Decide or Default? How Defaults, Information, and Financial Incentives Influence Sustainable Delivery Choices in a Field Experiment

Sanne Wolf[*1], **Paul Buijs**[1], **and Dirk Pieter van Donk**[1]

[1]***University of Groningen, Faculty of Economics and Business, Department of Operations***

Abstract

E-commerce delivery choices matter for environmental sustainability, yet checkout designs often steer consumers toward faster, less sustainable options. Research on shifting these choices has focused largely on information and financial incentives, with evidence drawn mainly from stated-preference and controlled settings whose findings may not carry over to actual checkout behavior. This study reports a large-scale field experiment with an e-commerce retailer, in which three commonly studied interventions are varied: (i) the preselection of a delivery option (default), (ii) the provision of sustainability information, and (iii) a small financial incentive. The empirical design allows identification of both the individual and combined effects of these interventions on the uptake of sustainable delivery. The results show that the sustainable default is by far the most effective intervention. Financial incentives and sustainability information both raise uptake only modestly. Interaction effects are predominantly negative, suggesting that interventions partly substitute for one another. Taken together, the pattern of effects is consistent with delivery choices being made with limited deliberation at checkout, where interventions requiring minimal effort are especially effective. For retailers, this implies that simple, low-cost changes to checkout design can substantially increase the uptake of sustainable delivery.

Keywords: Sustainable delivery, E-commerce, Choice architecture, Nudges, Field experiment

1. Introduction

Consider a consumer buying a pair of socks online as a Christmas gift for her father. At checkout, the webshop offers two delivery options: next-day delivery or delivery within three days. The slower option carries a small green leaf and the message: “By choosing this option, you lower the environmental impact of delivery". Would this be enough to make her wait two extra days? Would a 50-cent discount do more? And would any of this matter if the slower option were already preselected? These are the types of questions we aim to explore in this paper.

Despite the practical relevance of these questions, empirical evidence on whether and how consumers can be encouraged to choose the sustainable delivery option remains limited. The few studies on this topic mostly rely on controlled experiments and survey studies, and examine interventions that differ in the extent

[*]Corresponding author. Email: s.wolf@rug.nl

to which they require consumers to actively attend to and evaluate the delivery options at the moment of choice. Within this literature, studies have shown that providing sustainability information or offering financial incentives can increase uptake of the sustainable delivery option (Ignat and Chankov 2020, Agatz et al. 2021, Viet et al. 2023, Kokkinou et al. 2024, Dietl et al. 2024), but both these interventions require consumers to notice a message or price difference and weigh it against other aspects of the delivery options. Defaults work differently. By preselecting the sustainable delivery option, they can increase uptake without requiring consumers to first notice and weigh a message or price difference. Yet defaults have rarely been studied in sustainable e-commerce delivery, and the only three studies to date report conflicting results. Rai et al. (2021) and Cauwelier et al. (2025) find no effect, while Nijssen et al. (2023) show that preselecting the sustainable delivery option increases uptake from 7 percent to 27 percent.

The limited attention to defaults in sustainable e-commerce delivery is surprising, given that defaults have been extensively investigated in behavioral economics and rank among the most powerful tools for changing behavior (Jachimowicz et al. 2019). Strong default effects have been found across a variety of real-world settings, such as retirement savings, organ donation, and green electricity uptake (Madrian and Shea 2001, Johnson and Goldstein 2003, Pichert and Katsikopoulos 2008, Jachimowicz et al. 2019). At the same time, informational interventions have been found to produce small or null effects when tested in the field (Barabas and Jerit 2010), in contrast to the positive information effects consistently reported in the sustainable e-commerce delivery literature. Together, these insights raise the question of whether the current evidence on sustainable delivery choices, drawn almost entirely from controlled experiments and surveys, reflects how consumers actually behave at checkout.

To examine how consumers respond to interventions in a real e-commerce delivery setting, we conduct a field experiment that is, to our knowledge, the first of its kind in this domain. Several authors have called for such field experiments, noting that survey-based methods may not capture how consumers behave when the choice arises in a real purchase (Ignat and Chankov 2020, Agatz et al. 2021, Rai et al. 2021, Lee and Donohue 2026). Our field experiment ran at a Dutch e-commerce retailer, where customers at checkout were exposed to three interventions: sustainability information, a financial incentive, and a default, presented either alone or in combination. In total, the experiment produced 4,504 observations of delivery choices over eight months. Overall, we find that the default is by far the most powerful intervention, strongly raising uptake of sustainable delivery in every condition in which it appears. By contrast, sustainability information has little effect and financial incentives produce only a modest shift compared to the effect of the default. We furthermore find that adding information or a financial incentive once the sustainable delivery option is already the default yields little beyond what the default achieves alone.

This study makes two main contributions. First, it provides field-experimental evidence on the relative effectiveness of behavioral interventions for sustainable e-commerce delivery choices. In doing so, it provides the first such evidence in this setting and answers multiple recent calls across the literature. Second, our results show that the relative effectiveness of interventions is different compared to what prior literature suggests. Interventions that require consumers to notice and weigh information or price differences are comparatively weak, while the default, which requires neither, is far more effective. We argue that this pattern suggests a broader principle. An intervention works only if the processing it demands is actually present in the choice environment.

Our findings also carry direct societal implications. The environmental cost of last-mile delivery is

substantial (Jaller and Pahwa 2020, Holguín-Veras et al. 2018), yet our results suggest that a low-cost, easily implementable change—preselecting the sustainable delivery option at checkout—may be among the most effective tools available to mitigate it. If more consumers accept slower delivery, carriers gain flexibility to consolidate shipments and optimize fleet use, reducing both emissions and operational costs (Zeelenberg et al. 2026). Checkout design, in this sense, is not merely an interface decision but a lever retailers can use to lower the environmental footprint of their operations.

2. Theoretical Background

Research on sustainable e-commerce delivery has primarily examined two types of behavioral interventions: sustainability information and financial incentives. The controlled experiments or survey studies that investigated these interventions generally report positive effects. A third intervention, making the sustainable delivery option the default, has been extensively studied in behavioral economics and ranks among the most effective interventions in other domains, yet it has received remarkably little attention in this context. We consider this an important omission not only because defaults may be effective, but also because the conditions under which delivery choices are made in practice may systematically favor defaults over the interventions that have been studied most so far. To develop this argument, we first describe how each intervention operates and what the existing evidence shows, then characterize the decision environment of checkout, and finally derive predictions about the relative and combined effectiveness of these three interventions.

2.1. Defaults, Sustainability Information, and Financial Incentives

The following section describes how each of the three interventions is expected to work and reviews the relevant evidence from both the broader literature and the sustainable delivery literature specifically.

Defaults preselect one option while leaving all alternatives available. Rather than persuading consumers to choose differently, they work by changing the structure of the choice itself, making one option the starting point without restricting what the consumer can ultimately select (Thaler and Sunstein 2009). Their effectiveness is commonly understood to operate through three mechanisms: ease, endowment, and endorsement (Dinner et al. 2011, Jachimowicz et al. 2019), as discussed below.

First, the default is said to operate through *ease* as accepting a preselected option requires less effort and deliberation than actively overriding it (Choi et al. 2003, Johnson and Goldstein 2003, Waisman et al. 2026). When an option is preselected, consumers may not evaluate every available alternative separately, but instead simply assess whether the default itself is satisfactory (Johnson et al. 2012, Jachimowicz et al. 2019). This reflects the broader tendency to accept the first option that meets a sufficient threshold of acceptability, that is, the so-called *satisficing* choice (Simon 1955, Caplin et al. 2011). A preselected option in this case meets that threshold simply by being present, acceptable, and requiring no action to accept (Payne et al. 1993). When consumers have no strong reason to deviate, this path of least resistance becomes especially attractive (Choi et al. 2003).

Second, a default can also work through *endowment* as it becomes a psychological reference point, so that switching away from it feels like giving something up rather than gaining an alternative (Kahneman et al. 1991). The endowment effect is closely tied to so-called status quo bias, as people tend to weigh the costs of changing their situation more heavily than the costs of remaining in it, making the act of switching

feel more effortful and consequential than simply proceeding (Samuelson and Zeckhauser 1988). Because losses tend to loom larger than equivalent gains, deviations feel costlier than they would in a neutral choice between two equally presented options (Kahneman et al. 1991).

Third, a default may influence choices through *endorsement*, where the preselected option is read as an implied recommendation that the choice architect considers as appropriate or advisable, a cue consumers often draw on when forming their own judgment (McKenzie et al. 2006, Dinner et al. 2011). This signal carries particular weight when consumers lack strong prior preferences, leaving them receptive to external guidance (Krijnen et al. 2017, Madrian and Shea 2001, Choi et al. 2003, Blumenstock et al. 2018). The same signaling can also operate as a descriptive norm. Where an implied recommendation conveys what the platform advises, a descriptive norm conveys what is standard or expected, so that consumers may read a preselected option as reflecting what most people typically choose, which can make the sustainable option feel like the socially accepted choice (Everett et al. 2015, Ebeling and Lotz 2015).

Importantly, none of these mechanisms requires consumers to be convinced that the sustainable option is a better choice. Rather, they operate by making the preselected option easy to accept and mentally demanding to override (Smith et al. 2013, Jachimowicz et al. 2019). Evidence from other domains confirms that these mechanisms — ease, endowment, and endorsement — translate into substantial behavioral effects. Madrian and Shea (2001), for instance, show that automatically enrolling employees in a pension plan dramatically increases participation, with many retaining the default contribution rate long after enrollment. Johnson and Goldstein (2003) demonstrate substantially higher organ donation rates in countries where donation is the default. Pichert and Katsikopoulos (2008) find that a majority of consumers stick with a green electricity default even when a cheaper alternative is available. Surprisingly, despite this consistent evidence across domains, defaults remain almost unexamined in sustainable delivery, and the little evidence that exists is mixed (Rai et al. 2021, Nijssen et al. 2023, Cauwelier et al. 2025).

Sustainability information works by making the environmental consequences of a choice visible and salient, giving consumers a reason to prefer the more sustainable option (Rai et al. 2021, Viet et al. 2023). For such information to affect behavior, consumers must be motivated and able to carefully process it (Petty and Cacioppo 1986). In practice, this means consumers must work through a behavioral chain: noticing the message, interpreting its relevance, assigning weight to sustainability relative to competing attributes such as speed and cost, and then actively selecting the sustainable option (Bettman et al. 1998). This chain depends on consumers treating the delivery choice as a decision worth careful consideration and on their willingness to trade off environmental impact against delivery speed or other practical factors. In the context of sustainable delivery, information is commonly provided in the form of eco-labels, CO_2 statistics, or brief explanatory messages. Evidence from controlled experiments and surveys indicates that consumers are indeed able and motivated to engage with such information, and that these information-based interventions are effective in these hypothetical decision environments (e.g., Ignat and Chankov 2020, Agatz et al. 2021, Viet et al. 2023, Kokkinou et al. 2024, Amaya et al. 2025, Cauwelier et al. 2025, Lee and Donohue 2026).

Financial incentives lower the price of the sustainable option — or increase the price of the non-sustainable alternatives — giving consumers a direct economic reason to choose it. Acting on such a financial incentive still requires consumers to notice the price difference and judge whether the saving is worth it, but this is a comparatively easy evaluation. Because price is among the most salient and readily processed attributes at the point of purchase (Thaler 1985, Bordalo et al. 2013), a financial incentive

registers with relatively little effort. Unlike sustainability information, it does not require consumers to attend to or interpret an environmental message. In the sustainable delivery context, financial incentives have been studied primarily in the form of delivery cost reductions for the more sustainable delivery option (Agatz et al. 2021, Dietl et al. 2024, Kokkinou et al. 2024), and the evidence suggests they are effective.

Taken together, the three types of interventions differ in the degree to which they depend on consumers processing or deliberating on information at the moment of choice. Sustainability information requires the longest reasoning chain. Financial incentives rely on a shorter chain but still require some active processing. Defaults require the least active processing, because consumers can 'choose' the sustainable option by doing nothing. This distinction in how much active processing is required for each intervention becomes important once we move from survey and experimental settings to real checkout settings.

2.2. Decisions in Real Checkouts

A common concern in behavioral studies is that findings from controlled research settings may not accurately predict how interventions perform in the real world (Levitt and List 2007). Controlled experiments or surveys often create conditions that differ from natural decision environments in several aspects: the focal decision is isolated; participants know their choices are being studied; attention to the task is heightened; and participants may be self-selected (Levitt and List 2007, Al-Ubaydli and List 2015, Lonati et al. 2018). When investigating interventions in the field, these conditions often change, and with them, effects may shrink or even reverse, because an intervention only works if the mechanisms it relies on are present in the choice environment where the decision occurs (Shadish 2002).

The same logic applies to delivery choices in e-commerce. The checkout where delivery choices are actually made differs from the survey and controlled settings used to study them. Surveys present the delivery choice as the focal task, isolated from the broader transaction. In real checkouts, by contrast, finalizing the order is the focal task, and the delivery choice is only one subtask among cart confirmation, address entry, discount codes, and payment. The delivery decision therefore competes for attention with these other subtasks (Bettman et al. 1998), and attention may be further divided among the parallel activities online shoppers engage in (Atalay et al. 2017). Surveys can also make participants aware that their responses are being observed, encouraging more deliberate evaluation than a real purchase would and, where sustainability is the visible focus, biasing responses toward what is perceived as desirable behavior (Levitt and List 2007). Survey participants are furthermore typically self-selected individuals willing to engage carefully with the task, whereas consumers at checkout arrive intending to complete a purchase and often approach delivery choices habitually, with little active re-evaluation (Cai et al. 2021).

Taken together, these differences imply that the conditions under which informational interventions and financial incentives are effective in surveys, namely focused attention, deliberate evaluation, and task engagement, are less present in a real checkout (Levitt and List 2007). The checkout environment may therefore favor interventions that do not depend on these conditions, such as defaults. More generally, whether an intervention can influence the delivery choice in a real checkout depends on whether its mechanism can operate under divided attention, habitual processing, and limited task engagement.

2.3. Hypothesis Development

The preceding subsection described how the checkout environment is subject to limited deliberation. This shapes how each intervention is likely to perform and provides the basis for the hypotheses developed below. Among the three interventions, the default is expected to be the most effective in the checkout environment. As established in Section 2.1, defaults operate through three mechanisms — ease, endowment, and endorsement. Each of these mechanisms is well-suited to a setting in which consumers are unlikely to engage in careful deliberation, as a preselected sustainable option can guide the delivery choice simply by being present and acceptable (Johnson and Goldstein 2003, Sunstein 2015b). The signal it carries, that this is potentially the recommended or socially normal choice, further reinforces this effect, particularly when consumers have no a priori strong preferences about a delivery option (McKenzie et al. 2006, Dinner et al. 2011, Ebeling and Lotz 2015, Everett et al. 2015).

Sustainability information, by contrast, is expected to be less effective in the checkout environment. It requires consumers to notice the message, interpret it, assign weight to sustainability relative to other attributes, and incorporate it into their decision (Bettman et al. 1998). These steps are more likely to occur in surveys and controlled experiments — where the delivery choice is the focal task and attention is undivided — than in a live checkout, where the consumer's primary goal is to complete the transaction (Levitt and List 2007, Lonati et al. 2018). This is consistent with evidence from other domains showing that informational interventions that produce effects in controlled settings frequently yield weaker or null effects in the field (Barabas and Jerit 2010).

Financial incentives are less sensitive to the limited processing conditions of checkout than sustainability information, but they remain dependent on active processing. A financial incentive is more concrete and more salient than a sustainability message, particularly because consumers already attend to cost when completing a purchase (Thaler 1985, Bordalo et al. 2013). However, the consumer still needs to notice that the sustainable option is cheaper and decide that the financial incentive sufficiently compensates for foregoing the standard, usually faster or more convenient, delivery option. This makes financial incentives more likely to affect choice than sustainability information in a setting where deliberation is limited, but less effective than defaults, which require no active processing at all.

In summary, interventions differ in how much active processing they require, and this is what determines how they are expected to perform in the field. Defaults are well-suited to the checkout environment because the mechanisms through which they operate, ease, endowment, and endorsement, do not depend on careful deliberation: they can shape the delivery choice before any active evaluation begins. Sustainability information and financial incentives, by contrast, work only if consumers engage with the intervention and deliberate on the options, a condition less reliably met in the checkout environment than in controlled experiments or surveys. We therefore expect defaults to outperform sustainability information and financial incentives in the field. As can be concluded from the previous, our prediction does not rest on the assumption that consumers are indifferent to sustainability or price, but on the observation that the intervention demanding the least active processing is best suited to a setting where the delivery choice is marked by divided attention and limited deliberation.

Hypothesis 1. *Making sustainable delivery the default option increases its uptake more than either providing sustainability information or a financial incentive.*

The comparison between financial incentives and sustainability information follows the same logic.

Both require active processing, but financial incentives rely on a shorter and more straightforward reasoning chain (Bettman et al. 1998). Consumers do not need to interpret an environmental claim or link a delivery choice to broader sustainability outcomes. They only need to notice that one option — the sustainable one — is financially beneficial. Furthermore, because price is among the most salient and readily processed attributes at the point of purchase (Thaler 1985, Bordalo et al. 2013), we expect financial incentives to be more likely to affect real delivery choices than sustainability information. Sustainability information may still influence some consumers, particularly those already motivated to consider environmental impact, but its effectiveness depends more strongly on attention and interpretation — conditions that may not be present in real checkout environments (Barabas and Jerit 2010, Levitt and List 2007).

Hypothesis 2. *Providing a financial incentive increases the uptake of the sustainable delivery option more than sustainability information.*

The above logic also has implications for the effects of combined interventions. Behavioral economics research suggests that when interventions are combined, each cannot be assumed to retain the effect it would have in isolation, as they may reinforce each other or substitute for one another when they operate through overlapping mechanisms or when one intervention reduces the room for another to have an effect (Fang et al. 2023, Rodemeier and Löschel 2025). In sustainable e-commerce delivery, this is particularly relevant because checkout interventions are often easy to combine: a retailer can preselect an option, add an environmental label, and attach a discount to the same delivery alternative.

When the sustainable option is already the default, additional interventions have less room to operate. The default resolves the delivery choice for many consumers simply by being present and acceptable, not because they are indifferent to sustainability, but because in a low-deliberation environment, the mechanisms described above make proceeding with the preselected option the natural response (Johnson and Goldstein 2003, Jachimowicz et al. 2019). Crucially, the default does not move a random subset of consumers. It guides those who lack a strong prior preference and would otherwise have engaged only minimally, and it holds those who already prefer the sustainable option and are content to stay on it. For this second group, the default already secures the sustainable choice, so providing information has no preference left to change. The consumers who remain on the non-default option are therefore disproportionately those with an active preference for faster, non-sustainable delivery (Dinner et al. 2011). Information and financial incentives can only influence this residual group, but it is by construction the group least responsive to a nudge because consumers who have actively rejected the preselected option are unlikely to reverse that choice in response to an environmental label or a discount. Therefore, once a default is in place, information and financial incentives act on a smaller and more resistant population, so their additional effect is expected to be limited.

Hypothesis 3. *When the sustainable option is the default, sustainability information or a financial incentive raises uptake by less than it would if the sustainable option were not the default.*

3. Research Setting and Experimental Design

3.1. Setting and Delivery Choice Context

We conducted a field experiment in collaboration with a Dutch e-commerce retailer, implementing our behavioral interventions on the firm's website for customers in the Netherlands. This retailer specializes in

premium socks and related apparel, with orders fulfilled through a third-party parcel carrier. The number of orders during the experimental period (April 29, 2025, to December 31, 2025) exhibits clear seasonality, with higher order volumes in colder months, lower volumes in warmer months, and pronounced peaks around gift-giving occasions such as Christmas. Throughout the experimental period, the website and checkout interface remained unchanged, except for a platform-wide delivery price adjustment on November 15, which raised the shipping cost for both delivery options from €3.99 to €4.99.[1]

On the website of the retailer, the delivery choice module appears on the cart page and is visible without scrolling (see Figure 1). Within this module, customers face two home-delivery options presented as radio buttons: a fast option ("Standard delivery: ordered before 13:00, sent today") and a slower option ("Postponed: max. 3 workdays"). In this study, we define the slower option as the sustainable delivery option[2]. The rationale is that accepting a longer delivery window gives carriers greater operational flexibility, which Zeelenberg et al. (2026) show can support more sustainable delivery.

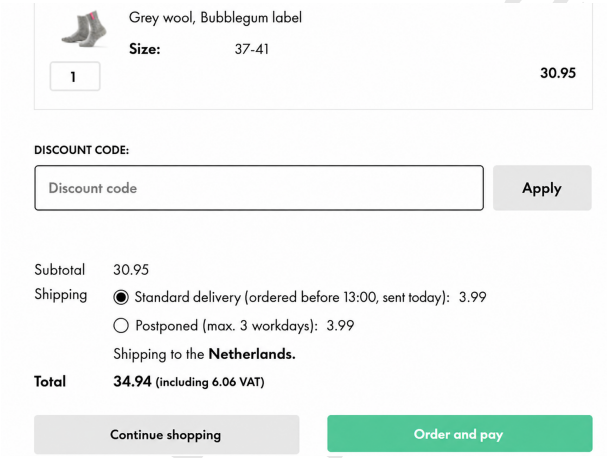


Figure 1: Checkout of e-commerce company

Notes: Screenshot of the checkout interface, translated from Dutch to English.

3.2. Interventions

In our field experiment, we implemented the interventions described above that are commonly investigated in the existing literature: defaults, sustainability information, and financial incentives (see Table 1). All interventions were applied within the retailer's checkout; the rest of the shopping experience remained unchanged.

First, the default intervention determined whether the sustainable or the standard option was preselected, and, due to the design of the website, the preselected option also appeared at the top of the list. Second, for the sustainability information intervention, we varied the description of the slow delivery option. In the baseline condition, no additional information was provided, and the option was labeled "Postponed delivery (max. 3 workdays)". In other conditions, the slow option was either labeled "Sustainable (max. 3 workdays)" or it was accompanied by a brief explanation: "Sustainable: The delivery driver is already in

[1] As a check, we included an indicator for observations after November 15; the results of this robustness check show that this change does not affect our main findings.

[2] We recognize that the sustainability of delivery options is debated in the literature and that parcel delivery remains environmentally burdensome overall. We therefore use "sustainable" in a relative sense, referring to the slower option as the comparatively more sustainable alternative.

your neighborhood (max. 3 workdays)". Third, for the financial incentive, we varied the discount on the sustainable delivery option across three levels: none, €0.50, and €1.00.

Table 1: Interventions and levels

Default	**Information**	**Financial incentive**
(1) Sustainable delivery option preselected (*Sustainable*)	(1) Postponed delivery: max. 3 workdays (*No info*)	(1) No incentive on sustainable delivery option (*€0*)
(2) Standard delivery option preselected (*Standard*)	(2) Sustainable: max. 3 workdays (*Label*)	(2) €0.50 incentive on sustainable delivery option (*€0.50*)
	(3) Sustainable: The delivery driver is already in the neighborhood: max. 3 workdays (*Explanation*)	(3) €1.00 incentive on sustainable delivery option (*€1*)

Note: The labels used in subsequent figures and tables are reported below each level in brackets and italics.

The experiment was conducted in two phases. In the first phase, we tested all combinations of the default intervention (two levels) and the information intervention (three levels), resulting in six treatments. This phase allows us to estimate the separate effects of defaults and information, as well as their interaction. In the second phase, we introduced financial incentives. We again varied the default setting and implemented three financial incentive levels. To reduce implementation costs, financial incentives were combined only with the "No-info" condition and the explanation information condition, and not with the simple label ("Sustainable (max. 3 workdays)"). We focus on these combinations because prior research suggests that simple labels are generally less effective than more explicit information (Nijssen et al. 2023, Viet et al. 2023). The second phase yielded eight additional treatments. Overall, this experimental design with 14 treatments allows us to estimate both main effects and selected interaction effects across interventions.

Finally, we implemented two additional treatments alongside the main experiment to provide a preliminary indication of the role of delivery time in shaping the probability of switching away from the default. In these treatments, the sustainable delivery option was extended from three to five delivery days and was tested both when preselected and when not preselected. As these treatments were not pre-registered, the resulting observations are excluded from the main analysis, although including them does not qualitatively change our results. We do include these observations in a post-hoc analysis.

3.3. Treatment Assignment and Implementation

In our research setting, the retailer required the delivery-choice module to remain uniform for all shoppers at any given time. This made individual-level randomization infeasible, a common constraint in operational field experiments where designs must be adapted to the realities of live systems (Gao et al. 2023). We therefore assigned treatments using time blocks, meaning that the same treatment was implemented for all shoppers within a given period, which in most cases was one calendar week. During periods of high traffic, we used shorter blocks to achieve a more balanced distribution of observations across treatments. In all cases, orders completed in the same time block were subject to the same treatment.

Treatments were assigned to time blocks rather than randomly across individual orders, so an estimated treatment effect could partly reflect temporal patterns in delivery preferences that happened to coincide

with that block. We addressed this through the design of the implementation schedule. The treatment sequence was set in advance, typically fixed about two months ahead, and implemented without mid-block adjustments. We also scheduled each treatment to span both weekdays and weekends, and most treatments to run across multiple seasons (spring/summer and autumn/winter), so that estimated effects are unlikely to be driven by day-of-week or seasonal variation alone (the full schedule is reported in Table 5, Appendix A). In the analysis, we control for time-varying factors that may influence delivery choice, including day of week, time of day, holidays, weather, and concurrent promotions.

3.4. Sample, Unit of Observation, and Data

Because the retailer only records completed transactions, the analysis focuses on delivery choices conditional on purchase completion. Our unit of observation is therefore the completed order. The final dataset contains 4,504 completed orders, distributed across the different treatments outlined above. We set a target of at least 100 orders per treatment in our pre-registration and met this threshold for all treatments, with a minimum of 106 orders per treatment. Statistical power depends on which two treatments are being compared, since they differ in sample size and in baseline uptake. Precision, therefore, varies across different comparisons.

The retailer provided an order-level dataset capturing the delivery choice and key transactional characteristics (for an overview see Appendix B). For each completed order, we observe the date and time of purchase, the delivery option chosen (standard or sustainable delivery), and the delivery fee charged. We furthermore observe the package type, indicating whether the parcel was delivered to the mailbox or to the front door. While the package type was not communicated to the consumer during checkout and is not stated elsewhere on the website, consumers may form expectations about the likely parcel type. Such expectations may affect delivery choice, since mailbox parcels do not require the recipient to stay home. We therefore control for the realized package type, assuming that consumers' expectations are reasonably aligned with the package type ultimately used. Furthermore, marketing activities may also influence delivery choice, hence, we control for whether a promotion, such as a discount code or a free product, was active (referred to as *promotion*). We additionally constructed an indicator for holiday and festive periods to account for seasonal gift-giving behavior that may affect delivery urgency (referred to as *urgency*). In addition, we control for daily temperature data to account for potential weather-related effects.

Our primary dependent variable is a binary indicator equal to 1 if the customer selected the sustainable delivery option and 0 if the customer selected the standard delivery option.

3.5. Ethics and Preregistration

Ethical review was conducted by the Institutional Review Board (IRB) of the University of Groningen, which reviewed the completed research data management plan and ethics checklist and issued a statement of no objection for the study [reference redacted for review]. The study was also preregistered on AsPredicted (Wharton Credibility Lab, University of Pennsylvania) prior to implementation. In line with typical digital field experiments, customers were not explicitly informed that they were part of a study.

4. Estimation Strategy and Results

The final sample included 4,504 observations across 14 treatments. Details on variables, descriptive statistics, and correlation can be found in Appendix B. Table 7 in Appendix B presents the proportion of orders completed with the sustainable delivery option selected per treatment, providing an overview of our main dependent variable.

4.1. Baseline Uptake

We estimate treatment effects using a logistic regression model that includes the interventions and a standard set of controls. This estimation allows us to apply maximum likelihood estimation to predict sustainable delivery uptake under different treatments. Given that interventions were assigned in time blocks, observations within a given block are not independent. We therefore report standard errors clustered at the time-block level. The estimated equation is

$$\begin{aligned} \ln\left[\frac{\Pr(S_i = 1)}{1 - \Pr(S_i = 1)}\right] = \alpha &+ \boldsymbol{\beta}_I^\top \text{Information}_i \\ &+ \boldsymbol{\beta}_P^\top \text{Incentive}_i \\ &+ \beta_D \, \text{Default}_i \\ &+ \boldsymbol{\gamma}^\top X_i \end{aligned} \tag{1}$$

where $S_i = 1$ indicates that consumer i completed the order with the sustainable delivery option selected. Thus, the left-hand side represents the log-odds that an order is completed with the sustainable delivery option selected. $Information_i$ is a vector capturing the information condition to which the consumer is exposed (no information, label, or explanation), $Incentive_i$ is a vector capturing the financial incentive level offered for the sustainable option (€0, €0.50, or €1.00), and $Default_i$ is an indicator equal to one when the sustainable option is the default and zero otherwise. The vector X_i contains the control variables included in the analysis. The controls include indicators for weekend and afternoon purchases, concurrent promotions, urgent scenarios, daily temperature, and package type (see Appendix B for further explanation). These controls account for predictable variation in purchasing conditions and marketing activity that might otherwise confound the estimated treatment effects.

Table 2 shows the results of our analysis. Model 0 includes only control variables and serves as a baseline. Model 1 presents the estimates of Equation (1) with the main independent variables and shows that sustainability information, financial incentives, and default interventions all have positive and statistically significant effects on the uptake of the sustainable delivery option[3].

As logit coefficients are not directly interpretable as effect sizes, we use post-estimation margins to translate the estimates from Equation (1) into predicted probabilities that an order is completed with the sustainable delivery option selected. To estimate the predicted probability for one intervention, we fix the other two interventions at their baseline level. For example, when examining information, we set the default to the standard option and the financial incentive level to zero and vary the focal intervention across its levels. For each level of an intervention, we estimate the predicted uptake of the sustainable

[3]Several controls change sign between Model 0 and Models 1–2. This is because variables such as Black Friday and promotion activity are concentrated within a small number of treatment blocks rather than distributed evenly across all fourteen conditions (see Appendix B, Table 7). Because we control for these variables directly rather than omitting them, this concentration is unlikely to bias the treatment estimates.

Table 2: Logistic Regression

	Model 0 *Sustainable delivery*	**Model 1** *Sustainable delivery*	**Model 2** *Sustainable delivery*
Main effects			
Default Sustainable		4.051***	4.873***
reference: no default		(0.192)	(0.229)
Information: Label		0.688**	0.763*
reference: no information		(0.301)	(0.419)
Information: Explanation		0.744***	1.337***
reference: no information		(0.163)	(0.278)
Financial Incentive: €0.50		1.058***	2.123***
reference: no incentive		(0.315)	(0.373)
Financial Incentive: €1.00		1.441***	2.339***
reference: no incentive		(0.281)	(0.568)
Interaction terms			
Default Sustainable × Label			0.112
			(0.432)
Default Sustainable × Explanation			-0.744**
			(0.317)
Default Sustainable × €0.50			-1.916***
			(0.358)
Default Sustainable × €1.00			-0.893**
			(0.391)
Explanation × €0.50			-0.418
			(0.379)
Explanation × €1.00			-1.083**
			(0.503)
Control variables			
Black Friday	-1.360***	0.339	0.370
reference: not ordered during Black Friday	(0.133)	(0.218)	(0.235)
Urgency	-0.040	-1.079***	-1.277***
reference: no urgency	(0.141)	(0.229)	(0.295)
Weekend order	0.620***	0.078	0.021
reference: ordered during the week	(0.070)	(0.113)	(0.103)
Afternoon	0.081	0.269***	0.290***
reference: ordered before 13:00	(0.065)	(0.096)	(0.087)
Promotion	-0.212***	0.537***	0.482**
reference: ordered when there was no active promotion	(0.077)	(0.205)	(0.204)
Temperature	0.004***	0.002	0.001
	(0.001)	(0.002)	(0.002)
Front door package	-0.254***	0.025	0.005
reference: mailbox package	(0.066)	(0.146)	(0.157)
Constant	-0.684***	-3.544***	-3.963***
	(0.118)	(0.262)	(0.348)
Model statistics			
Observations	4504	4504	4504
Pseudo R^2	0.046	0.459	0.471

Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

delivery option, using each observation's own values for the control variables, and then average these predicted probabilities across the sample. Figure 2 shows the expected sustainable delivery uptake per intervention. Because each intervention is evaluated from the same baseline, the figure is useful for comparing effectiveness across interventions.

The predicted probabilities show that when none of the interventions is active, the predicted uptake of the sustainable delivery option is only about 7%. This means that with the faster option preselected and the sustainable option carrying no label, explanation, or discount, the vast majority of customers leave the checkout with the standard, less sustainable delivery option. Compared against this baseline, the figure shows that all three interventions increase the uptake of the sustainable delivery option, but to differing degrees. Information and financial incentives lift uptake only modestly, leaving the sustainable option a minority choice. The default has a far larger effect, with a large majority accepting the sustainable delivery option.

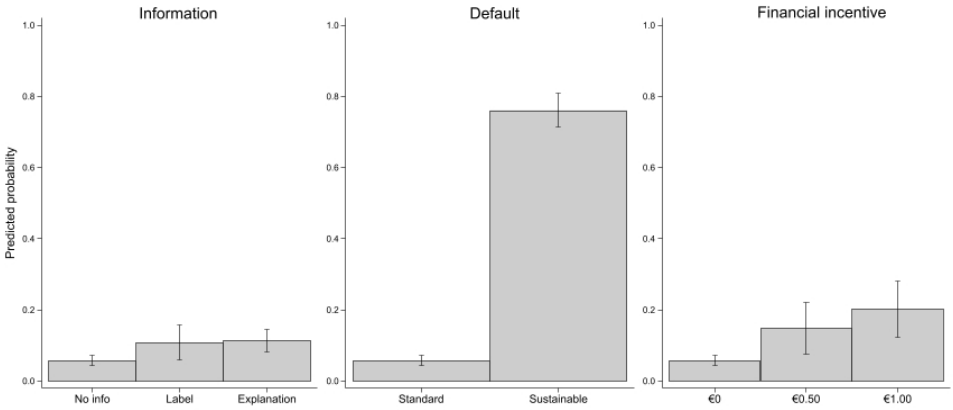


Figure 2: Average Predicted Uptake of the Sustainable Delivery Option

4.2. Relative Effectiveness

Hypothesis 1 predicted that the default raises uptake of the sustainable delivery option by more than either sustainability information or a financial incentive. Visually, Figure 2 already shows that the default is most effective by increasing the predicted uptake of the sustainable delivery option to around 75%. To test this relative effectiveness, we conduct pairwise comparisons among interventions (Newson 2013). The results confirm the ranking suggested by the figure and are consistent with Hypothesis 1 (see Table 3). The default raises sustainable delivery uptake significantly more than every information condition and every financial incentive condition, with differences of roughly 56 to 65 percentage points. Hypothesis 1 is therefore supported.

Hypothesis 2 predicted that financial incentives raise uptake by more than sustainability information. The estimates in Model 1 of Table 2 are consistent with this direction. The €0.50 and €1.00 incentives are both associated with higher predicted uptake than the information explanation condition, by 3.5 and 8.9 percentage points, respectively. In the pairwise comparisons (Table 3), however, neither difference is significant at the 5% level, though the €1.00 incentive is significant at the 10% level ($p = 0.062$). The direction of the point estimates thus suggests that financial incentives lead to higher sustainable delivery uptake than informational messages, and €1.00 is marginally significant, but we do not find

strong statistical support for Hypothesis 2.

Table 3: Pairwise Comparisons

Contrast	Difference	P-value
Default Sustainable vs. Information: Label	0.653	0.000
Default Sustainable vs. Information: Explanation	0.647	0.000
Default Sustainable vs. Financial incentive: €0.50	0.612	0.000
Default Sustainable vs. Financial incentive: €1.00	0.558	0.000
Financial incentive: €0.50 vs. Information: Explanation	0.035	0.417
Financial incentive: €1.00 vs. Information: Explanation	0.089	0.062

Notes: The "difference" column reports differences in predicted probabilities between interventions, with the first listed intervention always corresponding to the one associated with higher uptake of the sustainable delivery option.

4.3. Combined Effects

Hypothesis 3 predicts that when the sustainable option is the default, sustainability information or a financial incentive raises the uptake of the sustainable delivery option by less than it would if the sustainable option were not the default. We estimate an extended logit model that adds pairwise interactions among the default, sustainability information, and financial incentives to the main effects to test this. The interaction terms let the effect of one intervention depend on whether another is already in place, instead of assuming each intervention's effect stays the same regardless of which other interventions are also in place. Specifically, we estimate

$$\begin{aligned}\ln\left[\frac{\Pr(S_i=1)}{1-\Pr(S_i=1)}\right] = \alpha &+ \boldsymbol{\beta}_I^\top \text{Information}_i \\ &+ \boldsymbol{\beta}_P^\top \text{Incentive}_i + \beta_D\, \text{Default}_i \\ &+ \boldsymbol{\beta}_{IP}^\top (\text{Information}_i \times \text{Incentive}_i) \\ &+ \boldsymbol{\beta}_{DP}^\top (\text{Default}_i \times \text{Incentive}_i) \\ &+ \boldsymbol{\beta}_{ID}^\top (\text{Information}_i \times \text{Default}_i) \\ &+ \boldsymbol{\gamma}^\top X_i\end{aligned} \tag{2}$$

The results provide partial support for Hypothesis 3 (Model 2, Table 2). The interactions between the sustainable default and, respectively, the explanation, the €0.50 incentive, and the €1.00 incentive are all negative and statistically significant. For these interactions, the negative sign indicates that the incremental effect of sustainability information or a financial incentive is smaller once the sustainable option is already the default, supporting Hypothesis 3.

The other default interaction, (Default × Label), is not significant. This should not be read as evidence that the label and default combine independently. Instead, it may reflect limited precision or an effect too small to detect, particularly given the modest stand-alone effect of the label and the relatively small number of observations for these treatments.

We additionally observe a negative and significant interaction between the explanation and the €1.00 incentive. This combination does not involve the default and so falls outside Hypothesis 3, but the substitution pattern is interesting and consistent with Lee and Donohue (2026), who find that information

and financial incentives act as substitutes when both are present.

Taken together, the interaction estimates provide partial support for Hypothesis 3. In three out of four default interactions, we find significant negative effects, indicating that the effect of sustainability information or a financial incentive is smaller once the sustainable option is already the default. A caveat applies to the interpretation of these negative interactions, though. Because the default alone raises uptake to roughly 75%, there is limited room for a second intervention to produce a further increase. The negative interaction effects could therefore also partly reflect a mechanical ceiling, since the default already pushes uptake close to its maximum, leaving little room for other interventions to have an effect.

4.4. Post-Hoc Analysis

The analysis establishes that the default is a powerful lever for increasing the uptake of the sustainable delivery option. This strength, however, raises a question about how the default actually works. Our expectation for its effectiveness rests on the idea that defaults operate through ease, endowment, and endorsement, with consumers accepting the preselected option through a low-effort but still responsive process. Yet defaults can also work through inattention, with consumers staying on the preselected option regardless of what the alternative offers simply because they do not engage with the choice at all. If this second mechanism drives the effect we observe, the default would be exploiting inattention rather than guiding choice, raising concerns for consumer welfare and for the ethics of default-setting.

A first piece of evidence comes from urgent scenarios (Models 1 and 2 in Table 2), where consumers are significantly less likely to choose the slower delivery option. This suggests that when the package is urgent, consumers ensure they choose the faster delivery option. What our preceding analysis does not show, however, is whether people switch away from the default when the alternative simply becomes more attractive. To investigate this, we conduct a post-hoc analysis in which we redefine the dependent variable as an indicator for choosing the non-default option, equal to one when a consumer selects the non-default option and zero otherwise. We then relate switching away from the default to the relative attractiveness of the non-default option along two dimensions: its price and its delivery speed. We treat these as proxies for attractiveness under the standard assumptions that, all else equal, consumers prefer cheaper options and faster delivery. Specifically, we estimate

$$\begin{aligned} \ln\left[\frac{\Pr(ND_i = 1)}{1 - \Pr(ND_i = 1)}\right] = \alpha &+ \boldsymbol{\beta}_I^\top \text{Information}_i \\ &+ \beta_1 \,\text{Price}_i + \beta_2 \,\text{Time}_i \\ &+ \beta_D \,\text{Default}_i + \boldsymbol{\gamma}^\top X_i \end{aligned} \tag{3}$$

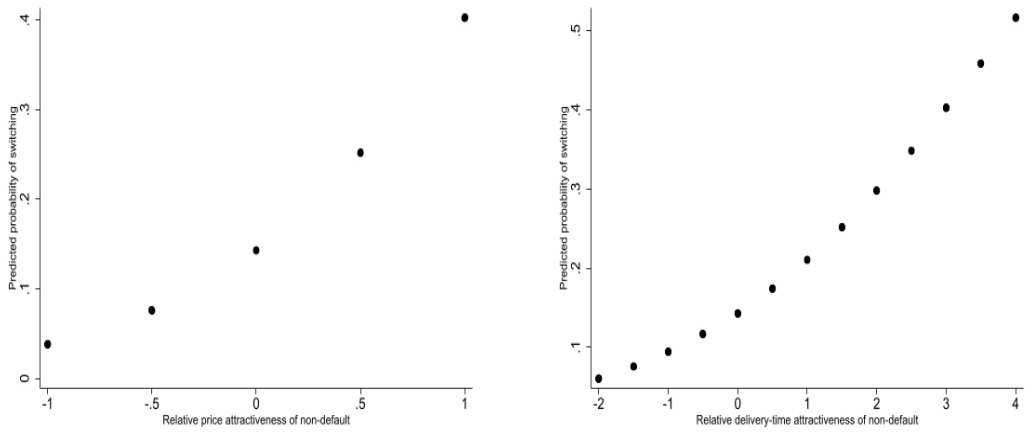
where ND_i indicates that consumer i selects the non-default delivery option. Thus, the left-hand side represents the log-odds of choosing the non-default delivery option. $Price_i$ captures the price advantage of the non-default option, measured as the default's delivery cost minus the delivery cost of the alternative option. A positive value means the non-default option is cheaper relative to the default, making it more financially attractive and switching more likely. $Time_i$ captures the delivery-time advantage of the non-default option, measured in days, where higher values indicate that the non-default option is faster relative to the default. A larger value, therefore, makes the non-default option more attractive and switching more likely. Similar to the other models, $Default_i$ is an indicator equal to one when the sustainable option is preselected and zero otherwise. This indicator is included as the likelihood of switching may

differ depending on which option is set as the default. X_i contains the same control variables as in the main analysis (for an overview, see Appendix B), and the model is estimated on the main sample using the same inference procedure as the core specifications.

Figure 3 plots the predicted probability of switching away from the default against two measures of how attractive the non-default option is: its price advantage and its delivery-time advantage. To trace switching across a wider range of delivery-time differences, the panels combine the main sample with additional observations where the non-default option was offered at five working days instead of three. Both panels reveal a clear positive relationship. Switching becomes more likely as the non-default option becomes cheaper and as it offers a greater speed advantage. Both price and delivery-time attractiveness also enter positively and significantly in the regression estimates (Appendix C). The relationship with delivery time is also visibly non-linear. Switching rises only gradually when the non-default offers a small speed advantage, but increases more steeply as that advantage grows, and the estimated coefficient on delivery-time attractiveness is considerably larger once the five-day observations are included (Model 2, Appendix C).

Aside from increased switching, Figure 3 also shows that switching never becomes a majority response. Even at the most attractive levels of the non-default option, the predicted probability of switching reaches only around 40 to 50 percent. In other words, even when the alternative is cheaper or faster, a substantial share of consumers remain with the default.

Taken together, these findings indicate that the default does not operate through inattention alone. At least a substantial share of consumers remain sensitive to the options and move away from the default when the alternative becomes sufficiently attractive, which is inconsistent with a process in which the preselected option is accepted without any processing. Interpreting the behavior of those who stay with the default is less straightforward. Some may remain with the default through a satisficing process, accepting it because it is good enough, while others may not engage with the choice at all and still others may stay deliberately, valuing the environmental benefit of the sustainable option even when the alternative is cheaper or faster. Because our analysis captures responsiveness only along price and delivery time, it cannot distinguish these cases.



(a) Price attractiveness (b) Time attractiveness

Figure 3: Predicted Probability of Switching Away From the Default

4.5. Robustness Checks

We conduct two additional robustness checks to assess whether our main findings are affected by the time-block treatment assignment or by potential spillovers across treatment blocks.

First, treatment was assigned across time blocks, not at the order level, making observations within the same time block non-independent. With only 36 time blocks, conventional cluster-robust standard errors can understate sampling variability and over-reject the null (Cameron et al. 2008, Roodman et al. 2019). We therefore re-assess the significance of our main estimates using Wild cluster bootstrap at the time-block level.[4] We use 9,999 replications and apply the procedure to all core specifications: the main-effects model, the interaction model, and the post-hoc switching analysis. Across nearly every specification, the bootstrap p-values are consistent with the baseline clustered inference. Two exceptions do not survive the stricter test: the main effect of the information label ($p = 0.142$) and the interaction between the default and the €1.00 incentive ($p = 0.073$). The label's insignificance is consistent with our broader finding that a bare label does little on its own. All other estimates relevant to our hypotheses retain their significance level (see Appendix D.1). We therefore find no indication that the main results are an artifact of overstated precision due to the limited number of time-block clusters.

Second, a potential concern is that exposure mismatch may arise across treatment blocks. Because treatment conditions were varied over time, some customers may have been exposed to one treatment but completed their purchase only after a new treatment became active. In our data, we observe only the time at which an order is completed, not when the checkout process was initiated. As a result, the treatment observed at the time of purchase may not correspond to the checkout environment initially seen by the customer, which could attenuate the estimated treatment effects. To investigate this concern, we conduct a robustness check in which we exclude orders placed during the first two days of each treatment block. The remaining sample consists of 2,639 observations. By re-estimating the analysis on this restricted sample, we focus on orders that are likely to always correspond to the checkout environment active at the time of purchase. Overall, the estimates in the restricted sample support our main findings, suggesting that exposure mismatch does not bias our results (see Appendix D.2). The default, the explanation condition, and the €1.00 incentive retain their sign, magnitude, and significance. Three weaker estimates lose precision in the restricted sample. The label and the €0.50 incentive weaken to the 10% level, and the interaction between the default and the €1.00 incentive loses significance. The restriction removes roughly 40% of observations, and these estimates concern treatments with relatively few orders, so reduced power is a likely explanation. The label and this interaction also weakened when using Wild cluster bootstrap, which is consistent with, though not proof of, these effects being genuinely small or absent.

5. Conclusion and Discussion

We conducted a field experiment to study how behavioral interventions can encourage the selection of the sustainable delivery option. Specifically, we compared the effectiveness of defaults, sustainability information, and financial incentives, as well as their interactions.

Three main findings emerge. First, setting the sustainable delivery option as the default has by far

[4]The procedure estimates the model under the null, then repeatedly flips the sign of each cluster's residuals at random (Rademacher weights) to build a bootstrap distribution for the test statistic. The observed statistic is compared to this distribution to obtain a p-value (Roodman et al. 2019).

the strongest effect on its uptake. Providing financial incentives and sustainability information both raise uptake only modestly, with a €1 discount showing a slightly larger, marginally significant, positive effect than sustainability information. Second, adding information or financial incentives on top of the default produces a negative interaction effect for nearly all intervention combinations, meaning that information and financial incentives are less effective when the sustainable delivery option is already the default. Third, we find that when the non-default delivery option becomes sufficiently attractive, consumers become more likely to switch away from the default.

5.1. Theoretical Implications

Our findings offer new insight into how behavioral interventions shape sustainable e-commerce delivery choices. Much of the existing literature on this topic implicitly assumes that consumers evaluate the available options deliberately, weighing attributes such as speed, cost, and environmental impact before choosing. Under that assumption, a consumer who deliberately compares options would act on their preferences regardless of which option was preselected. Those for whom sustainability outweighed the value of faster delivery would choose the slower, more sustainable option, while those for whom speed prevailed would choose the fast option — whether or not it was the default. By giving consumers clearer grounds for such a comparison, sustainability information should then be effective, as most earlier studies found (e.g., Agatz et al. 2021, Dietl et al. 2024, Amaya et al. 2025). Our results reverse this ranking. The default is by far the most influential intervention, while providing sustainability information and financial incentives had much smaller effects.

Our results, therefore, challenge the deliberative model of choice that much of this literature assumes. If consumers chose on the merits of the delivery options, the default should not have mattered as much, yet it was decisive, while sustainability information and financial incentives, which gave them more to deliberate upon, did little. This points to a choice in which the default itself carries substantial weight, rather than a choice made by weighing speed, cost, and environmental impact. This insight, however, does not mean that consumers mindlessly accept whatever the default presents. Our results suggest that consumers do respond to the relative attractiveness of the delivery options, moving away from the default as the non-default option becomes more attractive. Furthermore, our results also show that in urgent scenarios, many consumers switch to the fast delivery option, suggesting that when consumers need their package fast, they actively switch away from the slow delivery. Accordingly, the underlying choice process is most naturally read as *satisficing*, in which consumers accept the preselected option as long as it meets a threshold of acceptability rather than searching for the overall optimum. This also explains why adding sustainability information or a financial incentive on top of a default does little. If the preselected option already clears the threshold of acceptability, the choice is effectively settled, and an extra reason to choose it has little left to change. Consumers therefore switch away from the default only when it does not meet their threshold of acceptability or when an alternative becomes clearly superior (Caplin et al. 2011, Simon 1955, Stüttgen et al. 2012). Under such a decision process, the order in which options are presented matters because each option is judged against an acceptability threshold rather than against the other options, giving the first option encountered a structural advantage (Stüttgen et al. 2012).

The interpretation that consumers satisfice rather than deliberate also bears on the ethical debate surrounding defaults (e.g., Bovens 2009, Sunstein 2015a, Schubert 2017, Lemken 2024). The standard objection is that defaults exploit inattention to steer consumers into options they would not otherwise

choose. If what matters ethically is whether consumers end up with an option they find acceptable, a satisficing process meets that standard. Under satisficing, consumers stay with the default because it clears their threshold of acceptability, not because they have failed to notice the alternative, and they leave once the default no longer clears it. Our switching results are consistent with this, since consumers move away from the default as the alternative becomes more attractive. It is important to note, however, that in our field experiment, a large share of consumers stay with the default regardless of the alternative. In these cases, we cannot distinguish whether these consumers stay because they accept the default or because they did not notice the delivery option. This distinction opens a direction for future research where the exact underlying choice mechanisms surrounding defaults can be further investigated. In any case, we want to note that, in our context, the ethical defense of the default only holds as long as leaving the default is easy. When the delivery choice is buried among many other choices, consumers may stay even when the default falls short of what they would accept.

Taken together, our findings carry a broader implication. It is well established that intervention effectiveness does not simply hold across contexts (Kosters and Van der Heijden 2015, Hummel and Maedche 2019), though this has mostly been studied across populations and domains, and largely in the lab. Our field study results extend this idea. In the same checkout, under identical conditions, a structural default and an attention-based message produced very different effects. The diverging results therefore suggest that content alone does not explain intervention effectiveness. How much deliberation a setting demands from the consumer matters too, alongside who is nudged and where. We offer this idea as an interpretation rather than a settled result, and we encourage future research to test how widely this holds across other settings and intervention types. If it does hold more broadly, it means that designing for impact begins not with the intervention itself but with considering the conditions under which the choice is made.

5.2. Practical Implications

From a broader sustainability perspective, the most sustainable pair of socks is still the one already in one's drawer. However, for new purchases that do occur, our findings show that e-commerce retailers can substantially influence the environmental impact of delivery. More specifically, we show that the uptake of sustainable delivery options can be strongly increased if e-commerce retailers make the sustainable option the default option. Importantly, doing so is relevant not only from a sustainability perspective but also from an operational one. Shifting demand toward slower delivery options can provide carriers with more flexibility to consolidate shipments and optimize fleet use, reducing both emissions and operational costs (Jaller and Pahwa 2020, Zeelenberg et al. 2026). Checkout design thus provides firms with a tool to improve both environmental performance and the efficiency of delivery operations.

To assess the relevance of our results for practice, it is important to consider whether and what interventions retailers currently use to encourage more sustainable delivery choices. We therefore examined the delivery-choice interfaces of 100 large e-commerce retailers in the Netherlands, recording whether each used any intervention at checkout and, if so, which type (see Appendix E for the full method). This gives a picture of current practice against which the effectiveness we document can be compared. The pattern we observe runs counter to the effectiveness we find. Of the 100 retailers, 38 offered no comparatively more sustainable delivery option at checkout. Among the 62 retailers that did, financial incentives were the most common intervention, used by 32. Sustainability information was far rarer,

provided by 16, and only 8 made the sustainable option the default, while 23 used no intervention at all. This suggests that many retailers either do not recognize that checkout design can shift delivery choices or, if they do, assume consumers respond mostly to financial incentives. Preselection, which our results indicate is the strongest lever, is rare even among the retailers that could apply it. Firms may therefore be underutilizing a tool that could improve both the environmental performance and the operational flexibility of their delivery, at little cost and effort.

Some retailers may hesitate to use defaults, whether out of ethical concerns about undue steering (Lemken 2024) or out of concern that customers who overlook the preselection end up dissatisfied. Yet our analysis of current practice shows that the use of information and financial incentives is limited as well, and that even among retailers offering a more sustainable option, a substantial share use no intervention to encourage it. Many firms are therefore leaving the operational and environmental benefits of sustainable delivery unrealized, not because the available tools fail but because those tools go completely unused. Our results speak directly to this. All interventions we tested raised uptake of the sustainable delivery option, although the bare label's effect was small and not robust, suggesting that retailers have meaningful scope to reduce the environmental harm associated with e-commerce deliveries. Therefore, by moving from passively listing delivery options to deliberately shaping how those options are presented, firms can advance both the sustainability and the operational flexibility of their delivery operations.

These observations from current practice come from the Dutch market, and the specific interventions retailers use need not generalize to other e-commerce markets, since they partly reflect local conditions such as competitive norms and regulation. The relative effectiveness of interventions we document, however, is less likely to be context-specific. Defaults outperform sustainability information and financial incentives in our setting because the delivery choice likely receives limited deliberation. This feature of the choice process is not specific to Dutch consumers, so we expect the same ordering wherever the delivery decision elicits similar low deliberation. We treat this insight as an interpretation rather than a tested claim, and note that effect sizes may vary by product type, especially when delivery speed is more central to the purchase decision.

5.3. Limitations and Future Research

This study has several limitations, each of which points to a specific direction for future research.

First, while allowing us to observe delivery choices as they are actually made, our field setting limited the control we could exert over the study design. Treatments could not be randomized at the individual level but were instead implemented for all customers within a given time block. The main threat this raises is that a treatment's effect could be confounded with temporal patterns that coincide with when it was run. We addressed this through the design of the implementation schedule, fixing the treatment sequence in advance, spanning weekdays and weekends and multiple seasons, and controlling for time-varying factors such as holidays, weather, and promotions. Furthermore, we used Wild cluster bootstrap to address the concern that inference was distorted by the small number of time-block clusters. These measures mitigate but do not fully remove the constraints of time-block assignment. Individual-level randomization would allow sharper causal inference, which a fully randomized checkout experiment could provide.

Second, our interpretation of the decision-making process rests on observed choices rather than on direct evidence of how those choices were made. Even though our effect estimates do not depend on this interpretation, our account of the underlying process, that choices are made with limited deliberation and

that satisficing characterizes the choice process, is not tested empirically. The switching results provide indirect support, since consumers move away from the default when the alternative becomes sufficiently attractive, indicating at least some processing, but more direct measures of attention would allow a sharper test. Fisher (2023) shows that cursor-tracking data can capture when particular attributes begin to influence a decision, and such data, together with checkout dwell times, could distinguish consumers who actively accept the default from those who remain with it without processing the alternatives. The retailer in our study does not record this information. Future work would benefit from embedding dwell-time logging or mouse-tracking in the checkout, linking process measures to the same choice outcomes we study here. A related limitation concerns the default itself. On the retailer's website, the preselected option was always shown at the top of the list, and this could not be changed. As a result, our estimation cannot separate the effect of preselection from a position effect. Future work that varies order and preselection independently could establish how much each contributes to the default effect we observe.

Third, our information conditions captured only a few of the many ways environmental information could be conveyed. We tested a bare sustainability label and a brief explanatory message, but not, for instance, quantified carbon savings or other framings. This matters because Lee and Donohue (2026) and Cauwelier et al. (2025) show that the effectiveness of information depends heavily on how the environmental benefit is communicated. Our finding that information is comparatively weak, therefore, applies to the simple messages we tested, and stronger or differently framed information might perform better. Future work could examine which types of information are most effective in a live checkout.

Finally, our findings come from a single retailer in one product category, which limits their generalizability. Socks are typically low in delivery urgency, though they are also bought in more time-sensitive situations, such as gift-giving. Even so, consumers here may be relatively more willing to accept slower delivery than in more time-sensitive categories such as groceries, and the magnitude of our default effect is likely to some degree tied to this low-urgency context. Therefore, our results are likely generalizable to other low-urgency contexts where very fast delivery is not crucial, but less informative of intervention effectiveness in high-urgency product categories. Future research would benefit from investigating how behavioral interventions work for those product categories where fast delivery is more important. Such a follow-up study could establish whether our pattern of effects generalizes beyond low-urgency goods.

Acknowledgments

We would like to sincerely thank Noa, Jeroen, and Peter-Paul from Wuunder for their extensive support in implementing the experiment, their collaborative spirit, and the many valuable brainstorming sessions, and Perry and Soscha from Soxs for their enthusiasm, willingness to think along with us, and for allowing us to conduct the experiment in their company's checkout.

References

Agatz N, Fan Y, Stam D (2021) The impact of green labels on time slot choice and operational sustainability. *Production and Operations Management* 30(7):2285–2303.

Al-Ubaydli O, List JA (2015) On the generalizability of experimental results in economics. Fréchette GR, Schotter A, eds., *Handbook of Experimental Economic Methodology*, 420–462 (Oxford University Press).

Amaya J, Encarnacion T, Cantillo V (2025) Sustainable last mile delivery alternatives: Influencing factors and

willingness to use. *Transportation Research Part D: Transport and Environment* 139:104574.

Atalay AS, Bodur HO, Bressoud E (2017) When and how multitasking impacts consumer shopping decisions. *Journal of Retailing* 93(2):187–200.

Barabas J, Jerit J (2010) Are survey experiments externally valid? *American Political Science Review* 104(2):226–242.

Bettman JR, Luce MF, Payne JW (1998) Constructive consumer choice processes. *Journal of Consumer Research* 25(3):187–217.

Blumenstock J, Callen M, Ghani T (2018) Why do defaults affect behavior? Experimental evidence from Afghanistan. *American Economic Review* 108(10):2868–2901.

Bordalo P, Gennaioli N, Shleifer A (2013) Salience and consumer choice. *Journal of Political Economy* 121(5):803–843.

Bovens L (2009) The ethics of nudge. Grüne-Yanoff T, Hansson SO, eds., *Preference Change: Approaches from Philosophy, Economics and Psychology*, 207–219 (Springer).

Cai L, Yuen KF, Xie D, Fang M, Wang X (2021) Consumer's usage of logistics technologies: Integration of habit into the unified theory of acceptance and use of technology. *Technology in Society* 67:101789.

Cameron AC, Gelbach JB, Miller DL (2008) Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics* 90(3):414–427.

Caplin A, Dean M, Martin D (2011) Search and satisficing. *American Economic Review* 101(7):2899–2922.

Cauwelier K, Rai HB, Mommens K (2025) Thinking fast, slow delivery: Nudging sustainable choices among Gen Z consumers. *Research in Transportation Economics* 114:101657.

Choi JJ, Laibson D, Madrian BC, Metrick A (2003) Optimal defaults. *American Economic Review* 93(2):180–185.

Dietl M, Voigt S, Kuhn H (2024) From rush to responsibility: Evaluating incentives on online fashion customers' willingness to wait. *Transportation Research Part D: Transport and Environment* 133:104280.

Dinner I, Johnson EJ, Goldstein DG, Liu K (2011) Partitioning default effects: Why people choose not to choose. *Journal of Experimental Psychology: Applied* 17(4):332–341.

Ebeling F, Lotz S (2015) Domestic uptake of green energy promoted by opt-out tariffs. *Nature Climate Change* 5(9):868–871.

Everett JA, Caviola L, Kahane G, Savulescu J, Faber NS (2015) Doing good by doing nothing? The role of social norms in explaining default effects in altruistic contexts. *European Journal of Social Psychology* 45(2):230–241.

Fang X, Goette L, Rockenbach B, Sutter M, Tiefenbeck V, Schoeb S, Staake T (2023) Complementarities in behavioral interventions: Evidence from a field experiment on resource conservation. *Journal of Public Economics* 228:105028.

Fisher G (2023) Measuring the factors influencing purchasing decisions: Evidence from cursor tracking and cognitive modeling. *Management Science* 69(8):4558–4578.

Gao Y, Li M, Sun S (2023) Field experiments in operations management. *Journal of Operations Management* 69(4):676–701.

Holguín-Veras J, Encarnación T, González-Calderón CA, Winebrake J, Wang C, Kyle S, Herazo-Padilla N, Kalahasthi L, Adarme W, Cantillo V, Yoshizaki H, Garrido R (2018) Direct impacts of off-hour deliveries on urban freight emissions. *Transportation Research Part D: Transport and Environment* 61:84–103.

Hummel D, Maedche A (2019) How effective is nudging? A quantitative review on the effect sizes and limits of empirical nudging studies. *Journal of Behavioral and Experimental Economics* 80:47–58.

Ignat B, Chankov S (2020) Do e-commerce customers change their preferred last-mile delivery based on its sustainability impact? *The International Journal of Logistics Management* 31(3):521–548.

Jachimowicz JM, Duncan S, Weber EU, Johnson EJ (2019) When and why defaults influence decisions: A meta-analysis of default effects. *Behavioural Public Policy* 3(2):159–186.

Jaller M, Pahwa A (2020) Evaluating the environmental impacts of online shopping: A behavioral and transportation approach. *Transportation Research Part D: Transport and Environment* 80:102223.

Johnson EJ, Goldstein D (2003) Do defaults save lives? *Science* 302(5649):1338–1339.

Johnson EJ, Shu SB, Dellaert BG, Fox C, Goldstein DG, Häubl G, Larrick RP, Payne JW, Peters E, Schkade D, Wansink B, Weber E (2012) Beyond nudges: Tools of a choice architecture. *Marketing Letters* 23(2):487–504.

Kahneman D, Knetsch JL, Thaler RH (1991) Anomalies: The endowment effect, loss aversion, and status quo bias. *Journal of Economic Perspectives* 5(1):193–206.

Kokkinou A, Quak H, Mitas O, Mandemakers A (2024) Should I wait or should I go? Encouraging customers to make the more sustainable delivery choice. *Research in Transportation Economics* 103:101388.

Kosters M, Van der Heijden J (2015) From mechanism to virtue: Evaluating nudge theory. *Evaluation* 21(3):276–291.

Krijnen JM, Tannenbaum D, Fox CR (2017) Choice architecture 2.0: Behavioral policy as an implicit social interaction. *Behavioral Science & Policy* 3(2):1–18.

Lee Y, Donohue K (2026) Nudging green-but-slow shipping choices in online retail. *Management Science* URL http://dx.doi.org/10.1287/mnsc.2023.00239.

Lemken D (2024) Options to design more ethical and still successful default nudges: A review and recommendations. *Behavioural Public Policy* 8(2):349–381.

Levitt SD, List JA (2007) What do laboratory experiments measuring social preferences reveal about the real world? *Journal of Economic Perspectives* 21(2):153–174.

Lonati S, Quiroga BF, Zehnder C, Antonakis J (2018) On doing relevant and rigorous experiments: Review and recommendations. *Journal of Operations Management* 64:19–40.

Madrian BC, Shea DF (2001) The power of suggestion: Inertia in 401 (k) participation and savings behavior. *The Quarterly Journal of Economics* 116(4):1149–1187.

McKenzie CR, Liersch MJ, Finkelstein SR (2006) Recommendations implicit in policy defaults. *Psychological Science* 17(5):414–420.

Newson RB (2013) Attributable and unattributable risks and fractions and other scenario comparisons. *The Stata Journal* 13(4):672–698.

Nijssen SR, Pijs M, van Ewijk A, Müller BC (2023) Towards more sustainable online consumption: The impact of default and informational nudging on consumers' choice of delivery mode. *Cleaner and Responsible Consumption* 11:100135.

Payne JW, Bettman JR, Johnson EJ (1993) *The adaptive decision maker* (Cambridge University Press).

Petty RE, Cacioppo JT (1986) The elaboration likelihood model of persuasion. Berkowitz L, ed., *Advances in Experimental Social Psychology*, volume 19, 123–205 (Academic Press).

Pichert D, Katsikopoulos KV (2008) Green defaults: Information presentation and pro-environmental behaviour. *Journal of Environmental Psychology* 28(1):63–73.

Rai HB, Broekaert C, Verlinde S, Macharis C (2021) Sharing is caring: How non-financial incentives drive sustainable e-commerce delivery. *Transportation Research Part D: Transport and Environment* 93:102794.

Rodemeier M, Löschel A (2025) Information nudges, subsidies, and crowding out of attention: Field evidence from energy efficiency investments. *Journal of the European Economic Association* 23(4):1397–1430.

Roodman D, Nielsen MØ, MacKinnon JG, Webb MD (2019) Fast and wild: Bootstrap inference in Stata using boottest. *The Stata Journal* 19(1):4–60.

Samuelson W, Zeckhauser R (1988) Status quo bias in decision making. *Journal of Risk and Uncertainty* 1(1):7–59.

Schubert C (2017) Green nudges: Do they work? are they ethical? *Ecological Economics* 132:329–342.

Shadish WR (2002) Revisiting field experimentation: Field notes for the future. *Psychological Methods* 7(1):3–18.

Simon HA (1955) A behavioral model of rational choice. *The Quarterly Journal of Economics* 69(1):99–118.

Smith NC, Goldstein DG, Johnson EJ (2013) Choice without awareness: Ethical and policy implications of defaults. *Journal of Public Policy & Marketing* 32(2):159–172.

Stüttgen P, Boatwright P, Monroe RT (2012) A satisficing choice model. *Marketing Science* 31(6):878–899.

Sunstein C (2015a) The ethics of nudging. *Yale Journal on Regulation* 32(2):413 – 450.

Sunstein CR (2015b) *Choosing not to choose: Understanding the value of choice* (Oxford University Press, USA).

Thaler R (1985) Mental accounting and consumer choice. *Marketing Science* 4(3):199–214.

Thaler RH, Sunstein CR (2009) *Nudge: Improving decisions about health, wealth, and happiness* (Penguin).

Twinkle, Centre for Market Insights (2024) *Twinkle100 2024* (Woerden, The Netherlands: BBP Media), ranking of the 300 largest online retailers in the Netherlands.

Viet NQ, de Leeuw S, van Herpen E (2023) The impact of social vs environmental sustainability information disclosure on consumer choice of delivery time with varying sustainability concerns. *International Journal of Physical Distribution & Logistics Management* 53(11):26–52.

Waisman RM, Derksen T, Häubl G (2026) How default choice architecture impacts downstream behavior: A taxonomy, theoretical framework, and research agenda. *Consumer Psychology Review* 9(1):31–50.

Zeelenberg MJ, Kilic OA, Buijs P (2026) From willingness to wait to sustainable impact: Smoothing e-commerce delivery volume peaks. *Transportation Research Part D: Transport and Environment* 156:105349.

Appendix

A. Treatment Overview and Schedule

Table 4: Treatment Overview

		Interventions	
Treatment	**Information Level**	**Default**	**Financial Incentive**
Treatment 1 (T1)	Information label	Sustainable option	No incentive
Treatment 2 (T2)	Information label	Standard option	No incentive
Treatment 3 (T3)	Information explanation	Sustainable option	No incentive
Treatment 4 (T4)	Information explanation	Standard option	No incentive
Treatment 5 (T5)	No information	Sustainable option	No incentive
Treatment 6 (T6)	No information	Standard option	No incentive
Treatment 7 (T7)	No information	Sustainable option	€0.50 incentive
Treatment 8 (T8)	No information	Standard option	€0.50 incentive
Treatment 9 (T9)	Information explanation	Sustainable option	€0.50 incentive
Treatment 10 (T10)	Information explanation	Standard option	€0.50 incentive
Treatment 11 (T11)	No information	Sustainable option	€1.00 incentive
Treatment 12 (T12)	No information	Standard option	€1.00 incentive
Treatment 13 (T13)	Information explanation	Sustainable option	€1.00 incentive
Treatment 14 (T14)	Information explanation	Standard option	€1.00 incentive
Treatment 15 (T15)	Information explanation	Standard option	No incentive
Treatment 16 (T16)	Information explanation	Sustainable option	No incentive

Note. This table summarizes which interventions were active in each treatment condition. Treatments 15 (41 observations) and 16 (143 observations) are excluded from the main analysis and included only in the post-hoc analysis, as in these conditions, the sustainable option involved a delivery time of 5 working days instead of 3.

Table 5: Treatment Schedule

	Week number																																		
Treatment	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	1
Treatment 1	■	■																																		
Treatment 2			■			■							■		■																						
Treatment 3				■			■	■											■				■		■			■				■				
Treatment 4					■									■		■				■				■			■				■		■	■		
Treatment 5									■			■					■				■					■										
Treatment 6										■	■							■				■							■		■				■	■
Treatment 7																	■				■															
Treatment 8																		■				■														
Treatment 9																			■				■		■											
Treatment 10																				■				■									■			
Treatment 11																										■										
Treatment 12																													■				■			
Treatment 13																												■								
Treatment 14																											■						■			
Treatment 15																																		■		
Treatment 16																																				

Note 1. A filled cell indicates that the treatment was active at some point during that week. Treatments commonly started on Tuesday 9 A.M. and ended the week after on Tuesday 9 A.M. We fill the cell of the week in which the treatment started.

Note 2. In weeks 47, 50 and 51, we changed treatment more frequently due to the high number of orders (Black Friday and Christmas). In these weeks, treatments were on for 1, 2, or 3 days. We highlight these treatments by filling the cells with a lighter shade of gray.

Note 3. From the start of the financial-incentive treatments (week 34), two treatments ran in parallel at any given time. The reason is that the retailer waives shipping costs for orders above €50, so no financial incentive could be applied to those orders. Customers with orders above €50, therefore, saw the same treatment as others, but without the financial incentive component, while customers with orders of €50 or below saw the full treatment, including the incentive.

B. Data

B.1. Variable Overview

Table 6: Overview of Variables

Variable	Description	Measurement
Dependent variable		
Sustainable Choice	Indicates whether the sustainable delivery option was selected	1 = Sustainable delivery was selected 0 = Standard delivery was selected
Treatment variables (main analysis)		
Default	Indicates whether the sustainable option was the default option	1 = Sustainable default 0 = Standard default
Information	Indicates whether and what type of sustainability information was shown at checkout with the sustainable delivery option	2 = Information explanation shown 1 = Information label shown 0 = No information shown
Financial Incentive	Indicates whether and how much of a financial incentive was offered for the sustainable delivery option	2 = €1 incentive 1 = €0.50 incentive 0 = No incentive
Treatment variables (post-hoc analysis)		
Price Attractiveness	Indicates the relative price attractiveness of the non-default delivery option compared with the default delivery option	Computed as the delivery cost of the default option minus that of the non-default delivery option.
Delivery Time Attractiveness	Indicates the relative delivery-time attractiveness of the non-default delivery option compared with the default delivery option	Computed as the delivery time of the default option minus that of the alternative delivery option.
Control variables		
Afternoon	Indicates whether the order was placed before or after 13:00	1 = Ordered after 13:00 0 = Ordered before 13:00
Black Friday	Indicates whether the order was placed during the Black Friday period	1 = Ordered during Black Friday period (28/11–30/11) 0 = Otherwise
Front door package	Indicates whether the order was delivered in the mailbox or to the front door	0 = Mailbox package 1 = Front door package
Promotion	Indicates whether there was a promotion active when the order was placed	1 = Promotion was active 0 = No promotion was active
Temperature	Average daily temperature	Continuous variable measured in tenths of a degree Celsius
Urgency	Indicates whether an order was placed in the three days leading up to an event when time pressure is likely. Included events are Christmas, Sinterklaas (a Dutch gift-giving tradition in early December), Mother's Day, and Father's Day	1 = Order placed during an urgent period 0 = Otherwise
Weekend	Indicates whether the order was completed during the weekend	1 = Ordered during the weekend 0 = Ordered during a weekday

Note. This table provides an overview of the dependent variable, treatment variables, and control variables used in the analysis.

B.2. Descriptive Statistics

Table 7: Descriptive Statistics by Treatment

Variable		T1	T2	T3	T4	T5	T6	T7	T8	T9	T10	T11	T12	T13	T14	Overall
Observations	N	170	121	573	1255	470	942	125	106	122	121	150	120	120	109	4504
Sustainable choice	Mean	0.89	0.07	0.83	0.13	0.83	0.03	0.81	0.22	0.84	0.34	0.93	0.28	0.93	0.28	0.40
	SD	0.32	0.25	0.38	0.34	0.38	0.18	0.40	0.41	0.36	0.48	0.25	0.45	0.26	0.45	0.49
Black Friday	Mean	0.00	0.00	0.00	0.34	0.00	0.04	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.10
	SD	0.00	0.00	0.00	0.47	0.00	0.20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.31
Urgency	Mean	0.26	0.00	0.14	0.09	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.05
	SD	0.44	0.00	0.35	0.28	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.22
Weekend	Mean	0.07	0.16	0.42	0.40	0.39	0.21	0.10	0.22	0.30	0.30	0.50	0.23	0.47	0.16	0.32
	SD	0.26	0.37	0.49	0.49	0.49	0.41	0.31	0.41	0.46	0.46	0.50	0.42	0.50	0.36	0.47
Afternoon	Mean	0.70	0.59	0.60	0.61	0.60	0.67	0.52	0.51	0.55	0.48	0.64	0.53	0.58	0.63	0.61
	SD	0.46	0.49	0.49	0.49	0.49	0.47	0.50	0.50	0.50	0.50	0.48	0.50	0.50	0.48	0.49
Promotion	Mean	0.97	0.50	0.32	0.71	0.72	0.87	0.56	0.30	0.00	0.24	0.66	0.82	0.47	0.51	0.65
	SD	0.17	0.50	0.47	0.45	0.45	0.33	0.50	0.46	0.00	0.43	0.48	0.39	0.50	0.50	0.48
Temperature	Mean	13.11	16.43	9.57	8.09	9.93	7.43	9.65	12.67	12.63	12.71	9.87	6.93	6.43	7.89	9.12
	SD	3.41	2.33	5.44	4.46	4.86	3.75	6.81	2.12	2.09	2.94	1.63	3.04	4.45	2.81	4.74
Front door package	Mean	0.32	0.20	0.44	0.46	0.53	0.51	0.02	0.05	0.01	0.05	0.02	0.05	0.07	0.02	0.37
	SD	0.47	0.40	0.50	0.50	0.50	0.50	0.15	0.21	0.09	0.22	0.14	0.22	0.25	0.13	0.48

Note. The table reports means and standard deviations by treatment (T1–T14) and for the pooled sample (Overall, N = 4,504). Temperature is reported in degrees Celsius, converted from the raw measure recorded in tenths of a degree.

B.3. Correlation Table

Table 8: Correlation Matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Black Friday	1.00	-0.08	0.20	-0.02	0.25	-0.05	0.07	-0.16	-0.27	0.26	-0.17
(2) Urgency		1.00	-0.08	0.01	0.02	-0.06	-0.02	-0.01	0.07	0.18	-0.12
(3) Weekend order			1.00	-0.01	0.18	-0.14	0.05	0.08	0.07	0.12	-0.01
(4) Afternoon				1.00	0.06	-0.04	0.01	0.01	-0.02	-0.03	-0.04
(5) Promotion					1.00	-0.51	0.16	-0.12	-0.19	-0.24	-0.14
(6) Temperature						1.00	-0.11	0.11	0.15	0.01	0.01
(7) Front door package							1.00	-0.08	-0.06	-0.02	-0.34
(8) Sustainable choice								1.00	0.73	0.02	0.21
(9) Default sustainable									1.00	-0.04	0.15
(10) Information										1.00	-0.07
(11) Financial incentive											1.00

Note. This table reports pairwise correlations for the main analysis variables. Column numbers correspond to the row variables.

C. Post-Hoc Analysis

Table 9: Logistic Regression: Switching to Non-Default Option

	Model 1	**Model 2**
	Chose non-default option	*Chose non-default option*
Main effects		
Price attractiveness of non-default	1.430***	1.463***
	(0.252)	(0.243)
Delivery-time attractiveness of non-default	0.277***	0.488***
	(0.097)	(0.085)
Default Sustainable	0.151	-0.355
reference: standard is default	(0.341)	(0.324)
Information: Label	-0.340	-0.445
reference: no information	(0.277)	(0.273)
Information: Explanation	0.238	0.200
reference: no information	(0.217)	(0.205)
Control variables		
Black Friday	0.953***	0.978***
reference: not ordered during Black Friday	(0.223)	(0.222)
Urgency	0.343	0.331
reference: no urgency	(0.372)	(0.372)
Weekend order	-0.147	-0.193
reference: ordered during the week	(0.135)	(0.135)
Afternoon	-0.083	-0.129
reference: ordered before 13:00	(0.110)	(0.105)
Promotion	-0.339	-0.250
reference: ordered when there was no active promotion	(0.244)	(0.232)
Temperature	-0.001	-0.000
	(0.002)	(0.002)
Front door package	-0.308*	-0.442**
reference: mailbox package	(0.167)	(0.215)
Constant	-1.726***	-1.427***
	(0.349)	(0.377)
Model statistics		
Observations	4504	4688
Pseudo R^2	0.056	0.073

Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D. Robustness Analysis

D.1. Robustness Check 1: Wild Cluster Bootstrap

Table 10: Wild Cluster Bootstrap Inference

Model	Variable	Coef.	SE	p (bootstrap)
Main effects model				
	Default sustainable	4.051	0.192	0.000
	Information: Label	**0.688**	**0.301**	**0.142**
	Information: Explanation	0.744	0.163	0.001
	Financial Incentive: €0.50	1.058	0.315	0.004
	Financial Incentive: €1.00	1.441	0.281	0.000
Interaction model				
	Default Sustainable	4.873	0.229	0.000
	Information: Label	0.763	0.419	0.136
	Information: Explanation	1.337	0.278	0.000
	Financial Incentive: €0.50	2.123	0.373	0.000
	Financial Incentive: €1.00	2.339	0.568	0.003
	Default Sustainable × Label	0.112	0.432	0.818
	Default Sustainable × Explanation	-0.744	0.317	0.027
	Default Sustainable × €0.50	-1.916	0.358	0.000
	Default Sustainable × €1.00	**-0.893**	**0.391**	**0.073**
	Explanation × €0.50	-0.418	0.379	0.303
	Explanation × €1.00	-1.083	0.503	0.050
Switching model				
	Price attractiveness	1.430	0.252	0.000
	Time attractiveness	0.277	0.097	0.027
	Default sustainable	0.151	0.341	0.666

Note 1. This table reports coefficient estimates, cluster-robust standard errors, and Wild cluster bootstrap p-values. Bold coefficients indicate estimates that were significant at the 5% level in our main specifications, but are no longer significant based on bootstrap p-values.
Note 2. The **main effects model** should be compared with Model 1 in Table 2, the **interaction model** should be compared with Model 2 in Table 2, and the **switching model** should be compared with Model 1 in Table 9 in Appendix C.

D.2. Robustness Check 2: Treatment Spillovers

Table 11: Logistic Regression on Restricted Sample

	Model 0 *Sustainable delivery*	**Model 1** *Sustainable delivery*	**Model 2** *Sustainable delivery*
Main effects			
Default Sustainable		4.077***	4.942***
reference: no default		(0.303)	(0.240)
Information: Label		0.636*	0.665
reference: no information		(0.335)	(0.455)
Information: Explanation		0.876**	1.709***
reference: no information		(0.345)	(0.296)
Financial Incentive: €0.50		0.797*	1.565***
reference: no incentive		(0.439)	(0.539)
Financial Incentive: €1.00		1.452***	2.334***
reference: no incentive		(0.403)	(0.720)
Interaction terms			
Default Sustainable × Label			0.014
			(0.422)
Default Sustainable × Explanation			-1.233***
			(0.353)
Default Sustainable × €0.50			-1.575***
			(0.436)
Default Sustainable × €1.00			-0.066
			(0.686)
Explanation × €0.50			-0.130
			(0.468)
Explanation × €1.00			-1.746**
			(0.697)
Control variables			
Black Friday	-1.062	0.521	0.357
reference: not ordered during Black Friday	(0.826)	(0.366)	(0.283)
Urgency	-0.069	-1.069***	-1.227***
reference: no urgency	(0.955)	(0.395)	(0.431)
Weekend order	0.686*	0.007	-0.085
reference: ordered during the week	(0.414)	(0.162)	(0.145)
Afternoon	0.116	0.165	0.206*
reference: ordered before 13:00	(0.119)	(0.144)	(0.121)
Promotion	-0.140	0.482*	0.390
reference: ordered when there was no active promotion	(0.772)	(0.283)	(0.301)
Temperature	0.011**	0.003	0.002*
	(0.005)	(0.002)	(0.001)
Front door package	-0.261**	0.081	0.050
reference: mailbox package	(0.110)	(0.181)	(0.188)
Constant	-1.531**	-3.641***	-4.088***
	(0.725)	(0.317)	(0.312)
Model statistics			
Observations	2639	2639	2639
Pseudo R^2	0.073	0.493	0.507

Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E. Analysis of Intervention Use in Dutch Checkouts

To document how retailers currently present delivery options, we examined the checkouts of 100 large e-commerce retailers in the Netherlands. The retailers were drawn at random from an annual ranking of the 300 largest Dutch e-commerce retailers, compiled by the trade publication Twinkle and the Centre for Market Insights of the Amsterdam University of Applied Sciences (Twinkle and Centre for Market Insights 2024). These retailers account for the majority of online sales in the Netherlands, so the sample reflects the delivery interfaces most consumers encounter. Restaurants and other prepared-food delivery services were excluded before the random draw (4 in total), as the delivery choice in that setting involves very different considerations.

For each of the 100 retailers, we noted what delivery options they offer and whether they try to discourage or encourage either of the options through a default, sustainability information, or by providing a financial incentive. As only 16% of retailers label one delivery option as more sustainable than another, we classify the sustainable option ourselves. To classify the options, we follow the literature and treat as more sustainable the option that is delivered more slowly or within a longer delivery window, as this allows for shipment consolidation; the option delivered to a pick-up point or parcel locker, which consolidates orders at fewer stops and reduces failed deliveries; or the option delivered by bicycle or electric vehicle. In total, 62 out of 100 retailers offered a sustainable delivery option. Of these 62, 49 offered pick-up points and 33 offered in-store pick-up (20 of the 62 offered both). Longer delivery windows or bicycle or electric vehicle delivery were not explicitly offered by any retailer. Out of the 62 retailers that offered a more sustainable delivery option, 23 did not use any intervention. 16 retailers provided information about it in the form of either a label or an explanation, 32 provided a financial incentive, and 8 made the sustainable option the default. In the table below, we provide more detailed information on how many retailers use a specific intervention, a combination of interventions, or no interventions. We split the use of interventions by the type of sustainable delivery option a retailer offers: either a *pick-up point* option, which covers pick-up points and parcel lockers, or an *in-store pick-up*.

Table 12: Use of Interventions Across E-commerce Retailers

	Firms (n)	
Intervention	Pick-up Point	In-Store Pick-up
Information: Label	13	4
Information: Explanation	2	0
Financial Incentive €0.00 - €0.50	3	0
Financial Incentive €0.51 - €1.00	3	0
Financial Incentive €1.01 - €2.00	2	1
Financial Incentive > €2.00	4	22
Sustainable Default	5	3
Combined Interventions		
Information and Financial Incentive	4	3
Information and Default	0	0
Financial Incentive and Default	2	2

Notes: The table counts retailer–option-type pairs. A retailer offering both a pick-up point option and in-store pick-up appears in both columns, and a retailer using more than one intervention appears in each corresponding row. The Combined Interventions rows report retailers already included in the individual intervention rows above.