

H.2.d s/he does not feel disgusted.

H.3 The stated intention to adopt the SH product is higher the stronger the respondent's belief that, by adopting the SH product...

H.3.a s/he is seen by others as financially savvy.

H.3.b s/he seen by others as worthy of social approval.

H.3.c s/he is seen by others as environmentally conscious.

H.3.d s/he seen by others as not disgusted.

H.4 The stated intention to adopt the SH product is higher the stronger the respondent's belief that, by adopting the SH product, most of others from the same macro-region...

H.4.a feel they are financially savvy.

H.4.b feel they receive social approval.

H.4.c feel they are environmentally conscious.

H.4.d do not feel they are disgusted.

Finally, we advance two conjectures regarding the relationship between image perspectives, both motivated by the literature. First, insofar as individuals are more sensitive to the preservation of their social image than their self-image, and assuming that second-hand is more likely to pose image concerns than first-hand consumption, we conjecture that respondents will report lower scores for the four determinants under social image than self-image. We also expect social image scores to affect second-hand adoption more than self-image ones. We hence formulate the following hypotheses:

H.5.a Scores reported for the four determinants under social image are systematically lower than under self-image.

H.5.b The probability to adopt the second-hand product is more sensitive to social-image dimensions than self-image.

Second, given that in many social domains individuals tend to attribute less progressive values to others than those held collectively (pluralistic ignorance), we conjecture that this also occurs for second-hand. In particular, we expect respondents to report higher scores on the self-image determinants than second-order self-image ones, and to systematically underestimate the modal self-image scores elicited in their macro-regional subsample of reference. This leads to these final hypotheses:

H.6.a Scores reported on self-image determinants are systematically higher than those on second-order self-image determinants.

H.6.b There is a systematic underestimation of the modal self-image scores observed in respondents' macro-regional reference subsample.

Note that, with respect to our pre-registration, we inverted the order of *H.3* and *H.4* to improve the clarity in the presentation of results. For the same reason, we have also laid down Hypotheses *H.5* and *H.6* that formalize analyses previously stated only narratively.

2.4 Main Results

2.4.1 Product adoption across vignette attributes

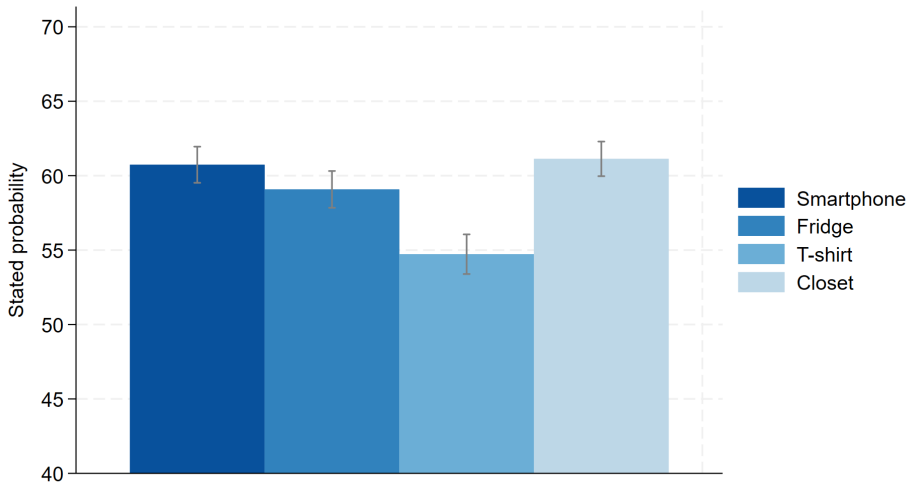
We start by analyzing how the stated probability of adopting the product described in the vignette varies across products, their condition and price. We had no expectations about potential differences in stated probabilities between products. However, it is reasonable to expect them to decrease in price and worsen conditions.

The average stated probabilities by product are graphed in Figure 2.1, and those by conditions and price levels in Figure 2.2. Regarding the former, we find that the average stated probabilities of taking a second-hand closet (61.13), a smartphone (60.73), or a refrigerator (59.08) do not differ significantly from one another, whereas

the average probability of taking a second-hand T-shirt is statistically significantly lower (54.72).[6]

With respect to conditions and price, as expected, there is a sharp decrease in probability when the product shifts from free to available at a low price, regardless of conditions. Likewise, the stated probability decreases when the condition varies from excellent to acceptable, for both price levels. In particular, when the product is free, the decrease in the average stated probability is statistically significant as the condition changes from excellent (63.38) to acceptable (60.20), but not when it shifts from excellent to good (64.40); the difference in probability between excellent and acceptable is exacerbated when the product is no longer free (55.41 versus 50.42). These trends hold for all products considered (see Figure B.1 and Figure B.2 in Section B.1).

Figure 2.1: Probability to take the product by type

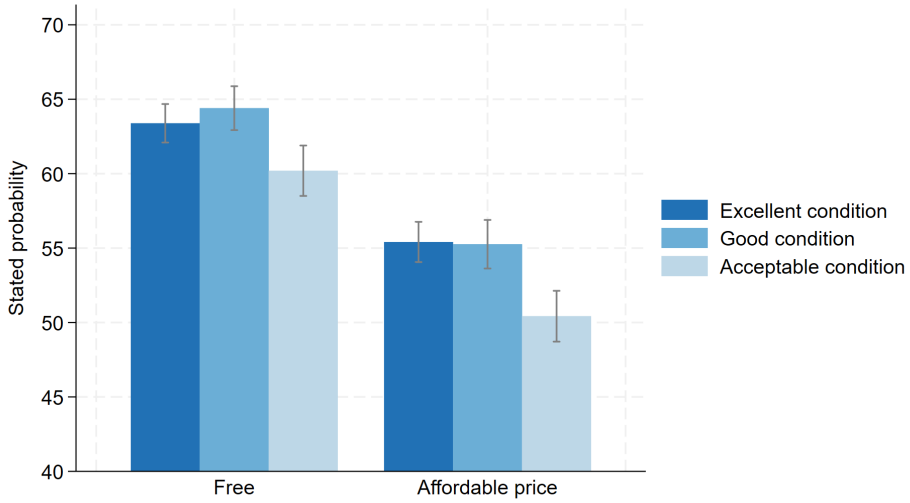


To test Hypothesis *H.1*, as for all other analyses involving the stated probability elicited in the vignette, we adopt fractional probit models after rescaling this variable to a unitary interval. Table 2.3 displays the estimation results for product condition and price, separately (Models 1 and 2) and jointly (Model 3), as well as their joint estimation controlled for product type (Model 4) and respondents' demographic traits (Model 5).

In line with Hypothesis *H.1.a*, the stated probability to take the vignette product

[6]This is confirmed by three pairwise Mann-Whitney rank-sum tests, for which the Šidák-Holm adjusted p-values fall below 0.001.

Figure 2.2: Probability to take the product by vignette attributes



is higher when shifting from acceptable to good and excellent conditions. Compared to the cheap-price condition, obtaining the product for free significantly increases the likelihood of choosing the second-hand good, supporting our hypothesis *H.1.b*. These effects are robust to the joint estimation of conditions and price and to several other controls, including the product type and respondents' demographic traits. As a further robustness check, we re-estimated the regression models by including interactions among the attributes (product, condition, and price level) to account for differences across the vignettes shown to respondents. The inclusion of these interaction terms does not alter the sign or statistical significance of the estimated effects on the probability of choosing the product described in the vignette (see the estimation results reported in Table B.11).

2.4.2 Product adoption across image dimensions and perspectives

We turn now to the investigation of how the probability to take the vignette product depends on the financial, social, environmental and hygiene dimensions of the three image perspectives. In keeping with other contributions eliciting social norms through matching coordination games (Barigozzi & Montinari, 2025; Fromell et al., 2021; Krupka & Weber, 2013), the values of the responses to the four-point items have been recoded as -1, -0.33, $+0.33$, and $+1$. Although this coding is standard in the literature, before adopting it we verified the implied ordering by estimating re-

Table 2.3: Vignette attributes and probability to take the product

	(1)	(2)	(3)	(4)	(5)
Excellent condition	0.102***		0.103***	0.107***	0.111***
	(0.020)		(0.020)	(0.020)	(0.019)
Good condition	0.120***		0.116***	0.113***	0.091***
	(0.022)		(0.021)	(0.022)	(0.021)
Free		0.227***	0.226***	0.227***	0.245***
		(0.016)	(0.016)	(0.016)	(0.015)
Smartphone				0.158***	0.122***
				(0.023)	(0.023)
Fridge				0.111***	0.192***
				(0.024)	(0.023)
Closet				0.166***	0.174***
				(0.023)	(0.022)
Constant	0.146***	0.101***	0.022	−0.088***	−0.791***
	(0.016)	(0.011)	(0.018)	(0.023)	(0.070)
Controls	No	No	No	No	Yes
Observations	10,496	10,496	10,496	10,496	10496

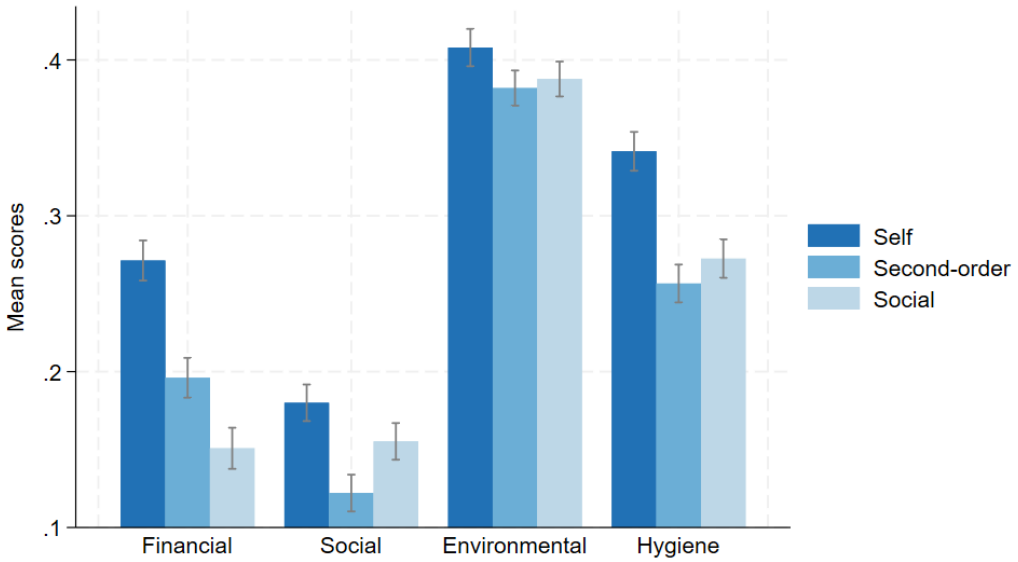
Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Excellent condition* and *Good condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. Controls in column (2). *Smartphone, Fridge, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification in Table B.4). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

gressions with category dummies; the coefficients displayed the expected monotonic pattern (see Table B.8 for Hypothesis 2, Table B.9 for Hypothesis 3, and Table B.10 for Hypothesis 4).

The distribution of the recoded scores elicited on the corresponding image items is graphed in Figure 2.3. The environmental dimension has the highest mean scores across all three image perspectives, and unlike the other dimensions, its scores remain relatively stable across perspectives. Within each image perspective, scores differ substantially across dimensions. Matched-pairs sign tests confirm that these within-perspective differences are highly statistically significant (Šidák-Holm adjusted p-values below 0.001), with one exception: the financial and social-approval dimensions of the social image yield identical mean scores (0.15).

We now proceed with the test of Hypotheses *H.2–H.4*, for which we conducted fractional probit model estimations on the stated probability of taking the vignette product using as regressors the scores on each determinant under self-image, social image and second-order self-image. Results of these estimations are reported,

Figure 2.3: Mean scores on image dimensions across perspectives



respectively, in Table 2.4, Table 2.5 and Table 2.6. In all these models, the dependent variable remains the standardized unitary stated probability to take the product described in the vignette. All specifications include vignette characteristics as regressors; as a robustness check, we also estimated models with vignette fixed effects (product × condition × price level) for the vignette seen by each respondent, and none of the results reported in this section changes in sign or significance (see Table B.15). Given that regressions estimate the impact on the dependent variable of a positive increase in their regressors, the image determinants are labeled as the positive extremes of the related scales, that is, as financial savvy, environmental consciousness, social approval and disgust insensitivity.

Table 2.4 shows that the four self-image dimensions are positively correlated with the probability of taking a second-hand product. Interestingly, environmental consciousness is the only self-image dimension that is not significantly associated with the probability of taking the product seen in the vignette, once the other dimensions of self-image are taken into account. This suggests that, conditional on financial, social, and hygiene considerations, the extent to which adopting a second-hand product enhances one's environmental self-image provides little additional explanatory power for adoption intentions. We return to this point when we compare the relative weight of image dimensions across perspectives below in Figure 2.4. Results do

Table 2.4: Self-image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.132***	0.127***	0.129***	0.125***
	(0.016)	(0.016)	(0.016)	(0.016)
Social approval	0.155***	0.156***	0.153***	0.111***
	(0.018)	(0.018)	(0.018)	(0.018)
Environmental consciousness	0.020	0.020	0.021	0.019
	(0.017)	(0.017)	(0.017)	(0.016)
Disgust insensitivity	0.406***	0.405***	0.403***	0.293***
	(0.017)	(0.017)	(0.017)	(0.017)
Excellent condition		0.079***	0.080***	0.091***
		(0.019)	(0.019)	(0.019)
Good condition		0.079***	0.075***	0.066***
		(0.021)	(0.021)	(0.020)
Free		0.219***	0.221***	0.235***
		(0.016)	(0.016)	(0.015)
Smartphone			0.093***	0.071***
			(0.023)	(0.022)
Fridge			0.103***	0.158***
			(0.022)	(0.022)
Closet			0.135***	0.148***
			(0.022)	(0.022)
Constant	0.029***	−0.147***	−0.230***	−0.491***
	(0.010)	(0.018)	(0.023)	(0.070)
Controls	No	No	No	Yes
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking a value in {−1, −0.33, +0.33, +1}. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.5). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

not change when controlling for product type, condition, and price, as well as for demographic characteristics: the sign and significance of coefficients are preserved. Overall, with the exception of *H.2.c*, which posited an effect of the environmental dimension of self-image on second-hand adoption, these results broadly support Hypothesis *H.2*.

These analyses are replicated, with comparable findings, for the image perspectives of social image (Table 2.5) and second-order self-image (Table 2.6), lending support to our Hypotheses *H.3* and *H.4*. However, unlike in the specification for self-image, the environmental dimension is positively and significantly associated with adoption intentions under both the social-image and second-order self-image

perspectives. In these two perspectives, the environmental dimension captures the belief that adopting a second-hand product would be regarded as a sustainable choice, respectively, by others evaluating one's behavior and by others evaluating their own. This finding suggests that environmental consciousness contributes additional explanatory power when adoption is evaluated through others' perspectives, but not once the other self-image dimensions are jointly considered. The fact that environmental consciousness behaves differently across image perspectives motivates us to explore this dimension more closely in the remainder of the paper.

Again as a robustness check, we re-estimated the regression models Table 2.4, Table 2.5, and Table 2.6 by including interactions among the image dimensions and the vignette attributes. The inclusion of these interaction terms does not alter the sign or statistical significance of the estimated effects on the probability of choosing the product described in the vignette (see the estimation results reported respectively in Table B.12, Table B.13, and Table B.14).

2.4.3 Relationships between social image and self-image

In previous sections (e.g., Section 2.2.3 and Section 2.3.4), we hinted at potential differences across perspectives in the scores reported on the image dimensions. In particular, we conjectured that social image scores were systematically lower than self-image ones (Hypothesis *H.5.a*) and that social image would be more predictive of second-hand adoption than self-image (Hypothesis *H.5.b*). We now explore both these questions.

2.4.3.1 Self-image versus social image

The distribution of the scores elicited on image items, under the perspectives of self-image and social image, are reported in Table 2.7. For each perspective, four columns report the proportion of respondents agreeing completely with A (Column "−−"), more with A than with B (Column "−"), more with B than A (Column "+") and completely with B (Column "++"). Test statistics (adjusted p-values) are reported in the last column of the table.

In line with our expectation, self-image is generally more positive than social image; the only exception occurs for the social-approval dimension, for which the

Table 2.5: Social image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.062***	0.060***	0.060***	0.045***
	(0.017)	(0.017)	(0.017)	(0.017)
Social approval	0.126***	0.131***	0.128***	0.082***
	(0.021)	(0.021)	(0.021)	(0.020)
Environmental consciousness	0.108***	0.103***	0.105***	0.080***
	(0.019)	(0.019)	(0.019)	(0.018)
Disgust insensitivity	0.250***	0.252***	0.248***	0.154***
	(0.018)	(0.018)	(0.018)	(0.017)
Excellent		0.083***	0.085***	0.097***
		(0.020)	(0.020)	(0.019)
Good		0.094***	0.091***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.233***	0.234***	0.247***
		(0.016)	(0.016)	(0.015)
Smartphone			0.099***	0.082***
			(0.023)	(0.022)
Fridge			0.094***	0.165***
			(0.023)	(0.023)
Closet			0.124***	0.144***
			(0.023)	(0.022)
Constant	0.096***	−0.096***	−0.175***	−0.615***
	(0.010)	(0.018)	(0.023)	(0.070)
Controls	No	No	No	Yes
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Social image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking values in {−1, −0.33, +0.33, +1}. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.6). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

two modal answers coincide at a partial agreement with statement B (Column "+"). Matched-pairs sign tests are strongly statistically significant for all dimensions (Šidák–Holm adjusted p-values ¡ 0.001). Ultimately, these results suggest that our respondents believe their self-image is more positively affected by second-hand adoption than their social image. Hypothesis *H.5.a* is thus confirmed.[7]

2.4.3.2 Relative weight of social image for adoption

A related question we investigated is whether the social-image perspective on a dimension is more predictive of second-hand adoption than self-image, in line with our

[7]We have limited the discussion to the total sample as these trends also hold across macro-regions. Scores and test statistics for regional trends are available in Table B.17.

Table 2.6: Second-order self-image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.110***	0.108***	0.109***	0.070***
	(0.017)	(0.017)	(0.017)	(0.017)
Social approval	0.076***	0.079***	0.075***	0.063***
	(0.019)	(0.019)	(0.019)	(0.018)
Environmental consciousness	0.128***	0.126***	0.128***	0.092***
	(0.018)	(0.018)	(0.018)	(0.017)
Disgust insensitivity	0.214***	0.213***	0.209***	0.129***
	(0.018)	(0.018)	(0.018)	(0.017)
Excellent		0.086***	0.088***	0.099***
		(0.020)	(0.020)	(0.019)
Good		0.095***	0.092***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.226***	0.227***	0.243***
		(0.016)	(0.016)	(0.015)
Smartphone			0.104***	0.085***
			(0.023)	(0.022)
Fridge			0.099***	0.170***
			(0.023)	(0.022)
Closet			0.130***	0.148***
			(0.023)	(0.022)
Constant	0.099***	−0.089***	−0.173***	−0.636***
	(0.010)	(0.018)	(0.023)	(0.070)
Controls	No	No	No	Yes
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Second-order self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking values in $\{-1, -0.33, +0.33, +1\}$. *Good condition* and *Excellent condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.7). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Distribution of scores under self-image versus social image

	Self-image				Social image				Sign test
Dimension	−−	−	+	++	−−	−	+	++	p-value
Financial	11.98	20.25	32.82	**34.96**	15.44	24.73	**31.55**	28.28	$< .001$
Social	10.61	24.73	**41.61**	23.05	10.84	26.97	**40.18**	22.01	$< .001$
Environmental	7.87	15.62	33.88	**42.64**	6.39	15.81	**40.92**	36.88	$< .001$
Hygiene	9.27	19.37	32.18	**39.18**	10.19	21.24	**35.99**	32.57	$< .001$

Note: Distribution of scores on the four image dimensions. Columns (−−, −, +, ++) indicate the proportion of respondents, respectively, completely agreeing with A, agreeing with A more than B, agreeing with B more than A, and completely agreeing with B. Modal scores are highlighted in bold. The last column reports Šidák–Holm adjusted p-values from sign tests.

Hypothesis *H.5.b*. To assess the relative importance of self-image and social image, we estimated four fractional probit models, one for each image dimension, including both the self-image and social-image scores for that dimension (specifications and

estimation results are available in Table B.16). Figure 2.4 displays the coefficients of these regressors.

Figure 2.4: The relative weight of image dimensions across perspectives

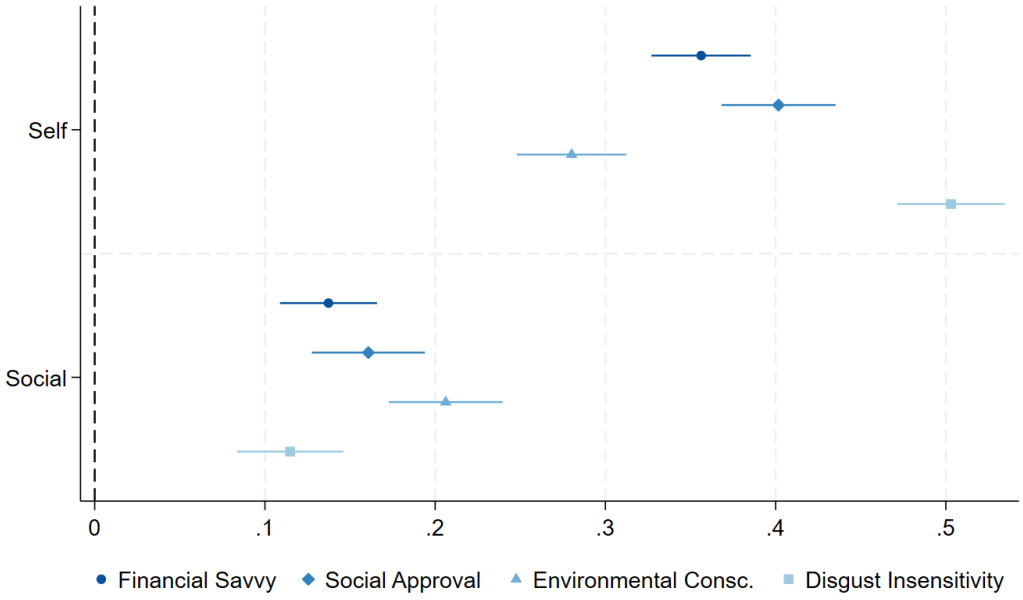


Figure 2.4 shows a remarkably consistent pattern in all four dimensions. The coefficient associated with self-image is always larger than the corresponding social-image coefficient. Wald tests reject the equality of the two coefficients for every dimension. The largest differences emerge for disgust insensitivity, followed by social approval and financial savvy, whereas the gap is smaller, although still statistically significant, for environmental consciousness.[8]

These findings, which contradict Hypothesis *H.5.b*, suggest that self-image is consistently more strongly associated with second-hand adoption intentions than social-image. In other words, respondents appear to care more about how adopting second-hand makes them feel about themselves than about how it makes them appear to others. This result is in stark contrast to the pattern typically observed in other behavioral domains, where social image is generally found to exert a stronger influence than self-image. (see, e.g., Bašić & Quercia, 2022; Ewers & Zimmermann, 2015; Rind & Benjamin, 1994).

The result for environmental consciousness represents a partial exception to the

[8]Wald tests show relevant differences for Financial Savvy (difference = 0.219; Šidák–Holm p ¡ 0.001), Social Approval (0.241; p ¡ 0.001), and Disgust Insensitivity (0.388; p ¡ 0.001), and a smaller, but still significant gap for Environmental Consciousness (0.074; p = 0.012).

general pattern. Although self-image remains more strongly associated with adoption intentions than social image, the difference between the two perspectives is considerably smaller than for the other image dimensions. This finding mirrors the descriptive evidence in Figure 2.3, where the environmental dimension shows both the highest mean scores and the smallest differences across the three image perspectives. Taken together, these results suggest that adopting a second-hand product is widely perceived as a sustainable choice, whose effects on respondents' self-evaluations and on how they expect to be perceived by others appear closely aligned. This may explain why the distinction between self-image and social image is considerably less pronounced for environmental consciousness than for the other image dimensions.

2.4.4 Pluralistic ignorance in second-hand adoption

The elicitation of second-order self-image, i.e., the respondents' beliefs about the self-image that prevails among others in their reference group, allows us to explore the possibility of pluralistic ignorance, that is, a systematic misperception of such prevailing self-image. As anticipated in Section 2.3 and Section 3.4, we conjectured that second-order self-image would be systematically lower than respondents' own self-image (*Hypothesis H.6.a*) and than the modal self-image score observed within their reference group (*Hypothesis H.6.b*). We explore both of these questions in the following.

2.4.4.1 Second-order versus first-order self-image

Table 2.8 reports the distribution of the scores on the image dimensions across self-image and second-order self-image. The table is structured in the same fashion as Table 2.7, with four columns for each perspective reporting proportions of respondents agreeing completely with A, more with A than with B, more with B than A, completely with B (respectively, Columns "−−", "−", "+", "++"), and with the last column reporting the adjusted p-values of the hypothesis tests.

The proportion of respondents agreeing more with B (either predominantly or totally) is always higher under self-image than second-order self-image (except for social approval, for which the modal answers match) and all pairwise differences

Table 2.8: Distribution of scores under self-image versus second-order self-image

	Self-image				Second-order self-image				Sign test
Dimension	−−	−	+	++	−−	−	+	++	p-value
Financial	11.98	20.25	32.82	**34.96**	12.65	24.70	**33.20**	29.45	$< .001$
Social	10.61	24.73	**41.61**	23.05	11.47	29.40	**38.42**	20.70	$< .001$
Environmental	7.87	15.62	33.88	**42.64**	6.50	16.33	**40.43**	36.74	$< .001$
Hygiene	9.27	19.37	32.18	**39.18**	9.57	23.09	**36.57**	30.77	$< .001$

Note: Distribution of scores on the four image dimensions across first-order and second-order self-image. Columns (−−, −, +, ++) indicate the (rounded) proportion of respondents, respectively, completely agreeing with A, agreeing with A more than B, agreeing with B more than A, and completely agreeing with B. Modal scores are highlighted in bold. The last column reports Šidák–Holm adjusted p-values from sign tests.

turn out to be statistically strongly significant, as evidenced by a series of proportion tests (p-values ¡ 0.001; Šidák-Holm adjusted; see Table B.18 for tests on the scores disaggregated per macro-region). Overall, these findings support the conjecture that people generally tend to attribute more positive values to themselves than to others, that is, that their self-image is more positively affected by adopting second-hand than how much it affects that of others (Hypothesis *H.6.a*).[9]

2.4.4.2 Beliefs about self-image modes

In this section, we further investigate the extent of the mismatch between respondents' beliefs about others' self-image and the actual modal self-image observed within their reference group. In Table 2.9, "Mismatch" represents the proportion of participants whose responses on the second-order self-image elicitation task did not match the modal responses on the self-image items observed in their macro-regional reference group for the same vignette. Overall, the mismatch is substantial across all four image dimensions: between 61% and 67% of respondents fail to correctly predict the modal self-image evaluation within their reference group (for disaggregation in macro-regions, see Table B.19). All scores are statistically significantly different from 50%, as evidenced by a series of one-sample binomial tests (all p-values ¡ 0.001).

Table 2.9 also reports the proportion of respondents who underestimated ("Below") or overestimated ("Above") the modal self-image response observed within their reference group for the corresponding vignette. Interestingly, the mismatch is

[9]Consistent with our pre-registration, we conducted matched-pairs chi-squared tests of marginal homogeneity (Stuart–Maxwell, the k×k generalization of McNemar) comparing the distributions of the four dimension scores between self- and social-image perspectives within each product. These tests assess whether the marginal distributions differ—i.e., whether responses under self-image are systematically shifted toward higher categories relative to social-image. Since these results are consistent with those already discussed in the text, we do not report them and instead focus on matched-pairs sign tests, whose interpretation is more intuitive and aligns with common practice in the literature (see, e.g., Barigozzi & Montinari, 2025).

mostly pessimistic, as respondents who made an incorrect prediction tend to underestimate more frequently than to overestimate how positively others privately evaluate the effect of second-hand adoption on their self-image. The proportion is extremely high for the environmental and hygiene dimensions (around 90% of mismatches), followed by the financial (84%) and the social dimensions (65%). Ultimately, these findings confirm the presence of pluralistic ignorance regarding others' beliefs about second-hand consumption and lend support to our Hypothesis *H.6.b.*

Table 2.9: Mismatch between second-order self-image and self-image mode

Dimension	Mismatch	Below	Above
Financial	66.96	84.00	16.00
Social	61.49	65.38	34.62
Environmental	61.10	90.29	9.71
Hygiene	65.04	89.40	10.60

Note: "Mismatch" reports the proportion of respondents whose second-order self-image response does not coincide with the modal self-image response observed within their reference group (macro-area) for the corresponding vignette. "Below" and "Above" identify respondents for whom their second-order response falls respectively below and above the modal self-image. All proportions are statistically significantly different from 0.50 (two-sided binomial tests; p ¡ 0.001 for all).

The evidence of pluralistic ignorance raises the question of whether the direction of the mismatch is correlated with the probability of taking a product. Table 2.10 explores this question. For all dimensions, underestimating the modal value is associated with lower adoption intentions than among respondents who hold accurate beliefs, suggesting that pessimistic misperceptions about others' self-image discourage second-hand adoption. In contrast, overestimating others' evaluations has a weaker and, in most cases, not statistically significant positive effect. An exception is the environmental dimension, where "above" misalignment is significantly negatively associated with adoption intentions. The social dimension, instead, is always significantly positively associated with the intention to adopt. One possible, suggestive interpretation of the former effect is that respondents who overestimate the modal value for the environmental consciousness dimension may perceive second-hand adoption as a less distinctive action. If adopting second-hand is considered an environmentally responsible choice by the majority of people in one's reference group, this behavior contributes less to one's self-image and therefore reduces the intention to adopt. In contrast, perceiving greater social approval tends to operate as a positive social cue, increasing stated adoption intentions.

Table 2.10: Second-order self-image and self-image mode mismatch on adoption

	(1)	(2)	(3)
Financial below	−0.057**	−0.085***	−0.055**
	(0.023)	(0.022)	(0.022)
Financial above	−0.008	0.063*	0.038
	(0.032)	(0.032)	(0.032)
Social below	−0.168***	−0.165***	−0.092***
	(0.020)	(0.020)	(0.019)
Social above	0.136***	0.095***	0.115***
	(0.026)	(0.026)	(0.025)
Environmental below	−0.133***	−0.139***	−0.089***
	(0.022)	(0.021)	(0.021)
Environmental above	−0.116***	−0.035	−0.030
	(0.039)	(0.040)	(0.038)
Disgust below	−0.143***	−0.137***	−0.078***
	(0.021)	(0.021)	(0.021)
Disgust above	−0.046	−0.002	−0.030
	(0.037)	(0.037)	(0.037)
Excellent condition		0.133***	0.131***
		(0.021)	(0.020)
Good condition		0.148***	0.117***
		(0.023)	(0.022)
Free		0.272***	0.267***
		(0.018)	(0.017)
Smartphone		0.158***	0.117***
		(0.025)	(0.024)
Fridge		0.103***	0.175***
		(0.024)	(0.023)
Closet		0.170***	0.175***
		(0.024)	(0.024)
Constant	0.473***	0.124***	−0.531***
	(0.022)	(0.031)	(0.078)
Controls	No	No	Yes
Observations	9,397	9,397	9,397

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below*; *Environmental above/below*; *Social above/below*; *Disgust above/below*: dummy variables equal 1 when the second-order self-image score on the dimension is, respectively, above/below the modal self-image score; the reference category is no mismatch between second-order self-image and the modal self-image score. *Excellent condition* and *Good condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.20). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.5 Exploratory analyses

2.5.1 Self- and social-image misalignment

Considering the greater predictive role of self-image relative to social image documented in Section 2.4.3.2, we investigated whether the probability to adopt second-

hand is affected by the direction of the "misalignment" between these two perspectives within the individual, and specifically whether it is higher when self-image scores are above those of social image and lower otherwise.

Table 2.11 reports the share of respondents for whom the scores on self and social image differed ("Misaligned"), and the proportion, among such respondents, of those for whom the self-image scores are below versus above the social-image ones. The table clearly shows that misalignment between self-image and social image is far from negligible: depending on the dimension, between 36% and 42% of respondents report different scores in the two perspectives. Interestingly, when misalignment occurs, it is more often in the direction of self-image exceeding social image (this trend holds across macro-regions, as documented by Table B.21). This pattern is strongest for financial considerations, where 64% of misaligned respondents report higher self-image than social-image scores, followed by hygiene, environmental, and social-approval considerations. One-population proportion tests confirm that these values differ significantly from 50% (p-values ¡ 0.001). Subjects hence tend to evaluate the impact of second-hand adoption on their own image more positively than they expect others to do.

Figure 2.5 plots the mean scores of social image for each score of self-image, and shows a more nuanced but consistent trend: social image tends to be lower than self-image for respondents reporting a positive self-image and higher otherwise, regardless of image dimension. Across all four dimensions, respondents seem to attribute less extreme evaluations to others than to themselves. When self-image is positive, social-image scores are generally lower, whereas when self-image is negative, social-image scores are generally higher.

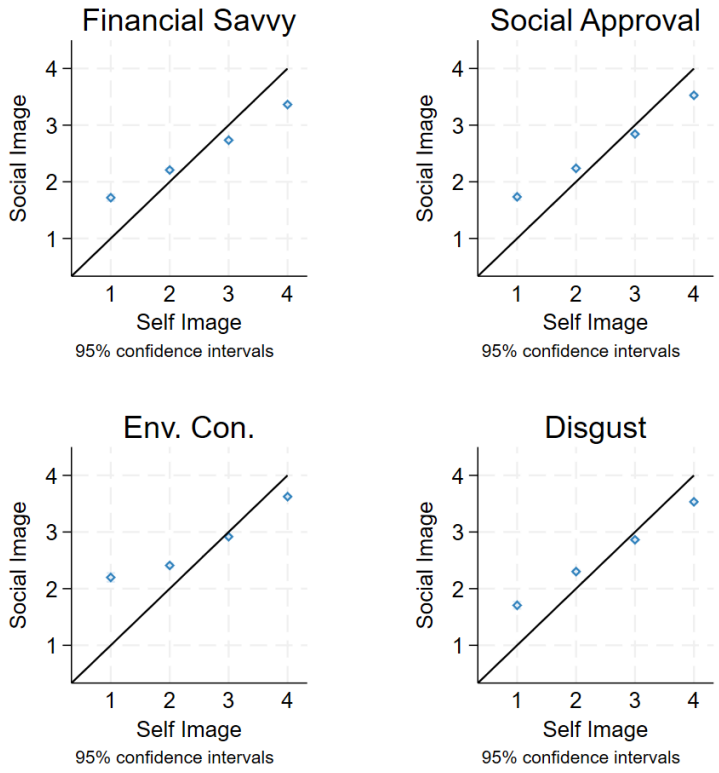
Table 2.11: Misalignment between self-image and social image

Dimension	Misaligned	Below	Above
Financial	42.37	35.96	64.04
Social	37.98	45.56	54.44
Environmental	36.08	43.36	56.64
Hygiene	36.12	39.54	60.46

Note: Proportion of respondents whose answers on the self-image items did not match the social-image score for the dimension ("Misaligned"). Columns "Below" and "Above" indicate the proportion of misaligned respondents who gave an answer for the self-image dimension than is lower or higher than the social-image one. All proportions are statistically significantly different from 0.50 (two-sided binomial tests; p ¡ 0.001 for all).

To test whether second-hand adoption intention is affected by the direction of

Figure 2.5: Misalignment between self-image and social image



misalignment, we constructed for each image dimension a dummy variable denoting whether self-image score is above or below the social image one, and used these dummies as regressors of fractional probits (Table 2.12). Partly in line with expectations, a self-image higher than a social image is associated with a higher probability of second-hand adoption, although the effect is statistically significant only for financial and hygiene considerations. For social-approval considerations, the effect is not robust to demographic controls. A self-image lower than the social image, instead, is associated with a lower probability of adoption, but this effect is statistically significant only for the environmental and hygiene dimensions.

Quite interestingly, also in this case, the environmental dimension follows a different pattern from the other image dimensions. While a strong negative association emerges when social-image scores exceed self-image scores, a weaker negative association is also observed when self-image scores exceed social-image scores, although the coefficient is no longer statistically significant when we control for demographic characteristics. This result is in line with our previous finding that environmental

consciousness is the image dimension with the smallest distinction between self-image and social image, suggesting that alignment between the two may matter more than the direction of the discrepancy.

With the exception of the environmental dimension, taken together, previous findings indicate that intention to adopt is not driven solely by the absolute level of self-image, but depends on the direction of the misalignment with social image. It may be worth noting that adoption intentions decline more strongly when the score of self-image is lower than the score for social image than how much they rise when the opposite occurs.

2.5.2 Self- and second-order self-image misalignment

While Section 2.4.4.2 focused on the accuracy of respondents' beliefs about others' private self-image, in this section we turn to the alignment between individual responses on own self-image and the self-image attributed to others, with the aim of investigating whether discrepancies between the two are associated with second-hand adoption intentions. Also in this case, we conjectured that the direction of this misalignment would be differentially associated with the probability of taking the vignette product.

Descriptive statistics in Table 2.13 show that self-image and second-order self-image are misaligned for a substantial share of respondents, ranging from 36% for the environmental dimension to about 40% for the social dimension. Among such misaligned respondents, most state higher self-image scores than those attributed to others (57-62%). Thus, respondents who perceive a difference between themselves and others tend to evaluate the effect of second-hand adoption on their own self-image more positively than they believe others would evaluate it for themselves. Descriptive statistics at the macro-region level are presented in Table B.23.

Figure 2.6 plots the average second-order self-image score for each level of self-image score, across dimensions. Similarly to the patterns displayed in Figure 2.5, across all dimensions, when respondents report a positive self-image, they tend to attribute a less positive self-image to others, whereas when they report a negative self-image, they tend to attribute a less negative self-image to others. In other terms, also in the comparison between self-image and second-order self-image, it seems that

Table 2.12: Self-image and social image misalignment on adoption

	(1)	(2)	(3)
Financial below	−0.025	−0.023	−0.024
	(0.024)	(0.025)	(0.024)
Financial above	0.053**	0.057***	0.060***
	(0.022)	(0.022)	(0.021)
Social below	−0.032	−0.036	−0.048**
	(0.024)	(0.023)	(0.023)
Social above	0.052**	0.043*	0.030
	(0.023)	(0.023)	(0.022)
Environmental below	−0.111***	−0.114***	−0.088***
	(0.025)	(0.024)	(0.024)
Environmental above	−0.054**	−0.050**	−0.028
	(0.023)	(0.022)	(0.021)
Disgust below	−0.183***	−0.171***	−0.131***
	(0.025)	(0.025)	(0.024)
Disgust above	0.124***	0.124***	0.084***
	(0.023)	(0.022)	(0.022)
Excellent condition		0.109***	0.113***
		(0.020)	(0.019)
Good condition		0.110***	0.089***
		(0.021)	(0.021)
Free		0.220***	0.240***
		(0.016)	(0.015)
Smartphone		0.160***	0.124***
		(0.023)	(0.022)
Fridge		0.114***	0.192***
		(0.023)	(0.023)
Closet		0.171***	0.179***
		(0.023)	(0.022)
Constant	0.239***	−0.075***	−0.750***
	(0.012)	(0.025)	(0.071)
Controls	No	No	Yes
Observations	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below; Environmental above/below; Social above/below; Disgust above/below*: dummy variables equal 1 when the self-image score on the dimension is, respectively, above/below the social-image score; the reference category is self-image equal to social image (alignment). *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Cheap Price* dummy variable equals 1 if the product can be taken at a convenient price, 0 otherwise. Baseline: *Free*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.22). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

respondents tend to attribute less extreme evaluations to others than to themselves.

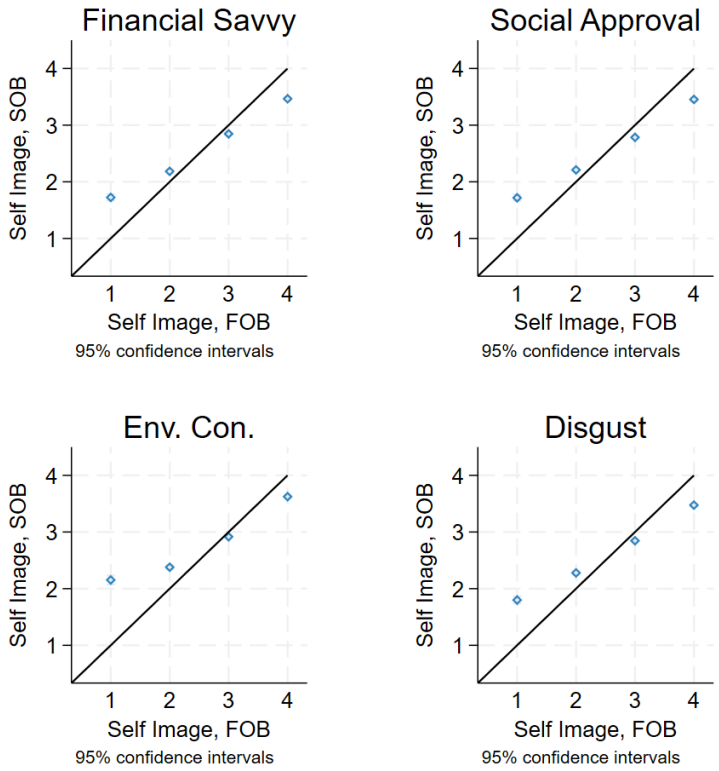
In keeping with what we did in the previous section, we estimated a series of fractional probit models using as regressors four binary variables, one for each image dimension, denoting whether the self-image score is above or below the second-order one (Table 2.14).

Table 2.13: Misalignment between self-image and second-order self-image

Dimension	Misaligned	Below	Above
Financial	39.33	39.20	60.80
Social	39.97	41.72	58.28
Environmental	36.15	42.51	57.49
Hygiene	37.70	37.58	62.42

Note: Proportion of respondents whose answers on the second-order self-image items did not match the self-image scores for the dimension ("Misaligned"). Columns "Below" and "Above" indicate the proportion of misaligned respondents whose self-image is, respectively, lower or higher than the second-order self-image. All proportions are statistically significantly different from 0.50 (two-sided binomial tests; p ¡ 0.001 for all).

Figure 2.6: Misalignment between second-order and first-order self-image



For all image dimensions, the same consistent pattern can be identified: respondents whose self-image scores are below their second-order self-image scores are significantly less likely to adopt the second-hand product than respondents for whom the two perspectives are aligned. This result is robust when controlling for both product characteristics and demographic controls. By contrast, reporting a self-image score above the second-order self-image one is associated with higher adoption intentions only for the hygiene dimension once demographic controls are included. The corresponding effects for the remaining dimensions either disappear or are not

Table 2.14: Self-image and second-order self-image misalignment on adoption

	(1)	(2)	(3)
Financial below	−0.063***	−0.058**	−0.063***
	(0.024)	(0.024)	(0.024)
Financial above	0.018	0.023	0.038*
	(0.023)	(0.023)	(0.022)
Social below	−0.061**	−0.069***	−0.066***
	(0.024)	(0.024)	(0.023)
Social above	0.067***	0.060***	0.018
	(0.023)	(0.023)	(0.022)
Environmental below	−0.122***	−0.117***	−0.087***
	(0.025)	(0.024)	(0.024)
Environmental above	−0.046**	−0.041*	−0.030
	(0.022)	(0.022)	(0.021)
Disgust below	−0.204***	−0.201***	−0.134***
	(0.026)	(0.026)	(0.025)
Disgust above	0.172***	0.174***	0.113***
	(0.023)	(0.023)	(0.022)
Excellent condition		0.109***	0.113***
		(0.020)	(0.019)
Good condition		0.111***	0.090***
		(0.021)	(0.021)
Free		0.222***	0.241***
		(0.016)	(0.015)
Smartphone		0.165***	0.127***
		(0.023)	(0.022)
Fridge		0.114***	0.191***
		(0.023)	(0.023)
Closet		0.175***	0.181***
		(0.023)	(0.022)
Constant	0.245***	−0.073***	−0.723***
	(0.012)	(0.025)	(0.070)
Controls	No	No	Yes
Observations	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below; Environmental above/below; Social above/below; Disgust above/below*: dummy variables equal 1 when the self-image score on the dimension is, respectively, above/below the second-order self-image score; the reference category is self-image equal to second-order self-image (alignment). *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Cheap Price* dummy variable equals 1 if the product can be taken at a convenient price, 0 otherwise. Baseline: *Free*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence (full specification available in Table B.24). Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

statistically significant. It is worth noting that the environmental-consciousness dimension again displays a distinct pattern. Differently from other dimensions, when self-image scores are higher than the second-order self-image ones, the probability of adoption is lower, even though the coefficient loses statistical significance once demographic characteristics are controlled for.

The results of Table 2.14 then reveal the same asymmetric pattern observed for

self- versus social-image misalignment (Table 2.12): relative to the case in which they are aligned, a self-image that falls below second-order self-image is associated with a larger and more robust reduction in adoption intentions than the corresponding increase observed when self-image exceeds the latter.

2.6 Discussion and conclusion

This paper set out to investigate how image considerations influence second-hand adoption by disentangling three distinct perspectives – self-image, social image, and second-order self-image – across four evaluative dimensions (financial, social, environmental, and hygiene), using a large-scale vignette experiment on a nationally representative Italian sample. While substantial evidence documents that financial, social, environmental, and hygiene considerations influence second-hand adoption, existing research has not systematically investigated how these determinants operate differently across image perspectives, nor how misalignments between perspectives influence adoption. Several findings emerge. While all image perspectives predict second-hand adoption, self-image is a consistently stronger predictor than both social image and second-order self-image across all four dimensions. The environmental dimension is the sole exception: it does not significantly predict adoption under self-image when the other dimensions are controlled for, yet becomes significant under social and second-order perspectives. Finally, participants systematically underestimate how positively others privately evaluate second-hand adoption – a pattern consistent with pluralistic ignorance – and the direction of the mismatch matters asymmetrically: underestimation is associated with reduced adoption intentions, more strongly than overestimation boosts them.

The behavioral patterns that emerged from the analysis validate our decision to disaggregate image into different dimensions and analyze their impact across diverse social perspectives. Eliciting a general measure of image would have obscured, on the one hand, the differential effect of the same image dimension across different perspectives and, on the other, the differential effect of the four dimensions within the same image perspective. In support of the former case, when self- and social-image measures for the same dimension are jointly included to predict second-hand

adoption, the self-image coefficient is always larger and Wald tests reject equality across all four dimensions – indicating that, for instance, feeling financially savvy is more important than appearing financially savvy or believing others feel financially savvy. In support of the latter case, image dimensions within the same perspective showed statistically significant differences in mean scores, and regression analyses indicate that they vary in their relevance for second-hand adoption. For example, under self-image, the four dimensions differ markedly in their predictive strength, with disgust insensitivity exerting the largest effect on adoption and environmental consciousness the smallest.

Perhaps the most theoretically striking result is that self-image consistently outweighs social image in predicting second-hand adoption, directly contradicting the broader literature documenting stronger behavioral effects of social image in domains such as honesty (Bašić & Quercia, 2022), compliance (Ewers & Zimmermann, 2015; Rind & Benjamin, 1994), and prosocial behavior (Bénabou & Tirole, 2006). The gap between the two perspectives is largest for hygiene and social-approval considerations, and smallest – although still significant – for environmental consciousness. This heterogeneity is itself informative: the near-parity of self- and social-image for the environmental dimension suggests that buying second-hand is broadly perceived as a sustainable choice whose reputational and private value are similarly appraised, while for hygiene and social approval the internal and external evaluations diverge more sharply.

A natural explanation for the overall dominance of self-image is that second-hand adoption typically unfolds in private or low-visibility contexts, which in turn decreases the relative weight of social image concerns (Falk, 2021). But a deeper shift may also be at play: as pre-owned goods are progressively reframed from markers of financial hardship to markers of environmental awareness and savviness (Gilal et al., 2024), buying second-hand may increasingly function as a signal to oneself of who one is rather than primarily a signal to others. Relatedly, the environmental dimension of self-image is the only one that does not independently predict adoption once the other self-image dimensions are controlled for, while remaining significant under social and second-order perspectives. This suggests that sustainability motivations in this domain may operate more as a social signal than as a privately

internalized value, consistent with evidence that green consumption is often driven by reputational rather than intrinsic concerns (Parguel et al., 2017).

Our findings point toward differentiated policy interventions tailored to the specific dimensions and perspectives that matter most for adoption. The dominance of self-image suggests that interventions should primarily boost private evaluations rather than leverage gains in reputation or status. Educational campaigns should focus on the two self-image dimensions with the strongest effects – hygiene and financial savvy – while deprioritizing environmental appeals. Environmental consciousness operates primarily as a social signal rather than a private value, so sustainability messaging is better targeted at perceived social image: communicating, for instance, that second-hand adoption allows one to broadcast the image of an environmentally conscious consumer. For hygiene, policymakers should make sanitation verifiable and salient through standardized cleaning protocols and third-party certificates. To leverage the financial dimension, advertisers could reframe second-hand adoption as a financially savvy choice by emphasizing its convenience over first-hand alternatives.

Finally, the evidence of pluralistic ignorance points to an additional lever: norm-correction campaigns that make prevailing private attitudes visible – for instance, by communicating that most consumers privately feel good about buying second-hand – could help unlock latent demand. Given that underestimating others' positive self-image significantly depresses adoption intentions, correcting this misperception may be among the most cost-effective tools available to policymakers seeking to scale second-hand markets.

As with any survey-based study, our findings may be subject to response biases, such as socially desirable responding and experimenter demand effects. The former bias occurs when respondents provide socially acceptable rather than truthful answers, potentially leading individuals to overstate sustainability attitudes or underreport disgust toward used goods. Experimenter demand effects arise when respondents infer the study's purpose and adjust their responses accordingly, potentially inflating reported willingness to adopt second-hand products. However, several features of our design mitigate these concerns. First, the survey was administered anonymously online via computer-assisted web interviewing (CAWI), a

setting that has been shown to reduce demand effects and social desirability bias (De Quidt et al., 2018; Kreuter et al., 2008). Second, respondents were unaware of subsequent questions and could not revise prior answers, which prevented retrospective adjustments to appear consistent. Third, and most importantly, our elicitation of second-order self-image employed an incentivized coordination game (Krupka & Weber, 2013), in which respondents earned monetary rewards for accurately predicting modal responses in their macro-region. This incentive mechanism has been specifically designed to elicit honest beliefs free from social desirability and demand effects, and recent methodological work confirms that the presence of monetary incentives in this task attenuates response biases relative to unincentivized measures (Fallucchi & Nosenzo, 2022; Fallucchi et al., 2026). We note that only second-order beliefs were incentivized, so residual bias cannot be ruled out for self- and social-image measures; nonetheless, our main conclusions on misalignments leverage the incentivized belief elicitation.

Several limitations of this study suggest directions for future research. First, our sample consists exclusively of Italian respondents. Thus, our findings might not generalize to contexts with different second-hand market maturity, regulatory environments, or social structures. For instance, second-hand clothing is particularly widespread in the United States but dramatically low in China (Xu et al., 2014), suggesting markedly different, country-specific mechanisms (Gilal et al., 2024). Cross-cultural replications of this study would help establish the external validity of our results and assess whether the relative weight of image perspectives and the observed misalignment asymmetries replicate across settings.

Second, while we find evidence of misalignments between image perspectives, our cross-sectional design cannot establish causal relationships. Field and lab experiments that exogenously manipulate a single image dimension or perspective while holding others constant – for example, treatments emphasizing private financial gains (self-image), verifiable hygiene upgrades (self-image), or the perceived social meaning of sustainability (social/second-order image) – would provide stronger causal evidence on how each lever affects adoption. Pre-registered interventions that separately manipulate these components and measure their effect on stated and revealed choices would be particularly informative.

Third, our vignette-based measure of adoption intentions may not perfectly predict actual behavior. Although stated intentions are strong predictors of behavior in many domains (for some examples, see Fishbein and Ajzen, 2011), studies measuring actual second-hand purchases would complement our findings and shed light on whether the image dynamics we identify translate into real-world consumption patterns. Embedding real-world behavioral outcomes – such as real purchase opportunities, platform sign-ups, or redemption of vouchers and discounts – within the survey or in follow-up field settings would strengthen external validity (see, e.g., the voucher choice experiment by Lorè et al., 2025). Linking survey responses to administrative or platform data (with consent) would offer further validation of revealed behavior.

Finally, our focus on the four determinants of second-hand consumption necessarily omits other potentially relevant image dimensions, such as nostalgia or materialism. Future research could expand the dimensional structure of image to examine whether the trends documented in this study persist in these domains and to incorporate additional motives (e.g., uniqueness-seeking or status signaling). Documenting whether self-image dominance, the relevance of environmental considerations in other-oriented perspectives, and asymmetric misalignment effects generalize to these added dimensions would help assess the breadth of our findings.

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Chapter 3

Environmental costs in second-hand clothing purchase: A voucher choice experiment

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Abstract

Although consumers may wish to act in environmentally responsible ways, informational barriers can prevent them from accurately understanding the true environmental costs of their consumption choices. We investigate how presenting different types of environmental costs (CO2 emissions, energy consumption, or water usage) affects willingness to purchase second-hand clothing. We measure changes in stated purchasing intentions and, using multiple price list, incentivized choices in a large, nationally representative sample of Italians ($N = 10,496$). We report three main findings. First, all information treatments promote second-hand adoption, but the choice of metric matters: water-based information generates the strongest effects than CO2—the dominant metric in sustainability communication. Second, pro-environmental behavior amplifies responsiveness, particularly for water-based information, while need for uniqueness acts as a barrier, with high-NFU individuals requiring more compensation to switch to second-hand. Third, local environmental conditions do not systematically moderate treatment effects. Our findings indicate that reframing environmental information from CO2 to more tangible metrics like water can meaningfully enhance sustainability campaigns, though reaching beyond already environmentally engaged consumers remains an open challenge.

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

JEL codes: D12 · D83 · D91 · Q50

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3.1 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 costs for the environment. Niinimäki et al., 2020 estimate that the fashion industry is responsible for 8–10% of global CO2 emissions (4–5 billion tonnes annually), surpassing the combined emissions of international aviation and maritime shipping (Leal Filho et al., 2022), and for approximately 20% of global industrial water pollution. Water consumption is equally striking: the fashion industry uses around 93 billion cubic meters of water per year, with a single cotton t-shirt requiring up to 2,700 litres to produce (European Parliament, 2023). Beyond emissions and water use, overproduction generates roughly 92 million tonnes of textile waste annually, a figure projected to reach 134 million tonnes by 2030 (Niinimäki et al., 2020).

This environmental burden is exacerbated by the proliferation of fast fashion (Bianchi, 2018). Since 2000, clothing production has roughly doubled while the average number of times a piece of clothing is worn has declined by around 36% (Leal Filho et al., 2022). More clothes are produced, used briefly, and discarded – a dynamic that continuously generates new environmental costs. Extending the active life of clothes already in circulation, therefore, can directly reduce these costs by deferring or displacing the production of new items. The acquisition of second-hand clothing is the most direct mechanism to achieve this (Guiot & Roux, 2010). Evidence suggests that purchasing 100 second-hand clothes is estimated to displace between 60 and 85 new clothing purchases, and that such displacement reduces toxic emissions caused by polyester cotton clothes by 45% and global warming impact of producing cotton t-shirts by 14% (Farrant et al., 2010). Recently, Klooster et al. (2024) estimated that choosing second-hand over new clothing reduces the impact of fashion on climate change by up to 42% and water scarcity by 35–53%.

Despite the beneficial environmental impact of second-hand clothing, adoption rates worldwide remain generally low. In the European Union, only 22% of post-consumer textile waste is collected separately for recycling or reuse, while the rest

[1] https://fashionunited.com/statistics/global-fashion-industry-statistics

is often burned or deposited in landfills[2]. Understanding what drives demand for second-hand clothing, and what inhibits it, is therefore a precondition for designing policies capable of accelerating the transition away from new clothing consumption. In this respect, a determinant that is receiving increasing attention is consumers' awareness of the environmental harms caused by the fashion industry. Empirical evidence suggests that consumers that are aware of the environmental costs associated with the production of new clothes show higher intention to purchase them second-hand (Borusiak et al., 2020; Negash & Akhbar, 2024; Pretner et al., 2021). However, research also shows that such awareness among consumers is currently low (Goworek et al., 2012; Harris et al., 2016) and that it may be difficult to increase it. For example, several contributions have tested the effect of providing information about carbon footprint, and they have found that consumers report difficulties in understanding CO2-based information (Dreist et al., 2024; Guenther et al., 2012; Hartikainen et al., 2014). Consequently, a stream of the literature has started investigating how such information can be expressed in metrics that can be more easily digested by consumers, for instance equating it with miles driven per gallon (Larrick et al., 2015) or minutes of light-bulb usage (Camilleri et al., 2019).

This paper investigates how the willingness to purchase second-hand clothing is affected by the expression of environmental costs through a framed field experiment involving two tasks.

In the first task, we ask respondents to guess the environmental cost avoided by not producing a new piece of clothing, in a quiz manner, and then we inform them about the correct figure. 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). All these externalities are involved in the production of new clothing and, unlike CO2, electric energy and water are everyday resources believed to be more relatable to individuals. Moreover, to facilitate comprehension of the measurement units involved, each is supplemented with an illustrative equivalence in a metric consistent with the environmental cost under consideration (e.g., 1 kg CO2e is equated to the distance in km that a medium-sized car has to drive to

[2] *Circular economy for textiles: Taking responsibility to reduce, reuse and recycle textile waste and boosting markets for used textiles*, European Commision Press Release https://ec.europa.eu/commission/presscorner/detail/en/ip_23_3635 [Accessed: 26.03.2024]

generate it). We also ask respondents whether, based on the information received, they would be less, equally, or more likely to purchase second-hand in their daily lives. Respondents in the control skip this task.

The second task involves a voucher choice. Respondents are presented with 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. For half of the sample, we kept the traditional-store voucher fixed and varied the amounts of the second-hand one, thereby eliciting the willingness to accept (WTA) to switch consumption from first-hand to second-hand clothing. For the other half, instead, we did the opposite and thus elicited willingness to pay (WTP) to switch from second-hand to first-hand clothing.

We find that, consistent with our hypotheses, providing information in terms of water or energy usage generates stronger effects on stated intention to purchase second-hand clothing and voucher choices than CO2-based information. Water-based information yields the largest treatment effects, with 43% of respondents declaring to be more likely to purchase (compared to 35% for energy and 30% for CO2), and reducing willingness to accept to forgo second-hand consumption 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. Interestingly, respondents exhibit consistent voucher choices across the two versions of the task, that is, relative to whether the first-hand voucher is set as the fixed option. This suggests that our results are not driven by a framing or an endowment effect, even though the magnitude of WTA and WTP varies with which option is designated as the option.

We also document important heterogeneity in treatment effects. Information provision is most effective among individuals who initially underestimate environmental benefits of favoring second-hand over first-hand (approximately 62% of our sample), while those with accurate prior beliefs show substantial resistance to updating their stated intention. Treatment effects are highest among people scoring high on the pro-environmental behaviors scale (PEBS; Menardo et al., 2020) and hindered by those exhibiting a high need for uniquness (NFU scale; Ruvio et al., 2008). Finally, considering that local environmental conditions may influence how respondents respond to

our treatments, we have linked their choices to administrative data on water scarcity, energy poverty, and air quality, at the provincial or regional level. Contrary to our expectations, we find limited evidence that local environmental conditions moderate treatment effects. Similarly, store proximity influences baseline voucher preferences but does not moderate information provision effects, indicating that our treatments are effective regardless of the perceived accessibility of the conduct.

The paper proceeds as follows. Section 3.2 reviews the relevant literature on environmental information provision. Section 3.3 details our experimental design. Section 3.4 presents our main findings on stated intentions and voucher choices. Section 3.5 examines heterogeneous treatment effects by prior beliefs and individual characteristics, and explores potential moderators including geographic location, store proximity, and local environmental conditions. Section 3.6 discusses the policy implications of our findings as well as the study limitations and some directions for future research.

3.2 Literature Review

A well-established literature has investigated the effectiveness of CO2-based information provision on consumer choices (see, e.g., the literature reviews by Liu et al., 2016; Rondoni and Grasso, 2021). What these contributions generally find is that information referencing CO2 emissions can be effective in fostering sustainable consumption, but its effectiveness appears modest. To our knowledge, the study by Pretner et al., 2021 is one of the few to examine the effect of CO2-based information on second-hand clothing choices. The study finds informing participants about the number of emissions prevented by choosing a second-hand hoodie over a new one increased willingness to pay (WTP) from \$11.90 to \$15.30 in the US sample and from €18.00 to €19.60 in the Italian sample. This limited effectiveness may be due to the difficulty that individuals report in relating to CO2-based information (Dreist et al., 2024; Guenther et al., 2012; Hartikainen et al., 2014; Rondoni & Grasso, 2021).

Previous research has found that communicating metrics in a comprehensible and relatable way enhances information provision (Camilleri & Larrick, 2014; Larrick et

al., 2015; Ungemach et al., 2018). In effect, carbon labels are more effective in fostering sustainable choices when they complement CO2 information with a traffic light system (Lohmann et al., 2022; Thøgersen & Nielsen, 2016) or provide an equivalence with other, more relatable measures, such as monetary saving (Newell & Siikamäki, 2014) or minutes of usage of a light bulb (Camilleri et al., 2019). Notably, Camilleri et al., 2019 find that a label reporting the light-bulb minutes equivalent of a serving of food items (as well as its ranking in terms of carbon footprint) led participants to purchase fewer cans of beef soup and report more accurate estimates for CO2 emissions caused by beef.

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 in alternative environmental externalities (CO2, energy, or water) and converting the metric for each externality into a familiar, but closely related, equivalent metric. The choice of energy and water as alternatives to CO2 is motivated by the observation that, 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, notably energy consumption and water usage.

To enhance information clarity, we express the environmental cost in terms of habitual or familiar actions, making the damage more concrete. We restated kilograms of CO2 as those emitted by an equivalent number of kilometers driven by a medium-sized gasoline car (Pace et al., 2025 for a similar conversion rationale). Regarding energy and water saving, we adopt respectively the equivalence of kWh to hours of a 100-watt light-bulb usage, and liters to minutes of open kitchen tap, since both are actions that individuals can easily relate to and experience in their daily lives (details of the implementation are discussed in Section 3.3.2).

By varying the type of environmental cost information, the present experiment contributes to the discussion on how to design informational campaigns aimed at raising environmental awareness to encourage consumers to adopt greener consumption patterns. Additionally, the experiment sees two elements of novelty. First, rather than simply providing information about environmental costs, it administers an information provision task in the form of a quiz, intending to enhance the respon-

dent's curiosity, engagement, and deliberation. Second, the experiment addresses the topic of conscious consumption choices, particularly in the textile industry, by adopting a voucher choice task. We are aware of other papers relying on voucher choices, such as in meat consumption (Pizzo et al., 2024), yet none do it for clothing.

3.3 Experimental Design

The information provision and voucher choice tasks were embedded in a larger survey about second-hand. 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 information provision and voucher choice tasks is available, respectively, in Section C.2.1 and Section C.2.2, and, together with the text of the full survey, in the project OSF repository.

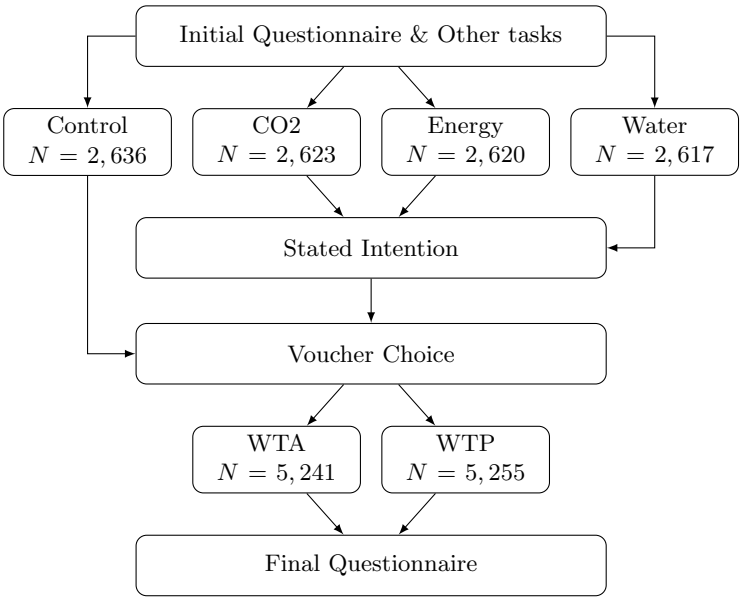
3.3.1 Procedure

The survey was implemented and administered by the survey company Scenari 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 provincial level with respect to the following sociodemographic variables: 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 geographic area (Northwest: 26.93%; Northeast: 19.76%; Center: 19.63%; South: 22.77%; Islands: 10.91%); see Table C.1 in Section C.1 for the full descriptive statistics.

During 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 (€0–20) based on their choices in the voucher task and a random draw (see details in Section 3.3.3). Figure 3.1 provides a visual summary of the experimental design, illustrating the sequence of tasks from random assignment to treatment through the elicitation of stated intentions and voucher choices.

[3]The OSF repository of the project can be accessed here: osf.io/wzcqh

Figure 3.1: Experimental Design Flowchart



3.3.2 Information provision task

The information provision task involved guessing how much resources could be saved in terms of CO2, electricity, or water when switching to second-hand clothing (from first-hand clothing). 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. Figure 3.2 shows an illustration of the task for the CO2e treatment, translated from Italian (see Section C.2.1 for the versions proposed in the other two treatments).

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 tap. Once they had expressed their guess, respondents were shown the true values in a separate screen: 4 kg CO2e, 17 kWh, and 340 liters, respectively. These values and equivalences adopted in the task were based on estimates from various scientific and institutional sources.[4] Finally,

[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).

Figure 3.2: Illustration of the information provision task

(a) Quiz

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

(b) Information provision and intention elicitation

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

following the information provision, respondents were asked to state whether they were less, equally, or more inclined to buy second-hand clothing themselves.[5]

3.3.3 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. We used two different versions of this MPL (see Section C.2.2 for the experimental screens). In the first version (Figure 3.3), the value of the first-hand voucher (Voucher A) is fixed across rows at €10, and the task elicits the respondent's willingness to accept (hereinafter WTA) compensation to switch from a first-hand voucher to a second-hand one (in this case, Voucher B) valued at 0 to €20. In the second 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, keeping the voucher values identical. The conceptual distinction between these two measures stems from the nature of the fixed option: WTA captures the compensation required when moving from a guaranteed option typically regarded

[5]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).

as superior (first-hand) to an alternative regarded as inferior (second-hand), while WTP captures the premium one is willing to pay for the reverse transition.[6] The first version of the task is thus the most intuitive for the interpretation of results, as it sets the first-hand store as the respondents' status quo; it is also the most widespread in similar experiments (for example, 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).

Respondents were required to select a single switching point in the price list, transitioning directly from one voucher option to the other, with no possibility of multiple or inconsistent switches. The voucher could be redeemed at two prominent first-hand and second-hand stores. We carefully selected these two stores to make sure they were comparable. Both stores have similar price levels and are spread across Italy, so they are widely available to our respondents. This was important because our participants come from all over the country. We will discuss how store proximity and accessibility might affect our results in Section 3.5.

Figure 3.3: 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

On a different screen, participants could opt out of the lottery. We included this option to see if information about environmental costs would encourage people to reduce their consumption rather than favoring second-hand items.

3.3.4 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

[6] For a more in-depth discussion of the differences between WTA and WTP, see Bateman et al., 2002.

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)* Do the different environmental cost framing 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 and the revealed true value. Specifically, we conjecture that, when respondents realize that they have underestimated the environmental cost of first-hand clothing, they might, in response, be willing to change their consumption pattern toward second-hand; likewise, those who realize they overestimated the cost might persevere in or increase their consumption of first-hand clothing. 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 treatment*
- *H.2.b higher in Energy than CO2 treatment*
- *H.2.c higher in Water than CO2 treatment*

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 the fixed option) 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 the fixed option) 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.

3.4 Main Results

3.4.1 Stated Intentions

Participants in the three treatments were asked their intention to buy second-hand after the information provision, we find that 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 3.1; furthermore, Figure C.1 in Section C.1 shows the same trend.

Table 3.1: Intention Distribution by Belief Accuracy

	Belief Accuracy		
Intention	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 ±15% tolerance interval around true values. Percentages represent column proportions within each belief accuracy category. Pearson $\chi^2 = 103.41$, $p < 0.001$.

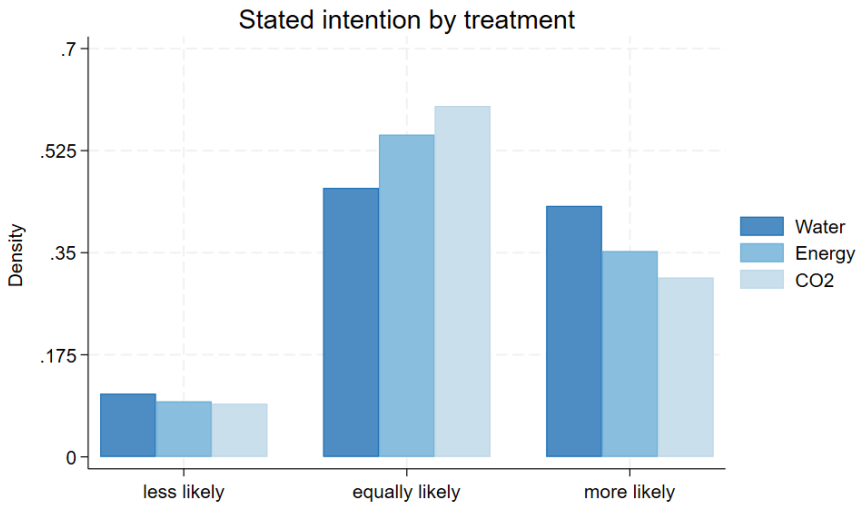
A chi-squared test shows that the intention and guesses are significantly associated, that is, that the relative stated intention to adopt second-hand changes based on underestimation or overestimation of costs (Pearson $\chi^2 = 103.41$, $p < 0.001$). Information provision appears most effective among 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 (62.26%) 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 3.4, the patterns just identified 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.

Figure 3.4: 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.

A chi-squared test confirms that these differences across treatments are statistically significant (Pearson $\chi^2 = 109.56$, $p < 0.001$; see Table C.2 in the Section C.1 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 signifi-*

cantly 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.

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 triggered. Table 3.2 reports the main specifications of these regressions, while the complete models including sociodemographic controls are presented in Table C.7 in Section C.1.

Table 3.2: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	(1)	(2)	(3)
Delta Belief	-0.004**	-0.001	-0.006***
	(0.002)	(0.002)	(0.002)
Water		0.371***	0.367***
		(0.056)	(0.059)
Energy		0.135***	0.132**
		(0.052)	(0.055)
Cut 1	-2.226***	-2.065***	-2.327***
	(0.038)	(0.046)	(0.142)
Cut 2	0.553***	0.726***	0.953***
	(0.024)	(0.039)	(0.139)
Controls	No	No	Yes
Observations	7860	7860	7860

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely). *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, and macro-region of residence (baseline categories set to the mode; full specification in Table C.7).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Model (1) examines the role of belief accuracy across all treatments. The negative and significant coefficient on Delta Belief (-0.004, $p < 0.005$) 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 (0.371, $p < 0.01$), followed by the Energy treatment (0.135, $p < 0.01$). These findings reinforce our earlier descriptive evidence that more tangible and relatable environmental metrics are more effective in shifting behavioral intentions.

Model (3) adds the full set of sociodemographic controls (age, gender, income, education, employment status, pro-environmental behavior, need for uniqueness, past adoption of second-hand, and macro-region of residence; see Table C.7 for the complete specification). The treatment effects remain virtually unchanged: Water (0.367, $p < 0.01$) and Energy (0.132, $p < 0.05$) continue to outperform CO2. Delta Belief becomes more strongly significant (-0.006, $p < 0.01$), suggesting that once observable heterogeneity is accounted for, belief accuracy plays a clearer role in moderating responsiveness to information provision.

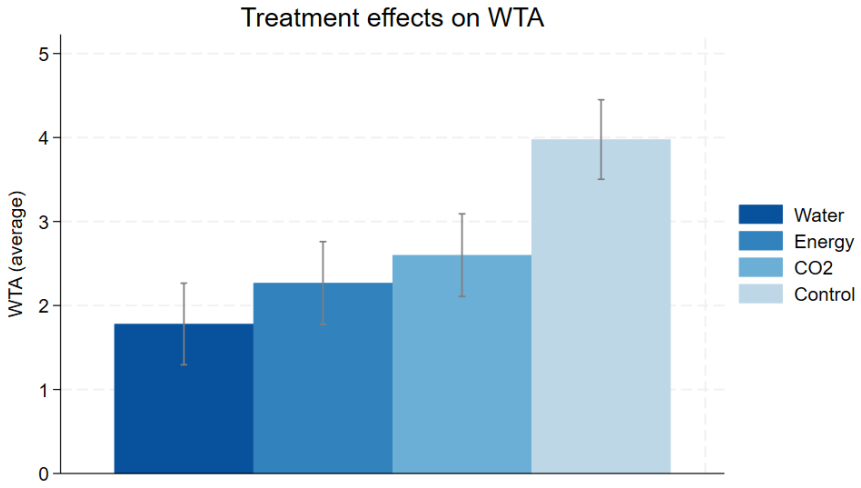
3.4.2 Willingness to Accept and Willingness to Pay Analysis

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 is the fixed option (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 fixed option value of €10, resulting in variables ranging from -€12.5 to €12.5.

Figure 3.5 and Figure 3.6 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

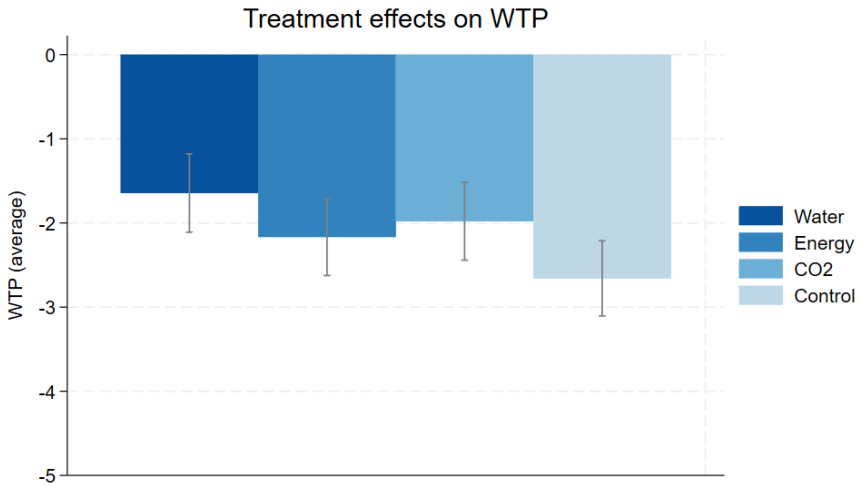
accuracy.

Figure 3.5: 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 3.6: 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.

3.4.2.1 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 3.3a 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.

Table 3.3b 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.

To further investigate the relative importance of information type versus belief accuracy, we estimate a series of Tobit regressions whose main specifications are reported in Table 3.4 (the complete models including sociodemographic controls are presented in Table C.8 in Section C.1). Column (1) confirms that all three information treatments significantly reduce WTA relative to Control, with Water showing the strongest effect (-4.294, $p < 0.01$), followed by Energy (-3.239, $p < 0.01$) and CO2 (-2.531, $p < 0.01$). Column (2) adds sociodemographic controls; the pattern remains consistent, with all three treatments retaining significance relative to Control, though the magnitudes are somewhat attenuated. Column (3) 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 (-2.079, $p < 0.01$), the effect of Energy is no longer statistically distinguishable from CO2. Delta Belief exhibits a negative and significant effect (-0.047, $p < 0.05$), indicating that respondents who were further from the true value show somewhat lower WTA. Column (4) adds controls to this specification: Water

Table 3.3: WTA: Pairwise Mean Comparisons (Bonferroni Adjusted)

(a) By Treatment

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

(b) By Belief Accuracy

	Underestimate	Accurate	Overestimate
Underestimate	—	−0.35	−0.47
Accurate		—	−0.12
Overestimate			—

Note: Values represent the difference in mean WTA between the column category and the row category (column mean minus row mean). Belief accuracy determined using ±15% tolerance interval around true values. Significance levels (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

remains significant (-1.572, $p < 0.05$), but the Delta Belief coefficient becomes insignificant, suggesting that the belief accuracy effect observed in column (3) is partly absorbed by observable individual characteristics.

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 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): *Belief accuracy does not significantly affect WTA. Pairwise comparisons reveal no differences across accuracy groups, and the Delta Belief coefficient, while significant without controls, vanishes once sociodemographic characteristics are included.*

Table 3.4: Tobit Regression Estimations of WTA for Second-Hand

	(1)	(2)	(3)	(4)
Water	-4.294***	-3.688***	-2.079***	-1.572**
	(0.669)	(0.623)	(0.683)	(0.628)
Energy	-3.239***	-2.145***	-0.963	0.007
	(0.681)	(0.633)	(0.688)	(0.635)
CO2	-2.531***	-2.249***		
	(0.680)	(0.631)		
Delta Belief			-0.047**	-0.015
			(0.024)	(0.022)
Constant	7.635***	14.498***	5.373***	13.069***
	(0.492)	(1.423)	(0.507)	(1.599)
Controls	No	Yes	No	Yes
Observations	5241	5074	3930	3788

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTA*, 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 treatment indicators. Baseline: *Control* in columns (1)–(2); *CO2* in columns (3)–(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. *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, macro-region of residence, distance to the nearest first-hand and second-hand store, and whether the closest store is second-hand (baseline categories set to the mode; full specification in Table C.8).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.4.2.2 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 3.5a 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.

Table 3.5b shows an interesting 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 set 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 3.5: WTP: Pairwise Mean Comparisons (Bonferroni Adjusted)

(a) By Treatment

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

(b) By Belief Accuracy

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

Note: Values represent the difference in mean WTP between the column category and the row category (column mean minus row mean). Belief accuracy determined using ±15% tolerance interval around true values. Significance levels (Bonferroni adjusted): * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The Tobit regressions in Table 3.6 provide further insights into these effects (Table 3.6 reports the main specifications of these regressions, while the complete models including sociodemographic controls are presented in Table C.8 in Section C.1). Column (1) shows that both Water (1.574, $p < 0.001$) and CO2 (1.097, $p < 0.05$) increase WTP relative to Control, but Energy does not. Column (2) adds sociodemographic controls; the pattern is confirmed, with Water (1.293, $p < 0.01$) and CO2 (1.196, $p < 0.05$) remaining significant. Column (3) uses CO2 as baseline and introduces Delta Belief, which exhibits a positive and marginally significant effect (0.032, $p < 0.10$): 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 inclined to pay more to return to first-hand options. Neither Water nor Energy significantly differs from CO2 in this specification. Column (4) adds controls: treatment effects

remain insignificant relative to CO2, and the Delta Belief coefficient also becomes insignificant, mirroring the pattern observed for WTA.

These findings provide limited support for our hypotheses:

Result 5 (H.4.a): *Information provision has more limited effects on WTP than on WTA. Water and CO2 coefficients are significant, while only Water-based information shows a robust effect relative to Control in pairwise comparisons. When using CO2 as baseline, no information type demonstrates a significant advantage.*

Result 6 (H.4.b): *Belief accuracy shows an association with WTP. While pairwise comparisons reveal significant differences across accuracy groups, the Delta Belief coefficient is only marginally significant without controls and vanishes once sociodemographic characteristics are included.*

Table 3.6: Tobit Regression Estimations of WTP for First-Hand

	(1)	(2)	(3)	(4)
Water	1.574***	1.293***	0.710	0.214
	(0.532)	(0.495)	(0.556)	(0.517)
Energy	0.751	0.779	-0.166	-0.322
	(0.529)	(0.494)	(0.548)	(0.509)
CO2	1.097**	1.196**		
	(0.532)	(0.494)		
Delta Belief			0.032*	0.022
			(0.017)	(0.016)
Constant	-4.206***	-7.740***	-3.328***	-6.578***
	(0.373)	(1.092)	(0.401)	(1.247)
Controls	No	Yes	No	Yes
Observations	5255	5105	3930	3817

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTP*, 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 treatment indicators. Baseline: *Control* in columns (1)–(2); *CO2* in columns (3)–(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. *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, macro-region of residence, distance to the nearest first-hand and second-hand store, and whether the closest store is second-hand (baseline categories set to the mode; full specification in Table C.8).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

To investigate more thoroughly the psychological drivers of responses to environmental information, we examine two pre-registered individual-level mechanisms: pro-environmental attitudes and need for uniqueness. We elicit those constructs using validated scales, respectively the Pro-Environmental Behavior Scale (PEBS) by Menardo et al., 2020, as adapted from Markle, 2013, and the Need for Uniqueness (NFU) scale by Ruvio et al., 2008, as adapted from Tian et al., 2001. The Pro-Environmental behavior scale (hereinafter, PEB) captures respondents' general orientation toward sustainable consumption practices, which we expect to amplify responsiveness to environmental messaging. Need for Uniqueness (hereinafter, NFU), by contrast, reflects individuals' desire to differentiate themselves from others through distinctive consumption choices. The PEB and NFU variables have been standardized (mean = 0, st.dev = 1).

Table 3.7 reports ordered logistic regressions examining how these mechanisms relate to changes in stated intention to purchase second-hand clothing. Column (1) includes, in addition to Delta Belief, PEB, treatment indicators (using CO2 as baseline), and their interactions. As expected, PEB strongly predicts increased intention to adopt second-hand (0.508, $p < 0.01$). Notably, the Water $\times$ PEB interaction is positive and highly significant (0.245, $p < 0.01$), indicating that the Water treatment is particularly effective among pro-environmentally oriented individuals. The corresponding interaction with Energy is not significant, suggesting that this amplification effect is specific to water-based information. This finding confirms that environmental messaging resonates most strongly with individuals already predisposed toward sustainable consumption, and that this resonance is especially pronounced for water-related information.

Column (2) includes NFU, treatment indicators, and their interactions. We find that NFU exhibits a negative and highly significant association with increased intention to buy second-hand (-0.356, $p < 0.01$). Individuals who score higher on NFU are significantly less likely to report increased willingness to adopt second-hand clothing after learning about environmental benefits. Neither the Water $\times$ NFU nor the Energy $\times$ NFU interactions are significant, indicating that the NFU

Table 3.7: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	(1)	(2)	(3)
Delta Belief	-0.004**	-0.002	-0.004**
	(0.002)	(0.002)	(0.002)
Water	0.340***	0.370***	0.342***
	(0.056)	(0.056)	(0.057)
Energy	0.099*	0.128**	0.098*
	(0.053)	(0.053)	(0.054)
Pro-Environmental Behavior	0.508***		0.453***
	(0.037)		(0.038)
Need for Uniqueness		-0.356***	-0.273***
		(0.038)	(0.039)
Water × Pro-Environmental Behavior	0.245***		0.259***
	(0.055)		(0.058)
Energy × Pro-Environmental Behavior	0.081		0.089
	(0.054)		(0.056)
Water × Need for Uniqueness		-0.009	0.060
		(0.057)	(0.059)
Energy × Need for Uniqueness		0.012	0.023
		(0.054)	(0.057)
Cut 1	-2.246***	-2.122***	-2.270***
	(0.050)	(0.047)	(0.050)
Cut 2	0.743***	0.734***	0.748***
	(0.040)	(0.039)	(0.040)
Observations	7860	7860	7860

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Pro-Environmental Behavior* and *Need for Uniqueness* are standardized scales. Interaction terms between treatment indicators and these scales are included. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

barrier operates uniformly across treatment conditions. 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 (3) includes both mechanisms and all interactions simultaneously. The effects of PEB (0.453, $p < 0.01$) and NFU (-0.273, $p < 0.01$) remain highly significant, confirming that these mechanisms operate independently of each other. The Water $\times$ PEB interaction remains significant (0.259, $p < 0.01$), while none of the NFU interactions reach significance. These results indicate an important asymmetry: while NFU acts as a uniform barrier across all treatment conditions, pro-environmental orientation selectively amplifies the effectiveness of water-based information. Water and Energy remain statistically significant with respect to CO2 throughout the model specifications.

Table 3.8 and Table 3.9 extend 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 (Table 3.8), column (1) includes PEB, treatment indicators (using Control as baseline), and their interactions. We observe that PEB significantly reduces WTA (-2.500, $p < 0.01$), meaning pro-environmentally oriented individuals require less compensation to switch from first-hand to second-hand vouchers. The Energy $\times$ PEB interaction is marginally significant and positive (1.283, $p < 0.10$), suggesting that the PEB effect on WTA is somewhat attenuated in the Energy condition relative to Control; no other PEB interactions are significant.

Column (2) includes NFU and its interactions. NFU significantly increases WTA (2.211, $p < 0.01$). High-NFU individuals demand greater compensation to switch to second-hand vouchers, reinforcing the pattern observed for stated intentions. This effect suggests that resistance to second-hand adoption among individuals with high NFU extends beyond stated preferences to consequential financial trade-offs. None of the NFU interactions are significant, indicating that this barrier operates uniformly

Table 3.8: Tobit Regression Estimations of WTA for Second-Hand

	(1)	(2)	(3)
Water	-4.272***	-4.294***	-4.279***
	(0.664)	(0.663)	(0.660)
Energy	-3.301***	-3.190***	-3.254***
	(0.677)	(0.676)	(0.673)
CO2	-2.735***	-2.513***	-2.691***
	(0.675)	(0.674)	(0.671)
Pro-Environmental Behavior	-2.500***		-2.166***
	(0.506)		(0.511)
Need for Uniqueness		2.211***	1.822***
		(0.489)	(0.493)
Water × Pro-Environmental Behavior	-0.149		-0.043
	(0.702)		(0.720)
Energy × Pro-Environmental Behavior	1.283*		1.193
	(0.731)		(0.739)
CO2 × Pro-Environmental Behavior	0.020		0.119
	(0.692)		(0.703)
Water × Need for Uniqueness		0.042	-0.172
		(0.691)	(0.710)
Energy × Need for Uniqueness		-0.712	-0.498
		(0.700)	(0.705)
CO2 × Need for Uniqueness		0.173	0.120
		(0.682)	(0.691)
Constant	7.705***	7.603***	7.671***
	(0.489)	(0.488)	(0.486)
Observations	5241	5241	5241

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTA*, 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 treatment indicators. Baseline: *Control*. *Pro-Environmental Behavior* and *Need for Uniqueness* are standardized scales. Interaction terms between treatment indicators and these scales are included. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

across treatments.

Column (3) includes both mechanisms simultaneously. PEB (−2.166, $p < 0.01$) and NFU (1.822, $p < 0.01$) both retain their significance and direction, confirming that these mechanisms operate independently. No interaction terms reach significance in this joint specification. As for the stated intention, all treatments remain statistically significant throughout the model specifications.

Turning to WTP (Table 3.9), the patterns reveal important asymmetries. Column (1) shows that PEB strongly increases WTP (2.037, $p < 0.01$). The positive coefficient suggests that pro-environmental individuals who have just been informed about environmental benefits find the imposed second-hand baseline more accept-

able 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. Notably, the Water × PEB interaction is positive and significant (1.089, $p < 0.05$), indicating that pro-environmental individuals respond particularly strongly to water-based information also in the WTP task—echoing the interaction pattern found for stated intentions.

Table 3.9: Tobit Regression Estimations of WTP for First-Hand

	(1)	(2)	(3)
Water	1.317**	1.574***	1.319**
	(0.521)	(0.532)	(0.521)
Energy	0.619	0.762	0.613
	(0.520)	(0.530)	(0.521)
CO2	1.023*	1.106**	1.018*
	(0.522)	(0.532)	(0.522)
Pro-Environmental Behavior	2.037***		2.117***
	(0.384)		(0.396)
Need for Uniqueness		-0.217	0.312
		(0.382)	(0.386)
Water × Pro-Environmental Behavior	1.089**		1.131**
	(0.541)		(0.556)
Energy × Pro-Environmental Behavior	0.183		0.119
	(0.558)		(0.573)
CO2 × Pro-Environmental Behavior	0.545		0.488
	(0.539)		(0.556)
Water × Need for Uniqueness		0.216	0.327
		(0.547)	(0.553)
Energy × Need for Uniqueness		-0.219	-0.245
		(0.543)	(0.549)
CO2 × Need for Uniqueness		-0.300	-0.215
		(0.542)	(0.550)
Constant	-4.076***	-4.206***	-4.071***
	(0.367)	(0.373)	(0.367)
Observations	5255	5255	5255

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTP*, 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 treatment indicators. Baseline: *Control*. *Pro-Environmental Behavior* and *Need for Uniqueness* are standardized scales. Interaction terms between treatment indicators and these scales are included. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column (2) shows that NFU has no significant association with WTP (−0.217, $p > 0.10$), in sharp contrast with the strong WTA effect. None of the NFU interactions are significant. 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 NFU barrier disappears, suggesting that uniqueness motives are activated primarily when individuals are asked to relinquish their natural default rather than when they are offered the opportunity to return to it.

Column (3) confirms these patterns: PEB remains strongly significant (2.117, $p < 0.01$), the Water $\times$ PEB interaction remains significant (1.131, $p < 0.05$), and NFU is again insignificant (0.312, $p > 0.10$). As in the main treatment analysis, Water and CO2 coefficients are significant against the Control.

Taken together, the analyses on stated intentions and voucher choices reveal a consistent picture. PEB operates as a robust amplifier of responsiveness across all outcomes: it increases stated intentions, reduces WTA, and raises WTP. Moreover, its effect is selectively enhanced for water-based information, as the Water $\times$ PEB interaction is significant for stated intentions (Table 3.7) and WTP (Table 3.9). NFU acts as a uniform barrier to second-hand adoption in stated intentions and WTA, but—importantly—this barrier does not extend to the WTP frame, where NFU shows no significant effect. Neither mechanism interacts significantly with treatment type for NFU, confirming that uniqueness motives reduce general receptivity to environmental information regardless of the specific metric used. These patterns are robust to the joint inclusion of both mechanisms (column 3 in all tables).

Result 7 (PEB): *Pro-environmental behavior consistently amplifies responsiveness to environmental information across stated intentions, WTA, and WTP. The Water $\times$ PEB interaction is significant for stated intentions and WTP, indicating that water-based information is particularly effective among pro-environmentally oriented individuals.*

Result 8 (NFU): *Need for Uniqueness operates as a barrier to second-hand adoption in stated intentions and WTA, but not in the WTP frame. This barrier is uniform across treatment conditions, with no significant NFU $\times$ treatment interactions.*

3.5 Additional Results and Robustness Checks

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

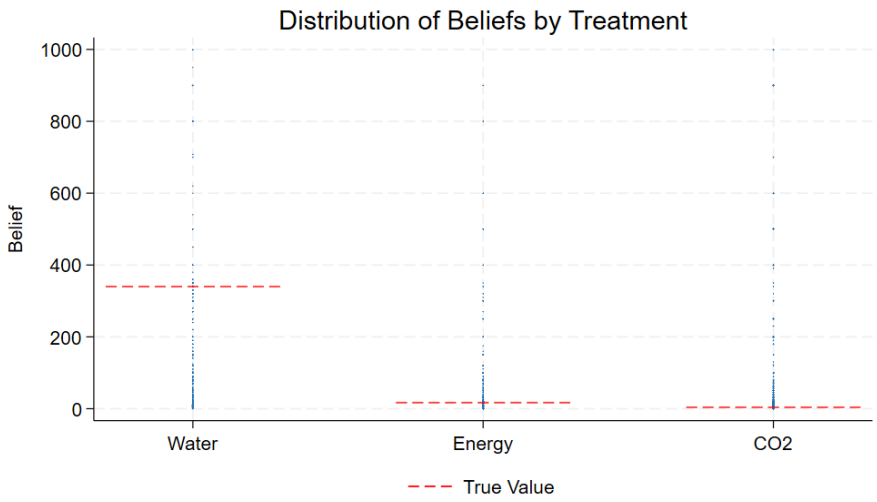
Figure 3.7 presents scatter plots showing the distribution of respondents' initial beliefs against the true values for each environmental externality. Several patterns emerge from this visualization. First, there is substantial heterogeneity in the distribution of prior beliefs across all three environmental costs, 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.

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.

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 3.8 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 3.9 presents violin plots

Figure 3.7: 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.

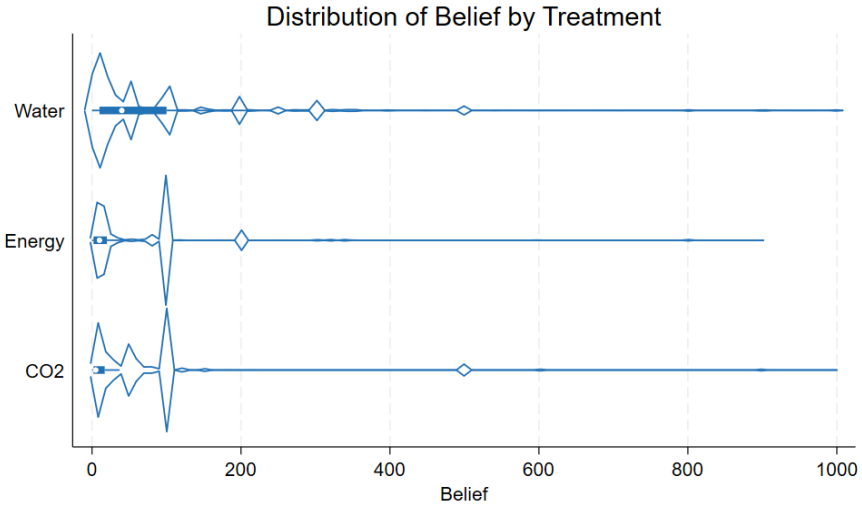
of Delta Belief – the standardized distance between respondents' beliefs and the true values, computed as (belief − true value) / true value – for each treatment. For CO2 and energy, eliminating outliers produces distributions that remain reasonably balanced between under- and overestimation, preserving the essential structure of belief heterogeneity. However, for the water treatment, outlier removal has a larger 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

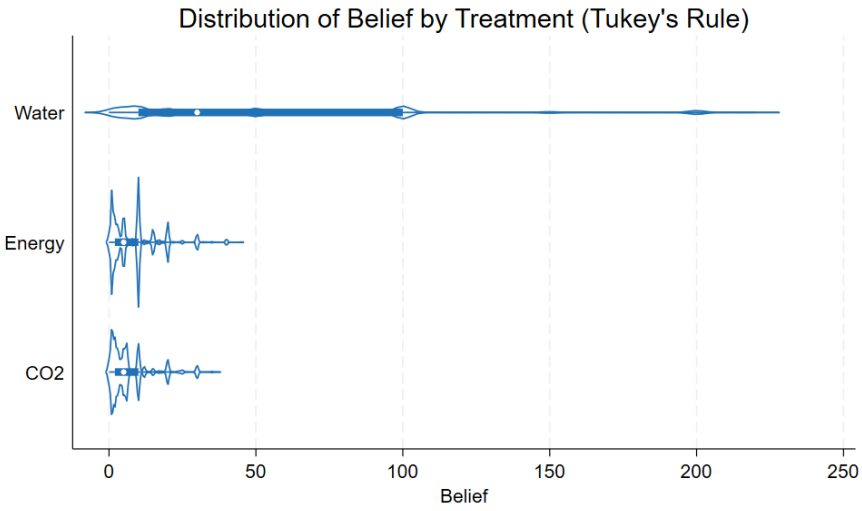
to preserve sample composition and comparability across treatments. However, we provide complete robustness analyses excluding outliers in Section C.1, allowing readers to assess the sensitivity of our findings to this specification choice.

Figure 3.8: Distribution of Beliefs: Full Sample and Outliers Removed

(a) Full Sample



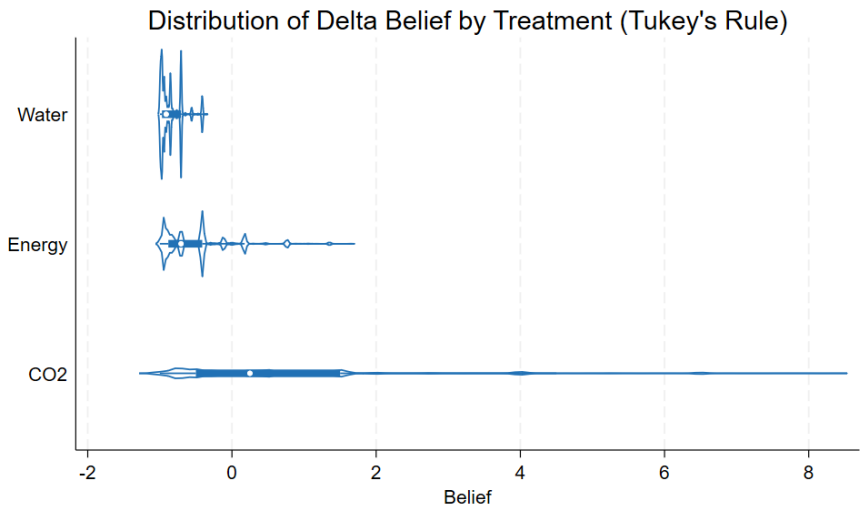
(b) Outliers Removed (Tukey's Rule)



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 × IQR, Q3 + 1.5 × IQR). Each violin shows the probability density of belief values, with wider sections indicating higher concentration of responses.

Result 9 (Belief heterogeneity): *Respondents' prior beliefs exhibit substantial heterogeneity across all treatments, with the majority systematically underestimating environmental benefits.*

Figure 3.9: 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 × IQR, Q3 + 1.5 × IQR). Negative values indicate underestimation; positive values indicate overestimation.

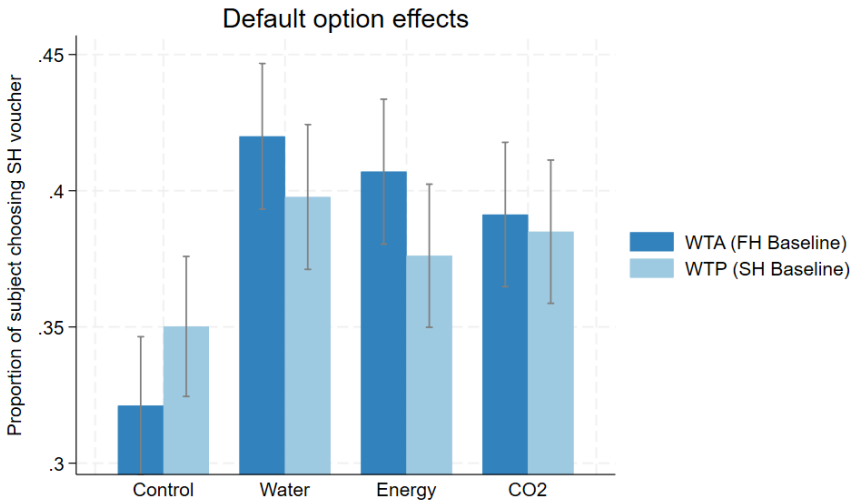
3.5.2 Default Options Across MPL Frames

A key feature of our experimental design is the use of two multiple price list (MPL) frames: one with first-hand (FH) as the fixed option and another with second-hand (SH) as the fixed option. 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 fixed option 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 preference for the type (either FH, or SH) net of its monetary value. 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 3.10 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 3.10. Pooling across all treatments, 38.5%

Figure 3.10: 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.

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 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 (Brown et al., 2024; Marzilli Ericson & Fuster, 2014), respondents in our experiment appear to exhibit relatively stable preferences when choosing between equally-valued vouchers, regardless of which option is designated as the baseline. This consistency strengthens the inter-

pretation 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 3.10: 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	0.418
Control	0.321	0.350	0.029	−1.58	0.114
Water	0.420	0.398	−0.022	1.16	0.247
Energy	0.407	0.376	−0.031	1.62	0.106
CO2	0.391	0.385	−0.006	0.33	0.740

Note: Two-sample proportion tests comparing the share of indifferent respondents (switching at the baseline value of €10) across the two elicitation tasks. *WTA (Baseline FH)* reports the proportion of respondents indifferent between the first-hand and second-hand voucher in the WTA task; *WTP (Baseline SH)* reports the corresponding proportion in the WTP task. *Difference* is WTP minus WTA proportion. Tests are reported for the full sample and separately by treatment group.
Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.*

3.5.3 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 islands of Pantelleria and La Maddalena, 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 postal code, 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 3.11 reports Tobit regressions examining how store proximity moderates treatment effects on WTA. Column (1) includes the treatment indicators and the binary indicator for which store type is closer. The results reveal an interesting pattern: respondents for whom the second-hand store is the closest option exhibit significantly higher WTA (1.247, $p < 0.01$), 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) adds interactions between treatment indicators and the SH Closest dummy. None of the interactions are significant, indicating that relative proximity does not differentially moderate specific treatment effects. The SH Closest main effect also loses significance in this specification.

Column (3) 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 (4) adds interactions between treatment indicators and the continuous distance measures. The Energy × FH Distance interaction is negative and significant (-0.088, $p < 0.05$), and the Energy × SH Distance interaction is positive and marginally significant (0.056, $p < 0.10$), suggesting that the Energy treatment becomes somewhat more effective when first-hand stores are farther away and less effective when second-hand stores are farther away. No other distance interactions are significant.

Critically, across all four specifications, the treatment effects remain stable (with the exception of CO2 in Column (2)). The consistency of these coefficients across specifications indicates that store proximity does not substantially moderate treat-

Table 3.11: Tobit Regression Estimations of WTA for Second-Hand

	(1)	(2)	(3)	(4)
Water	-3.996***	-4.216***	-4.004***	-3.967***
	(0.665)	(1.025)	(0.666)	(0.935)
Energy	-2.459***	-2.826***	-2.466***	-2.271**
	(0.676)	(1.018)	(0.676)	(0.961)
CO2	-2.667***	-1.629	-2.652***	-2.441***
	(0.677)	(1.047)	(0.677)	(0.942)
SH Closest	1.247***	1.432		
	(0.477)	(0.976)		
Water × SH Closest		0.382		
		(1.346)		
Energy × SH Closest		0.662		
		(1.361)		
CO2 × SH Closest		-1.784		
		(1.372)		
FH Distance (in km)			-0.018	0.022
			(0.014)	(0.027)
SH Distance (in km)			0.007	-0.016
			(0.011)	(0.022)
Water × FH Distance (in km)				-0.042
				(0.038)
Energy × FH Distance (in km)				-0.088**
				(0.039)
CO2 × FH Distance (in km)				-0.032
				(0.039)
Water × SH Distance (in km)				0.028
				(0.030)
Energy × SH Distance (in km)				0.056*
				(0.033)
CO2 × SH Distance (in km)				0.013
				(0.032)
Constant	6.834***	6.724***	7.694***	7.583***
	(0.562)	(0.751)	(0.546)	(0.670)
Observations	5074	5074	5074	5074

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTA*, 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 treatment indicators. Baseline: *Control*. *SH Closest* is a dummy equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code. *FH Distance* and *SH Distance* denote the driving distance (in km) from the respondent's postal code to the nearest first-hand and second-hand store, respectively. Interaction terms between treatment indicators and store-proximity variables are included.
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ment effects—information provision appears equally effective regardless of store accessibility.

Table 3.12 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) adds interactions with SH Closest; none are significant. Column (3) uses continuous distance measures; neither is significant. Column (4) adds distance interactions; none reach significance.

Table 3.12: Tobit Regression Estimations of WTP for First-Hand

	(1)	(2)	(3)	(4)
Water	1.571***	1.597*	1.569***	1.279*
	(0.533)	(0.825)	(0.533)	(0.760)
Energy	0.707	0.336	0.700	0.475
	(0.529)	(0.832)	(0.529)	(0.749)
CO2	1.103**	0.679	1.100**	1.090
	(0.534)	(0.819)	(0.533)	(0.750)
SH Closest	-0.013	-0.343		
	(0.383)	(0.757)		
Water × SH Closest		-0.061		
		(1.081)		
Energy × SH Closest		0.637		
		(1.078)		
CO2 × SH Closest		0.737		
		(1.080)		
FH Distance (in km)			-0.003	0.006
			(0.012)	(0.023)
SH Distance (in km)			-0.008	-0.020
			(0.009)	(0.017)
Water × FH Distance (in km)				-0.015
				(0.034)
Energy × FH Distance (in km)				-0.000
				(0.033)
CO2 × FH Distance (in km)				-0.018
				(0.032)
Water × SH Distance (in km)				0.024
				(0.025)
Energy × SH Distance (in km)				0.010
				(0.023)
CO2 × SH Distance (in km)				0.013
				(0.026)
Constant	-4.075***	-3.881***	-3.860***	-3.734***
	(0.438)	(0.587)	(0.419)	(0.518)
Observations	5105	5105	5105	5105

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTP*, 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 treatment indicators. Baseline: *Control*. *SH Closest* is a dummy equal to 1 if the second-hand store is closer than the first-hand store to the respondent's postal code. *FH Distance* and *SH Distance* denote the driving distance (in km) from the respondent's postal code to the nearest first-hand and second-hand store, respectively. Interaction terms between treatment indicators and store-proximity variables are included.
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

As with WTA, treatment effects remain stable across specifications, with Water always significant and CO2 only in the specification with no interaction effects. 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 on WTA): *While relative store proximity affects baseline WTA (with closer second-hand stores paradoxically increasing WTA), it does not substantially moderate treatment effects. Notably, there are mild interaction effects of store proximity and Energy.*

Result 12 (Store proximity on WTP): *Store proximity does not influence, nor moderate treatment effects, in WTP.*

3.5.4 Geographic Heterogeneity: Regional Variation in Treatment Effects

Our analysis thus far has examined treatment effects pooling across respondents of diverse geographic areas. 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 stated intention and voucher choice analyses (Table 3.2, Table 3.4 and Table 3.6) 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 Section C.1.

Despite the limitations in statistical power, several suggestive patterns emerge from the regional analysis that warrant discussion.

Starting with stated intentions (Table C.9), the Water treatment significantly outperforms CO2 in all macro-regions except the Islands (the macro-region with the smallest sample size), with the Center showing the largest coefficient (0.676, $p < 0.01$). The Energy treatment is significant only in the Center (0.250, $p < 0.05$) and South (0.200, $p < 0.10$), while showing no advantage over CO2 in the Northwest, Northeast, or Islands. This pattern confirms that the superiority of water-based information is geographically robust, while the relative effectiveness of energy versus CO2 messaging is more regionally specific.

Turning to WTA (Table C.10), 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.

An interesting regional specificity appears in the Islands macro-region, where the Energy treatment exhibits a strong effect on WTA (7.21, $p < 0.01$ in column 9), nearly double the magnitude observed in other regions. Even after controlling for 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 (more on this topic in Section 3.5.5).

Northeast and Center regions emerges as the most uniformly responsive area, with all three information treatments showing significant effects in the baseline specification. Conversely, the South shows a more selective pattern: while Water maintains

strong effects, neither Energy nor CO2 reach significance.

Turning to WTP (Table C.11), the regional patterns are different and generally much weaker. Notable exceptions occur in the South, where water is the only treatment showing a robust positive effect on WTP (3.14, $p < 0.01$ in column 7), though this effect weakens when controlling for belief updating (2.203, $p < 0.10$ in column 8); and in the Northwest, where CO2 shows significance (2.03, $p < 0.05$). Elsewhere, Water and CO2 are not significant, and Energy fails to reach significance in any region.

Several important caveats apply to these regional findings. The sample sizes for individual macro-regions range from 428 to 2,103 observations across the different specifications, 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 $\times$ 8 treatments) 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 suggests that local environmental conditions and economic contexts may meaningfully shape how individuals respond to different types of environmental information. In the next section, we 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.

3.5.5 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 vari-

ables 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 (NO_2), ozone (O_3), 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 3.13 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.

The results provide limited evidence that local environmental conditions moderate treatment effects in the hypothesized directions. Table C.12 and Table C.13 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

Table 3.13: 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 ($\mu g/m^3$, provincial)	10,466	23.52	4.41	10	35.6
O_3 concentration ($\mu g/m^3$, provincial)	10,214	59.43	7.81	44	95
PM10 concentration ($\mu g/m^3$, provincial)	10,466	18.79	5.14	4	33.31
PM2.5 concentration ($\mu g/m^3$, 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 as the vote share for green parties in the 2024 EU elections; *Flooding risk* as the percentage of provincial land area at risk (2020–2021); *Water scarcity* as a dummy equal to 1 if the province declared a water emergency in 2021; *Energy Poverty Index* at the regional level (2020); air quality variables (*NO_2*, *O_3*, *PM10*, *PM2.5*) as average annual concentrations at the provincial level (2020–2021).

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 variable (HPH) shows no significant main effect or interaction for WTA or WTP. Interestingly, provinces with water usage restrictions show lower baseline WTP (-2.094, $p < 0.01$), though the corresponding WTA coefficient (-1.381) does not reach significance. However, the Water treatment interaction with water scarcity, while positive, does not reach conventional significance for WTA and is only marginally significant for WTP (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 for either WTA or WTP. However, the Energy × Modified LIHC interaction is negative and marginally significant for WTA (-0.261, $p < 0.10$), tentatively suggesting that the Energy treatment may be somewhat more effective in regions with higher energy poverty. This effect does not appear for WTP, and given the multiple comparisons involved,

should be interpreted cautiously.

For the air quality specifications (column 4), several notable patterns emerge for WTA. NO_2 concentration shows a positive and significant main effect (0.206, $p < 0.01$), and PM2.5 shows a negative and significant main effect (-0.342, $p < 0.01$), indicating that air quality indicators are associated with baseline WTA levels. The CO2 $\times$ O_3 interaction is positive and significant (0.173, $p < 0.05$), but this suggests that higher ozone concentrations make the CO2 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 do not systematically moderate treatment effects on voucher choices in the hypothesized directions. There are few marginally significant interactions that emerge (water scarcity $\times$ Water for WTP, Energy Poverty $\times$ Energy for WTA, both $p < 0.10$; CO2 $\times$ O_3 for WTA, $p < 0.05$ but opposite the expected sign). This pattern suggests that the effectiveness of different environmental framings is not strongly contingent on whether respondents face corresponding local environmental challenges.*

3.6 Discussion and Conclusion

This paper investigates how different types of environmental information affects individuals' willingness to adopt second-hand clothing. Using a large, nationally representative sample of Italian consumers ($N = 10{,}496$), we combine stated intentions with incentivized voucher choices to provide a comprehensive assessment of information framing effects.

Our results can be summarized as follows. Water-based information generates the strongest behavioral responses across all outcomes, increasing stated intention among 43% of respondents (versus 35% for energy and 31% for CO2) and reducing WTA by approximately 20% (€2.20) relative to control. Energy shows intermediate effectiveness, while CO2—the dominant metric in sustainability communication—is the least effective frame. These treatment rankings are robust across specifications and outcome measures. Belief accuracy matters for stated intentions, where underestimators respond most strongly, but does not significantly affect incentivized choices once sociodemographic controls are included, suggesting that observable characteristics jointly determine both prior beliefs and behavioral responsiveness. Pro-environmental behavior amplifies treatment effects across all outcomes, with a particularly strong interaction with water-based information for stated intentions and WTP. Need for uniqueness acts as a uniform barrier in stated intentions and WTA but, notably, not in the WTP frame—consistent with uniqueness motives being activated when individuals are asked to relinquish their natural default rather than when offered the opportunity to return to it. WTA and WTP measures yield asymmetric results: treatment effects are substantially attenuated when second-hand is artificially imposed as the status quo, raising concerns about WTP elicitation in contexts where the baseline is not naturally experienced as the default. Finally, neither local environmental conditions (water scarcity, energy poverty, air quality, pro-environmental political attitudes) nor store proximity systematically moderate treatment effects, suggesting that information operates through context-independent cognitive channels rather than through resonance with locally-salient problems.

These findings contribute to several strands of the literature. We extend work on environmental information provision (Allcott & Taubinsky, 2015; Jessoe & Rap-

son, 2014) by showing that not only whether but *which* environmental externalities are presented critically determines effectiveness—the widespread reliance on CO2 metrics may reflect scientific convention rather than psychological impact. We contribute to the literature on heterogeneous treatment effects by documenting that belief accuracy channels. Our results on pro-environmental behavior and need for uniqueness add to research on individual moderators of pro-environmental interventions, revealing an asymmetry between WTA and WTP that speaks to debates on reference dependence and the WTA-WTP gap. Finally, the null results on contextual moderation challenge context-dependent persuasion models and suggest limits to the intuition that environmental messages resonate more when matched to local problems.

From a policy perspective, our results suggest that shifting from CO2-based to water-based environmental messaging could meaningfully enhance the effectiveness of sustainability campaigns at no additional cost. This recommendation is especially relevant for sectors like fashion and textiles, where water impacts are substantial and intuitively meaningful to consumers. The finding that treatment effects do not vary with local environmental conditions implies that standardized national campaigns can be deployed without costly geographic customization—water-based messaging appears equally effective in water-scarce and water-abundant regions. However, the concentration of effects among already pro-environmentally oriented individuals highlights the limits of information-based interventions. For high-NFU consumers who resist environmental messaging, alternative framings emphasizing individuality and counter-mainstream identity—rather than environmental virtue—may prove more effective. Our methodological findings also caution that WTP estimates imposing counterfactual defaults may underestimate demand for sustainable options, potentially biasing policy evaluations toward underinvestment in green alternatives.

Several limitations should be acknowledged. We do not observe actual purchasing behavior: while the voucher task is consequential, redemption and follow-through remain unobserved, and field experiments tracking real purchases would strengthen external validity. Our local environmental condition measures are aggregated at provincial or regional levels and may not capture individual-level variation in exposure or subjective salience. Finally, our one-shot information provision differs

from real-world campaigns involving repeated exposure and social reinforcement; longitudinal designs would illuminate dynamic effects and optimal messaging frequency. Future research should also pursue process-tracing methods to identify why water and energy metrics outperform CO2—whether through stronger emotional responses, easier visualization, or reduced cognitive load—and develop alternative message frames capable of engaging consumer segments that are resistant to environmental appeals.

In sum, this paper demonstrates that the choice of environmental metric matters substantially for the effectiveness of sustainability communication. Water-based information consistently outperforms energy and CO2, across stated intentions and incentivized choices in a large representative sample. Pro-environmental orientation amplifies these effects while need for uniqueness acts as a barrier, and local environmental conditions do not moderate them. For policymakers, the implication is straightforward: reframing environmental information in terms of water rather than CO2 can enhance the behavioral impact of sustainability campaigns promoting second-hand consumption, though reaching beyond already environmentally engaged consumers remains an open challenge requiring complementary strategies.

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Conclusion

This dissertation set out from a question of pressing practical relevance: why do consumers who overwhelmingly say they want to reduce waste so often fail to do so, and what, specifically, stands in their way? The scale of what is at stake motivated this question in the opening chapter of this dissertation: over 58 million tonnes of food and nearly 7 million tonnes of textiles are discarded in the EU each year, much of it while still edible or wearable, and it is precisely this scale that makes the answer worth pursuing carefully rather than assuming it away. A common policy instinct is to treat the attitude-behavior gap as an information problem: tell consumers more, or tell them more clearly, and behavior will follow. The three chapters of this dissertation test that instinct directly, in two settings where the stakes are real and the mechanisms are measurable. What emerges is that the instinct turns out to be only half right. Information does matter, but its specific form is what moves behavior; and much of what stands between favorable attitudes and sustainable choices is less a lack of facts than a misalignment between what consumers are told and how they see themselves, and each other, when acting on it. The remainder of this concluding chapter sets out what this means, first by summarizing what each chapter found, then by drawing out what the three findings mean when read together, and finally by turning that synthesis into concrete guidance for policy.

Chapter 1 shows that, for perishable food, the calendar date drives consumer valuation regardless of the type of date mark used, and that educating consumers about date-mark meaning has an asymmetric effect: it lowers valuation of items nearing a safety-relevant threshold but fails to raise valuation of items that remain usable, a pattern traced to inattention and asymmetric native understanding of the two label types.

Chapter 2 shows that self-image is a stronger and more consistent predictor of

second-hand adoption than social image or beliefs about others' private evaluations, with the environmental dimension the notable exception, and documents robust pluralistic ignorance: consumers systematically underestimate how favorably others privately regard second-hand adoption, which in turn depresses their own adoption intentions.

Chapter 3 shows that, among three environmental metrics used to frame the cost of new clothing, water-based information is the most effective at increasing willingness to purchase second-hand clothing, outperforming the CO2 metric that dominates real-world sustainability communication, with effects concentrated among already pro-environmentally oriented consumers and dampened by a high need for uniqueness.

Read individually, these three sets of results answer three separate empirical questions. Read together, however, they point to a set of contributions that no single chapter could establish on its own. Two patterns recur across the three chapters, and a third emerges only when the first two are read side by side.

A first pattern is that changing how the same underlying information is presented can be inert or highly consequential, and which of the two is not obvious in advance. Chapter 1 shows that varying the type of date mark attached to a perishable food item (best-before versus use-by) has essentially no effect on consumers' native valuation, even though the two labels are legally intended to convey different messages. Chapter 3 documents the opposite pattern in a different domain: all the environmental metric used to frame the cost of a new garment (CO2, energy, or water) have a significant effect on the willingness to purchase second-hand clothing. Where information does move behavior, moreover, it need not move it uniformly: Chapter 1 also shows that educating consumers about date-mark meaning has a real but asymmetric effect, reducing valuation of food nearing a safety-relevant threshold while leaving valuation of still-usable food unchanged. Read together, these results caution against treating *providing information* as a single, predictable lever: whether a given piece of information moves behavior, and along which margin, has to be established empirically for each case rather than assumed.

A second pattern, specific to the two second-hand chapters, is that what predicts adoption is anchored in how consumers see themselves, not in how they believe

others see them. Chapter 2 shows that self-image is a stronger and more consistent predictor of second-hand adoption than either social image or second-order self-image (beliefs about others' private views), across all four dimensions studied; the chapter also documents a robust misalignment effect, whereby adoption drops when a respondent's self-image falls below their second-order self-image, pointing to self-perception, rather than social approval as such, as the behaviorally binding channel. Chapter 3 echoes this from a different angle: independently of which environmental metric a respondent sees, their own pro-environmental orientation and need for uniqueness, both identity-linked traits, are directly associated with stated intentions and incentivized willingness to pay or accept, respondents higher in pro-environmental orientation report greater willingness to switch to second-hand, while those with a stronger need for uniqueness show the opposite pattern. In both chapters, in other words, second-hand adoption is better explained by consumers' relationship to themselves than by reputational or social-signaling concerns.

Taken together, these two patterns point to a third, more general implication for policy design. Because the effectiveness of information cannot be predicted from its content alone, and because, at least for second-hand goods, the more reliable lever is self-directed rather than socially directed, policymakers should not assume that any accurate information will work, nor default to appeals about how a choice will be perceived by others. Interventions are more likely to succeed when they are tested empirically and framed around how consumers see themselves.

The synthesis that emerges is therefore not that information and persuasion are ineffective, but that their effectiveness cannot be assumed and has to be established for each specific message and margin: the metric that happens to be more relatable rather than the one already in wide use, self-directed framing rather than appeals to social approval, and safety-consistent rather than waste-consistent educational framing where the two are in tension. This heterogeneity, documented consistently across a food-waste and two second-hand-consumption settings, is the central contribution that only becomes visible when the three chapters are read as a single body of evidence.

These broader contributions are not only of academic interest: each of them translates into a concrete lever that policymakers, retailers, and communication

campaigns can act on.

First, on **date-label design**, the finding that date-mark type does not independently affect valuation, while education has an asymmetric, safety-favoring effect, cautions against policy proposals that treat date-mark harmonization or simplification alone as an effective food-waste reduction tool. If the objective is specifically to reduce discarding of still-safe food, interventions will need to target consumers' native understanding of best-before labels directly, rather than relying on general point-of-sale education that, this dissertation shows, consumers apply asymmetrically across label types.

Second, on **environmental communication campaigns**, the result that water-based framing outperforms the CO2 metric that currently dominates sustainability communication implies that shifting the default metric used in campaigns promoting second-hand clothing, and, plausibly, other sustainable consumption categories, could increase their behavioral impact at no additional cost. Because treatment effects in Chapter 3 do not vary with local environmental conditions, this recommendation does not require costly geographic customization: a national campaign built around water impacts should be similarly effective across regions with very different water stress levels.

Third, on second-hand adoption campaigns, the finding from Chapter 2 that self-image dominates social image as a predictor of adoption suggests that campaigns are likely to be more effective when they strengthen consumers' private sense of making a savvy, hygienic, or otherwise personally rewarding choice, rather than when they emphasize how the choice will be perceived by others. A second, more tentative implication follows from the chapter's misalignment result: because adoption drops when a respondent's self-image falls below their belief about others' private views, correcting the (largely inaccurate) belief that others privately disapprove of second-hand purchasing, for instance by communicating survey evidence that most consumers privately view it favorably, is a plausible low-cost complement to self-directed messaging. This dissertation did not test such a norm-correction intervention directly, and it is flagged here as a promising hypothesis for future research rather than as a result the evidence already establishes.

These recommendations should, however, be read against the limitations of the

evidence they rest on.

Three limitations apply across the dissertation as a whole. First, on external validity, all three studies were conducted in an Italian or German context; while the mechanisms identified, namely, inattention, asymmetric label understanding, self- versus social-image weighting, and metric-specific responsiveness to environmental information, have no obvious reason to be geographically bounded, the specific magnitudes reported here (for instance, the size of the water-versus-CO2 gap in Chapter 3, or the extent of pluralistic ignorance in Chapter 2) may not generalize directly to consumer populations with different retail institutions, labeling conventions, or baseline attitudes toward sustainability.

Second, on stated versus revealed preferences, the three chapters occupy different points on this spectrum, and this heterogeneity is itself a limitation when the chapters are read together. Chapter 1 relies on an incentivized valuation task throughout; Chapter 3 combines stated intentions with an incentivized voucher choice, allowing a direct comparison between the two; Chapter 2, by contrast, relies on a vignette experiment eliciting stated adoption intentions rather than incentivized choices, since the design's purpose would have been difficult to achieve with a single incentivized outcome. As with any survey-based measure, Chapter 2's results may therefore be subject to socially desirable responding or experimenter-demand effects to a degree that the incentivized chapters are less exposed to.

Third, none of the three chapters observes actual purchasing behavior outside the experimental setting: the voucher task in Chapter 3 is consequential but redemption and follow-through remain unobserved, the in-store valuation task in Chapter 1 does not track subsequent food consumption or disposal, and the vignette task in Chapter 2 measures intentions rather than completed purchases. Field experiments that track real, longer-horizon purchasing and consumption decisions would strengthen the external validity of the behavioral patterns documented here.

These limitations notwithstanding, the findings of this dissertation open several avenues that future research is well positioned to pursue.

Four directions follow naturally from the findings of this dissertation. The most immediate is to test interactions between the informational and perceptual barriers studied separately across the three chapters: for instance, combining water-based

environmental information (Chapter 3) with a social-norm correction communicating others' true, largely favorable private attitudes toward second-hand adoption (Chapter 2) to test whether the two levers are complementary or substitutable, and whether their combination is particularly effective for the high-need-for-uniqueness consumers who resist environmental appeals alone. A second direction is to extend the approach to **other domains** of circular consumption beyond food and clothing: repair versus replacement decisions for durable and electronic goods being a natural candidate, given that similar informational (repair-cost labeling) and perceptual (image of repairing versus replacing) barriers plausibly apply. A third direction is **longitudinal design**: all three chapters rely on single-shot interventions, and it remains an open question whether the effects of date-mark education, image-based messaging, or metric-specific environmental framing persist, decay, or interact with repeated exposure over time. Finally, the dissertation's in-store and large-sample survey designs represent a step toward external validity, but **large-scale field experiments** that track actual purchasing and consumption behavior over time, rather than incentivized but one-off valuation and choice tasks, would be the natural next step to confirm that the behavioral patterns identified here translate into measurable reductions in food and textile waste at scale.

Waste, this dissertation suggests, is rarely a failure of caring. Consumers who want to do the right thing are stopped less by indifference than by a mismatch, identifiable and correctable, between what they are told and how they actually experience the choice in front of them. None of the interventions this dissertation points to is expensive or exotic: a better-chosen date label, a more relatable environmental metric, a corrected belief about what one's neighbors quietly already do. None requires consumers to want something they do not already want. The shoppers in Chapter 1 already prefer not to waste food, the respondents in Chapter 2 already regard second-hand purchasing favorably in private, and the consumers in Chapter 3 already respond to environmental appeals when the appeal is one they can relate to; what was missing, in each case, was not the underlying preference but a message calibrated to it. And each rests on causal, field-tested evidence rather than on intuition about what ought to work. If circular consumption is, as this dissertation opened by arguing, one of the defining resource-allocation puzzles of contemporary

consumption, its conclusion is an optimistic one: closing the gap between attitude and action is not, in cases like these, a matter of changing what people want, but of finally getting the message right.

Author Contribution Statement

As required in §10 (3) of the Curriculum for the joint PhD Program Economics and Statistics (Doctoral Program) at the Faculty of Economics and Statistics at the University of Innsbruck and the Faculty of Social and Economic Sciences at the Johannes Kepler University Linz (Complete version as of 1 October 2021; Original version published in the University of Innsbruck Bulletin of 24 June 2021, Issue 84, No. 884), the doctoral candidate's own contribution for each project (i.e., specialist article) are pointed out in the following list. For this, the "CRediT author statement" is used as a guideline.

Chapter 1: Date Marking and Consumer Valuation of Perishable Food Around the Expiry Date

1. **Methodology:** Contributed to the development and implementation of the experimental design, including the operationalization of the date-mark treatments, the information intervention, and the modified multiple price list elicitation procedure.
2. **Software:** Prepared and implemented the software-based components of the experimental procedure and data management workflow.
3. **Investigation:** Contributed to the empirical implementation of the study, including the preparation and execution of the experimental procedures.
4. **Project Administration:** Coordinated the day-to-day organization of the empirical activities, including logistical planning, coordination of research tasks, and monitoring of data collection procedures.
5. **Data Curation:** Managed, cleaned, documented, and prepared the experimental data for empirical analysis.
6. **Formal Analysis:** Conducted the statistical and econometric analyses used to estimate the effects of expiry dates, date-mark type, and consumer education on willingness to pay.
7. **Visualization:** Produced the figures, tables, and graphical representations used to present the empirical results.
8. **Writing – Original Draft:** Drafted the first version of the manuscript.
9. **Writing – Review & Editing:** Contributed to revising the manuscript and integrating feedback from external reviewers during the drafting process.

Chapter 2: Image Considerations and Second-Hand Adoption

1. **Methodology:** Contributed to the design of the vignette experiment.

2. **Data Curation:** Cleaned, documented, and prepared the survey data for empirical analysis, including the construction of variables and analysis-ready datasets.

3. **Formal Analysis:** Performed the statistical and econometric analyses used to examine the relationship between image perspectives and intentions to adopt second-hand goods.

4. **Visualization:** Produced the tables and graphical representations used to report and interpret the empirical findings.

5. **Writing – Original Draft:** Drafted the results sections of the manuscript, including the presentation and interpretation of the empirical analyses.

Chapter 3: Environmental Information and Willingness to Purchase Second-Hand Clothing

1. **Conceptualization:** Contributed to the development and refinement of the research question concerning the role of environmental cost metrics in second-hand clothing adoption.

2. **Methodology:** Designed the experimental protocol, including the environmental information treatments based on CO2 emissions, energy consumption, and water usage, as well as the stated-intention outcomes and the incentivized multiple price list choice task.

3. **Project Administration:** Coordinated the organization and implementation of the study, including the management of timelines, survey implementation procedures, and research team communication.

4. **Data Curation:** Cleaned, documented, and prepared the survey and experimental-choice data for empirical analysis, including the construction of variables and analysis-ready datasets.

5. **Formal Analysis:** Conducted the statistical and econometric analyses used to estimate treatment effects and examine heterogeneity by pro-environmental behavior, need for uniqueness, and local environmental conditions.

6. **Visualization:** Produced the tables and graphical representations used to present the main results and heterogeneity analyses.

7. **Writing – Original Draft:** Drafted the main sections of the manuscript, excluding the literature review, including the experimental design, empirical strategy, results, and discussion.

Appendix A

True to type? EU-style date marking and the valuation of perishable food

A.1 Flowcharts of experimental designs

Figure A.1: Flowchart of the preparatory survey

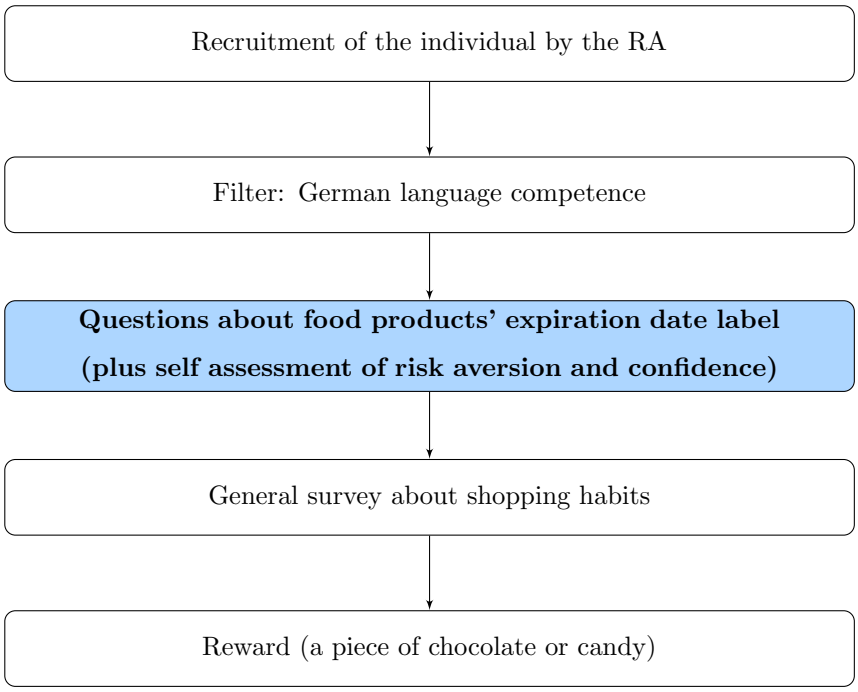


Figure A.2: Flowchart of the modified MPL Experiment

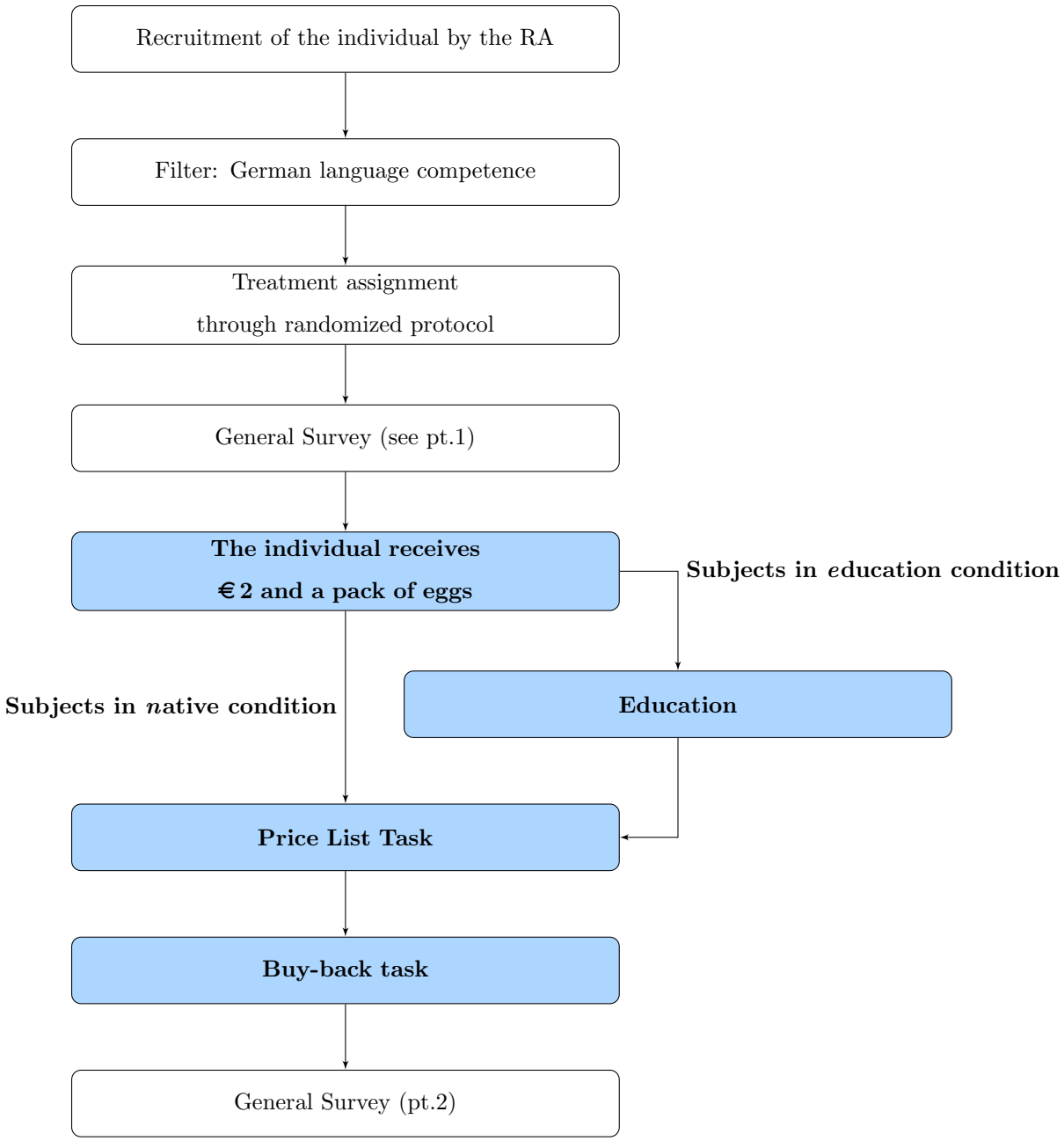


Figure A.3: Flowchart of the Attentiveness Experiment

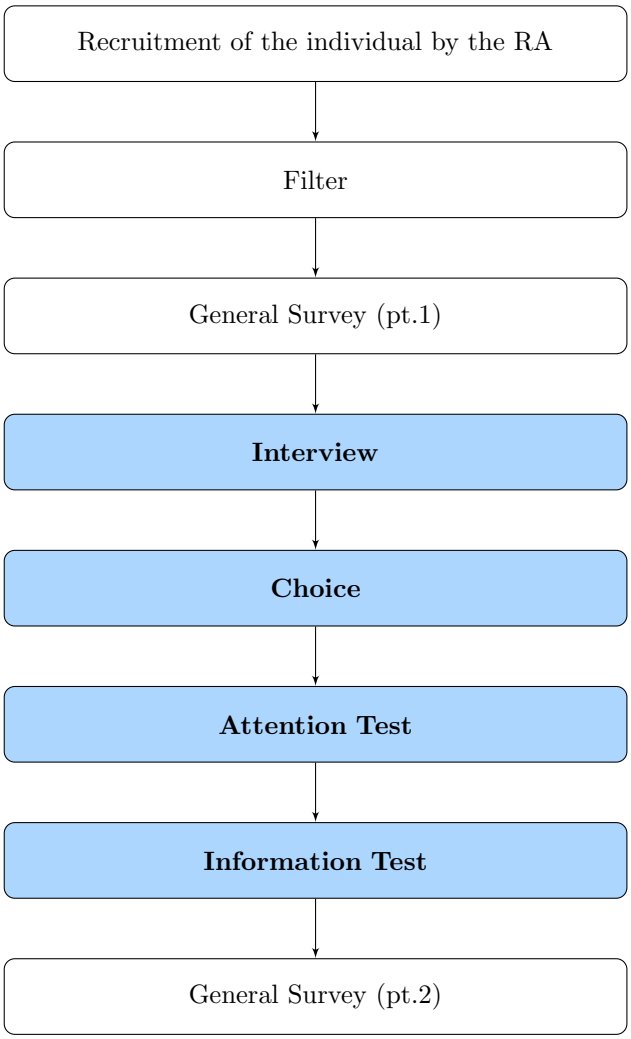
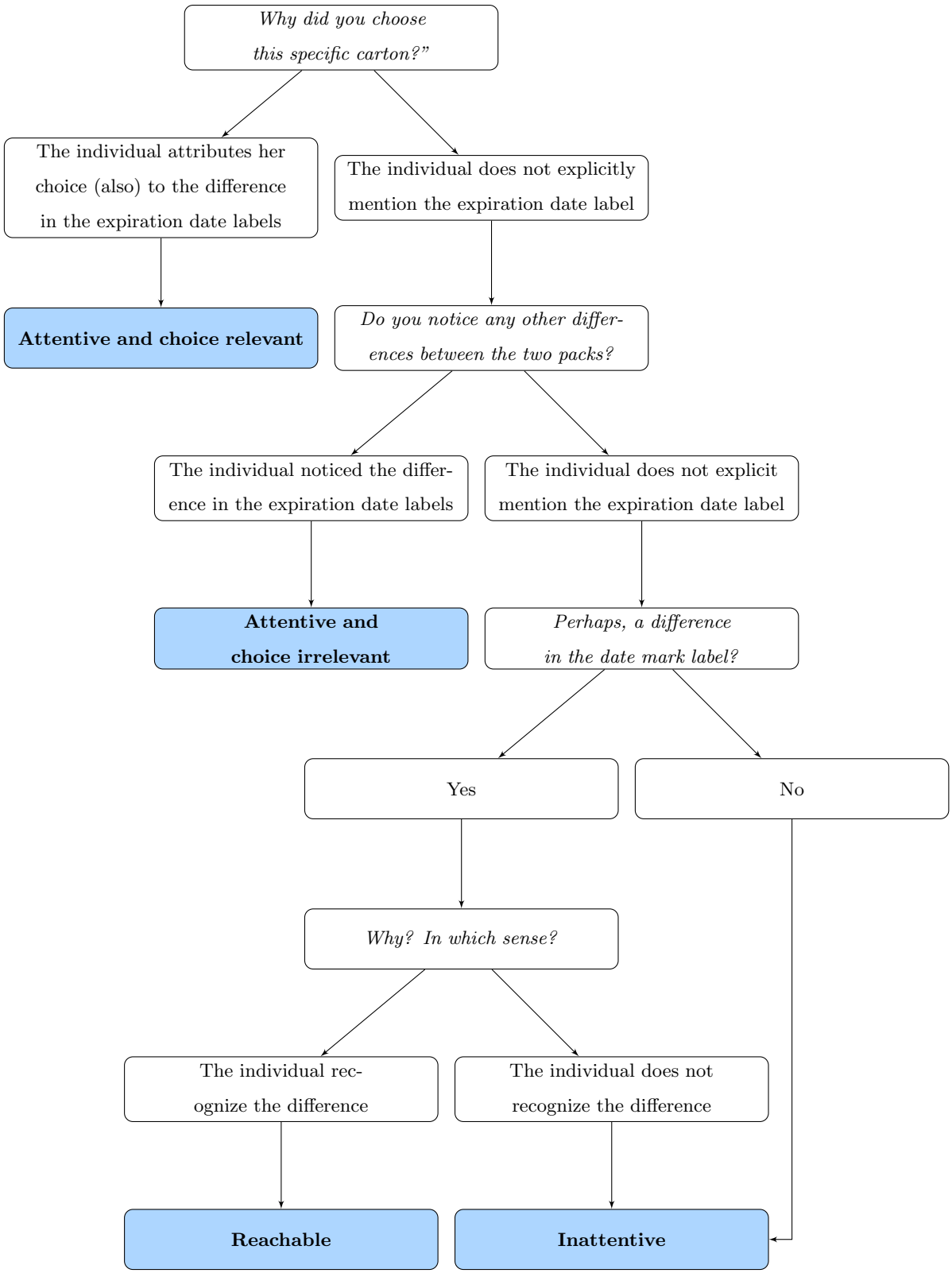


Figure A.4: Flowchart of the Attention Test (Inattention Experiment)



The questions for the Information test are as follows

1. *In shops, packaged groceries are sold with two labels indicating expiration dates. One is "Best Before", the other is "Use By". What do you think:*
    - The two date marks have different messages for the consumer.
    - The two date marks have the same meaning for the consumer.
2. *The "use by" date means:*
    - The food will be safe to eat up to this date and should not be eaten past this date (UB)
    - The food can be consumed after this date, but it may no longer be at its best quality (BB)
    - The food can be used after this date only if the packaging is not damaged (CD1)
    - The food will be safe to eat from this date on and should be eaten past this date (CD2)
3. *The "best before" date means:*
    - The food will be safe to eat up to this date and should not be eaten past this date (UB)
    - The food can be consumed after this date, but it may no longer be at its best quality (BB)
    - The food can be used after this date only if the packaging is not damaged (CD1)
    - The food will be safe to eat from this date on and should be eaten past this date (CD2)

The order of the two questions and the order of the possible options (equal in the two question for each individual in order to avoid possible confusion) is random across the subjects. The answers used in these two questions are the definition of the UB label and of the BB and two confounding definitions (CDs). The label definitions in English and in German are the official ones used in the Eurobarometer, 2015.

A.2 Survey questions

General Survey (pt. 1)

1. Do you regularly buy groceries?
    - Yes
    - No
    - I prefer not to answer
2. Would you saying that you are doing the most grocery shopping of your household?
    - Yes
    - No
    - I prefer not to answer
3. How many people, including yourself, live in your household?
    - 1
    - 2
    - 3
    - 4
    - 5 or more
    - I prefer not to answer
4. *If more than one person in the household:* Is someone in your household vegetarian?
    - Yes
    - No
    - I prefer not to answer
5. Are you vegetarian?
    - Yes
    - No
    - I prefer not to answer
6. *If more than one person in the household:* Is someone in your household vegan?
    - Yes
    - No
    - I prefer not to answer
7. Are you vegan?
    - Yes
    - No

- I prefer not to answer

8. If we may ask: Could you assign yourself to one of the following age groups?
    - 18-25
    - 26-35
    - 36-45
    - 46-55
    - 56-65
    - 66 or more
    - I prefer not to answer
9. Do you have children?
    - Yes
    - No
    - I prefer not to answer
10. Which gender do you associate with?
    - Female
    - Male
    - Other
    - I prefer not to answer
11. Which gender do you associate with?
    - Female
    - Male
    - Other
    - I prefer not to answer
12. What is the highest educational attainment you have achieved?
    - *Kein Schulabschluss* - no High School Diploma.
    - *Hauptschulabschluss* - Hauptschule Diploma, awarded after 9 years of Education.
    - *Realschulabschluss* - Realschule Diploma, awarded after 10 years of Education.
    - *Abitur* - High School Diploma, awarded after 12 years of Education and necessary prerequisite for University.
    - *Berufsschulabschluss* - Apprenticeship, awarded after 3-years education cycle in which the student can learn a craft work from a trainer (an expert in the field) and attend a vocational school.
    - *Hochschulabschluss* - University Degree, with no difference whether it is Bachelor or Master.

- *Promotion* - Ph.D.

13. You are currently...
    - Studying/Training
    - Self-employed
    - Employed
    - Retired
    - Unemployed
    - I prefer not to answer.

General Survey (pt. 2)

N.B. The second part of the general survey is only administered to subjects in the modified MPL experiment.

1. What do you usually pay most attention to when you buy groceries at the grocery store:
    - Produced in Germany
    - Sustainable or environmentally friendly production
    - Price
    - Brand
    - Other [please specify].
2. Do you or anyone else in your household like to eat eggs?
    - Yes
    - No
3. Do you or anyone else in your household have an egg allergy?
    - Yes
    - No
4. Is food sometimes thrown away in your household?
    - Yes
    - No
5. Who is typically responsible for discarding such food?
    - Me
    - Someone else

Appendix B

Image Considerations in Second-Hand Adoption: A Vignette Experiment

B.1 Additional analyses

B.1.1 Balance tables

Table B.1: Distribution of demographic variables across vignette products

	T-shirt	Closet	Refrigerator	Smartphone	Joint F-test
Mean					
Age	50.21	50.09	50.10	48.33	$< .001 * **$
Pro-Environmental Behavior	3.38	3.37	3.37	3.34	0.219
Need for Uniqueness	2.97	2.95	2.94	2.92	0.154
Proportion					
Gender					
Woman	0.503	0.502	0.548	0.495	$< .001 * **$
Man	0.497	0.498	0.452	0.505	$< .001 * **$
Macro-region					
Northwest	0.291	0.283	0.236	0.267	$< .001 * **$
Northeast	0.180	0.201	0.232	0.179	$< .001 * **$
Central	0.185	0.198	0.206	0.196	0.265
South	0.238	0.204	0.220	0.250	$< .001 * **$
Islands	0.107	0.114	0.106	0.109	0.778
Education					
Middle School (or lower)	0.096	0.093	0.081	0.077	0.039**
High School	0.544	0.542	0.557	0.538	0.531
University	0.313	0.314	0.315	0.327	0.652
PhD	0.047	0.051	0.046	0.058	0.221
Employment status					
Worker (blue collar)	0.072	0.077	0.077	0.073	0.853
Employed	0.394	0.385	0.425	0.421	0.005***
Self-employed	0.110	0.123	0.107	0.127	0.050*
Student	0.065	0.065	0.056	0.060	0.464
Homemaker	0.089	0.088	0.091	0.087	0.943
Retired	0.214	0.181	0.175	0.154	$< .001 * **$
Looking for a job	0.043	0.058	0.056	0.058	0.043**
Other	0.014	0.023	0.013	0.020	0.022**
Income					
Don't know/Prefer not to answer	0.193	0.184	0.166	0.166	0.021**
Less than €1,000	0.041	0.047	0.046	0.046	0.775
From €1,000 to €1,500	0.113	0.108	0.111	0.100	0.394
From €1,500 to €2,000	0.160	0.161	0.167	0.167	0.842
From €2,000 to €3,000	0.216	0.229	0.242	0.234	0.153
From €3,000 to €5,000	0.185	0.188	0.183	0.187	0.958
From €5,000 to €10,000	0.058	0.052	0.052	0.063	0.222
Over €10,000	0.033	0.032	0.032	0.036	0.813

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Distribution of demographic variables across vignette conditions

	Excellent	Good	Acceptable	Joint F-test
Mean				
Age	49.80	49.66	49.49	0.707
Pro-Environmental Behavior	3.36	3.37	3.37	0.960
Need for Uniqueness	2.95	2.94	2.95	0.691
Proportion				
Gender				
Woman	0.522	0.499	0.510	0.139
Man	0.478	0.501	0.490	0.139
Macro-region				
Northwest	0.269	0.267	0.273	0.869
Northeast	0.199	0.198	0.195	0.943
Central	0.198	0.193	0.197	0.873
South	0.224	0.227	0.233	0.667
Islands	0.110	0.115	0.102	0.272
Education				
Middle School (or lower)	0.089	0.090	0.079	0.260
High School	0.549	0.552	0.533	0.288
University	0.314	0.312	0.329	0.280
PhD	0.048	0.047	0.059	0.072*
Employment status				
Worker (blue collar)	0.078	0.073	0.072	0.595
Employed	0.400	0.403	0.420	0.240
Self-employed	0.112	0.115	0.127	0.152
Student	0.064	0.061	0.057	0.511
Homemaker	0.088	0.091	0.087	0.829
Retired	0.188	0.180	0.169	0.127
Looking for a job	0.053	0.060	0.048	0.119
Other	0.017	0.016	0.019	0.630
Income				
Don't know/Prefer not to answer	0.180	0.174	0.176	0.765
Less than €1,000	0.044	0.046	0.045	0.952
From €1,000 to €1,500	0.103	0.116	0.108	0.206
From €1,500 to €2,000	0.169	0.160	0.159	0.428
From €2,000 to €3,000	0.229	0.231	0.232	0.924
From €3,000 to €5,000	0.185	0.185	0.188	0.954
From €5,000 to €10,000	0.057	0.053	0.058	0.598
Over €10,000	0.033	0.035	0.033	0.824

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Distribution of demographic variables across vignette price levels

	Free	Affordable price	Joint F-test
Mean			
Age	49.51	49.88	0.223
Pro-Environmental Behavior	3.37	3.36	0.876
Need for Uniqueness	2.93	2.96	0.048**
Proportion			
Gender			
Woman	0.513	0.509	0.682
Man	0.487	0.491	0.682
Macro-region			
Northwest	0.269	0.269	0.971
Northeast	0.198	0.197	0.974
Central	0.190	0.205	0.050*
South	0.229	0.226	0.657
Islands	0.114	0.103	0.071*
Education			
Middle School (or lower)	0.085	0.088	0.585
High School	0.545	0.545	0.988
University	0.318	0.317	0.958
PhD	0.052	0.049	0.534
Employment status			
Worker (blue collar)	0.071	0.080	0.060*
Employed	0.410	0.402	0.399
Self-employed	0.117	0.117	0.957
Student	0.063	0.059	0.346
Homemaker	0.086	0.092	0.226
Retired	0.182	0.179	0.746
Looking for a job	0.055	0.052	0.487
Other	0.017	0.019	0.450
Income			
Don't know/Prefer not to answer	0.180	0.173	0.344
Less than €1,000	0.045	0.045	0.946
From €1,000 to €1,500	0.104	0.113	0.178
From €1,500 to €2,000	0.167	0.160	0.395
From €2,000 to €3,000	0.234	0.225	0.296
From €3,000 to €5,000	0.178	0.196	0.018**
From €5,000 to €10,000	0.059	0.053	0.217
Over €10,000	0.033	0.034	0.768

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.1.2 Additional figures

Figure B.1: Probability to take the product by type and condition

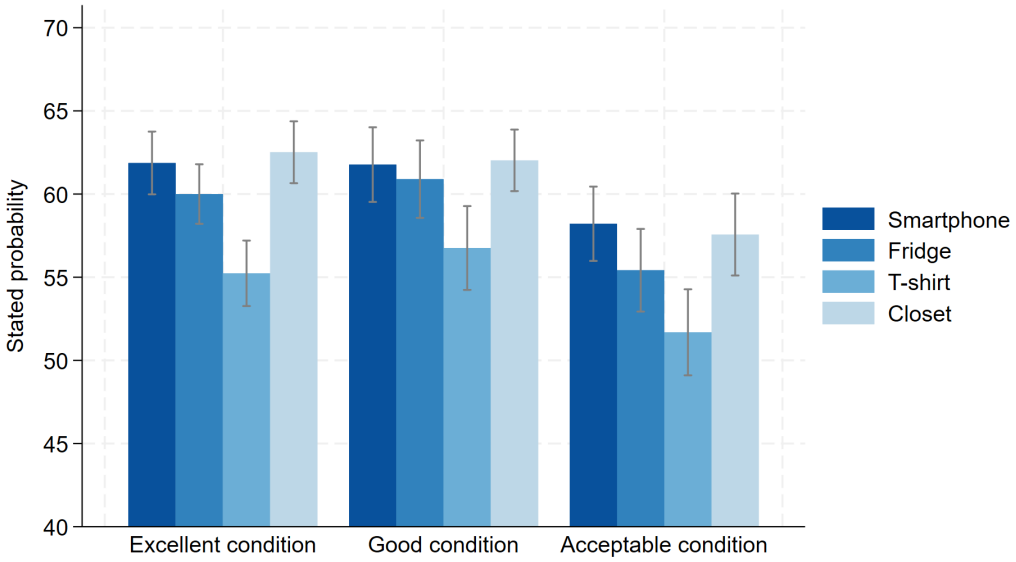
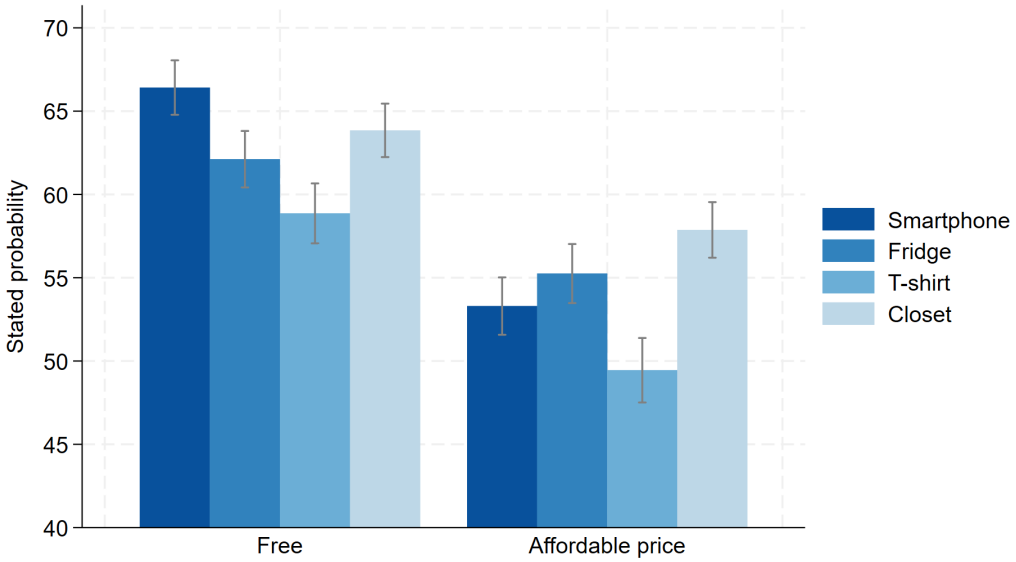


Figure B.2: Probability to take the product by type and price



B.1.3 Extended baseline specifications (full controls)

Table B.4: Vignette attributes and probability to take the product

	(1)	(2)	(3)	(4)	(5)
Excellent condition	0.102***		0.103***	0.107***	0.111***
	(0.020)		(0.020)	(0.020)	(0.019)
Good condition	0.120***		0.116***	0.113***	0.091***
	(0.022)		(0.021)	(0.022)	(0.021)
Free		0.227***	0.226***	0.227***	0.245***
		(0.016)	(0.016)	(0.016)	(0.015)
Smartphone				0.158***	0.122***
				(0.023)	(0.023)
Fridge				0.111***	0.192***
				(0.024)	(0.023)
Closet				0.166***	0.174***
				(0.023)	(0.022)
Age					−0.003***
					(0.001)
Man					0.079***
					(0.016)
Need for Uniqueness					−0.021**
					(0.011)
Pro-Environmental Behavior					0.117***
					(0.012)
Don't know/Prefer not to answer					−0.032
					(0.025)
Less than €1,000					0.015
					(0.043)
From €1,000 to €1,500					0.013
					(0.029)
From €1,500 to €2,000					−0.016
					(0.025)
From €3,000 to €5,000					−0.024
					(0.024)
From €5,000 to €10,000					−0.091**
					(0.038)
Over €10,000					−0.085*
					(0.049)
Middle School (or lower)					0.025
					(0.030)

University					−0.053***
					(0.018)
PhD					−0.147***
					(0.039)
Worker (blue collar)					0.012
					(0.031)
Self-employed					−0.050*
					(0.027)
Student					−0.023
					(0.037)
Homemaker					−0.044
					(0.032)
Retired					−0.066**
					(0.029)
Looking for a job					−0.078**
					(0.037)
Other employment status					−0.038
					(0.064)
Yes, I have already purchased them					0.865***
					(0.021)
Yes, but I have never purchased them					0.695***
					(0.020)
Yes, but I would not repurchase them					0.446***
					(0.029)
North-East					0.084***
					(0.023)
Central					0.121***
					(0.023)
South					0.065***
					(0.022)
Islands					0.036
					(0.028)
Constant	0.146***	0.101***	0.022	−0.088***	−0.791***
	(0.016)	(0.011)	(0.018)	(0.023)	(0.070)
Observations	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Excellent condition* and *Good condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Fridge, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to

North-West. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Self-image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.132***	0.127***	0.129***	0.125***
	(0.016)	(0.016)	(0.016)	(0.016)
Social approval	0.155***	0.156***	0.153***	0.111***
	(0.018)	(0.018)	(0.018)	(0.018)
Environmental consciousness	0.020	0.020	0.021	0.019
	(0.017)	(0.017)	(0.017)	(0.016)
Disgust insensitivity	0.406***	0.405***	0.403***	0.293***
	(0.017)	(0.017)	(0.017)	(0.017)
Excellent condition		0.079***	0.080***	0.091***
		(0.019)	(0.019)	(0.019)
Good condition		0.079***	0.075***	0.066***
		(0.021)	(0.021)	(0.020)
Free		0.219***	0.221***	0.235***
		(0.016)	(0.016)	(0.015)
Smartphone			0.093***	0.071***
			(0.023)	(0.022)
Fridge			0.103***	0.158***
			(0.022)	(0.022)
Closet			0.135***	0.148***
			(0.022)	(0.022)
Age				−0.004***
				(0.001)
Man				0.061***
				(0.016)
Need for Uniqueness				−0.044***
				(0.011)
Pro-Environmental Behavior				0.065***
				(0.012)
Don't know/Prefer not to answer				−0.069***
				(0.024)
Less than €1,000				0.053
				(0.042)
From €1,000 to €1,500				0.030
				(0.029)
From €1,500 to €2,000				0.007
				(0.024)
From €3,000 to €5,000				−0.033
				(0.024)

From €5,000 to €10,000				−0.052
				(0.038)
Over €10,000				−0.055
				(0.052)
Middle School (or lower)				0.032
				(0.029)
University				−0.056***
				(0.018)
PhD				−0.077**
				(0.039)
Worker (blue collar)				0.003
				(0.031)
Self-employed				−0.065**
				(0.026)
Student				−0.084**
				(0.035)
Homemaker				−0.076**
				(0.032)
Retired				−0.095***
				(0.028)
Looking for a job				−0.092**
				(0.037)
Other employment status				−0.096
				(0.060)
Yes, I have already purchased them				0.617***
				(0.022)
Yes, but I have never purchased them				0.509***
				(0.020)
Yes, but I would not repurchase them				0.450***
				(0.029)
North-East				0.087***
				(0.023)
Center				0.121***
				(0.023)
South				0.087***
				(0.022)
Islands				0.036
				(0.027)
Constant	0.029***	−0.147***	−0.230***	−0.491***
	(0.010)	(0.018)	(0.023)	(0.070)

Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking a value in $\{-1, -0.33, +0.33, +1\}$. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Social image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.062***	0.060***	0.060***	0.045***
	(0.017)	(0.017)	(0.017)	(0.017)
Social approval	0.126***	0.131***	0.128***	0.082***
	(0.021)	(0.021)	(0.021)	(0.020)
Environmental consciousness	0.108***	0.103***	0.105***	0.080***
	(0.019)	(0.019)	(0.019)	(0.018)
Disgust insensitivity	0.250***	0.252***	0.248***	0.154***
	(0.018)	(0.018)	(0.018)	(0.017)
Excellent		0.083***	0.085***	0.097***
		(0.020)	(0.020)	(0.019)
Good		0.094***	0.091***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.233***	0.234***	0.247***
		(0.016)	(0.016)	(0.015)
Smartphone			0.099***	0.082***
			(0.023)	(0.022)
Fridge			0.094***	0.165***
			(0.023)	(0.023)
Closet			0.124***	0.144***
			(0.023)	(0.022)
Age				−0.004***
				(0.001)
Man				0.063***
				(0.016)
Need for Uniqueness				−0.029***
				(0.011)
Pro-Environmental Behavior				0.087***
				(0.012)
Don't know/Prefer not to answer				−0.053**
				(0.025)
Less than €1,000				0.025
				(0.043)
From €1,000 to €1,500				0.024
				(0.029)
From €1,500 to €2,000				−0.007
				(0.025)
From €3,000 to €5,000				−0.022
				(0.024)

From €5,000 to €10,000				−0.059
				(0.038)
Over €10,000				−0.067
				(0.050)
Middle School (or lower)				0.022
				(0.029)
University				−0.053***
				(0.018)
PhD				−0.114***
				(0.039)
Worker (blue collar)				0.012
				(0.031)
Self-employed				−0.047*
				(0.027)
Student				−0.054
				(0.036)
Homemaker				−0.053*
				(0.032)
Retired				−0.083***
				(0.029)
Looking for a job				−0.092**
				(0.037)
Other employment status				−0.044
				(0.062)
Yes, I have already purchased them				0.727***
				(0.022)
Yes, but I have never purchased them				0.595***
				(0.020)
Yes, but I would not repurchase them				0.439***
				(0.029)
North-East				0.086***
				(0.023)
Center				0.108***
				(0.023)
South				0.077***
				(0.022)
Islands				0.038
				(0.027)
Constant	0.096***	−0.096***	−0.175***	−0.615***
	(0.010)	(0.018)	(0.023)	(0.070)

Observations	10,496	10,496	10,496	10,496

Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Social image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking values in $\{-1, -0.33, +0.33, +1\}$. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Second-order self-image and probability to take the vignette product

	(1)	(2)	(3)	(4)
Financial savvy	0.110***	0.108***	0.109***	0.070***
	(0.017)	(0.017)	(0.017)	(0.017)
Social approval	0.076***	0.079***	0.075***	0.063***
	(0.019)	(0.019)	(0.019)	(0.018)
Environmental consciousness	0.128***	0.126***	0.128***	0.092***
	(0.018)	(0.018)	(0.018)	(0.017)
Disgust insensitivity	0.214***	0.213***	0.209***	0.129***
	(0.018)	(0.018)	(0.018)	(0.017)
Excellent		0.086***	0.088***	0.099***
		(0.020)	(0.020)	(0.019)
Good		0.095***	0.092***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.226***	0.227***	0.243***
		(0.016)	(0.016)	(0.015)
Smartphone			0.104***	0.085***
			(0.023)	(0.022)
Fridge			0.099***	0.170***
			(0.023)	(0.022)
Closet			0.130***	0.148***
			(0.023)	(0.022)
Age				−0.004***
				(0.001)
Man				0.068***
				(0.016)
Need for Uniqueness				−0.030***
				(0.011)
Pro-Environmental Behavior				0.090***
				(0.012)
Don't know/Prefer not to answer				−0.044*
				(0.024)
Less than €1,000				0.034
				(0.043)
From €1,000 to €1,500				0.030
				(0.029)
From €1,500 to €2,000				−0.000
				(0.025)
From €3,000 to €5,000				−0.025
				(0.024)

From €5,000 to €10,000				−0.059
				(0.038)
Over €10,000				−0.060
				(0.051)
Middle School (or lower)				0.027
				(0.029)
University				−0.053***
				(0.018)
PhD				−0.111***
				(0.039)
Worker (blue collar)				0.004
				(0.031)
Self-employed				−0.051*
				(0.027)
Student				−0.058
				(0.036)
Homemaker				−0.057*
				(0.032)
Retired				−0.079***
				(0.029)
Looking for a job				−0.095**
				(0.037)
Other employment status				−0.066
				(0.062)
Yes, I have already purchased them				0.752***
				(0.022)
Yes, but I have never purchased them				0.611***
				(0.020)
Yes, but I would not repurchase them				0.447***
				(0.029)
North-East				0.089***
				(0.023)
Center				0.115***
				(0.023)
South				0.069***
				(0.022)
Islands				0.028
				(0.027)
Constant	0.099***	−0.089***	−0.173***	−0.636***
	(0.010)	(0.018)	(0.023)	(0.070)

Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Second-order self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking values in $\{-1, -0.33, +0.33, +1\}$. *Good condition* and *Excellent condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.1.4 Alternative coding of image variables

Table B.8: Self-image and probability to take the vignette product (image dummies)

	(1)	(2)	(3)	(4)
Financial (−−)	−0.227***	−0.218***	−0.221***	−0.227***
	(0.035)	(0.035)	(0.035)	(0.034)
Financial (−)	−0.269***	−0.257***	−0.261***	−0.234***
	(0.029)	(0.029)	(0.029)	(0.029)
Financial (+)	−0.252***	−0.243***	−0.244***	−0.238***
	(0.023)	(0.023)	(0.023)	(0.022)
Social (−−)	−0.301***	−0.309***	−0.305***	−0.204***
	(0.035)	(0.035)	(0.035)	(0.033)
Social (−)	−0.123***	−0.131***	−0.127***	−0.079***
	(0.021)	(0.021)	(0.021)	(0.021)
Social (++)	0.003	−0.005	−0.006	0.016
	(0.024)	(0.024)	(0.024)	(0.024)
Environmental (−−)	−0.023	−0.024	−0.024	−0.019
	(0.038)	(0.038)	(0.038)	(0.037)
Environmental (−)	−0.068**	−0.069**	−0.074**	−0.063**
	(0.029)	(0.029)	(0.029)	(0.029)
Environmental (+)	−0.185***	−0.181***	−0.181***	−0.148***
	(0.021)	(0.021)	(0.021)	(0.021)
Disgust (−−)	−0.840***	−0.840***	−0.833***	−0.615***
	(0.040)	(0.040)	(0.040)	(0.039)
Disgust (−)	−0.439***	−0.440***	−0.437***	−0.308***
	(0.028)	(0.028)	(0.028)	(0.027)
Disgust (+)	−0.198***	−0.201***	−0.201***	−0.148***
	(0.022)	(0.022)	(0.022)	(0.022)
Excellent		0.079***	0.080***	0.090***
		(0.019)	(0.019)	(0.019)
Good		0.081***	0.078***	0.068***
		(0.021)	(0.021)	(0.020)
Free		0.213***	0.214***	0.228***
		(0.016)	(0.016)	(0.015)
Smartphone			0.100***	0.077***
			(0.023)	(0.022)
Fridge			0.103***	0.159***
			(0.022)	(0.022)
Closet			0.133***	0.146***
			(0.022)	(0.022)

Age	−0.004***
	(0.001)
Man	0.059***
	(0.016)
Need for Uniqueness	−0.042***
	(0.011)
Pro-Environmental Behavior	0.058***
	(0.012)
Don't know/Prefer not to answer	−0.071***
	(0.024)
Less than €1,000	0.048
	(0.042)
From €1,000 to €1,500	0.023
	(0.029)
From €1,500 to €2,000	0.003
	(0.024)
From €3,000 to €5,000	−0.037
	(0.023)
From €5,000 to €10,000	−0.051
	(0.037)
Over €10,000	−0.067
	(0.051)
Middle School (or lower)	0.025
	(0.029)
University	−0.058***
	(0.018)
PhD	−0.087**
	(0.039)
Worker (blue collar)	−0.001
	(0.031)
Self-employed	−0.063**
	(0.026)
Student	−0.085**
	(0.035)
Homemaker	−0.072**
	(0.032)
Retired	−0.081***
	(0.028)
Looking for a job	−0.096***
	(0.037)
Other employment status	−0.099*

				(0.060)
Yes, I have already purchased them				0.615***
				(0.022)
Yes, but I have never purchased them				0.511***
				(0.021)
Yes, but I would not repurchase them				0.444***
				(0.029)
North-East				0.085***
				(0.023)
Center				0.122***
				(0.023)
South				0.087***
				(0.022)
Islands				0.036
				(0.027)
Constant	0.769***	0.592***	0.508***	0.118
	(0.021)	(0.025)	(0.029)	(0.075)
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. Self-image regressors are dummy variables for each category of the four-point scale (−−, −, +, ++) for each dimension (*Financial*, *Social*, *Environmental*, and *Disgust*). Baseline: modal category within each dimension. *Excellent condition* and *Good condition* are dummy variables equal to 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Affordable price*. *Smartphone*, *Fridge*, and *Closet* are dummy variables equal to 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Social image and probability to take the vignette product (image dummies)

	(1)	(2)	(3)	(4)
Financial (−−)	−0.028	−0.030	−0.030	0.008
	(0.032)	(0.032)	(0.032)	(0.030)
Financial (−)	0.015	0.011	0.011	0.022
	(0.023)	(0.023)	(0.023)	(0.022)
Financial (++)	0.126***	0.117***	0.117***	0.124***
	(0.026)	(0.026)	(0.026)	(0.026)
Social (−−)	−0.252***	−0.258***	−0.251***	−0.157***
	(0.038)	(0.038)	(0.038)	(0.036)
Social (−)	−0.067***	−0.068***	−0.067***	−0.035
	(0.023)	(0.023)	(0.023)	(0.022)
Social (++)	0.019	0.022	0.022	0.022
	(0.028)	(0.028)	(0.028)	(0.027)
Environmental (−−)	−0.027	−0.030	−0.035	−0.038
	(0.041)	(0.041)	(0.041)	(0.039)
Environmental (−)	−0.044*	−0.033	−0.036	−0.019
	(0.025)	(0.025)	(0.025)	(0.024)
Environmental (++)	0.148***	0.146***	0.144***	0.100***
	(0.023)	(0.023)	(0.023)	(0.022)
Disgust (−−)	−0.397***	−0.400***	−0.393***	−0.247***
	(0.038)	(0.038)	(0.038)	(0.036)
Disgust (−)	−0.150***	−0.158***	−0.153***	−0.089***
	(0.024)	(0.024)	(0.024)	(0.023)
Disgust (++)	0.122***	0.121***	0.121***	0.074***
	(0.024)	(0.024)	(0.024)	(0.024)
Excellent		0.084***	0.086***	0.097***
		(0.020)	(0.020)	(0.019)
Good		0.094***	0.091***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.232***	0.233***	0.245***
		(0.016)	(0.016)	(0.015)
Smartphone			0.097***	0.082***
			(0.023)	(0.022)
Fridge			0.093***	0.164***
			(0.023)	(0.023)
Closet			0.120***	0.142***
			(0.023)	(0.022)
Age				−0.004***
				(0.001)

Man	0.062***
	(0.016)
Need for Uniqueness	−0.027**
	(0.011)
Pro-Environmental Behavior	0.087***
	(0.012)
Don't know/Prefer not to answer	−0.053**
	(0.025)
Less than €1,000	0.025
	(0.043)
From €1,000 to €1,500	0.024
	(0.029)
From €1,500 to €2,000	−0.008
	(0.025)
From €3,000 to €5,000	−0.022
	(0.024)
From €5,000 to €10,000	−0.063
	(0.038)
Over €10,000	−0.072
	(0.051)
Middle School (or lower)	0.017
	(0.029)
University	−0.054***
	(0.018)
PhD	−0.114***
	(0.039)
Worker (blue collar)	0.009
	(0.031)
Self-employed	−0.046*
	(0.027)
Student	−0.057
	(0.036)
Homemaker	−0.054*
	(0.032)
Retired	−0.079***
	(0.029)
Looking for a job	−0.091**
	(0.037)
Other employment status	−0.044
	(0.062)
Yes, I have already purchased them	0.728***

				(0.022)
Yes, but I have never purchased them				0.595***
				(0.021)
Yes, but I would not repurchase them				0.436***
				(0.029)
North-East				0.087***
				(0.023)
Center				0.108***
				(0.023)
South				0.077***
				(0.022)
Islands				0.038
				(0.027)
Constant	0.228***	0.041*	−0.039	−0.557***
	(0.016)	(0.022)	(0.027)	(0.072)
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. Social image regressors are dummy variables for each category of the four-point scale (−−, −, +, ++) for each dimension (*Financial*, *Social*, *Environmental*, and *Disgust*). Baseline: modal category within each dimension. *Excellent condition* and *Good condition* are dummy variables equal to 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Affordable price*. *Smartphone*, *Fridge*, and *Closet* are dummy variables equal to 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10: Second-order self-image and probability to take the vignette product (image dummies)

	(1)	(2)	(3)	(4)
Financial (−−)	−0.086***	−0.091***	−0.093***	−0.032
	(0.033)	(0.033)	(0.033)	(0.032)
Financial (−)	−0.019	−0.024	−0.025	−0.009
	(0.024)	(0.023)	(0.023)	(0.023)
Financial (++)	0.157***	0.148***	0.146***	0.117***
	(0.026)	(0.026)	(0.026)	(0.026)
Social (−−)	−0.168***	−0.173***	−0.166***	−0.118***
	(0.036)	(0.036)	(0.036)	(0.034)
Social (−)	−0.009	−0.013	−0.010	0.009
	(0.022)	(0.022)	(0.022)	(0.021)
Social (++)	0.022	0.020	0.020	0.049*
	(0.027)	(0.027)	(0.027)	(0.027)
Environmental (−−)	−0.065*	−0.070*	−0.069*	−0.030
	(0.039)	(0.039)	(0.039)	(0.038)
Environmental (−)	−0.028	−0.025	−0.027	0.005
	(0.024)	(0.024)	(0.024)	(0.023)
Environmental (++)	0.175***	0.172***	0.175***	0.144***
	(0.023)	(0.023)	(0.023)	(0.022)
Disgust (−−)	−0.428***	−0.427***	−0.423***	−0.287***
	(0.039)	(0.038)	(0.038)	(0.036)
Disgust (−)	−0.127***	−0.129***	−0.127***	−0.075***
	(0.024)	(0.024)	(0.024)	(0.022)
Disgust (++)	0.051**	0.049**	0.046*	0.011
	(0.025)	(0.025)	(0.025)	(0.024)
Excellent		0.088***	0.090***	0.101***
		(0.020)	(0.020)	(0.019)
Good		0.094***	0.091***	0.080***
		(0.021)	(0.021)	(0.020)
Free		0.224***	0.225***	0.240***
		(0.016)	(0.016)	(0.015)
Smartphone			0.105***	0.087***
			(0.023)	(0.022)
Fridge			0.099***	0.170***
			(0.023)	(0.022)
Closet			0.127***	0.147***
			(0.023)	(0.022)
Age				−0.004***
				(0.001)

Man	0.067***
	(0.016)
Need for Uniqueness	−0.029***
	(0.011)
Pro-Environmental Behavior	0.090***
	(0.012)
Don't know/Prefer not to answer	−0.048*
	(0.025)
Less than €1,000	0.035
	(0.043)
From €1,000 to €1,500	0.028
	(0.029)
From €1,500 to €2,000	−0.003
	(0.025)
From €3,000 to €5,000	−0.029
	(0.024)
From €5,000 to €10,000	−0.064*
	(0.038)
Over €10,000	−0.069
	(0.051)
Middle School (or lower)	0.025
	(0.029)
University	−0.053***
	(0.018)
PhD	−0.110***
	(0.039)
Worker (blue collar)	0.007
	(0.031)
Self-employed	−0.046*
	(0.027)
Student	−0.058
	(0.036)
Homemaker	−0.053
	(0.032)
Retired	−0.071**
	(0.029)
Looking for a job	−0.094**
	(0.037)
Other employment status	−0.068
	(0.062)
Yes, I have already purchased them	0.754***

				(0.022)
Yes, but I have never purchased them				0.609***
				(0.020)
Yes, but I would not repurchase them				0.437***
				(0.029)
North-East				0.090***
				(0.023)
Center				0.116***
				(0.023)
South				0.068***
				(0.022)
Islands				0.034
				(0.028)
Constant	0.219***	0.038*	−0.047*	−0.591***
	(0.016)	(0.022)	(0.027)	(0.072)
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. Second-order self-image regressors are dummy variables for each category of the four-point scale (−−, −, +, ++) for each dimension (*Financial*, *Social*, *Environmental*, and *Disgust*). Baseline: modal category within each dimension. *Excellent condition* and *Good condition* are dummy variables equal to 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Affordable price*. *Smartphone*, *Fridge*, and *Closet* are dummy variables equal to 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.1.5 Interactions with vignette attributes and fixed effects

Table B.11: Vignette attributes and probability to take the product (with interactions)

	(1)	(2)	(3)	(4)	(5)
Excellent condition	0.125***	0.089**		0.120**	0.116**
	(0.028)	(0.042)		(0.060)	(0.057)
Good condition	0.122***	0.128***		0.130*	0.092
	(0.030)	(0.047)		(0.067)	(0.063)
Free	0.248***		0.238***	0.255***	0.274***
	(0.031)		(0.034)	(0.066)	(0.063)
Smartphone		0.165***	0.097***	0.122**	0.082
		(0.044)	(0.033)	(0.061)	(0.060)
Fridge		0.094**	0.146***	0.137**	0.216***
		(0.046)	(0.034)	(0.065)	(0.062)
Closet		0.149***	0.212***	0.204***	0.245***
		(0.046)	(0.033)	(0.066)	(0.065)
Excellent condition × Free	−0.042			−0.037	−0.026
	(0.040)			(0.083)	(0.078)
Good condition × Free	−0.011			0.010	0.026
	(0.043)			(0.093)	(0.087)
Excellent condition × Smartphone		0.005		−0.018	0.001
		(0.057)		(0.080)	(0.077)
Good condition × Smartphone		−0.036		−0.036	−0.038
		(0.063)		(0.088)	(0.084)
Excellent condition × Fridge		0.028		0.009	0.003
		(0.058)		(0.082)	(0.077)
Good condition × Fridge		0.012		0.030	0.071
		(0.064)		(0.092)	(0.088)
Excellent condition × Closet		0.039		0.046	0.019
		(0.058)		(0.083)	(0.079)
Good condition × Closet		−0.013		−0.032	−0.051
		(0.062)		(0.089)	(0.085)
Free × Smartphone			0.103**	0.104	0.089
			(0.047)	(0.087)	(0.084)
Free × Fridge			−0.061	−0.056	−0.047
			(0.047)	(0.091)	(0.087)
Free × Closet			−0.082*	−0.081	−0.143
			(0.046)	(0.092)	(0.088)
Excellent condition × Free × Smartphone				0.003	0.007
				(0.113)	(0.108)
Excellent condition × Free × Fridge				0.011	−0.002
				(0.115)	(0.108)
Excellent condition × Free × Closet				−0.030	0.022
				(0.116)	(0.110)
Good condition × Free × Smartphone				−0.032	−0.001
				(0.124)	(0.119)
Good condition × Free × Fridge				−0.058	−0.113
				(0.128)	(0.121)
Good condition × Free × Closet				0.014	0.057
				(0.123)	(0.117)
Constant	0.011	0.042	−0.014	−0.107**	−0.807***
	(0.022)	(0.033)	(0.025)	(0.048)	(0.081)
Controls	No	No	No	No	Yes
Observations	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Excellent condition* and *Good condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Fridge, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Controls* include demographic variables such as age, gender, need for uniqueness score, pro-environmental behavior, income, education, past adoption of and disposition toward second-hand, and macro-region of residence. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: Self-image and probability to take the vignette product (with interactions)

	(1)	(2)	(3)	(4)	(5)
Financial savvy	0.366***				0.069
	(0.036)				(0.045)
Social approval		0.520***			0.209***
		(0.039)			(0.052)
Environmental consciousness			0.319***		−0.083*
			(0.039)		(0.047)
Disgust insensitivity				0.592***	0.488***
				(0.037)	(0.047)
Excellent condition	0.082***	0.082***	0.100***	0.083***	0.076***
	(0.021)	(0.020)	(0.024)	(0.022)	(0.025)
Good condition	0.079***	0.091***	0.103***	0.105***	0.083***
	(0.023)	(0.022)	(0.026)	(0.024)	(0.027)
Free	0.191***	0.221***	0.183***	0.188***	0.170***
	(0.017)	(0.017)	(0.019)	(0.018)	(0.020)
Smartphone	0.152***	0.127***	0.143***	0.125***	0.112***
	(0.025)	(0.024)	(0.028)	(0.027)	(0.029)
Fridge	0.119***	0.107***	0.113***	0.115***	0.099***
	(0.025)	(0.024)	(0.028)	(0.025)	(0.029)
Closet	0.178***	0.149***	0.168***	0.172***	0.155***
	(0.025)	(0.024)	(0.028)	(0.025)	(0.028)
Excellent condition × Financial savvy	0.048				0.042
	(0.031)				(0.040)
Good condition × Financial savvy	0.025				0.035
	(0.033)				(0.042)
Free × Financial savvy	0.110***				0.108***
	(0.025)				(0.033)
Smartphone × Financial savvy	−0.021				0.004
	(0.036)				(0.048)
Fridge × Financial savvy	−0.034				−0.053
	(0.036)				(0.047)
Closet × Financial savvy	−0.070**				−0.062
	(0.035)				(0.046)
Excellent condition × Social approval		0.023			−0.022
		(0.034)			(0.044)
Good condition × Social approval		−0.012			−0.004
		(0.037)			(0.047)
Free × Social approval		0.033			−0.086**
		(0.028)			(0.036)

Smartphone × Social approval		−0.072*			0.010
		(0.040)			(0.052)
Fridge × Social approval		−0.055			−0.009
		(0.040)			(0.052)
Closet × Social approval		−0.088**			0.002
		(0.040)			(0.052)
Excellent condition × Environmental consc.			0.012		0.002
			(0.034)		(0.041)
Good condition × Environmental consc.			0.007		0.051
			(0.036)		(0.043)
Free × Environmental consc.			0.096***		0.043
			(0.027)		(0.033)
Smartphone × Environmental consc.			0.024		0.090*
			(0.039)		(0.049)
Fridge × Environmental consc.			0.009		0.084*
			(0.039)		(0.048)
Closet × Environmental consc.			−0.011		0.090*
			(0.039)		(0.047)
Excellent condition × Disgust insensitivity				0.011	−0.001
				(0.032)	(0.041)
Good condition × Disgust insensitivity				−0.061*	−0.099**
				(0.035)	(0.044)
Free × Disgust insensitivity				0.106***	0.069**
				(0.026)	(0.033)
Smartphone × Disgust insensitivity				−0.121***	−0.183***
				(0.038)	(0.049)
Fridge × Disgust insensitivity				−0.054	−0.065
				(0.037)	(0.047)
Closet × Disgust insensitivity				−0.114***	−0.133***
				(0.037)	(0.048)
Constant	−0.158***	−0.133***	−0.206***	−0.228***	−0.209***
	(0.024)	(0.023)	(0.028)	(0.025)	(0.028)
Observations	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking a value in $\{-1, -0.33, +0.33, +1\}$. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Social image and probability to take the vignette product (with interactions)

	(1)	(2)	(3)	(4)	(5)
Financial savvy	0.308***				0.078
	(0.035)				(0.050)
Social approval		0.363***			0.095
		(0.039)			(0.059)
Environmental consciousness			0.294***		0.010
			(0.041)		(0.052)
Disgust insensitivity				0.411***	0.311***
				(0.037)	(0.050)
Excellent	0.092***	0.096***	0.090***	0.086***	0.081***
	(0.020)	(0.020)	(0.023)	(0.022)	(0.024)
Good	0.100***	0.102***	0.101***	0.106***	0.091***
	(0.022)	(0.022)	(0.025)	(0.023)	(0.025)
Free	0.227***	0.230***	0.189***	0.223***	0.187***
	(0.016)	(0.017)	(0.019)	(0.018)	(0.019)
Smartphone	0.126***	0.107***	0.129***	0.095***	0.099***
	(0.024)	(0.024)	(0.027)	(0.026)	(0.028)
Fridge	0.101***	0.088***	0.097***	0.097***	0.104***
	(0.024)	(0.024)	(0.028)	(0.025)	(0.028)
Closet	0.146***	0.137***	0.153***	0.134***	0.136***
	(0.024)	(0.024)	(0.027)	(0.025)	(0.027)
Excellent × Financial savvy	−0.013				−0.018
	(0.030)				(0.043)
Good × Financial savvy	−0.028				−0.009
	(0.032)				(0.045)
Free × Financial savvy	0.016				−0.025
	(0.024)				(0.035)
Smartphone × Financial savvy	0.024				0.003
	(0.035)				(0.050)
Fridge × Financial savvy	0.035				−0.004
	(0.035)				(0.051)
Closet × Financial savvy	−0.002				0.028
	(0.034)				(0.049)
Excellent × Social approval		0.001			0.008
		(0.034)			(0.050)
Good × Social approval		−0.021			0.009
		(0.037)			(0.053)
Free × Social approval		0.022			−0.036
		(0.028)			(0.041)

Smartphone × Social approval		0.044			0.073
		(0.039)			(0.059)
Fridge × Social approval		0.063			0.120**
		(0.039)			(0.060)
Closet × Social approval		−0.007			0.004
		(0.040)			(0.059)
Excellent × Environmental consciousness			0.014		0.020
			(0.035)		(0.045)
Good × Environmental consciousness			0.006		0.049
			(0.037)		(0.049)
Free × Environmental consciousness			0.097***		0.135***
			(0.028)		(0.037)
Smartphone × Environmental consciousness			0.033		−0.009
			(0.041)		(0.053)
Fridge × Environmental consciousness			0.019		−0.022
			(0.041)		(0.054)
Closet × Environmental consciousness			0.002		0.010
			(0.041)		(0.052)
Excellent × Disgust insensitivity				−0.005	−0.017
				(0.032)	(0.044)
Good × Disgust insensitivity				−0.051	−0.075
				(0.035)	(0.047)
Free × Disgust insensitivity				0.051*	0.022
				(0.026)	(0.036)
Smartphone × Disgust insensitivity				0.012	−0.038
				(0.038)	(0.052)
Fridge × Disgust insensitivity				−0.010	−0.071
				(0.037)	(0.051)
Closet × Disgust insensitivity				−0.032	−0.072
				(0.037)	(0.051)
Constant	−0.104***	−0.111***	−0.180***	−0.153***	−0.152***
	(0.023)	(0.024)	(0.027)	(0.025)	(0.027)
Observations	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Social image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking a value in $\{-1, -0.33, +0.33, +1\}$. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.14: Second-order self-image and probability to take the vignette product (with interactions)

	(1)	(2)	(3)	(4)	(5)
Financial savvy	0.275***				0.005
	(0.036)				(0.049)
Social approval		0.375***			0.163***
		(0.039)			(0.055)
Environmental consciousness			0.282***		0.057
			(0.040)		(0.050)
Disgust insensitivity				0.408***	0.281***
				(0.038)	(0.050)
Excellent	0.096***	0.105***	0.101***	0.095***	0.088***
	(0.020)	(0.020)	(0.023)	(0.021)	(0.024)
Good	0.097***	0.109***	0.110***	0.101***	0.090***
	(0.022)	(0.022)	(0.025)	(0.023)	(0.025)
Free	0.214***	0.221***	0.183***	0.214***	0.190***
	(0.017)	(0.016)	(0.019)	(0.017)	(0.019)
Smartphone	0.118***	0.117***	0.124***	0.108***	0.095***
	(0.024)	(0.024)	(0.027)	(0.025)	(0.028)
Fridge	0.101***	0.091***	0.110***	0.106***	0.105***
	(0.024)	(0.024)	(0.027)	(0.025)	(0.028)
Closet	0.148***	0.144***	0.163***	0.147***	0.137***
	(0.024)	(0.023)	(0.027)	(0.025)	(0.027)
Excellent × Financial savvy	−0.011				0.016
	(0.031)				(0.043)
Good × Financial savvy	−0.015				0.004
	(0.033)				(0.045)
Free × Financial savvy	0.070***				0.030
	(0.025)				(0.034)
Smartphone × Financial savvy	0.062*				0.141***
	(0.037)				(0.049)
Fridge × Financial savvy	0.023				0.084*
	(0.036)				(0.050)
Closet × Financial savvy	−0.006				0.092*
	(0.036)				(0.049)
Excellent × Social approval		−0.033			−0.039
		(0.033)			(0.048)
Good × Social approval		−0.049			−0.049
		(0.036)			(0.050)
Free × Social approval		0.068**			0.015

		(0.027)			(0.038)
Smartphone × Social approval		−0.030			−0.081
		(0.039)			(0.055)
Fridge × Social approval		−0.023			−0.055
		(0.040)			(0.057)
Closet × Social approval		−0.104***			−0.129**
		(0.039)			(0.054)
Excellent × Environmental consciousness			0.002		0.024
			(0.035)		(0.044)
Good × Environmental consciousness			−0.002		0.034
			(0.037)		(0.046)
Free × Environmental consciousness			0.109***		0.084**
			(0.028)		(0.035)
Smartphone × Environmental consciousness			0.052		0.049
			(0.040)		(0.051)
Fridge × Environmental consciousness			−0.012		−0.036
			(0.041)		(0.050)
Closet × Environmental consciousness			−0.018		0.006
			(0.040)		(0.049)
Excellent × Disgust insensitivity				−0.036	−0.033
				(0.033)	(0.043)
Good × Disgust insensitivity				−0.040	−0.030
				(0.035)	(0.047)
Free × Disgust insensitivity				0.064**	−0.004
				(0.027)	(0.036)
Smartphone × Disgust insensitivity				−0.042	−0.113**
				(0.039)	(0.052)
Fridge × Disgust insensitivity				−0.028	−0.026
				(0.038)	(0.051)
Closet × Disgust insensitivity				−0.069*	−0.059
				(0.038)	(0.051)
Constant	−0.113***	−0.098***	−0.181***	−0.144***	−0.150***
	(0.024)	(0.023)	(0.027)	(0.024)	(0.027)
Observations	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Second-order self-image: Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity*: discrete variables taking a value in {−1, −0.33, +0.33, +1}. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.15: Probability to take the product by perspective (with vignette fixed effects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-image	Self-image	Social image	Social image	Second-order	Second-order
Financial savvy	0.047***	0.043***	0.022***	0.015***	0.040***	0.023***
	(0.007)	(0.006)	(0.004)	(0.005)	(0.007)	(0.006)
Social approval	0.055***	0.038***	0.047***	0.029***	0.028***	0.022***
	(0.007)	(0.006)	(0.009)	(0.008)	(0.009)	(0.007)
Environmental consc.	0.007	0.006	0.039***	0.028***	0.048***	0.032***
	(0.007)	(0.006)	(0.009)	(0.007)	(0.007)	(0.007)
Disgust insensitivity	0.152***	0.106***	0.095***	0.056***	0.080***	0.047***
	(0.009)	(0.007)	(0.005)	(0.005)	(0.008)	(0.007)
Age		−0.001***		−0.001***		−0.001***
		(0.000)		(0.000)		(0.000)
Man		0.021***		0.022***		0.024***
		(0.006)		(0.006)		(0.006)
Need for Uniqueness		−0.015***		−0.010**		−0.011**
		(0.004)		(0.004)		(0.004)
Pro-Environmental Beh.		0.023***		0.031***		0.032***
		(0.004)		(0.004)		(0.004)
Don't know/No answer		−0.023**		−0.018*		−0.014
		(0.009)		(0.009)		(0.009)
Less than €1,000		0.016		0.008		0.011
		(0.013)		(0.014)		(0.014)
From €1,000 to €1,500		0.011		0.009		0.011
		(0.009)		(0.009)		(0.009)
From €1,500 to €2,000		0.002		−0.002		−0.000
		(0.008)		(0.008)		(0.008)
From €3,000 to €5,000		−0.011		−0.007		−0.008
		(0.009)		(0.009)		(0.010)
From €5,000 to €10,000		−0.016		−0.019		−0.019
		(0.014)		(0.015)		(0.015)
Over €10,000		−0.020		−0.024		−0.022
		(0.017)		(0.017)		(0.017)
Middle School (or lower)		0.012		0.008		0.010
		(0.013)		(0.015)		(0.015)
University		−0.019***		−0.018***		−0.018***
		(0.006)		(0.005)		(0.006)
PhD		−0.026*		−0.039**		−0.038**
		(0.014)		(0.015)		(0.015)
Worker (blue collar)		0.000		0.003		0.000

		(0.009)		(0.009)		(0.009)
Self-employed		−0.023***		−0.017**		−0.018**
		(0.008)		(0.008)		(0.008)
Student		−0.028**		−0.018		−0.019
		(0.012)		(0.014)		(0.013)
Homemaker		−0.026**		−0.019*		−0.020*
		(0.011)		(0.011)		(0.011)
Retired		−0.032***		−0.029***		−0.027**
		(0.009)		(0.010)		(0.010)
Looking for a job		−0.032**		−0.033**		−0.034**
		(0.015)		(0.014)		(0.014)
Other employment status		−0.034		−0.016		−0.024
		(0.022)		(0.024)		(0.024)
Already purchased		0.226***		0.271***		0.281***
		(0.010)		(0.014)		(0.013)
Never purchased		0.191***		0.227***		0.233***
		(0.010)		(0.012)		(0.011)
Would not repurchase		0.168***		0.168***		0.171***
		(0.012)		(0.011)		(0.011)
North-East		0.030***		0.030***		0.031***
		(0.006)		(0.006)		(0.006)
Center		0.041***		0.038***		0.040***
		(0.008)		(0.009)		(0.009)
South		0.030***		0.027***		0.024***
		(0.007)		(0.008)		(0.008)
Islands		0.012		0.013		0.010
		(0.010)		(0.010)		(0.010)
Constant	0.512***	0.423***	0.538***	0.383***	0.539***	0.375***
	(0.003)	(0.023)	(0.003)	(0.025)	(0.002)	(0.024)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,496	10,496	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation with vignette fixed effects. Models (1) and (2) use respondents' *self-image* evaluations on each image dimension as the dependent variable; Models (3) and (4) use *social image* evaluations; and Models (5) and (6) use *second-order self-image* evaluations. Dependent variable. *Probability of taking the product*, ranging between 0 and 1. Regressors. *Financial savvy*, *Social approval*, *Environmental consciousness*, and *Disgust insensitivity*: discrete variables taking a value in $\{-1, -0.33, +0.33, +1\}$. *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. *Fixed effects* indicate vignette fixed effects. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.1.6 Mismatch and misalignments across image perspectives

Table B.16: Relative weights of image dimensions across self and social image

	(1)	(2)	(3)	(4)
Financial savvy (self)	0.356***			
	(0.015)			
Financial savvy (social)	0.137***			
	(0.014)			
Environmental consciousness (self)		0.280***		
		(0.016)		
Environmental consciousness (social)		0.206***		
		(0.017)		
Social approval (self)			0.402***	
			(0.017)	
Social approval (social)			0.161***	
			(0.017)	
Disgust insensitivity (self)				0.503***
				(0.016)
Disgust insensitivity (social)				0.115***
				(0.016)
Constant	0.118***	0.038***	0.138***	0.034***
	(0.009)	(0.010)	(0.008)	(0.009)
Observations	10,496	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. Each column reports a separate model including the *self-image* and the corresponding *social image* score for the indicated dimension (Financial savvy; Environmental consciousness; Social approval; Disgust insensitivity). These scores are discrete variables taking values in $\{-1, -0.33, +0.33, +1\}$. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.17: Image scores per macro-region under self-image versus social image

Determinant	Self-image −−	Self-image −	Self-image +	Self-image ++	Social image −−	Social image −	Social image +	Social image ++	Sign test p-value
Panel A: North West									
Financial	10.93	20.16	33.64	**35.27**	15.53	24.62	**32.54**	27.31	< .001
Social	10.36	24.44	**43.16**	22.04	10.47	28.69	**39.30**	21.54	< .001
Environmental	7.50	14.29	35.37	**42.84**	6.01	14.93	**42.70**	36.36	< .001
Hygiene	9.13	19.56	34.49	**36.82**	10.22	22.14	**37.32**	30.31	< .001
Panel B: North East									
Financial	12.44	19.05	32.06	**36.45**	16.06	25.84	**30.95**	27.15	< .001
Social	10.41	24.20	**42.14**	23.24	10.66	27.00	**40.60**	21.75	.004
Environmental	7.76	15.00	33.61	**43.64**	6.27	15.00	**42.14**	36.60	< .001
Hygiene	9.35	19.00	30.52	**41.13**	10.46	20.97	**34.91**	33.65	< .001
Panel C: Center									
Financial	10.53	20.78	33.50	**35.19**	12.23	24.66	**33.20**	29.90	< .001
Social	10.05	24.37	**42.14**	23.45	9.61	25.10	**43.06**	22.23	.149
Environmental	7.14	15.49	33.40	**43.98**	5.58	14.95	39.22	**40.24**	.037
Hygiene	8.69	18.50	31.99	**40.83**	8.20	19.13	**38.40**	34.27	< .001
Panel D: South									
Financial	13.56	20.42	**33.22**	32.80	16.49	25.19	**30.59**	27.74	< .001
Social	11.26	26.32	**40.29**	22.13	11.97	27.11	**39.50**	21.42	.017
Environmental	8.54	17.20	33.35	**40.92**	7.41	17.49	**39.87**	35.23	< .001
Hygiene	9.67	20.21	32.80	**37.32**	10.96	22.64	**34.52**	31.88	< .001
Panel E: Islands									
Financial	13.01	21.31	30.13	**35.55**	17.73	22.18	29.17	**30.92**	< .001
Social	11.27	23.76	**38.60**	26.38	11.97	25.76	**37.82**	24.45	.100
Environmental	8.91	16.94	32.66	**41.48**	6.90	17.47	**39.56**	36.07	.100
Hygiene	9.69	19.39	28.56	**42.36**	11.62	20.35	33.45	**34.59**	< .001

Note: Distribution of scores on the four image dimensions per macro-region. Columns (−−, −, +, ++) indicate the proportion of respondents, respectively, completely agreeing with A, agreeing with A more than B, agreeing with B more than A, and completely agreeing with B. Modal scores are highlighted in bold. The last column reports Šidák–Holm adjusted p-values from sign tests.

Table B.18: Image scores per macro-region under self-image versus second-order self-image

	Self-image				Second-order self-image				Sign test
Determinant	−−	−	+	++	−−	−	+	++	p-value
Panel A: North West									
Financial	10.93	20.16	33.64	**35.27**	11.96	25.33	**34.56**	28.16	$< .001$
Social	10.36	24.44	**43.16**	22.04	11.35	30.60	**39.55**	18.50	$< .001$
Environmental	7.50	14.29	35.37	**42.84**	6.65	14.96	**41.85**	36.54	$< .001$
Hygiene	9.13	19.56	34.49	**36.82**	9.52	23.81	**38.70**	27.98	$< .001$
Panel B: North East									
	Self-image				Second-order self-image				Sign test
Determinant	−−	−	+	++	−−	−	+	++	p-value
Financial	12.44	19.05	32.06	**36.45**	12.92	25.89	**32.59**	28.59	$< .001$
Social	10.41	24.20	**42.14**	23.24	11.19	30.09	**37.56**	21.17	$< .001$
Environmental	7.76	15.00	33.61	**43.64**	6.70	16.35	**40.74**	36.21	$< .001$
Hygiene	9.35	19.00	30.52	**41.13**	10.46	23.48	**36.26**	29.80	$< .001$
Panel C: Center									
	Self-image				Second-order self-image				Sign test
Determinant	−−	−	+	++	−−	−	+	++	p-value
Financial	10.53	20.78	33.50	**35.19**	11.31	24.51	**34.76**	29.42	$< .001$
Social	10.05	24.37	**42.14**	23.45	9.90	30.68	**38.16**	21.26	$< .001$
Environmental	7.14	15.49	33.40	**43.98**	5.63	15.58	**41.70**	37.09	$< .001$
Hygiene	8.69	18.50	31.99	**40.83**	7.96	22.91	**36.46**	32.67	$< .001$
Panel D: South									
	Self-image				Second-order self-image				Sign test
Determinant	−−	−	+	++	−−	−	+	++	p-value
Financial	13.56	20.42	**33.22**	32.80	13.51	24.18	**32.72**	29.58	$< .001$
Social	11.26	26.32	**40.29**	22.13	12.47	28.41	**38.24**	20.88	.001
Environmental	8.54	17.20	33.35	**40.92**	6.86	17.70	**37.95**	37.49	.024
Hygiene	9.67	20.21	32.80	**37.32**	9.58	23.14	**36.78**	30.50	$< .001$
Panel E: Islands									
	Self-image				Second-order self-image				Sign test
Determinant	−−	−	+	++	−−	−	+	++	p-value
Financial	13.01	21.31	30.13	**35.55**	14.50	22.36	29.17	**33.97**	.013
Social	11.27	23.76	**38.60**	26.38	13.01	24.98	**38.08**	23.93	.013
Environmental	8.91	16.94	32.66	**41.48**	6.55	18.17	**39.30**	35.98	.013
Hygiene	9.69	19.39	28.56	**42.36**	10.92	20.87	31.62	**36.59**	.001

Note: Distribution of scores on the four image dimensions per macro-region across first-order (self-image) and second-order self-image. Columns (−−, −, +, ++) indicate the proportion of respondents, respectively, completely agreeing with A, agreeing with A more than B, agreeing with B more than A, and completely agreeing with B. Modal scores are highlighted in bold. The last column reports Šidák–Holm adjusted p-values from sign tests.

Table B.19: Mismatch between second-order self-image and modes of self-image

Macro-region	Dimension	Mismatch	Below	Above
North West (N = 2,827)	Financial	67.89	84.37	15.63
	Social	60.06	66.43	33.57
	Environmental	61.19	90.89	9.11
	Hygiene	65.33	83.98	16.02
North East (N = 2,074)	Financial	68.47	87.09	12.91
	Social	62.73	66.33	33.67
	Environmental	61.44	92.43	7.57
	Hygiene	67.91	96.48	3.52
Center (N = 2,060)	Financial	67.18	86.71	13.29
	Social	61.65	61.97	38.03
	Environmental	61.35	91.08	8.92
	Hygiene	65.53	90.59	9.41
South (N = 2,390)	Financial	65.64	77.53	22.47
	Social	61.76	66.19	33.81
	Environmental	60.29	87.79	12.21
	Hygiene	63.31	85.99	14.01
Islands (N = 1,145)	Financial	64.62	86.07	13.93
	Social	61.91	65.49	34.51
	Environmental	61.63	88.77	11.23
	Hygiene	62.10	94.37	5.63

Note: "Mismatch" reports the proportion of respondents whose second-order self-image response does not coincide with the modal self-image response observed within their reference group (macro-region) for the corresponding vignette. "Below" and "Above" identify respondents for whom their second-order response falls, respectively, below and above the modal self-image.

Table B.20: Mismatch between second-order self-image and self-image mode on adoption

	(1)	(2)	(3)
Financial below	−0.057**	−0.085***	−0.055**
	(0.023)	(0.022)	(0.022)
Financial above	−0.008	0.063*	0.038
	(0.032)	(0.032)	(0.032)
Social below	−0.168***	−0.165***	−0.092***
	(0.020)	(0.020)	(0.019)
Social above	0.136***	0.095***	0.115***
	(0.026)	(0.026)	(0.025)
Environmental below	−0.133***	−0.139***	−0.089***
	(0.022)	(0.021)	(0.021)
Environmental above	−0.116***	−0.035	−0.030
	(0.039)	(0.040)	(0.038)
Disgust below	−0.143***	−0.137***	−0.078***
	(0.021)	(0.021)	(0.021)
Disgust above	−0.046	−0.002	−0.030
	(0.037)	(0.037)	(0.037)
Excellent condition		0.133***	0.131***
		(0.021)	(0.020)
Good condition		0.148***	0.117***
		(0.023)	(0.022)
Free		0.272***	0.267***
		(0.018)	(0.017)
Smartphone		0.158***	0.117***
		(0.025)	(0.024)
Fridge		0.103***	0.175***
		(0.024)	(0.023)
Closet		0.170***	0.175***
		(0.024)	(0.024)
Age			−0.004***
			(0.001)
Man			0.068***
			(0.017)
Need for Uniqueness			−0.022*
			(0.011)
Pro-Environmental Behavior			0.094***
			(0.012)
Don't know/Prefer not to answer			−0.030
			(0.026)

Less than €1,000	0.026
	(0.046)
From €1,000 to €1,500	0.029
	(0.031)
From €1,500 to €2,000	−0.002
	(0.026)
From €3,000 to €5,000	−0.028
	(0.025)
From €5,000 to €10,000	−0.077*
	(0.041)
Over €10,000	−0.095*
	(0.053)
Middle School (or lower)	0.025
	(0.031)
University	−0.041**
	(0.019)
PhD	−0.132***
	(0.043)
Worker (blue collar)	0.001
	(0.032)
Self-employed	−0.036
	(0.028)
Student	−0.064*
	(0.039)
Homemaker	−0.075**
	(0.034)
Retired	−0.068**
	(0.030)
Looking for a job	−0.097**
	(0.039)
Other employment status	−0.014
	(0.069)
Yes, I have already purchased them	0.792***
	(0.023)
Yes, but I have never purchased them	0.647***
	(0.021)
Yes, but I would not repurchase them	0.461***
	(0.031)
North-East	0.097***
	(0.026)
Central	0.115***

			(0.025)
South			0.060**
			(0.024)
Islands			0.030
			(0.030)
Constant	0.473***	0.124***	−0.531***
	(0.022)	(0.031)	(0.078)
Observations	9,397	9,397	9,397

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below*; *Environmental above/below*; *Social above/below*; *Disgust above/below*: dummy variables equal 1 when the second-order self-image score on the dimension is, respectively, above/below the modal self-image score; the reference category is no mismatch between second-order self-image and the modal self-image score. *Excellent condition* and *Good condition* dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*). *Free* dummy variable equals 1 if the product can be taken for free, 0 otherwise. Baseline: *Cheap Price*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.21: Misalignment between self-image and social image

Macro-region	Dimension	Misalignment	Below	Above
North West ($N = 2,827$)	Financial	44.39	34.82	65.18
	Social	37.74	44.61	55.39
	Environmental	34.98	42.06	57.94
	Hygiene	35.44	37.62	62.38
North East ($N = 2,074$)	Financial	43.64	31.71	68.29
	Social	39.34	44.85	55.15
	Environmental	37.27	42.69	57.31
	Hygiene	37.13	39.74	60.26
Center ($N = 2,060$)	Financial	41.41	39.04	60.96
	Social	37.33	47.33	52.67
	Environmental	35.97	45.61	54.39
	Hygiene	34.66	42.58	57.42
South ($N = 2,390$)	Financial	40.25	37.94	62.06
	Social	37.24	45.96	54.04
	Environmental	35.82	42.29	57.71
	Hygiene	36.82	39.89	60.11
Islands ($N = 1,145$)	Financial	41.22	37.50	62.50
	Social	38.78	45.27	54.73
	Environmental	37.38	45.79	54.21
	Hygiene	37.12	37.88	62.12

Note: Proportion of respondents for each macro-region whose answer on the self-image elicitation task did not match the social-image score for the determinant ("Misalignment"). Columns "Below" and "Above" indicate the proportion of misaligned respondents whose self-image is, respectively, lower or higher than their social-image score for the determinant.

Table B.22: Self-image and social image misalignment on adoption

	(1)	(2)	(3)
Financial below	−0.025	−0.023	−0.024
	(0.024)	(0.025)	(0.024)
Financial above	0.053**	0.057***	0.060***
	(0.022)	(0.022)	(0.021)
Social below	−0.032	−0.036	−0.048**
	(0.024)	(0.023)	(0.023)
Social above	0.052**	0.043*	0.030
	(0.023)	(0.023)	(0.022)
Environmental below	−0.111***	−0.114***	−0.088***
	(0.025)	(0.024)	(0.024)
Environmental above	−0.054**	−0.050**	−0.028
	(0.023)	(0.022)	(0.021)
Disgust below	−0.183***	−0.171***	−0.131***
	(0.025)	(0.025)	(0.024)
Disgust above	0.124***	0.124***	0.084***
	(0.023)	(0.022)	(0.022)
Excellent condition		0.109***	0.113***
		(0.020)	(0.019)
Good condition		0.110***	0.089***
		(0.021)	(0.021)
Free		0.220***	0.240***
		(0.016)	(0.015)
Smartphone		0.160***	0.124***
		(0.023)	(0.022)
Fridge		0.114***	0.192***
		(0.023)	(0.023)
Closet		0.171***	0.179***
		(0.023)	(0.022)
Age			−0.003***
			(0.001)
Man			0.082***
			(0.016)
Need for Uniqueness			−0.030***
			(0.011)
Pro-Environmental Behavior			0.114***
			(0.012)
Don't know/Prefer not to answer			−0.032
			(0.025)

Less than €1,000	0.021
	(0.043)
From €1,000 to €1,500	0.017
	(0.029)
From €1,500 to €2,000	−0.010
	(0.025)
From €3,000 to €5,000	−0.028
	(0.024)
From €5,000 to €10,000	−0.088**
	(0.038)
Over €10,000	−0.081
	(0.049)
Middle School (or lower)	0.030
	(0.030)
University	−0.056***
	(0.018)
PhD	−0.137***
	(0.039)
Worker (blue collar)	0.007
	(0.031)
Self-employed	−0.061**
	(0.027)
Student	−0.031
	(0.036)
Homemaker	−0.052
	(0.032)
Retired	−0.070**
	(0.029)
Looking for a job	−0.076**
	(0.037)
Other employment status	−0.057
	(0.064)
Yes, I have already purchased them	0.850***
	(0.021)
Yes, but I have never purchased them	0.683***
	(0.020)
Yes, but I would not repurchase them	0.463***
	(0.029)
North-East	0.088***
	(0.023)
Central	0.131***

			(0.023)
South			0.069***
			(0.022)
Islands			0.039
			(0.028)
Constant	0.239***	−0.075***	−0.750***
	(0.012)	(0.025)	(0.071)
Observations	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below; Environmental above/below; Social above/below; Disgust above/below*: dummy variables equal 1 when the self-image score on the dimension is, respectively, above/below the social-image score; the reference category is self-image equal to social image (alignment). *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Cheap Price* dummy variable equals 1 if the product can be taken at a convenient price, 0 otherwise. Baseline: *Free*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.23: Misalignment between self-image and second-order self-image

Region	Determinant	Misalignment	Below	Above
North West (N = 2,827)	Financial	40.68	36.61	63.39
	Social	41.03	39.05	60.95
	Environmental	36.65	41.02	58.98
	Hygiene	39.05	36.32	63.68
North East (N = 2,074)	Financial	40.89	36.32	63.68
	Social	42.77	42.39	57.61
	Environmental	36.64	40.79	59.21
	Hygiene	39.92	34.42	65.58
Center (N = 2,060)	Financial	39.85	39.10	60.90
	Social	39.32	41.60	58.40
	Environmental	36.89	42.24	57.76
	Hygiene	36.65	38.15	61.85
South (N = 2,390)	Financial	36.90	43.42	56.58
	Social	38.45	43.96	56.04
	Environmental	34.90	46.04	53.96
	Hygiene	36.28	40.14	59.86
Islands (N = 1,145)	Financial	37.29	43.33	56.67
	Social	36.59	42.96	57.04
	Environmental	35.28	42.82	57.18
	Hygiene	35.20	40.94	59.06

Note: Proportion of respondents for each macro-region whose answers on the second-order self-image items did not match their self-image scores for the determinant ("Misalignment"). Columns "Below" and "Above" indicate the proportion of misaligned respondents whose self-image is, respectively, lower or higher than their second-order self-image.

Table B.24: Misalignment between first-order and second-order self-image on adoption

	(1)	(2)	(3)
Financial below	−0.063***	−0.058**	−0.063***
	(0.024)	(0.024)	(0.024)
Financial above	0.018	0.023	0.038*
	(0.023)	(0.023)	(0.022)
Social below	−0.061**	−0.069***	−0.066***
	(0.024)	(0.024)	(0.023)
Social above	0.067***	0.060***	0.018
	(0.023)	(0.023)	(0.022)
Environmental below	−0.122***	−0.117***	−0.087***
	(0.025)	(0.024)	(0.024)
Environmental above	−0.046**	−0.041*	−0.030
	(0.022)	(0.022)	(0.021)
Disgust below	−0.204***	−0.201***	−0.134***
	(0.026)	(0.026)	(0.025)
Disgust above	0.172***	0.174***	0.113***
	(0.023)	(0.023)	(0.022)
Excellent condition		0.109***	0.113***
		(0.020)	(0.019)
Good condition		0.111***	0.090***
		(0.021)	(0.021)
Free		0.222***	0.241***
		(0.016)	(0.015)
Smartphone		0.165***	0.127***
		(0.023)	(0.022)
Fridge		0.114***	0.191***
		(0.023)	(0.023)
Closet		0.175***	0.181***
		(0.023)	(0.022)
Age			−0.003***
			(0.001)
Man			0.080***
			(0.016)
Need for Uniqueness			−0.029***
			(0.011)
Pro-Environmental Behavior			0.110***
			(0.012)
Don't know/Prefer not to answer			−0.037
			(0.025)

Less than €1,000	0.021
	(0.043)
From €1,000 to €1,500	0.015
	(0.029)
From €1,500 to €2,000	−0.014
	(0.025)
From €3,000 to €5,000	−0.027
	(0.024)
From €5,000 to €10,000	−0.091**
	(0.038)
Over €10,000	−0.087*
	(0.049)
Middle School (or lower)	0.026
	(0.030)
University	−0.056***
	(0.018)
PhD	−0.136***
	(0.039)
Worker (blue collar)	0.015
	(0.031)
Self-employed	−0.057**
	(0.027)
Student	−0.032
	(0.036)
Homemaker	−0.047
	(0.032)
Retired	−0.071**
	(0.029)
Looking for a job	−0.076**
	(0.037)
Other employment status	−0.048
	(0.064)
Yes, I have already purchased them	0.835***
	(0.021)
Yes, but I have never purchased them	0.672***
	(0.020)
Yes, but I would not repurchase them	0.461***
	(0.029)
North-East	0.086***
	(0.023)
Central	0.127***

			(0.023)
South			0.073***
			(0.022)
Islands			0.043
			(0.027)
Constant	0.245***	−0.073***	−0.723***
	(0.012)	(0.025)	(0.070)
Observations	10,496	10,496	10,496

Note: Fractional probit regression estimation. Dependent variable. *Probability of taking the second-hand product*: variable ranging between 0 and 1. Regressors. *Financial above/below; Environmental above/below; Social above/below; Disgust above/below*: dummy variables equal 1 when the self-image score on the dimension is, respectively, above/below the second-order self-image score; the reference category is self-image equal to second-order self-image (alignment). *Excellent condition* and *Good condition* are dummy variables equal 1 if the condition is the one described, 0 otherwise. Baseline: *Acceptable condition*. *Cheap Price* dummy variable equals 1 if the product can be taken at a convenient price, 0 otherwise. Baseline: *Free*. *Smartphone, Refrigerator, Closet* dummy variables equal 1 if the product belongs to that category, 0 otherwise. Baseline: *T-shirt*. *Age* is measured in years. *Man* is a dummy variable equal to 1 if the respondent reported being a man and 0 otherwise. *Income* categories are measured relative to the omitted category *From €2,000 to €3,000*; *Education* categories relative to *High school*; *Employment* status categories relative to *White-collar worker*; second-hand experience categories relative to respondents who would never buy second-hand products; and macro-regions relative to *North-West*. Standard errors clustered at the individual level are reported in parentheses. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Vignette and statements

[Translated from Italian. Randomized elements reported in square brackets.]

Please, read the following text and fill in the required fields.

Imagine you need to buy a *[T-shirt / refrigerator / closet / smartphone]*. You find the model you are interested in, *[a light blue, 100% cotton T-shirt, in your size / an LG with 384ℓ capacity, measures 59 × 203 × 68 cm, and energy class A++ / a wooden closet, with dimensions 226 × 212 × 56 cm, 3 compartments (90, 45, 90 cm), two double doors and one single door / an Honor 90, Android system, with 6.7" display, 256GB physical storage and 8GB RAM, 200MP back camera and 50MP front camera]*, second-hand. The product is in *[excellent condition / good condition / acceptable condition]*, has been sanitized, and you can pick it up *[at an affordable price / free of charge]*.

1. Referring to the situation described, what is the probability that you decide to take the product? Indicate a number from 0 to 100.

   [New page]

2. At each row of the following table you find two opposite statements, A and B. We ask you to indicate whether you agree more with statement A or B.

   "If I decided to take the product described..."

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel I am in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

[New page]

3. We asked the previous question to a representative sample of the Italian population. In your opinion, how did other respondents from the same macro-region answer to this question? Remember that, by "macro-region", we mean the five geographic areas: North-west (Liguria, Lombardia, Piemonte, Valle d'Aosta), North-East (Emilia-Romagna, Friuli-Venezia Giulia, Provincia Autonoma di Trento, Provincia Autonoma di Bolzano, Veneto), Center (Lazio, Marche, Toscana, Umbria), South (Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia), and Islands (Sardegna, Sicilia). Note also that, for this question, you may receive an additional reward of €4, as follows: for each row, if your answer matches the one given most frequently in the sample of your macro-region, you will receive €1. We will draw 400 participants to whom we will give this additional reward.

   [Same table of statements as in the previous question is shown here]

   [New page]

4. At each row of the following table you find two opposite statements, A and B. We ask you to indicate whether you agree more with statement A or B.

   "If I decided to take the product described, others would think that..."

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel that I was in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

Appendix C

Environmental costs in second-hand clothing purchase: A voucher choice experiment

C.1 Additional Tables and Figures

C.1.1 Balance Table

Table C.1: Balance Table

	WTA CO2	WTA Energy	WTA Water	WTA Control	WTP CO2	WTP Energy	WTP Water	WTP Control	Joint F-test
Mean									
Age	49.00	49.83	49.66	50.12	48.94	49.74	50.61	49.50	0.120
Pro-Environmental Behavior	-0.05	0.02	0.03	0.02	-0.02	0.01	0.04	-0.06	0.083*
Need for Uniqueness	0.00	-0.02	0.01	0.01	0.01	0.01	-0.03	-0.01	0.934
Proportion									
Gender									
Woman	0.509	0.517	0.537	0.480	0.500	0.542	0.508	0.501	0.033**
Man	0.491	0.483	0.463	0.520	0.500	0.458	0.492	0.499	0.033**
Macro-region									
Northwest	0.279	0.259	0.273	0.291	0.260	0.273	0.262	0.259	0.520
Northeast	0.200	0.198	0.187	0.182	0.210	0.206	0.195	0.203	0.647
Central	0.184	0.200	0.213	0.191	0.200	0.183	0.207	0.192	0.466
South	0.225	0.233	0.213	0.230	0.216	0.230	0.231	0.243	0.677
Islands	0.113	0.109	0.115	0.107	0.114	0.108	0.105	0.103	0.968
Education									
Middle School (or lower)	0.074	0.101	0.083	0.088	0.074	0.076	0.091	0.106	0.014**
High School	0.577	0.554	0.532	0.552	0.534	0.550	0.529	0.536	0.220
University	0.309	0.294	0.332	0.308	0.339	0.319	0.326	0.312	0.236
PhD	0.041	0.050	0.053	0.052	0.053	0.056	0.054	0.047	0.727

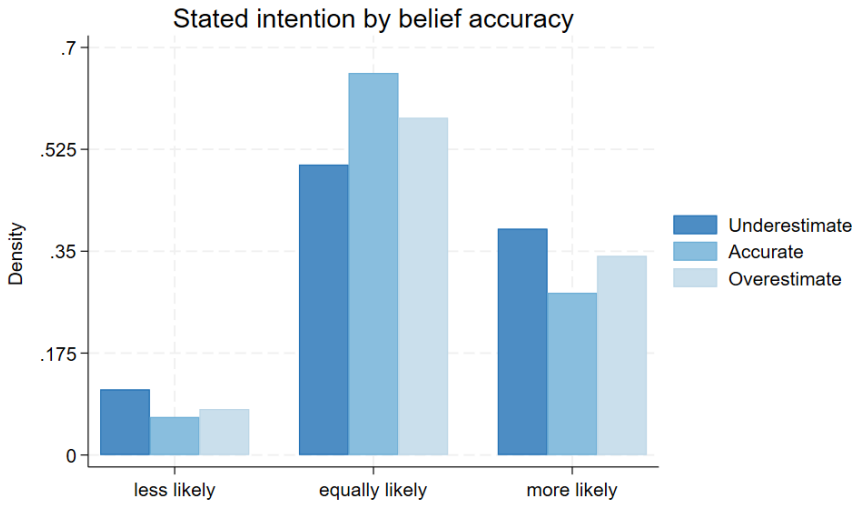
Employment status									
Worker (blue collar)	0.074	0.089	0.072	0.072	0.069	0.073	0.077	0.073	0.660
Employed	0.433	0.366	0.420	0.426	0.424	0.423	0.375	0.384	$< .001 * **$
Self-employed	0.116	0.112	0.118	0.102	0.125	0.107	0.124	0.131	0.324
Student	0.054	0.064	0.062	0.056	0.064	0.059	0.056	0.075	0.368
Homemaker	0.094	0.093	0.075	0.085	0.084	0.101	0.079	0.097	0.245
Retired	0.155	0.192	0.178	0.189	0.164	0.170	0.216	0.183	0.002***
Looking for a job	0.054	0.074	0.056	0.056	0.056	0.044	0.052	0.039	0.008***
Other	0.021	0.010	0.018	0.013	0.014	0.024	0.021	0.018	0.112
Income									
Don't know/Prefer not to answer	0.182	0.176	0.180	0.156	0.181	0.196	0.170	0.178	0.321
Less than €1,000	0.042	0.060	0.047	0.048	0.039	0.037	0.040	0.047	0.112
From €1,000 to €1,500	0.119	0.103	0.108	0.114	0.095	0.102	0.110	0.114	0.566
From €1,500 to €2,000	0.165	0.160	0.165	0.175	0.160	0.164	0.161	0.160	0.970
From €2,000 to €3,000	0.208	0.242	0.234	0.230	0.244	0.224	0.238	0.223	0.428
From €3,000 to €5,000	0.184	0.171	0.182	0.185	0.196	0.188	0.188	0.192	0.865
From €5,000 to €10,000	0.060	0.058	0.053	0.059	0.052	0.047	0.066	0.055	0.559
Over €10,000	0.040	0.030	0.030	0.034	0.033	0.043	0.028	0.031	0.331

Note: Balance table reporting sample means (for continuous variables) and proportions (for categorical variables) across treatment arms and elicitation tasks (WTA and WTP). Each row reports group-level statistics for the eight treatment–task cells (CO2, Energy, Water, Control × WTA, WTP). The last column reports the p-value from a joint F-test of equality of means across all eight groups.

Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.2 Intention by belief accuracy

Figure C.1: 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 ±15% tolerance interval around true values. Density represents the proportion of respondents in each intention category within each belief accuracy group.

C.1.3 Intention by treatment

Table C.2: Intention Distribution by Treatment

	Treatment		
Intention	Water	Energy	CO2
Less likely	284 (10.85%)	249 (9.50%)	239 (9.11%)
Equally likely	1,207 (46.12%)	1,447 (55.23%)	1,578 (60.16%)
More likely	1,126 (43.03%)	924 (35.27%)	806 (30.73%)
Total	2,617 (100%)	2,620 (100%)	2,623 (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 column proportions within each treatment condition. Pearson $\chi^2(4) = 109.56$, $p < 0.001$.

C.1.4 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 × IQR or above Q3 + 1.5 × IQR).

Table C.3: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	(1)	(2)	(3)
Delta Belief	-0.025	0.050**	-0.019
	(0.017)	(0.020)	(0.022)
Water		0.476***	0.357***
		(0.066)	(0.070)
Energy		0.186***	0.117*
		(0.061)	(0.064)
Need for Uniqueness			-0.145***
			(0.028)
Pro-Environmental Behavior			0.491***
			(0.030)
Age			-0.008***
			(0.002)
Man			0.034
			(0.053)
Don't know/Prefer not to answer			-0.473***
			(0.078)
Less than €1,000			-0.674***
			(0.132)
From €1,000 to €1,500			-0.239**
			(0.096)
From €1,500 to €2,000			-0.181**
			(0.081)
From €3,000 to €5,000			-0.054
			(0.077)
From €5,000 to €10,000			-0.364***
			(0.135)
Over €10,000			0.207
			(0.171)
Middle School (or lower)			-0.100
			(0.101)
University			-0.049

			(0.059)
PhD			-0.219*
			(0.133)
Worker (blue collar)			-0.202**
			(0.097)
Self-employed			-0.034
			(0.085)
Student			-0.278**
			(0.124)
Homemaker			-0.170*
			(0.098)
Retired			-0.358***
			(0.094)
Looking for a job			-0.314**
			(0.125)
Other			-0.106
			(0.195)
Yes, I have already purchased them			1.382***
			(0.062)
Yes, but I have never purchased them			1.115***
			(0.071)
Yes, but I would not repurchase them			0.712***
			(0.100)
Northeast			0.066
			(0.071)
Central			0.221***
			(0.073)
South			0.164**
			(0.073)
Islands			-0.144
			(0.092)
Cut 1	-2.167***	-1.974***	-2.352***
	(0.040)	(0.049)	(0.153)
Cut 2	0.553***	0.763***	0.889***
	(0.025)	(0.043)	(0.151)
Observations	6698	6698	6698

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Sample excludes outliers in stated beliefs, identified using Tukey's interquartile range rule. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely). *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, and macro-region of residence (baseline categories set to the mode).

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

Table C.4: Tobit Regression Estimations of WTA for Second-Hand and WTP for First-Hand

	WTA		WTP	
	(1)	(2)	(3)	(4)
Water	-3.632***	-1.505*	1.059**	0.602
	(0.646)	(0.778)	(0.510)	(0.630)
Energy	-2.168***	0.001	0.558	-0.008
	(0.666)	(0.757)	(0.523)	(0.603)
CO2	-2.208***		0.895*	
	(0.664)		(0.519)	
Delta Belief		-0.048		0.335
		(0.244)		(0.209)
Need for Uniqueness	0.899***	0.874***	0.660***	0.638***
	(0.262)	(0.306)	(0.202)	(0.241)
Pro-Environmental Behavior	-1.487***	-1.422***	1.679***	1.791***
	(0.273)	(0.313)	(0.215)	(0.254)
SH Closest	1.223**	0.790	0.095	0.246
	(0.582)	(0.668)	(0.459)	(0.546)
FH Distance (in km)	-0.010	-0.030	-0.004	-0.007
	(0.016)	(0.019)	(0.014)	(0.017)
SH Distance (in km)	-0.013	-0.002	0.000	0.005
	(0.014)	(0.016)	(0.011)	(0.013)
Age	0.031	0.022	-0.018	-0.010
	(0.023)	(0.026)	(0.018)	(0.021)
Man	-2.580***	-2.687***	1.020**	0.710
	(0.494)	(0.567)	(0.400)	(0.475)
Don't know/Prefer not to answer	0.705	1.203	-2.022***	-2.033***
	(0.771)	(0.887)	(0.602)	(0.722)
Less than €1,000	1.291	1.813	-0.278	0.808
	(1.279)	(1.426)	(1.044)	(1.266)
From €1,000 to €1,500	-0.255	-0.049	-1.653**	-1.821**
	(0.881)	(1.023)	(0.738)	(0.883)
From €1,500 to €2,000	-0.170	0.554	-0.157	-0.247
	(0.764)	(0.903)	(0.602)	(0.711)
From €3,000 to €5,000	0.084	0.209	-0.547	-0.308
	(0.731)	(0.853)	(0.544)	(0.646)
From €5,000 to €10,000	-0.046	0.292	0.464	0.752
	(1.140)	(1.342)	(0.925)	(1.104)

Over €10,000	-0.948	-1.712	-0.052	-0.045
	(1.385)	(1.561)	(1.183)	(1.430)
Middle School (or lower)	-0.785	-1.017	0.398	-0.242
	(0.951)	(1.098)	(0.745)	(0.922)
University	-0.109	0.115	-0.441	-0.706
	(0.556)	(0.649)	(0.443)	(0.530)
PhD	-2.477**	-3.701***	-0.347	-1.205
	(1.183)	(1.376)	(0.876)	(1.009)
Worker (blue collar)	-0.908	-1.609	1.689**	1.383
	(0.880)	(0.980)	(0.768)	(0.923)
Self-employed	0.117	0.002	1.847***	2.605***
	(0.826)	(0.956)	(0.602)	(0.737)
Student	-1.337	-2.283*	2.320***	1.860*
	(1.070)	(1.231)	(0.877)	(1.101)
Homemaker	-0.873	-1.340	0.060	0.265
	(1.007)	(1.183)	(0.790)	(0.938)
Retired	-0.464	-0.057	1.453**	1.913**
	(0.892)	(1.033)	(0.707)	(0.837)
Looking for a job	-1.266	-0.535	-0.322	-1.327
	(1.088)	(1.260)	(1.052)	(1.226)
Other	-0.813	-0.139	5.062***	3.634*
	(2.073)	(2.371)	(1.575)	(1.930)
Yes, I have already purchased them	-12.222***	-12.443***	9.619***	9.774***
	(0.597)	(0.688)	(0.471)	(0.563)
Yes, but I have never purchased them	-9.291***	-10.273***	7.257***	7.409***
	(0.668)	(0.778)	(0.524)	(0.621)
Yes, but I would not repurchase them	-7.264***	-6.962***	6.436***	6.570***
	(0.880)	(1.014)	(0.707)	(0.822)
Northeast	-0.980	-1.351*	-0.046	-0.639
	(0.713)	(0.811)	(0.555)	(0.667)
Central	-0.514	-0.827	0.792	0.281
	(0.686)	(0.785)	(0.561)	(0.653)
South	-0.310	-0.225	-0.928*	-1.025
	(0.681)	(0.812)	(0.546)	(0.644)
Islands	1.425	1.613	-1.045	-1.415
	(0.923)	(1.065)	(0.755)	(0.913)
Constant	14.368***	13.065***	-8.833***	-8.305***

	(1.521)	(1.780)	(1.162)	(1.367)
Controls	Yes	Yes	Yes	Yes
Observations	4517	3231	4534	3246

Note: Tobit regression estimation with standard errors clustered at the individual level. Sample excludes outliers in stated beliefs, identified using Tukey's interquartile range rule. Dependent variables: *WTA* (columns 1–2) and *WTP* (columns 3–4), both 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). For WTA, negative values indicate lower WTA (greater willingness to accept second-hand); for WTP, positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors. *Water* and *Energy* are treatment indicators. Baseline: *Control* in columns (1) and (3); *CO2* in columns (2) and (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. *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, macro-region of residence, distance to the nearest first-hand and second-hand store, and whether the closest store is second-hand (baseline categories set to the mode).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.5 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 × IQR or above Q3 + 1.5 × IQR).

Table C.5: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	(1)	(2)	(3)
Delta Belief	-0.003*	-0.001	-0.006***
	(0.002)	(0.002)	(0.002)
Water		0.362***	0.372***
		(0.058)	(0.061)
Energy		0.136**	0.129**
		(0.054)	(0.057)
Need for Uniqueness			-0.158***
			(0.026)
Pro-Environmental Behavior			0.494***
			(0.028)
Age			-0.009***
			(0.002)
Man			0.031
			(0.051)
Don't know/Prefer not to answer			-0.426***
			(0.074)
Less than €1,000			-0.596***
			(0.133)
From €1,000 to €1,500			-0.200**
			(0.093)
From €1,500 to €2,000			-0.138*
			(0.077)
From €3,000 to €5,000			-0.014
			(0.074)
From €5,000 to €10,000			-0.325**
			(0.128)
Over €10,000			0.264
			(0.164)
Middle School (or lower)			-0.130
			(0.098)
University			-0.046

			(0.057)
PhD			-0.265**
			(0.128)
Worker (blue collar)			-0.175*
			(0.096)
Self-employed			-0.029
			(0.081)
Student			-0.330***
			(0.117)
Homemaker			-0.173*
			(0.093)
Retired			-0.307***
			(0.090)
Looking for a job			-0.277**
			(0.116)
Other			-0.082
			(0.192)
Yes, I have already purchased them			1.311***
			(0.059)
Yes, but I have never purchased them			1.039***
			(0.070)
Yes, but I would not repurchase them			0.737***
			(0.097)
Northeast			0.033
			(0.069)
Central			0.250***
			(0.070)
South			0.140**
			(0.070)
Islands			-0.070
			(0.087)
Cut 1	-2.225***	-2.066***	-2.375***
	(0.040)	(0.048)	(0.147)
Cut 2	0.572***	0.743***	0.925***
	(0.025)	(0.040)	(0.145)
Observations	7283	7283	7283

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Sample excludes outliers in survey completion time, identified using Tukey's interquartile range rule. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely). *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, and macro-region of residence (baseline categories set to the mode).

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

Table C.6: Tobit Regression Estimations of WTA for Second-Hand and WTP for First-Hand

	WTA		WTP	
	(1)	(2)	(3)	(4)
Water	-3.718***	-1.718***	1.377***	0.075
	(0.644)	(0.649)	(0.509)	(0.533)
Energy	-1.954***	0.089	0.992*	-0.322
	(0.656)	(0.659)	(0.508)	(0.524)
CO2	-2.144***		1.382***	
	(0.651)		(0.510)	
Delta Belief		-0.013		0.021
		(0.022)		(0.017)
Need for Uniqueness	0.803***	0.839***	0.612***	0.596***
	(0.252)	(0.289)	(0.193)	(0.225)
Pro-Environmental Behavior	-1.435***	-1.289***	1.577***	1.684***
	(0.267)	(0.300)	(0.209)	(0.241)
SH Closest	1.108*	0.631	-0.103	-0.007
	(0.568)	(0.641)	(0.448)	(0.520)
FH Distance (in km)	-0.015	-0.037**	-0.010	-0.012
	(0.016)	(0.018)	(0.013)	(0.016)
SH Distance (in km)	-0.006	0.006	0.003	0.006
	(0.013)	(0.015)	(0.010)	(0.012)
Age	0.028	0.022	-0.035**	-0.035*
	(0.022)	(0.025)	(0.017)	(0.020)
Man	-2.646***	-2.721***	0.764**	0.433
	(0.480)	(0.541)	(0.386)	(0.446)
Don't know/Prefer not to answer	0.603	1.009	-2.040***	-2.032***
	(0.737)	(0.835)	(0.579)	(0.678)
Less than €1,000	0.678	1.391	-0.347	0.492
	(1.263)	(1.392)	(1.048)	(1.249)
From €1,000 to €1,500	0.193	0.622	-1.468**	-1.171
	(0.862)	(0.975)	(0.723)	(0.851)
From €1,500 to €2,000	0.005	0.670	-0.202	-0.172
	(0.733)	(0.849)	(0.576)	(0.661)
From €3,000 to €5,000	0.624	0.944	-0.547	-0.180
	(0.720)	(0.824)	(0.532)	(0.616)
From €5,000 to €10,000	0.535	1.167	-0.185	0.042
	(1.078)	(1.227)	(0.877)	(1.016)

Over €10,000	-1.060	-2.007	0.421	0.642
	(1.376)	(1.514)	(1.141)	(1.348)
Middle School (or lower)	-0.757	-1.006	0.374	-0.189
	(0.926)	(1.050)	(0.729)	(0.883)
University	-0.144	-0.097	-0.311	-0.455
	(0.541)	(0.618)	(0.428)	(0.501)
PhD	-2.486**	-3.509***	-0.190	-0.728
	(1.132)	(1.297)	(0.852)	(0.964)
Worker (blue collar)	-0.842	-1.632*	1.242	0.765
	(0.858)	(0.942)	(0.762)	(0.896)
Self-employed	0.159	-0.089	1.547***	1.930***
	(0.806)	(0.915)	(0.580)	(0.688)
Student	-1.288	-1.957*	1.669**	1.288
	(1.024)	(1.155)	(0.823)	(0.989)
Homemaker	-1.386	-2.016*	0.185	0.409
	(0.955)	(1.095)	(0.762)	(0.885)
Retired	-0.562	-0.157	1.786***	2.261***
	(0.871)	(0.988)	(0.685)	(0.791)
Looking for a job	-1.272	-0.808	0.057	-0.852
	(1.050)	(1.194)	(1.000)	(1.130)
Other	-1.339	-0.351	4.177***	2.444
	(2.107)	(2.399)	(1.506)	(1.780)
Yes, I have already purchased them	-12.388***	-12.798***	9.731***	9.957***
	(0.580)	(0.656)	(0.454)	(0.528)
Yes, but I have never purchased them	-9.318***	-10.390***	7.127***	7.369***
	(0.659)	(0.757)	(0.511)	(0.594)
Yes, but I would not repurchase them	-7.291***	-7.237***	6.191***	6.400***
	(0.856)	(0.969)	(0.672)	(0.765)
Northeast	-1.087	-1.533**	-0.139	-0.727
	(0.691)	(0.774)	(0.537)	(0.633)
Central	-0.937	-1.406*	0.686	0.288
	(0.665)	(0.745)	(0.539)	(0.613)
South	-0.346	-0.305	-0.754	-0.906
	(0.656)	(0.763)	(0.532)	(0.614)
Islands	0.735	0.505	-0.762	-1.140
	(0.900)	(1.019)	(0.715)	(0.839)
Constant	14.498***	13.467***	-7.825***	-6.425***

	(1.477)	(1.662)	(1.119)	(1.279)
Controls	Yes	Yes	Yes	Yes
Observations	4710	3505	4757	3539

Note: Tobit regression estimation with standard errors clustered at the individual level. Sample excludes outliers in survey completion time, identified using Tukey's interquartile range rule. Dependent variables: *WTA* (columns 1–2) and *WTP* (columns 3–4), both 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). For WTA, negative values indicate lower WTA (greater willingness to accept second-hand); for WTP, positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors. *Water* and *Energy* are treatment indicators. Baseline: *Control* in columns (1) and (3); *CO2* in columns (2) and (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. *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, macro-region of residence, distance to the nearest first-hand and second-hand store, and whether the closest store is second-hand (baseline categories set to the mode).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.6 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.

Table C.7: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	(1)	(2)	(3)
Delta Belief	-0.004**	-0.001	-0.006***
	(0.002)	(0.002)	(0.002)
Water		0.371***	0.367***
		(0.056)	(0.059)
Energy		0.135***	0.132**
		(0.052)	(0.055)
Need for Uniqueness			-0.160***
			(0.025)
Pro-Environmental Behavior			0.487***
			(0.027)
Age			-0.008***
			(0.002)
Man			0.022
			(0.049)
Don't know/Prefer not to answer			-0.456***
			(0.072)
Less than €1,000			-0.626***
			(0.124)
From €1,000 to €1,500			-0.204**
			(0.089)
From €1,500 to €2,000			-0.152**
			(0.074)
From €3,000 to €5,000			-0.058
			(0.071)
From €5,000 to €10,000			-0.306**
			(0.123)
Over €10,000			0.190
			(0.157)
Middle School (or lower)			-0.129
			(0.092)

University			-0.026
			(0.055)
PhD			-0.233*
			(0.123)
Worker (blue collar)			-0.161*
			(0.091)
Self-employed			-0.039
			(0.078)
Student			-0.296**
			(0.115)
Homemaker			-0.123
			(0.090)
Retired			-0.302***
			(0.087)
Looking for a job			-0.277**
			(0.114)
Other			-0.083
			(0.178)
Yes, I have already purchased them			1.324***
			(0.057)
Yes, but I have never purchased them			1.078***
			(0.067)
Yes, but I would not repurchase them			0.726***
			(0.092)
Northeast			0.061
			(0.066)
Central			0.265***
			(0.067)
South			0.169**
			(0.068)
Islands			-0.068
			(0.084)
Cut 1	-2.226***	-2.065***	-2.327***
	(0.038)	(0.046)	(0.142)
Cut 2	0.553***	0.726***	0.953***
	(0.024)	(0.039)	(0.139)
Observations	7860	7860	7860

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely). *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, and macro-region of residence (baseline categories set to the mode).

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

Table C.8: Tobit Regression Estimations of WTA for Second-Hand and WTP for First-Hand

	WTA		WTP	
	(1)	(2)	(3)	(4)
Water	-3.688***	-1.572**	1.293***	0.214
	(0.623)	(0.628)	(0.495)	(0.517)
Energy	-2.145***	0.007	0.779	-0.322
	(0.633)	(0.635)	(0.494)	(0.509)
CO2	-2.249***		1.196**	
	(0.631)		(0.494)	
Delta Belief		-0.015		0.022
		(0.022)		(0.016)
Need for Uniqueness	0.895***	0.877***	0.601***	0.579***
	(0.244)	(0.279)	(0.188)	(0.219)
Pro-Environmental Behavior	-1.386***	-1.294***	1.655***	1.745***
	(0.256)	(0.288)	(0.202)	(0.233)
SH Closest	1.184**	0.791	-0.075	0.019
	(0.549)	(0.619)	(0.434)	(0.504)
FH Distance (in km)	-0.010	-0.027	-0.006	-0.008
	(0.015)	(0.018)	(0.013)	(0.015)
SH Distance (in km)	-0.008	0.003	-0.001	0.003
	(0.013)	(0.015)	(0.010)	(0.012)
Age	0.033	0.028	-0.034**	-0.032
	(0.021)	(0.024)	(0.017)	(0.019)
Man	-2.602***	-2.691***	0.847**	0.558
	(0.463)	(0.523)	(0.375)	(0.434)
Don't know/Prefer not to answer	0.367	0.706	-2.032***	-2.031***
	(0.717)	(0.807)	(0.566)	(0.662)
Less than €1,000	0.921	1.238	-0.152	0.855
	(1.208)	(1.328)	(0.995)	(1.181)
From €1,000 to €1,500	-0.332	-0.183	-1.276*	-1.277
	(0.827)	(0.941)	(0.696)	(0.813)
From €1,500 to €2,000	-0.293	0.291	-0.053	-0.063
	(0.709)	(0.815)	(0.560)	(0.644)
From €3,000 to €5,000	0.295	0.495	-0.490	-0.243
	(0.690)	(0.789)	(0.515)	(0.599)
From €5,000 to €10,000	0.524	1.055	-0.142	-0.078
	(1.056)	(1.208)	(0.865)	(1.007)

Over €10,000	-1.073	-1.762	0.684	0.907
	(1.312)	(1.455)	(1.106)	(1.302)
Middle School (or lower)	-0.739	-0.927	0.383	-0.200
	(0.886)	(0.998)	(0.712)	(0.861)
University	-0.203	-0.047	-0.457	-0.698
	(0.522)	(0.595)	(0.415)	(0.486)
PhD	-2.603**	-3.609***	-0.299	-0.944
	(1.098)	(1.246)	(0.826)	(0.935)
Worker (blue collar)	-0.847	-1.451	1.158	0.723
	(0.830)	(0.912)	(0.738)	(0.870)
Self-employed	0.354	0.328	1.553***	2.037***
	(0.781)	(0.887)	(0.566)	(0.673)
Student	-1.275	-2.040*	1.863**	1.411
	(1.003)	(1.130)	(0.806)	(0.971)
Homemaker	-1.487	-2.069**	-0.145	-0.033
	(0.922)	(1.052)	(0.742)	(0.859)
Retired	-0.527	-0.197	1.504**	1.867**
	(0.836)	(0.947)	(0.666)	(0.772)
Looking for a job	-1.326	-0.697	-0.358	-1.177
	(1.023)	(1.166)	(0.946)	(1.066)
Other	-1.479	-1.099	3.984***	2.297
	(1.975)	(2.221)	(1.470)	(1.751)
Yes, I have already purchased them	-12.453***	-12.711***	9.596***	9.745***
	(0.561)	(0.635)	(0.441)	(0.514)
Yes, but I have never purchased them	-9.599***	-10.593***	7.143***	7.297***
	(0.632)	(0.722)	(0.496)	(0.575)
Yes, but I would not repurchase them	-7.603***	-7.484***	6.169***	6.260***
	(0.828)	(0.936)	(0.658)	(0.748)
Northeast	-1.047	-1.360*	-0.010	-0.432
	(0.668)	(0.747)	(0.524)	(0.616)
Central	-0.792	-1.133	0.891*	0.520
	(0.644)	(0.723)	(0.522)	(0.595)
South	-0.191	-0.038	-0.828	-0.848
	(0.635)	(0.736)	(0.517)	(0.598)
Islands	1.146	1.216	-0.923	-1.194
	(0.870)	(0.985)	(0.701)	(0.822)
Constant	14.498***	13.069***	-7.740***	-6.578***

	(1.423)	(1.599)	(1.092)	(1.247)
Observations	5074	3788	5105	3817

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variables: *WTA* (columns 1–2) and *WTP* (columns 3–4), both 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). For WTA, negative values indicate lower WTA (greater willingness to accept second-hand); for WTP, positive values indicate higher WTP (greater willingness to pay for first-hand). Regressors. *Water* and *Energy* are treatment indicators. Baseline: *Control* in columns (1) and (3); *CO2* in columns (2) and (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. *Demographic controls* include age, gender, need for uniqueness score, pro-environmental behavior, income, education, employment status, past adoption of second-hand, macro-region of residence, distance to the nearest first-hand and second-hand store, and whether the closest store is second-hand (baseline categories set to the mode).
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.7 Regression analysis: Macroregions

Table C.9: Ordered Logistic Regression on Change in Intention to Purchase Second-Hand

	North West	North East	Center	South	Islands
	(1)	(2)	(3)	(4)	(5)
Delta Belief	-0.008***	-0.003	0.004	0.000	0.008
	(0.003)	(0.004)	(0.003)	(0.003)	(0.006)
Water	0.308***	0.406***	0.676***	0.218*	0.217
	(0.109)	(0.130)	(0.123)	(0.116)	(0.165)
Energy	0.080	0.048	0.250**	0.200*	0.072
	(0.103)	(0.116)	(0.123)	(0.110)	(0.150)
Cut 1	-2.173***	-2.138***	-2.190***	-1.974***	-1.779***
	(0.092)	(0.106)	(0.110)	(0.098)	(0.118)
Cut 2	0.825***	0.765***	0.757***	0.536***	0.789***
	(0.076)	(0.088)	(0.089)	(0.083)	(0.107)
Observations	2103	1566	1555	1767	869

Note: Ordered logit regression estimation with standard errors clustered at the individual level. Separate estimation by macro-region of residence. Dependent variable. *Change in intention to purchase more second-hand clothes*: categorical variable 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* and *Energy* are treatment indicators. Baseline *CO2*. *Cut 1* and *Cut 2* denote the estimated threshold parameters separating the three categories of the ordered dependent variable (less likely / equally likely / more likely).

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

Table C.10: 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***	-2.134*	-4.697***	-1.632	-5.517***	-1.365	-3.829***	-3.180**	-4.718**	-1.870
	(1.254)	(1.271)	(1.531)	(1.469)	(1.455)	(1.449)	(1.419)	(1.574)	(2.233)	(2.252)
Energy	-2.917**	-1.617	-3.089**	0.002	-2.882*	1.263	-2.252	-1.547	-7.212***	-4.286*
	(1.295)	(1.295)	(1.500)	(1.422)	(1.537)	(1.517)	(1.384)	(1.531)	(2.350)	(2.355)
CO2	-1.779		-3.345**		-4.266***		-0.910		-3.275	
	(1.283)		(1.476)		(1.536)		(1.457)		(2.222)	
Delta Belief		-0.100**		-0.046		-0.022		-0.022		-0.070
		(0.040)		(0.048)		(0.068)		(0.036)		(0.051)
Constant	7.797***	6.515***	7.317***	4.181***	7.442***	3.255***	6.881***	6.279***	9.811***	6.952***
	(0.927)	(0.946)	(1.127)	(1.046)	(1.119)	(1.105)	(0.987)	(1.176)	(1.645)	(1.647)
Observations	1443	1062	1005	766	1032	782	1180	879	581	441

Note: Tobit regression estimation with standard errors clustered at the individual level. Separate estimation by macro-region of residence: North West (columns 1–2), North East (columns 3–4), Center (columns 5–6), South (columns 7–8), Islands (columns 9–10). Dependent variable: *WTA*, 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 treatment indicators. Baseline: *Control* in odd columns; *CO2* in even columns. *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. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.11: 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	-0.371	0.168	-0.372	1.271	1.267	3.139***	2.203*	1.290	1.553
	(0.985)	(0.992)	(1.155)	(1.276)	(1.212)	(1.208)	(1.134)	(1.255)	(1.834)	(1.862)
Energy	1.442	-0.518	-0.194	-0.779	0.718	0.662	1.172	0.081	-0.098	0.004
	(0.995)	(0.995)	(1.068)	(1.178)	(1.193)	(1.180)	(1.187)	(1.290)	(1.818)	(1.830)
CO2	2.030**		0.688		0.232		1.361		0.361	
	(1.034)		(1.105)		(1.210)		(1.170)		(1.657)	
Delta Belief		0.009		0.025		0.030		0.071		0.087**
		(0.022)		(0.056)		(0.026)		(0.059)		(0.036)
Constant	-4.514***	-2.522***	-3.586***	-3.106***	-3.024***	-2.982***	-5.416***	-4.460***	-4.175***	-4.417***
	(0.740)	(0.750)	(0.739)	(0.879)	(0.873)	(0.868)	(0.789)	(0.929)	(1.226)	(1.229)
Observations	1384	1041	1069	800	1028	773	1210	888	564	428

Note: Tobit regression estimation with standard errors clustered at the individual level. Separate estimation by macro-region of residence: North West (columns 1–2), North East (columns 3–4), Center (columns 5–6), South (columns 7–8), Islands (columns 9–10). Dependent variable: *WTP*, 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 treatment indicators. Baseline: *Control* in odd columns; *CO2* in even columns. *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. Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1.8 Regression analysis: Local Environmental Conditions

Table C.12: Tobit Regression Estimations of WTA for Second-Hand

	(1)	(2)	(3)	(4)
Water	-6.433***	-2.607***		
	(2.365)	(0.890)		
Energy	-3.800		0.948	
	(2.370)		(1.189)	
CO2	-2.192			-9.560
	(2.378)			(7.276)
Green vote share	-0.012			
	(0.276)			
Water × Green vote share	0.355			
	(0.379)			
Energy × Green vote share	0.093			
	(0.381)			
CO2 × Green vote share	-0.056			
	(0.385)			
HPH		0.054		
		(0.053)		
Water × HPH		0.012		
		(0.114)		
Days of partial suspension (dummy)		-1.381		
		(0.861)		
Water × Days of partial suspension (dummy)		2.100		
		(1.746)		
Modified LIHC			0.059	
			(0.072)	
Energy × Modified LIHC			-0.261*	
			(0.149)	
NO2 Concentration				0.206***
				(0.073)
CO2 × NO2 Concentration				-0.086
				(0.145)
PM210 Concentration				0.064
				(0.124)
CO2 × PM210 Concentration				-0.199
				(0.240)
PM2.5 Concentration				-0.342***
				(0.132)

CO2 × PM2.5 Concentration				0.397
				(0.267)
O3 Concentration				-0.066
				(0.044)
CO2 × O3 Concentration				0.173**
				(0.087)
Constant	7.709***	5.592***	4.932***	8.558**
	(1.711)	(0.453)	(0.582)	(3.655)
Observations	5241	5208	5241	5000

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTA*, 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). Each column estimates a separate specification including only the treatment relevant to the environmental context variable. Column (1): *Water* treatment interacted with provincial *Green vote share* (2024 EU elections). Column (2): *Water* treatment interacted with provincial *HPH* (hours of partial water suspension) and *Days of partial suspension* (dummy). Column (3): *Energy* treatment interacted with regional *Modified LIHC* (energy poverty index). Column (4): *CO2* treatment interacted with provincial air quality indicators (NO_2, *PM10*, *PM2.5*, O_3 concentrations). Baseline: *Control* in all columns.
Significance of coefficients: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.13: Tobit Regression Estimations of WTP for First-Hand

	(1)	(2)	(3)	(4)
Water	3.691*	0.396		
	(1.913)	(0.704)		
Energy	-0.083		0.913	
	(1.883)		(0.920)	
CO2	1.422			-2.228
	(1.909)			(5.631)
Green vote share	0.363			
	(0.222)			
Water × Green vote share	-0.358			
	(0.304)			
Energy × Green vote share	0.133			
	(0.303)			
CO2 × Green vote share	-0.062			
	(0.308)			
HPH		-0.034		
		(0.043)		
Water × HPH		0.022		
		(0.086)		
Days of partial suspension (dummy)		-2.094***		
		(0.683)		
Water × Days of partial suspension (dummy)		2.734*		
		(1.424)		
Modified LIHC			-0.045	
			(0.057)	
Energy × Modified LIHC			-0.147	
			(0.119)	
NO2 Concentration				-0.054
				(0.054)
CO2 × NO2 Concentration				0.014
				(0.110)
PM210 Concentration				-0.123
				(0.093)
CO2 × PM210 Concentration				-0.130
				(0.188)
PM2.5 Concentration				0.089
				(0.100)
CO2 × PM2.5 Concentration				0.203
				(0.204)

O3 Concentration				-0.026
				(0.034)
CO2 × O3 Concentration				0.038
				(0.068)
Constant	-6.353***	-3.107***	-2.993***	0.723
	(1.364)	(0.355)	(0.464)	(2.814)
Observations	5255	5217	5255	5028

Note: Tobit regression estimation with standard errors clustered at the individual level. Dependent variable: *WTP*, 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). Each column estimates a separate specification including only the treatment relevant to the environmental context variable. Column (1): *Water* treatment interacted with provincial *Green vote share* (2024 EU elections). Column (2): *Water* treatment interacted with provincial *HPH* (hours of partial water suspension) and *Days of partial suspension* (dummy). Column (3): *Energy* treatment interacted with regional *Modified LIHC* (energy poverty index). Column (4): *CO2* treatment interacted with provincial air quality indicators (NO_2, *PM10*, *PM2.5*, O_3 concentrations). Baseline: *Control* in all columns.

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

C.2 Experimental Screens

C.2.1 Information provision task

Figure C.2: 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 C.3: 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 C.4: 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.

10ℓ = 2 minutes of open kitchen tap

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

C.2.2 Voucher choice task

Figure C.5: 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 H&M	**Voucher B** MERCATINO	**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 C.6: 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 MERCATINO	**Voucher B** H&M	**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

Appendix D

Survey Experiment Materials for Chapters 2 and 3

This appendix reports the full English translation of the survey experiment presented in Chapter 2 and Chapter 3. The former analyses the results from the Vignette task, while the latter analyses the results from the quiz task and the voucher task. Table D.1 maps each section of the questionnaire below to the chapter that analyzes it.

Notation

The survey text is reported using the following notation:

- *[randomization]* indicates elements that were randomized across participants.
- **(response field)** indicates fields completed by respondents.
- [system information] indicates information on the functioning of the survey system and flow.
- *(Likert scale construction)* indicates how Likert scales were constructed or displayed.

Structure of this appendix

This questionnaire was administered once, to a single nationally representative Italian sample, and different blocks of it feed into Chapter 2 and Chapter 3. Table D.1 summarizes which section is used by which chapter; the same information is repeated as a short tag under each section heading below, and each chapter cross-references the relevant section directly (e.g., "see Appendix D, Section D.3") at the point where the corresponding questions are first used.

Table D.1: Mapping of questionnaire sections to chapters

Section	**Used in**
Welcome page; Initial questions	Shared, Chapter 2 and Chapter 3
Discrete choice experiment	Used in a separate paper not included in this dissertation
Vignette	Chapter 2
Likert scale	Used in a separate paper not included in this dissertation
Quiz task	Chapter 3
Voucher Task (MPL)	Chapter 3
Final questions	Shared covariates, Chapter 2 and Chapter 3

D.1 Welcome page

Welcome!

The study you are about to participate in is part of a research project on purchasing behavior conducted by a team of researchers from the University of Rome Tor Vergata and the University of Innsbruck, funded by the National Recovery and Resilience Plan (NRRP) and carried out in collaboration with Scenari SRL, which manages its implementation. By agreeing to participate you are contributing to our research.

It is very important for the success of our research that you read the questions very carefully before answering and that you answer honestly.

Filling out the questionnaire takes about 15 minutes. For the success of our study, it is important that the questionnaire is done in its entirety, that is, all fields are filled in.

The study has received the certificate of good standing of the Institutional Review Board (LegenIRB) of the University of Innsbruck. Our study will give you the opportunity to express your opinion and will be completely anonymous: data collected through Scenari SRL will be processed in accordance with current privacy legislation and the GDPR. Specifically, the data will be anonymized and sent to the group of researchers who will perform aggregate analyses.

Your participation in the study is voluntary. Your name will never be disclosed to the researchers' group and it will not be possible for the researchers to identify you in any way and at any time even after the study is conducted.

Please also remember that there are no right or wrong answers: answer freely and tell us about your likes and dislikes.

D.2 Initial questions

- Insert your age: **(numeric field)**
- The gender you identify with is:
    - Male
    - Female
    - Other

D.3 Discrete choice experiment

Indicate your preference among the following purchase options, each with the characteristics listed. When the product is second-hand, you will be given its condition (excellent, good, acceptable), and it is always intended to be checked and sanitized. Transport and shipping are free of charge. Suppose you need to buy *[a refrigerator / a closet / a smartphone / a T-shirt]* and you have found a model that interests you. You are considering *[an LG with 384L capacity, measures 59 x 203 x 68 cm, and energy class A++ / a wooden closet, with dimensions 226 x 212 x 56 cm, 3 compartments (90, 45, 90 cm), two double doors and one single door / an Honor 90, Android system, with 6.7'' display, 256GB physical storage and 8GB RAM, 200MP back camera and 50MP front camera / a light blue, 100% cotton T-shirt, in your size].*
Please indicate the option you would choose[1]:

Characteristics	**Option A** 	**Option B** 
Conditions	New	Second-hand in excellent conditions
Warranty	Yes	No
Seller	Online store	Physical store
Price	€1.200	€900
	☐ Option A	☐ Option B

[1] In this task, all the attributes of the two options are randomized. Respondents see six pairs of choices with random attributes, keeping constant the product.

D.4 Vignette

Please, read the following text and fill in the required fields.[2]

Imagine you need to buy a *[T-shirt / refrigerator / closet / smartphone]*. You find the model you are interested in, *[a light blue, 100% cotton T-shirt, in your size / an LG with 384ℓ capacity, measures 59 × 203 × 68 cm, and energy class A++ / a wooden closet, with dimensions 226 × 212 × 56 cm, 3 compartments (90, 45, 90 cm), two double doors and one single door / an Honor 90, Android system, with 6.7" display, 256GB physical storage and 8GB RAM, 200MP back camera and 50MP front camera]*, second-hand. The product is in *[excellent condition / good condition / acceptable condition]*, has been sanitized, and you can pick it up *[at an affordable price / free of charge]*.

1. Referring to the situation described, what is the probability that you decide to take the product? Indicate a number from 0 to 100.

   [New page]

2. At each row of the following table you find two opposite statements, A and B. We ask you to indicate whether you agree more with statement A or B.

   "If I decided to take the product described..."

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel I am in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

[New page]

[2] In this task, respondents only see one vignette.

3. We asked the previous question to a representative sample of the Italian population. In your opinion, how did other respondents from the same macro-region answer to this question? Remember that, by "macro-region", we mean the five geographic areas: North-west (Liguria, Lombardia, Piemonte, Valle d'Aosta), North-East (Emilia-Romagna, Friuli-Venezia Giulia, Provincia Autonoma di Trento, Provincia Autonoma di Bolzano, Veneto), Center (Lazio, Marche, Toscana, Umbria), South (Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia), and Islands (Sardegna, Sicilia). Note also that, for this question, you may receive an additional reward of €4, as follows: for each row, if your answer matches the one given most frequently in the sample of your macro-region, you will receive €1. We will draw 400 participants to whom we will give this additional reward.

   [Same table of statements as in the previous question is shown here]

   [New page]

4. At each row of the following table you find two opposite statements, A and B. We ask you to indicate whether you agree more with statement A or B.

   "If I decided to take the product described, others would think that..."

	Agree completely with A	Agree more with A than B	Agree more with B than A	Agree completely with B
A = I would feel that I was in financial distress B = I would feel that I was using money responsibly	☐	☐	☐	☐
A = I would feel embarrassment B = I would have the approval of others	☐	☐	☐	☐
A = I would feel that I had not made a sustainable choice B = I would feel that I had made a sustainable choice	☐	☐	☐	☐
A = I would feel disgusted in using it B = I would not feel disgusted in using it	☐	☐	☐	☐

D.5 Likert scale

- Do you go to physical thrift stores?
  - No, I do not go to them
  - Yes, I go to them, only to buy
  - Yes, I go to them, just to sell
  - Yes, I go to them both to sell and to buy
- Do you use online platforms for second-hand goods?
  - No, I do not use them
  - Yes, I use them, only to buy
  - Yes, I use them, just to sell
  - Yes, I use them both to sell and to buy
- Have you ever exchanged second-hand products with family, friends or acquaintances?
  - No, I have never exchanged them
  - Yes, I have only donated them
  - Yes, I have only received them
  - Yes, I have both donated and received them
- Have you ever bought, or would you buy, the following second-hand products?

  (For each product category, Likert scale: 1 = No, I would not buy them; 2 = Yes I have, but I would not buy them again; 3 = Yes I would, but I have never bought them; 4 = Yes, I have already bought them)

  a. Clothing
  b. Electronic devices (cellphone, TV, computer, etc.)
  c. Furniture (sofa, table, chairs, etc.)
  d. Household appliances (oven, dishwasher, etc.)

- Concerning CLOTHING, how important or unimportant do you consider the following factors to be when considering whether or not to buy a second-hand product?[3]

  (For each statement, Likert scale: 1 = very irrelevant; 2 = irrelevant; 3 = neither relevant nor irrelevant; 4 = relevant; 5 = very relevant)

  a. Hygiene considerations and health risks
  b. The quality of clothing
  c. The appearance of clothing
  d. What would others (friends, relatives, web users) think of my choice
  e. The shame I might feel about buying secondhand clothing
  f. The price difference between a new and a secondhand piece of clothing
  g. The lack of CO2 emissions associated with the production of a new piece of clothing
  h. Previous purchasing experiences (mine and others)
  i. The availability of stores where secondhand clothing can be purchased
  j. The habit of buying new clothing

- Concerning ELECTRONIC DEVICES (cell phone, TV, computer, etc.), how important or unimportant do you consider the following factors to be when considering whether or not to buy a second-hand product?

  (For each statement, Likert scale: 1 = very irrelevant; 2 = irrelevant; 3 = neither relevant nor irrelevant; 4 = relevant; 5 = very relevant)

  a. Hygiene considerations and health risks
  b. The quality of the device
  c. Its use by the previous owner
  d. The performance and memory of the device
  e. The appearance/design of the device
  f. What would others (friends, relatives, web users) think of my choice
  g. The shame I might feel about buying a secondhand electronic device
  h. The price difference between a new and a secondhand electronic device
  i. The lack of CO2 emissions associated with the production of a new electronic device
  j. Previous purchasing experiences (mine and others)
  k. The availability of stores where you can buy secondhand electronic devices
  l. The habit of buying new electronic devices

[3] In this section of the survey, respondents only see two of the four product categories randomly.

- Concerning FURNITURE (sofa, table, chairs, etc.), how important or unimportant do you consider the following factors to be when considering whether or not to buy a second-hand product?

  (For each statement, Likert scale: 1 = very irrelevant; 2 = irrelevant; 3 = neither relevant nor irrelevant; 4 = relevant; 5 = very relevant)

  a. Hygiene considerations and health risks
  b. The quality of the furniture
  c. Its use by the previous owner
  d. The appearance/design of the furniture
  e. What would others (friends, relatives, web users) think of my choice
  f. The shame I might feel about buying secondhand furniture
  g. The price difference between a new and a secondhand piece of furniture
  h. The lack of CO2 emissions associated with the production of a new piece of furniture
  i. Previous purchasing experiences (mine and others)
  j. The availability of stores where to buy secondhand furniture and the ease of transportation
  k. The habit of buying new furniture

- Concerning HOUSEHOLD APPLIANCES (oven, dishwasher, etc.), how important or unimportant do you consider the following factors to be when considering whether or not to buy a second-hand product?

  (For each statement, Likert scale: 1 = very irrelevant; 2 = irrelevant; 3 = neither relevant nor irrelevant; 4 = relevant; 5 = very relevant)

  a. Hygiene considerations and health risks
  b. The quality of the household appliance
  c. Its use by the previous owner
  d. The appearance/design of the household appliance
  e. The energy efficiency of the household appliance
  f. What would others (friends, relatives, web users) think of my choice
  g. The shame I might feel about buying a secondhand household appliance
  h. The price difference between a new and a secondhand household appliance
  i. The lack of CO2 emissions associated with the production of a new household appliance
  j. Previous purchasing experiences (mine and others)
  k. The availability of stores where to buy secondhand appliances and ease of transportation
  l. The habit of buying new household appliances

D.6 Quiz task

D.6.1 Control

[No text nor questions; respondent advances to next section]

D.6.2 Treatments

The production of new goods impacts the environment. Buying second-hand goods avoids the production of new goods and consequently reduces the environmental impact of their production process. In the next question, you will find out the extent of the reduction in environmental impact due to the purchase of second-hand clothing.

D.6.2.1 CO2

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 emission you can avoid by not producing a new one?
It is possible to avoid an amount of kilograms of CO2 emitted (kg CO2e) equal to: **(numeric field)**
[New page]

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

D.6.2.2 Energy

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 piece of clothing, do you know how much usage of energy you can avoid by not producing a new one?
It is possible to avoid an amount of kilowatt-hour (kWh) equal to: **(numeric field)**
[New page]

The exact estimate is about 17kWh, or about 170 hours of usage of a 100-watt bulb.

D.6.2.3 Water

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

10ℓ = 2 minutes of open kitchen sink

When you buy a second-hand piece of clothing, do you know how much usage of water you can avoid by not producing a new one?
It is possible to avoid an amount of liters (ℓ) equal to: **(numeric field)**
[New page]

The exact estimate is about 340 liters of water or about 17 minutes of open sink.

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

- ○ I am more inclined
- ○ I am equally inclined
- ○ I am less inclined

D.7 Voucher Task (MPL)

Below, we present 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.

D.7.1 WTA

Voucher A H&M	**Voucher B** MERCATINO	**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

D.7.2 WTP

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

[New page]

You can freely choose to participate or not participate in the lottery by checking one of the following boxes.

- ○ I want to participate in the lottery
- ○ I do not want to participate in the lottery

D.8 Final questions

D.8.1 Need for uniqueness

Indicate how much you agree with the following statements, on a scale of (1) Completely disagree to (5) Completely agree.

D.8.1.1 Creative choice

a. I often combine possessions in such a way that I create a personal image for myself that can't be duplicated.

b. I often try to find a more interesting version of run-of-the-mill products because I enjoy being original.

c. I actively seek to develop my personal uniqueness by buying special products or brands

d. Having an eye for products that are interesting and unusual assists me in establishing a distinctive image.

D.8.1.2 Unpopular choice

a. When it comes to the products I buy and the situations in which I use them, I have often broken customs and rules

b. I have often violated the understood rules of my social group regarding what to buy or own

c. I have often gone against the understood rules of my social group regarding when and how certain products are properly used

d. I enjoy challenging the prevailing taste of people I know by buying something they wouldn't seem to accept

D.8.1.3 Avoidance of similarity

a. When a product I own becomes popular among the general population, I begin using it less

b. I often try to avoid products or brands that I know are bought by the general population

c. As a rule, I dislike products or brands that are customarily purchased by everyone

d. The more commonplace a product or brand is among the general population, the less interested I am in buying it

D.8.2 Pro-environmental behavior scale

a. How often do you switch off standby modes of appliances or electronic devices? *("Never"(1), "Rarely"(2), "Sometimes"(3), "Usually"(4), "Always"(5))*

b. How often do you cut down on heating or air conditioning to limit energy use? *("Never"(1), "Rarely"(2), "Sometimes"(3), "Usually"(4), "Always"(5))*

c. How often do you limit your time in the shower in order to conserve water? *("Never"(1), "Rarely"(2), "Sometimes"(3), "Usually"(4), "Always"(5))*

d. How often do you wait until you have a full load to use the washing machine or dishwasher? *("Never"(1), "Rarely"(2), "Sometimes"(3), "Usually"(4), "Always"(5))*

e. During the past year have you volunteered or contributed money to an environmental, conservation, or wildlife protection group? *("Yes"(5), "No"(1))*

f. How often do you talk to others about their environmental behavior? *("Never"(1), "Rarely"(2), "Sometimes"(3), "Often"(4), "Constantly"(5))*

g. During the past year have you decreased the amount of meat you consume? *("Yes"(5), "No"(1), "I don't eat meat"(5))*

h. During the past year how often have you walked, cycled or used public transportation instead of driving *("Never"(1), "Occasionally"(3), "Frequently"(5))*

D.8.3 Additional socio-demographic characteristics

- Please indicate which figure your family's total monthly net income is closest to.
    - More than 10,000 euros
    - 5,000 to 10,000 euros
    - 3,000 to 5,000 euros
    - 2,000 to 3,000 euros
    - 1,500 to 2,000 euros
    - 1,000 to 1,500 euros
    - Less than 1000 euros
    - Don't know/Prefer not to answer
- What is your educational qualification?
    - Middle school or below
    - High School Diploma
    - University degree (bachelor's, master's) or equivalent
    - Ph.D. or equivalent
- What is your current occupation?
    - Self-employed
    - Employee
    - Worker
    - Unemployed
    - Other
- [If *Unemployed* or *Other* in the previous question] Would you say you are...
    - Homemaker
    - Student (full-time)
    - Retired person
    - Looking for a job
    - Other
- What province do you currently live in? (dropdown menu)
- Can you indicate the ZIP code of the city where you live? **(numeric field)**

- How would you define the place where you live?
    - A rural/country area
    - An urban area
- How many inhabitants does your city have?
    - less than 10,000
    - between 10,000 and 50,000
    - between 50,000 and 250,000
    - more than 250,000
- How many people does your household consist of, including yourself? **(numeric field)**
- Within your household, how many minors under the age of 18 are present? **(numeric field)**