How do defaults affect information acquisition and processing?*

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Abstract

We investigate whether default nudges affect the willingness of decision-makers to acquire and process information about the choice options in a discrete choice situation. In an online experiment, where about 2,300 participants choose between two donation options worth $100, we vary the availability of information on the options and the presence of a default or, more generally, a preselected option. We find that the presence of a preselected option – self-selected or exogenously determined as a default nudge – induces decision-makers to remain ignorant, as they request less information on the choice options when given the option to do so. Moreover, we show that the presence of such a status quo in the form of a preselected option makes participants more likely to disregard available information than in the absence of a preselected option. Our results cannot be explained by rational inattention or laziness. These findings show that default nudges may not be as innocent as often assumed, and more generally, they highlight the need for choice architects, regulators, legislation, and management to take the consequences of a preselected option, such as defaults and own choices made earlier in time, on information acquisition and processing into account when considering the design of a decision situation.

JEL Classifications: C90, D01, D83, D91, M21

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1 Introduction

Default nudges are often considered a potent, but rather innocent, tool of choice architects (Thaler and Sunstein, 2008). Since Samuelson and Zeckhauser (1988) have documented the power of the status quo, this insight has been successfully used, for example, by setting a default in the context of retirement savings, charitable giving, and electricity contracts (Carroll et al., 2009; Schulz et al., 2018; Fowlie et al., 2021). The use of such default nudges can be justified as they are considered consistent with the concept of Libertarian Paternalism (Thaler and Sunstein, 2003), and for their low cost and ease in implementation, even though their effectiveness has been questioned recently (see, e.g., DellaVigna and Linos, 2022). Another recent criticism is that nudges – often referred to as sludges in this context (Thaler, 2018) – are not always used to align the decision-makers' intentions with their actions. Instead, they are sometimes employed to steer decision-makers towards acting in the choice architect's interest. If a default *additionally* affected the way information was acquired and processed, this would seriously challenge the innocence of default nudges, and potentially even qualify them as a manipulation tool. Yet, such an interaction between information acquisition and processing, and the status quo in form of a preselected choice option (including default nudges and own choices made at an earlier point in time)[1] has not been investigated to date.

This gap is surprising not only given the high relevance for regulation, public authorities, management, or everyday life, but also for the well-documented fact that decision-makers sometimes avoid or ignore available information in a selective way (see, e.g., the reviews by Golman et al., 2017, or Gabaix, 2019). For example, decision-makers have been shown to strategically select information in a self-serving manner (Dimant et al., 2024), and even prefer to avoid information about their own health (Oster et al., 2013), intelligence (Eil and Rao, 2011; Möbius et al., 2022), or morality (Dana et al., 2007; Grossman, 2014; Kajackaite, 2015; Grossman and Van Der Weele, 2017; Momsen and Ohndorf, 2020; Serra-Garcia and Szech, 2022; Vu et al., 2023).[2] Yet, the documented behavior in these studies serves to protect a certain self-image, and it remains understudied how information acquisition and processing may be affected when it is neither rational to stay inattentive to information (e.g., Gabaix, 2019), nor helping one's self-image.[3]

In this paper, we investigate whether a preselected choice option in a discrete decision-making scenario – where the preselection is either self-chosen or exogenously allocated

[1] We consider default nudges a special case of preselected options in general: They are set by someone else, e.g., the choice architect, while in other situations the status quo in form of a preselected option is determined for example by the decision-makers' own previous choices. Our research focus and findings thus extend beyond defaults in the narrower sense, and to ease exposition, we use the terms default and status quo as synonyms for a preselected option when there is no risk of confusion.

[2] See Hertwig and Engel (2016) for a systematic discussion of the different types and functions of deliberate ignorance.

[3] A notable exception that we are aware of is Ambuehl (2024): They experimentally study the impact of incentives on information search, and document that incentives cause biased information search.

such as in a default nudge setting – may induce decision-makers to discard information about the choice options. We document and investigate information acquisition together with the degree to which available information is processed for making a decision in situations with and without preselected choice options. We alter whether an option is preselected and, if so, how it is determined – randomly or self-selected based on vague information, corresponding to a default nudge situation or a self-chosen status quo from a choice made at an earlier point in time. In addition, we vary whether subjects are immediately presented with all information about the possible options, or whether they actively need to reveal these pieces of information. In our setup, additional information affects the perceived attractiveness of the choice options. Our experimental approach thus allows us to causally identify the consequences of the presence of a default (defined in the broader sense as any type of preselected option), on information search and processing. The variation of the default, i.e., whether it is self-selected or randomly allocated, helps to address (and rule out) a variety of well-known mechanisms, such as perceiving the default as a recommendation (e.g., Altmann et al., 2022), which could trigger *rational* inattention (Gabaix, 2019).

Two closely related papers also study the interplay between default effects and information, yet from very different angles: In Grossman (2014) *defaults determine the availability of information on the externalities of the choice options*. Hence, information avoidance serves as an excuse to act (in)morally, and information defaults are shown to be of additional help in that regard. Our paper, in contrast, is not about investigating the determinants of information avoidance as an excuse, as there are no externalities in the sense of Grossman (2014). Instead, we investigate the influence of defaults on information acquisition and processing when there is *no conflict of interest* between one's own interest and that of a third-party. In our study, the *default applies to which of the choice options is pre-selected*, and discarding information may result in choosing an option that is not aligned with own preferences – very much in contrast to the setting in Grossman (2014). Bar-Gill and Ben-Shahar (2021) theoretically study how information costs affect the stickiness of defaults. While our study adds empirical evidence regarding the stickiness of defaults (see the discussion in the next paragraph), we are more interested in the reverse direction, i.e., we study if the presence of a default affects information acquisition and processing.

Investigating the effects of a preselected choice option on information acquisition and processing is important beyond the debate on the innocence of default nudges. Results may also put empirical findings from previous research on default effects into perspective that traditionally abstracts from information search altogether (see, e.g., Blumenstock et al., 2018). In this case, we may add another explanation – namely altered information acquisition and processing – for the power of defaults and the status quo beyond the so far documented ones, such as inertia (Johnson et al., 2012), endorsement (McKenzie et al., 2006; Altmann et al., 2022), and reference dependence (Kahneman and Tversky, 1979). Most importantly perhaps, if the status quo in the form of a preselected option – be it a default nudge or a self-selected choice at an earlier point in time – negatively affects information behavior, choice architects would need to think carefully about information provision and presentation: In case of such an effect, defaults – desirable or not – and preselected options more generally, may not only have larger effects on choices than previously thought, they might also be used as a manipulation tool or their distortive quality may be abused, respectively. Thus, interaction effects must be considered by regulators to avoid abusive use. In particular, circling back to the innocence of default nudges, such an effect would mean that *slugdes* might not only work via the well-documented channels of inertia, implicit recommendations, and loss aversion alone, but also via distorted information acquisition and processing, which would seriously call into question considering nudges as a tool in line with Libertarian Paternalism.

In the experiment, subjects choose between two options that determine how $100 are split between different charities and organizations, with or without one of the options being preselected. We consider charities to abstract from consequences that might intervene with the subjects' personal payoff and to cleanly isolate the mechanisms at play. The setting is thus comparable to public procurement and many situations in corporations and organizations, where the money to allocate is independent of the decision-makers' own payoffs (at least in a direct sense). Besides, it proxies all those (repeated) decision situations in business or in private, where the price between alternatives is sufficiently similar, such as when deciding for a new smartphone or laptop, or in case of the leasing rate for the next company car, or regarding the destination of the next family holiday.

The composition of the options depends on the subjects' preferences elicited at the beginning of the experiment.[4] The two largest shares of both options are donated to the most-preferred charities, albeit in a different order: In one option, the largest share is donated to the charity ranked first, and the second-largest share to the charity ranked second. In the other option, the order is swapped, making this option appear dominated at first sight. The main difference lies in the charities to which smaller shares are donated. Here the option attributing the largest share to the charity ranked first contains 'bad apples' ('Bad-Apples-Option'), as it attributes donations to charities that were ranked last, while the other option does not include these charities ('No-Bad-Apples-Option'). As more than 90% of our subjects explicitly opposed donations to the charities they ranked last, and as the money allocated to these charities exceeds the difference between the amounts allocated to the first and second ranked charity, learning about these 'bad apples' in the 'Bad-Apples-Option' should result in choosing the 'No-Bad-Apples-Option'.

After confronting subjects with information only on the two largest shares of the two options (and before information on the smaller shares is made available), either one of the

[4]For a similar experimental approach, see Engel et al. (2023)

options is randomly preselected as default by the computer, respectively preselected by participants themselves, or no option is preselected, depending on the assigned treatment. In the treatment where the composition of the options is initially concealed, we then examine participants' information acquisition. Finally, we investigate which option they choose, and how it is affected by the available information.

Our treatments work as intended: Presenting the 'Bad-Apples-Option' as default increases the share of subjects choosing the 'Bad-Apples-Option' by 26 percentage points, while a default on the 'No-Bad-Apples-Option' reduces this share by 32 percentage points.[5] Similarly, in our hidden information treatment, only about 20% reveal all the information that participants in the full information treatment see upfront.

We have two main findings: First, we find that the presence of a default regarding the choice options (or a preselected option, more generally) induces ignorance: When participants have the possibility to remain ignorant about the choice options in the treatment where the (full) composition of the options is initially concealed, they reveal significantly less information when an option has been preselected. While participants with preselection demand additional information on the choice options on average about 5 times, those without preselection do so about one more time; an increase of about 17%. We observe this result with slightly varying effect sizes for both possible ways in which the preselected option is determined, self-selected and by random allocation mirroring a default nudge setting. In addition, we observe this pattern also for a variety of alternative measures capturing information search behavior, such as the time spent on the information page.

Second, we find that the presence of a preselected option decreases the degree to which information is processed, which induces subjects to make worse choices. Learning that one option contains donations to the least-preferred charity in the presence of a preselected option has a significantly lower effect on final choices than in the absence of a preselected option. While having this information reduces the 'risk' of making the 'wrong' choice by more than half without a preselection, it is reduced by less than 33% in the presence of a preselection. Hence, decision-makers are more likely to disregard information that questions the preselected option.

Importantly, the nature of the preselection does not matter for this result: Whether the default is self-selected or has been randomly allocated, mimicking a default nudge scenario, does not make a difference. This nuance rules out that the effect in our setting is due to interpreting the preselected option as a recommendation (as discussed in, e.g., McKenzie et al., 2006; Blumenstock et al., 2018; Jachimowicz et al., 2019; Altmann et al., 2022), unless participants perceive their own previous choice as a recommendation, which seems rather implausible. Consequently, *rational inattention* (e.g., Caplin and

[5] These arguably large default effects are within the range of what Samuelson and Zeckhauser (1988) report: They document increases in the share of (non-incentivized) choices of up to 53 percentage points due to an option being presented as the default.

Dean, 2015; Gabaix, 2014, 2019; Bhui and Jiao, 2023) cannot explain the full pattern we observe as inattention would only be rational if the (self-selected) default was perceived as recommendation.

It is noteworthy that when the preselection is the 'No-Bad-Apples-Option', a non-negligible share of subjects *actively switches away* from their preselection, despite having information about the 'bad apples' displayed. Consequently, we observe the same negative tendency in which defaults alter the value of information in the decision-making process as when the 'Bad-Apples-Option' is preselected. This insight rules out that our finding can be explained by inertia.[6]

We contribute to various strands of literature. First, we add to the debate on defaults (e.g., DellaVigna and Linos, 2022), in particular on the innocence of default nudges and the adequacy of their use more generally (Thaler, 2018). While the debate has mostly focused on the result, namely the choice that is eventually implemented, and has highlighted that it may not always be in the decision-maker's best interest, our results provide complementary insights by showing that decision-makers may simply be manipulated and additionally be 'nudged' to pay little attention to the decision problem.

In a more general sense, our paper is the first to investigate the interaction of defaults (as a preselection of a choice to be implemented) and information acquisition and processing, and connects separate strands of literature – the one on information avoidance and ignorance (Loewenstein, 2006; Golman et al., 2017; Gabaix, 2019), and the one on defaults (Samuelson and Zeckhauser, 1988) and the mechanisms of default effects (Dinner et al., 2011; Blumenstock et al., 2018).[7] Starting with the latter one, using an incentivized experiment, our second contribution is to identify distorted information acquisition – here in the form of participants choosing to remain ignorant – as additional channel through which defaults affect our decisions. The current evidence on the effects of defaults on choices is mostly limited to endorsement (McKenzie et al., 2006), reference dependence (Kahneman and Tversky, 1979), or inertia (Johnson et al., 2012). These reasons differ starkly from information avoidance and ignorance.[8] Importantly, inertia as investigated in previous work refers to the reluctance to implement another choice than the default to avoid switching costs. The behavioral pattern we discover, however, may be described as inertia regarding the willingness to devote cognitive effort to the decision problem, i.e., to acquire and process relevant information when an option is preselected, or to changing one's mind. As most experiments investigate defaults while presenting decision-makers with all information necessary to make an educated choice, our results thus suggest that

[6]Even without this insight, we note that inertia would need to cause an asymmetrical effect depending on the presence of a default to explain our findings.

[7]Note again that the related study by Grossman (2014) considers both a different question and setting, in which there is no default or preselection of one of the choice options to be implemented, and as such cannot speak to, e.g., the mechanisms of default effects, in contrast to our study.

[8]See also Eidelman and Crandall (2012), and Jachimowicz et al. (2019) for additional reviews.

default effects observed in experimental decision contexts are severely underestimated compared to reality, as they abstract from information behavior.

A third contribution is our finding that the presence of a preselected choice option decreases the degree to which information is processed, thereby affecting final choices. It speaks to the literature on how decision-makers selectively process information when it is not avoided (Babcock et al., 1995; Exley, 2020; Gneezy et al., 2020; Schwardmann et al., 2022; Saccardo and Serra-Garcia, 2023). We add to this literature by documenting that, with a preselected option, decision-makers discard information that questions the superiority of the option that seemed superior when the default was allocated, where decision-makers most likely invested cognitive resources to think about the decision for the first time. In that aspect, our results complement the literature emerging from psychological research on confirmation bias (e.g., Charness and Dave, 2017).

Our results are informative for organizations and individuals alike, as they are relevant whenever decisions are to be taken in the presence of a preselected option, including default nudges, or an own choice made at an earlier point in time. Decision-makers might profit from our findings by building the habit always to search for information in case stakes are reasonably high, even if an active choice is not needed, and always to question whether they would have evaluated the decision situation in the same manner, had no preselected option existed. Organizations might think of strategies to convert these insights into processes, for example by automating research and by dividing research and decision-making across individuals, without communicating the current choice.

Most importantly, perhaps, our findings are policy-relevant in the sense that choice scenarios might be designed in an abusive manner. In addition to all default nudge scenarios that one can think of, for example, contracts that are automatically renewed as well as preselected options for example in a purchase environment might need regulation with respect to information provision. In light of our results, consumers might profit from an easy-to-digest, easy-to-access and easy-to-compare provision of the relevant information in the way it has been enforced, e.g., for consumer credits in the EU,[9] as this might prevent an abuse of the default effect due to information avoidance, and decrease the scope of information interpretation. Clearly, this includes the special case of default nudges, for which, if implemented at scale, ethical approval of an entity independent of the choice architect should become the norm in light of our results.

2 Experimental Design and Procedures

The experiment was tailored to, and run with, US subjects. The main task in our study asked subjects to decide between two options of which each split $100 among different US

[9]See, e.g., `https://en.wikipedia.org/wiki/Annual_percentage_rate`.

charities and organizations. These options were tailored to the subjects' preferences (but not necessarily aligned with them). In a between-subjects manner, we varied whether subjects immediately observed all necessary pieces of information (FullInformation) or whether these pieces of information needed to be revealed (HiddenInformation). In addition, we altered whether one option was preselected (DefaultTreatment) and, if so, how the default was determined: randomly, or self-selected after receiving only little information. In total, the experiment consisted of six treatments implemented in a 2 (FullInformation vs. HiddenInformation) by 3 (NoDefault vs. RandomDefault vs. Self-SelectedDefault) design (see Table 1).

We chose this experimental setting for several reasons: First, we did not want payoff-concerns to interfere with the effects, and cleanly isolating the mechanisms at play in a setting with own payoffs would significantly increase the required number of treatments. Moreover, we are convinced that the experimental setting captures the main characteristics of a plethora of decision situations where the monetary consequences for the decision-maker are constant across the alternatives. Consider, for example, the case of public procurement, or when ordering a service or product on behalf of one's employer. While the consequences of their choices are relevant to the decision-maker, there is often no difference in the costs that the decision-maker has to bear. Similarly, in many cases, costs are very comparable between the choice alternatives in business, but also in private, where decision-makers in fact have to pay themselves. For many of these situations there is a previous (own) choice that can just be repeated without exerting effort to gather information on the available options (that may not be exactly the same as when the last choice was implemented). Examples include the choice of a construction company, of a supplier for a certain part in manufacturing, of a service provider for, e.g., catering or technical solutions, of a new smartphone or laptop, of the next company car to be leased, or even of the location for the next family holiday. In all these situations, costs that the decision-maker has to bear can either be identical (and if only so for deciding on behalf of someone else) or are very similar. Moreover, all these situations can be recurring such that there often is a previous – possibly own – choice that may serve – in abstract terms – as a default choice and that can just be implemented again. Though very stylized and abstract, our design proxies many relevant decision situations.

Stage 1: Learning about Subjects' Preferences towards the Charities and Organizations in our Experiment

At the beginning of the experiment, we elicited the subjects' preferences about nine charities and organizations[10] by asking them to split hypothetically – without further research – $100 between these charities and, possibly, a tenth charity of their own choice. The listed

[10] In the following, the words 'charities' and 'organizations' are used interchangeably.

organizations were American Nuclear Society, Nuclear Information and Resource Service (anti-nuclear group), Biology Fortified, Inc. (promoting genetically modified organisms), Non-GMO Project (label for food free of genetically modified organisms), Democrats, Republicans, Doctors Without Borders, the Nature Conservancy, and UNICEF.

Providing a short description (one sentence max.) of the organizations' main purposes according to their websites or Wikipedia, we ensured a sufficient level of familiarity with the involved organizations (see the instructions and screenshots in Online Appendix C). The order in which organizations were presented was randomized between subjects.

We asked subjects to attribute at most $40 to a single organization. As we technically ensured compliance with this rule, this resulted in donations to at least three organizations. In case of ties among the preferred organizations, subjects had to specify which organization they preferred most. Similarly, we asked participants to identify the organization they supported least among those to which they did not attribute any money, and to select those to which they *strictly* opposed donations.

Some of the charities and organizations supported conflicting goals, and could be considered controversial, such as the American Nuclear Society and the anti-nuclear group 'Nuclear Information and Resource Service'. Yet, all of them were officially registered in the US and tax-exempt as 501(c) organizations (except for the political parties). We included these controversial organizations with conflicting goals, as well as the political parties, to offer a list of options among which almost everybody could find one they supported and one they did not, even to the degree that they opposed a donation. Subjects indeed behaved as anticipated: More than 90% of our subjects explicitly opposed a donation to their least-preferred organization, and more than 97.5% either donated to only one charity in a pair of organizations with conflicting goals, or explicitly opposed a donation, or both. Having stronger rather than merely weak preferences towards (some of) the presented organizations was important for our research question.

We used this design feature for an attention/bot check: Subjects who attributed money to all four organizations of the two pairs with conflicting goals were not allowed to proceed with the study. Note that donations to both political parties need not be a sign of inattention, as one might wish to support the democratic system. See Section A in the Online Appendix for details on the attention checks, (automatic) exclusion rules and behavior of excluded subjects.

Construction of Choice Options With and Without 'Bad Apples'

Based on the elicited preferences on the charities and organizations in our experiment as just described, we constructed two donation options: Each split $100 between 11 charities where the share attributed to each charity was influenced by the subject's stated preferences. Specifically, the two options consist of up to five charities for which we have obtained preferences on the individual level from Stage 1, which are complemented by up to eight popular charities.[11] We picked these organizations to mimic the characteristics of those from Stage 1 that did not support conflicting goals. We included ALSAC (St. Jude Children's Research Hospital), American Red Cross, American Society for the Prevention of Cruelty to Animals, Feeding America, Natural Resources Defense Council, Save the Children, Task Force for Global Health, and the WWF.

For both options, the two largest shares were donated to the subject's most-preferred charities; yet, the order was swapped. For the at first glance superior option, the largest share ($30) was donated to the charity ranked first and the second-largest share ($25) to the charity ranked second. Accordingly, for the at first glance inferior option, the largest share went to the charity ranked second and the second-largest share to the charity ranked first.

The crucial differences between the options lay in the smaller donations. The at first glance superior option contained significant donations to two charities to which the subjects explicitly did not allocate any money ('Bad-Apples-Option'): In total, $12 were allocated to these organizations, with $8 to the least-preferred one. More than 90% strictly opposed any donation in their name to this charity, and 4% of the remaining subjects at least declared it their least-preferred option, while donating a positive amount to the charity pursuing the opposing goal.[12] For the at first glance inferior option, the 'No-Bad-Apples-Option', the respective shares were donated to uncontroversial charities. Thus, the 'No-Bad-Apples-Option' consists of eight uncontroversial charities, and those three to which the participant allocated the three highest donation amounts in Stage 1. In short, we obtain the 'Bad-Apples-Option', by replacing two uncontroversial charities from the 'No-Bad-Apples-Option' with two charities to which the participant did not want to allocate any money in Stage 1.

As the additional donation of $5 to the preferred charity comes with a donation of $12 to the 'bad apples' in the 'Bad-Apples-Option', we expect that subjects who have considered the information on the composition of the options have – on average – an incentive to behave according to their expressed opposition to the 'bad apples', and choose the 'No-Bad-Apples-Option'. We instructed participants that the two options might differ from their preferences stated in Stage 1, in order not to create any false expectations (see Figure 3 in Online Appendix C for instructions/screenshots). Attention to the instructions was ensured both with so-called 'honeypots' and attention checks, allowing us to filter out machines or subjects who did not pay attention to the instructions, respectively (see Section A in the Online Appendix for details).

[11] We obtain a preference ranking for at least five organizations from the charities listed above as we require positive donation amounts to three of them, and as the attention check in combination with the (pairs of) controversial charities ensures that there are at least two without a donation; in addition, we ask for the most and least preferred charity.

[12] Recall that we picked pairs of controversial charities with conflicting goals.

Table 1: Number of Participants per Treatment

		Default			
		NoDefault	Random Default	Self-Selected Default	All
Info	FullInformation	384	384	377	1145
Info	HiddenInformation	386	387	382	1155
	All	770	771	759	2300

Stage 2: Provision of Incomplete Information and Allocation of Defaults

After subjects had stated their preferences, they received *incomplete* information on the two options: They learned to which charities each option allocated the two largest shares (but not the corresponding amounts). Given these pieces of information, one option – the 'Bad-Apples-Option' – appeared to dominate the other – the 'No-Bad-Apples-Option' –, as the largest share was donated to the subject's most-preferred organization, and no 'bad apples' could be seen at this stage (see Figures 6-8 in Online Appendix C for screenshots). After presenting these (incomplete) pieces of information, our first treatment variation came into play (see Table 1). We implemented three treatment variations regarding the presence of a default, and, if applicable, how it was determined (self-selected or randomly determined) in a between-subjects design (see Table 1).

In the NoDefault condition, subjects were shown the incomplete information, as just described, and were informed that they could choose at a later stage. In the RandomDefault condition, one of the options was randomly preselected as default. Subjects were informed that the selection had been randomly performed by the computer, and that they would have the possibility to change this choice at a later stage. To be able to assess whether it makes a difference which option was selected as default, we assigned the 'Bad-Apples-Option' with a 66% probability, assuming that the majority would select this option in the Self-SelectedDefault condition (which was the case).

In the Self-SelectedDefault condition, subjects chose one of the options based on the incomplete information. They were informed about the possibility to change this choice at a later stage. The purpose of the RandomDefault condition was to induce a clean default. Furthermore, it captures those situations where the default is determined by a third party, i.e., a choice architect. The purpose of the Self-SelectedDefault condition was to contrast the results obtained with the RandomDefault condition, using a setting where the preselection cannot rationally be interpreted as a recommendation. This allows us to investigate whether interpreting the default as a recommendation might drive our results.

After receiving incomplete information, and possibly learning about a preselected option, or preselecting one option oneself, subjects were given the opportunity to inform themselves about the full composition of the two choice options in the information stage, which came next.

Stage 3: Information Stage: Variation of Information Provision

Following the page where incomplete information was provided and defaults, if applicable, were determined, our second treatment variation came into play (see Table 1). Here, subjects could obtain additional information. In one condition – the FULLINFORMATION condition – all information was immediately presented and subjects could directly observe all charities both options consisted of. Participants thus had all information necessary to make an informed decision. As they could immediately observe that the — at first sight — preferable option (the 'Bad-Apple-Option') included charities they did not wish to support, they could use this information and choose their truly preferred option.

In the second condition – the HIDDENINFORMATION condition – only the charities to which the largest share would be donated were immediately visible in the information stage, and further information was provided on demand.[13] For each option, subjects had the possibility to click a button labelled 'Show Details' to reveal the names and shares of the other charities contained in the option. Thus, there was one button per option and subjects could decide whether to use one, both, or none of the buttons. When clicked, the button revealed the identity of the charity to which the next-smaller share would be donated, and its respective share in the option. Hence, the button revealed additional information in a step-wise manner. Per click, the name and the share of the next charity was revealed. Revelation was ordered in the sense that the second-largest share and the corresponding charity were revealed first; another click of the same button revealed the third-largest share and the respective charity, and so on. Subjects could decide whether and how often to click each of the two buttons. To reveal the first bad apple, they had to click the button associated with the "Bad-Apples-Option" three times; the second bad apple was revealed after another four clicks, i.e., after seven clicks in total. They could proceed to the next page and make their choice without revealing any new information, or after having revealed some, but not all, information, or after revealing the full composition of both options. To reveal the full composition of both option, 20 clicks (ten per option) were necessary.

The options were depicted as pie charts located side by side on the subjects' decision screens, and we randomized on which side of the screen which option was shown.[14]

[13]See Figures 9 and 10 in Online Appendix C for screenshots of the decision screens.
[14]See the supplementary material in Section C for screenshots of the decision screens.

Stage 4: Decision Stage

After the information stage, the two options were presented again with all the information that had been shown in the information stage. That is, those in the FullInformation condition were presented with all charities that the options consisted of including their corresponding shares, and those in the HiddenInformation condition were shown all information that they had revealed in the information stage.

Subjects were then asked to select the option according to which donations should be implemented, if their choice was randomly selected as payoff-relevant. Thus, participants with a Self-SelectedDefault or a RandomDefault could just stick to the previously selected option, or switch to the other one, while subjects in the NoDefault condition always had to make an active choice.

To implement a decision at this stage, subjects had to solve a small, but tedious puzzle, i.e., they had to write a list of letters in reversed order. This task was used to implement decision and switching costs, and it was already announced before the information stage (see Figure 11 in the Online Appendix for an example). These switching or decision costs were implemented in all treatments. They were introduced to investigate if – depending on the default – the will to implement a certain decision could potentially be more powerful than inertia triggered by switching costs.

Questionnaire

After subjects had made their final donation decision, we collected data on demographic variables (age, gender, education, and state of residence) in a questionnaire, together with a standard survey question on altruism (Falk et al., 2023). We also elicited time (following, e.g., Falk et al., 2018) and risk preferences (following Schneider and Sutter, 2020) experimentally.

Experimental Procedure

The experiment was run in November and December 2020 with US subjects. It was programmed in oTree (Chen et al., 2016), and subjects were recruited using MTurk. To be eligible for the study, we required an acceptance rate of over 96% in previously completed tasks. In addition, we implemented our own bot filter using so-called honeypots and attention checks (see Section A in the Online Appendix for details), where we filtered out 430, or close to 20%, of subjects arriving at our study, where bots accounted for about 16% of those filtered out. Subjects earned a show-up fee of \$0.80 and \$1.60 (in expectation) for completing the about 15-minutes-long study. The donations associated with the selected option were carried out with a probability of 1%; otherwise, the donations remained hypothetical. This was clearly announced to the subjects (see the instructions and screenshots in Online Appendix C).

3 Results

We first investigate whether the presence of a preselected option affects information search behavior, before we investigate the effects on considering and processing information in a second step.

In both cases, we start with a brief overview over our experimental outcomes, before we proceed to the regression analysis. Table 2 contains summary statistics of the main outcomes of interest: In column [1], we show these statistics for the full sample; in columns [2] and [3], we divide the sample by whether or not a participant has been randomly allocated to the conditions without preselection (NoDefault) and with preselection (pooling the RandomDefault and Self-SelectedDefault conditions).

Table 2: Descriptive Statistics: Experimental Outcomes

	[1] Full Sample		[2] NoDefault condition		[3] Default conditions	
	Mean (SD)	Obs.	Mean (SD)	Obs.	Mean (SD)	Obs.
Information Search Behavior (HiddenInformation Treatment)						
No. of Clicks	5.30	1155	5.72	386	5.09	769
	(7.66)		(7.91)		(7.52)	
Revealing New Info (=1)	0.26	1155	0.28	386	0.25	769
	(0.44)		(0.45)		(0.43)	
Revealing Critical Info (=1)	0.24	1155	0.26	386	0.23	769
	(0.43)		(0.44)		(0.42)	
Information Search Behavior (All Treatments)						
Seconds on Page	49.54	2300	51.72	770	48.44	1530
	(41.63)		(47.65)		(38.21)	
Choice Behavior						
Final Choice: No-Bad-Apples-Option (=1)	0.34	2298	0.38	768	0.32	1530
	(0.47)		(0.49)		(0.47)	
Critical Info Available: Final Choice: No-Bad-Apples-Option (=1)	0.43	1421	0.52	485	0.39	936
	(0.50)		(0.50)		(0.49)	
Critical Info Not Available: Final Choice: No-Bad-Apples-Option (=1)	0.19	877	0.16	283	0.20	594
	(0.39)		(0.36)		(0.40)	

3.1 Do Defaults Induce Ignorance?

To investigate whether the presence of a default option – self-selected or randomly allocated – affects information acquisition, we compare the number of clicks to reveal new information with and without default. The first row of Table 2 presents descriptive information on this outcome. Looking at the plain numbers, we already observe a pattern in this outcome: Subjects reveal less information on the charities in an option in the presence of a preselection, as is evident from the number of clicks to reveal new information: While on average, participants click 5.3 times to reveal information (column [1]), this number amounts to 5.7 in the condition without preselected option (column [2]), which is about 12% higher than in the condition with preselection (5.1, see column [3]).

In Table 3 we present regression results on this outcome, more specifically, results of a negative binomial regression of the number of clicks to reveal new information on the presence of a preselected option. We use a negative binomial regression because the outcome is discrete and skewed.[15] Pooling all participants, that is, those who chose their preselected option themselves (SELF-SELECTEDDEFAULT) as well as those who had their default randomly allocated (RANDOMDEFAULT), irrespective of the preselected option (Bad-Apples-Option or No-Bad-Apples-Option), we find that – correcting for covariates such as gender, education, risk aversion and patience – the presence of a preselected option results in clicking the button to reveal information 1.4 times less often, as average marginal effects indicate (Column 2 of Table 3, labelled 'AME'). Columns 3 to 10 show the coefficients and corresponding (average marginal) effects of the presence of a default option when looking at the different default options in isolation (Columns 3 to 6), and when separately looking at the different ways of allocating the preselected option (Columns 7 to 10). Coefficients are all very similar, and the presence of a preselected option has the same effect in a statistical sense (i.e., there is no statistically significant difference) no matter if we assign the default randomly (RANDOMDEFAULT condition) or if participants chose the preselected option themselves (SELF-SELECTEDDEFAULT condition), or if the preselected option is the Bad-Apples-Option, or the No-Bad-Apples-Option. We obtain very similar, although somewhat smaller, effects when using OLS regressions or when we do not adjust for individual characteristics such as age, gender, and so on (see Tables 8 and 9 in Online Appendix B): In all specifications, the presence of a preselected option reduces the number of clicks by about one click (or about 17%), keeping all else constant.

We can investigate whether the presence of a default affects information search behavior in a more encompassing way. In addition to the number of clicks to reveal new information about the two donation options, information search behavior can be measured using binary outcomes capturing whether or not participants reveal new information (more than 1 click),[16] and whether or not they reveal critical information, that is, to uncover the first bad apple (more than 2 clicks), as well as using the time in seconds that participants spend on the information page (which we can even investigate for those participants in the FULLINFORMATION treatment).

Descriptive information on these outcomes is presented in rows 2 to 4 of Table 2. We observe the same descriptive pattern as for the number of clicks: The share of participants who reveal new information or the share of those who reveal critical information by clicking on the button to reveal information is higher among those who have not been exposed to a preselected option by about 3 percentage points, or 13%, in both cases. Lastly, participants with preselection spend less time in the information stage (see 'Stage 3' in Section 2): While those with preselection spend about 48 seconds on the page to inform

[15]OLS regressions yield similar results, although, as expected, with a slightly reduced precision; see Table 8 in Online Appendix B for these results.

[16]Note that the first click revealed the identity of the second-largest charity, which has been communicated in Stage 2 before. Hence, to obtain new information regarding the charities that an option consists of, at least 2 clicks are necessary.

Table 3: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by The Number of Clicks to Reveal New Information

	Pooled		Preselected Option				Allocation of Preselection			
			Bad-Apples-Option		No-Bad-Apples-Option		RandomDefault		Self-Selected Default	
	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME
With Preselection	−0.25***	−1.36**	−0.24**	−1.37**	−0.28**	−1.50*	−0.22*	−1.20*	−0.27**	−1.52**
	(0.10)	(0.54)	(0.10)	(0.58)	(0.14)	(0.78)	(0.11)	(0.63)	(0.11)	(0.62)
Constant	3.10***		3.27***		2.57***		2.90***		3.13***	
	(0.47)		(0.50)		(0.66)		(0.60)		(0.55)	
N	1145	1145	958	958	571	571	767	767	762	762

Notes: This table shows the coefficients and corresponding average marginal effects (AME) from negative binomial regressions of the number of clicks to reveal new information on indicator variables for the presence of a preselected option (HiddenInformation treatment). In Columns 1 and 2, we report the results from all participants in the HiddenInformation treatment (minus 10 who did not finish the questionnaire) and pool both ways of determining a preselected option (RandomDefault or Self-SelectedDefault) as well as both possible preselections ('Bad-Apples-Option' and 'No-Bad-Apples-Option'). In Columns 3 to 6, we look at the two possible preselected options in isolation ('Bad-Apples-Option' and 'No-Bad-Apples-Option', respectively), and in Columns 7 to 10, we investigate the ways of allocating a preselected option in isolation (RandomDefault and Self-SelectedDefault, respectively). Note that in all columns, regressions have to include N = 386 participants (minus 2 who did not finish the questionnaire) who were not exposed to a default in addition to those of interest (see Table 1 for the corresponding numbers), for which reason numbers do not add up. In all regressions, we control for risk aversion, impatience, altruism, gender, age and age squared, education, the hours that have passed since 8am (local time), whether or not the session was conducted on a weekend, and whether or not the participant was among the fastest 25% of participants of our study. See Tables 7, 8, and 9 in Online Appendix B for detailed regression results, results from OLS regressions, and results without adjusting for individual characteristics. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

themselves about the choice options (column [3]), those without preselection spend about 52 seconds (column [2]), or roughly 7% more seconds, on that page.

We turn to the regression analysis of these figures. Table 4 summarizes the results. In short: Controlling for individual and session characteristics, the presence of a preselected option makes it about 5 percentage points less likely to reveal any additional information (Columns 3 and 4, results from a logit regression), makes it similarly less likely to reveal any critical information (Columns 5 and 6, results from a logit regression), and makes participants spend about 4 seconds less on the information page (Columns 7 and 8, results from a negative binomial regression).

Table 4: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by Different Outcomes

	No. of Clicks		Revealing New Info (=1)		Revealing Critical Info (=1)		Seconds on Page	
	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME
With Preselection	−0.252***	−1.359**	−0.315**	−0.052**	−0.316*	−0.049*	−0.077***	−3.837***
	(0.097)	(0.538)	(0.156)	(0.026)	(0.162)	(0.025)	(0.029)	(1.443)
Constant	3.096***		0.893		1.111		3.715***	
	(0.473)		(0.760)		(0.800)		(0.136)	
N	1145	1145	1145	1145	1145	1145	2275	2275

Notes: This table shows the coefficients and corresponding average marginal effects (AME) of indicator variables for the presence of a preselected option when regressing different measures for information search behavior on these indicator variables for all subjects in the HiddenInformation treatment in Columns 1 to 6, and all subjects in the HiddenInformation and the FullInformation treatments in Columns 7 and 8 (that is, pooling the possible default options, and pooling possible ways of allocating a preselected option, i.e., the RandomDefault and Self-SelectedDefault conditions). In Columns 1 and 2, the measure of information search behavior is the number of clicks, as in Table 3, and the coefficient results from a negative binomial regression. In Columns 3 to 6, the measures of information search behavior are binary, and indicate whether or not any information additional to the information shown in Stage 2 (when defaults are allocated) has been revealed or whether critical information (that is, information on a bad apple) has been revealed, respectively. These coefficients result from logit regressions. In Columns 7 and 8, information search is measured by the number of seconds spent on the information page. As this number is skewed and discrete, we apply a negative binomial regression, from which the reported coefficient results. In all regressions, we control for risk aversion, impatience, altruism, gender, age and age squared, education, the hours that have passed since 8am (local time), whether or not the session was conducted on a weekend, and whether or not the participant was among the fastest 25% of participants of our study. See Tables 10, 11, and 12 in Online Appendix B for detailed regression results, results from OLS regressions, and results without adjusting for individual characteristics. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We conclude with our first result:

Result 1. *When given the option to remain ignorant about the choice options, the presence of a default – self-selected or randomly allocated – increases ignorance about the available options. We observe this independent of the way the default has been determined (randomly allocated or self-chosen) and the option that has been preselected ('No-Bad-Apples-Option' or 'Bad-Apples-Option').*

3.2 Do Defaults Affect Information Processing?

Having established that the presence of a (self-selected or randomly allocated) default leads participants to remain ignorant more often, we now turn our attention to the potentially differential value people attach to the available information in their decision-making process in the presence of a preselection compared to situations without a default.

We again start our investigation with a look at the descriptive numbers presented in Table 2, panel *'Choice Behavior'*. The descriptive numbers in the first row can only serve as a first indication, as we cannot attribute *all* the difference in behavior that we observe between those in the DEFAULT treatments and those in the NODEFAULT treatment to differences in information processing (since final choices are obviously affected by defaults, too). This point notwithstanding, in the first row, pooling the information treatments, i.e., the FULLINFORMATION and the HIDDENINFORMATION treatment, we see that the 'No-Bad-Apples-Option' is chosen 6 percentage points, or close to 19%, more often in absence of a preselected option compared to the condition where participants have been exposed to a preselection.

Our main interest is to decompose this number with respect to available information, and investigate whether or not a *variation in available information* is associated with the same change in behavior in the presence and in absence of a preselection. To this end, we have to compare the increase in the share of those who chose the 'No-Bad-Apples-Option' due to the availability of critical information – that is, information about the bad apples – in absence and in the presence of a default (last two rows in Table 2). While in absence of a preselection, the availability of critical information is associated with an increase in 'No-Bad-Apples'-choices of 36 percentage points (52% - 16%, see Column [2], second-to-last and last row in Table 2), in the presence of a default, the increase amounts to only about half as much, i.e., 19 percentage points (39% - 20%, see Column [3], second-to-last and last row in Table 2).

We turn to investigating this in a regression setup, where we compare the effect of having critical information displayed about the composition of the 'Bad-Apples-Option' on choosing the 'No-Bad-Apples-Option' between those with a default, and those without. We do so using a logit regression and pooling both possible default options ('Bad-Apples-Option' and 'No-Bad-Apples-Option') as well as both ways to allocate a default (RANDOMDEFAULT and SELF-SELECTEDDEFAULT conditions).[17] Having processed the critical information – namely that the 'Bad-Apples-Option' contains unwanted charities, we expect participants to choose the 'No-Bad-Apples-Option' – the one aligned with own preferences – with a higher likelihood.

We find, however, that the change in the likelihood to choose the 'No-Bad-Apples-Option' that is induced by critical information is affected by the presence of a preselected

[17] See Tables 16 and 17 in Online Appendix B, and the discussion below, for separate analyses.

option: Columns 1 and 4 of Table 5 present the coefficients of critical information from the (logit) regression of choosing the 'No-Bad-Apples-Option'. We see that the logit coefficients are significantly larger without a default (first line) than in the presence of a default (second line). Here, this implies a higher value attached to information in the decision-making process without default. We observe this result independently of pooling participants in the FullInformation and the HiddenInformation treatments (Column 1) or looking at the HiddenInformation treatment only (Column 4), and the differences are almost identical in size (second-to-last row in Table 5).

How to interpret the logit coefficients, and how large is this effect? Columns 2 and 5 present the (average) marginal effects corresponding to the logit coefficients, which express in percentage points the increase in choosing the 'No-Bad-Apples-Option' that is associated with having critical information regarding the bad apples displayed – without a default (first row) and in the presence of a preselected option (second row). In the medical literature, this measure is interpreted as *absolute risk reduction (ARR)*,[18] as it expresses how much the 'risk' of choosing the 'Bad-Apples-Option' is reduced in absolute terms (i.e., in percentage *points*) by the display of critical information.[19]

In line with our descriptive results, the regression analysis confirms that while in absence of a default, critical information (i.e., the information that the 'Bad-Apples-Option' contains bad apples) reduces the 'risk' of choosing the 'Bad-Apples-Option' by 36 to 44 percentage points (Columns 2 and 5), the risk reduction due to information in the presence of a default is about half as large, i.e., almost 20 percentage points lower, and significantly so. We reach this conclusion if we only focus on those who actively revealed the displayed critical information themselves (HiddenInformation treatment), as well as if we consider all participants who had the critical information displayed including those who had all information displayed upfront (FullInformation treatment).

When comparing 'risk reductions' (or marginal treatment effects regarding binary outcomes) such as the (percentage-point) reduction in the likelihood to choose the 'Bad-Apples-Option' that is associated with the display of critical information, it is good practice at least in the medical literature to additionally consider potentially different *baseline risks*: Dividing the absolute, percentage-point 'risk' reduction with respect to choosing the 'Bad-Apples-Option' by the baseline risk (i.e., the share of those who chose the 'Bad-Apples-Option' without having critical information displayed) yields a measure known as *relative risk reduction (RRR)* in the medical literature.[20]

[18]Note that, in absolute terms, here expressed in percentage points, a reduction and an increase in an outcome are the same – in particular this is also the case for probabilities.

[19]The *ARR* is usually introduced as $\mathrm{ARR} = \mathrm{SB}_{\mathrm{No\ Info}} - \mathrm{SB}_{\mathrm{Info}}$, where $\mathrm{SB}_{\mathrm{No\ Info}}$ is the share of subjects choosing the 'Bad-Apples-Option' in absence of critical information (base category, or *baseline risk*), and $\mathrm{SB}_{\mathrm{Info}}$ is the respective share in the presence of critical information.

[20]This measure is usually introduced by dividing the *ARR* as defined in Footnote 19 by $\mathrm{SB}_{\mathrm{No\ Info}}$, i.e., by the baseline 'risk'.

Qualitatively, the results are the same. We report the RRR associated with having critical information about the 'Bad-Apples-Option' displayed in Columns 3 and 6 of Table 5. In relative terms, the risk to choose the 'Bad-Apples-Option' is reduced by 53% in absence of a preselection (Columns 3 and 6), but only by 29% to 32% when there is a default; a (significant) difference of 21 to 24 percentage points. Results are very similar without adjusting for individual characteristics such as age, gender, etc., especially with respect to the differential value that available information has for the decision-making process with and without a default (see Table 18 in Online Appendix B).

It is noteworthy that this pattern, i.e., that information matters less in the presence of a default, is found both when considering RANDOMLY determined defaults in isolation, as well as SELF-SELECTEDDEFAULTS in isolation; in fact, the relative risk reductions (RRR) (that account for the different baseline risks resulting from the different shares of participants with a preselected 'No-Bad-Apples-Option') and the differences in these RRRs induced by the presence of a preselected option are very similar in both DEFAULT treatments (e.g., 22% and 18% in the HIDDENINFORMATION condition with RANDOMDEFAULTS and SELF-SELECTEDDEFAULTS, respectively; see Column 5 in Tables 14 and 15 in Online Appendix B).

Thus, as we observe almost the same behavior with a SELF-SELECTEDDEFAULT and with an exogenously allocated default in the RANDOMDEFAULT condition, we conclude that this result – that information is considered and processed less in the presence of a default – is not just a consequence of interpreting the preselected option as a recommendation.

A second remarkable insight is that, focusing on the HIDDENINFORMATION treatment, we see that the value attached to critical information is lower in the presence of a default both when the preselected option is the 'No-Bad-Apples-Option' and when it is the 'Bad-Apples-Option' (although significantly so only for the 'Bad-Apples-Option', despite being the 'smaller' estimate).[21] In particular when comparing the value of critical information for choosing the 'No-Bad-Apples' option between the two possible defaults, i.e., when one default is the 'No-Bad-Apples-Option' and the other is the 'Bad-Apples-Option', accounting for the baseline risk is important, as the first-mentioned default already 'nudges' participants towards choosing the 'No-Bad-Apples-Option', thus the scope for an absolute reduction in risk is limited. Accounting for the baseline risk makes a comparison across the two default options possible: In relative terms, the risk reduction due to information is similar for the two possible default options, and so are the differences in risk reduction induced by having the critical information available (e.g., 24% and 19% in the HIDDENINFORMATION condition with the 'No-Bad-Apples-Option' and the 'Bad-Apples-Option' as default, respectively; note that the first difference is insignificant

[21]Note that the number of observations for the 'Bad-Apples-Option' as default exceeds the number of observations for a 'No-Bad-Apples' default.

Table 5: The Value of Information for Decision Making With and Without Preselected Option

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.774***	0.364***	0.529***	2.158***	0.442***	0.528***
	(0.189)	(0.031)	(0.085)	(0.277)	(0.054)	(0.103)
(2) –With Preselection (=1)	1.235***	0.183***	0.286***	1.614***	0.260***	0.323***
	(0.178)	(0.023)	(0.054)	(0.244)	(0.044)	(0.073)
Constant	−0.928*			−1.672**		
	(0.515)			(0.772)		
Difference between (1) and (2)	0.540***	0.181***	0.242***	0.545**	0.182***	0.205**
	(0.000)	(0.000)	(0.000)	(0.036)	(0.006)	(0.017)
Observations	2273	2273	2273	1144	1144	1144

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with and without preselected option, for all participants in our study (minus 27 who did not finish the study) in Column 1, and all participants in the HiddenInformation treatment (minus 11 who did not finish the study) in Column 4. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without preselected option. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of critical information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Note that in all columns, regressions have to include N = 386 participants (minus 2 who did not finish the questionnaire) who were not exposed to a default in addition to those of interest (see Table 1 for the corresponding numbers), for which reason numbers do not add up. In all regressions, we control for risk aversion, impatience, altruism, gender, age and age squared, education, the hours that have passed since 8am (local time), whether or not the session was conducted on a weekend, and whether or not the participant was among the fastest 25% of participants of our study. See Table 13 in the Online Appendix B for detailed regression results, and Tables 14 to 17 for results when restricting the sample to the RandomDefault, the Self-SelectedDefault, or those who were facing the 'Bad-Apples-Option' or the 'No-Bad-Apples-Option' as default, respectively. Finally, Table 18 in the Online Appendix B shows results without adjusting for individual characteristics.

Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

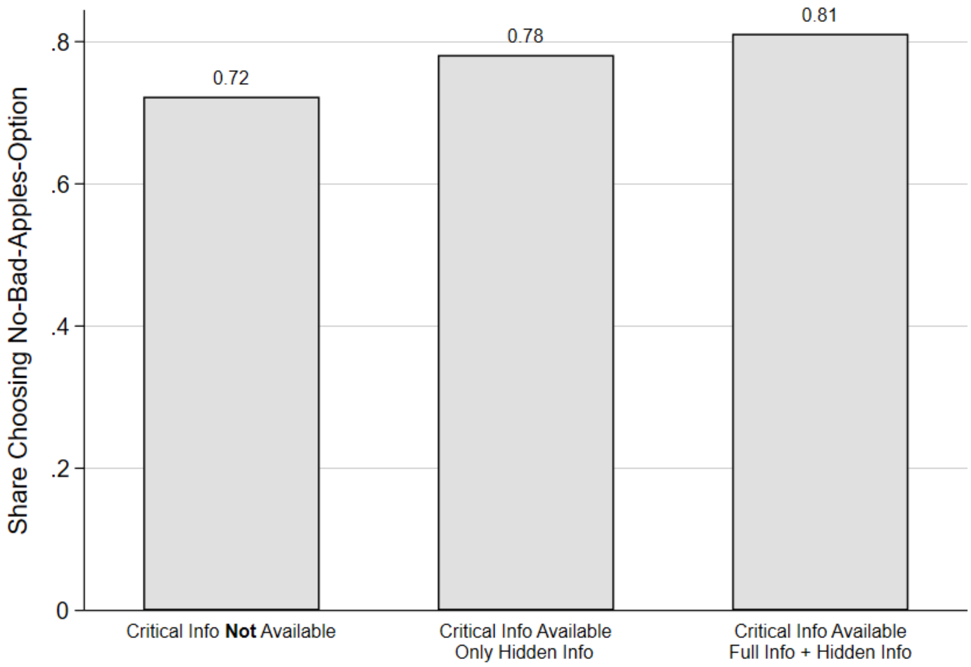

Figure 1: Choosing the 'No-Bad-Apples-Option' When It Has Been Set as Default (Randomly Allocated or Self-Selected) – With and Without Critical Information Displayed: Inertia (Not Switching) Would Be Expected With Critical Information Available, but Active Switching is Observed for About 20% of Participants.

Note: The share corresponding to the right-most bar is almost the same when looking at the FullInformation condition in isolation (82%), see Figure 2 in Online Appendix B.

mainly due to a very low number of participants who chose the 'Bad-Apples-Option' in the Self-SelectedDefault condition).[22]

Observing a very similar picture for both possible preselections, i.e., a lower value attached to critical information in the presence of both a 'No-Bad-Apples' and a 'Bad-Apples' default (even if insignificant in the first case) is more surprising than it initially seems: Should we not expect that critical information, i.e., information on the bad apples, has a larger effect on final choices if participants can just stick to the preselected option, as is the case when the 'No-Bad-Apples-Option' has been set as the default? At least this is not what we observe: With the 'No-Bad-Apples-Option' as a default, the observed pattern – that the presence of a default induces a negative tendency regarding the degree to which information is considered and processed – is caused by about 20% of the participants with this preselected option who *actively switch away* from the preselected option despite having the critical information about the bad apples available.

This pattern is depicted in Figure 1: Without critical information, 72% of the participants choose the 'No-Bad-Apples-Option' when it has been set as default. When critical information is available – either due to the exogenous treatment variation or due to the subjects' revelation behavior, the share of subjects sticking with the preselected 'No-

[22]See Tables 17 and 16 in Online Appendix B for these results.

Bad-Apples-Option' increases. Yet, in the HiddenInformation treatment, still 22% of the subjects actively switch away from the 'No-Bad-Apples-Option' to choose the 'Bad-Apples-Option' instead, despite having information on the bad apples displayed. When pooling across our Info treatments, we observe roughly the same picture: 19% of the subjects actively decide in favor of the 'Bad-Apples-Option' (18% in the FullInformation condition), even though just doing nothing would be more in line with their preferences – given they process the available information. Thus, we observe how the mechanism investigated here *outweighs* the default effect that one expects to see. We highlight this insight, even though the difference in relative risk reductions with a preselected 'No-Bad-Apples-Option' and without preselection is insignificant, as we see this as a strong indicator that the lower value attached to information in the presence of a default is not driven by inertia *alone*, and as another manifestation of defaults affecting information processing in our study: In the presence of a preselected option, individuals discount or even discard information that questions the superiority of the option they perceive as superior when defaults are allocated. We thus summarize:

Result 2. *Defaults decrease the degree to which information is processed. We find this independent of the way in which the preselected option is determined (randomly allocated or self-chosen). Moreover, in terms of magnitude, the decrease is similar for both possible default options ('No-Bad-Apples-Option' or 'Bad-Apples-Option'), although insignificant in the first case.*

4 Discussion and Alternative Explanations

Online Data Collection A frequent concern with online data collection is choice quality. We have addressed this issue in several ways that we are about to describe, but first, we want to note that the conditions are the same across treatments. Moreover, it is sometimes argued that online data collection involves a higher degree of noise. This would mean that results from online experiments are more conservative, since significant results are, all else equal, harder to obtain.

These arguments notwithstanding, we have undertaken three measures to address data quality: First, as explained in the last paragraph of Section 2, we have only allowed participation if participants' acceptance rate was over 96% in previously completed tasks. Second, we included attention checks throughout the experiment, and third, we implemented bot checks to filter out data entries from machines (see Section A in the Online Appendix for details).

As our data is in line with expectation and earlier results e.g., on the power of defaults, these measures have arguably been successful: We find robust evidence of a strong default effect, and it is less strong than what has been observed with unincentivized tasks (see, e.g., Samuelson and Zeckhauser, 1988).[23] In addition, we find that people react to information, and that information on average decreases the likelihood to choose the 'Bad-Apples-Option' (see Table 2 for a plain comparison).

Laziness/Inertia/Switching-Costs A well-documented channel through which defaults affect behavior is inertia (Blumenstock et al., 2018): Decision-makers are too lazy to switch away from the default and incur the corresponding switching costs. We document behavior that is complementary, but different from the well-known inertia explanation in two key aspects that cannot be explained this way.

The first aspect is even independent of any switching cost: The degree of information revelation differs in the presence of a default. The only relevant cost in this situation is the effort of clicking on a button, and this cost does not differ across treatment conditions. Yet, keeping 'costs' constant, we observe behavior that differs in the presence and in absence of a preselected option.

The second aspect is that the behavior documented here even *outweighs* the effect of switching costs when they work in different directions. For instance, when the (random) default option is the 'No-Bad-Apples-Option', we observe participants actively switching away from this chosen option. Thus, just as when the default option is the 'Bad-Apples-Option', defaults induce them to disregard the available information and make an, in this case, *active* suboptimal decision. That is, even when laziness or inertia cannot be the explanation, we document the same behavior regarding ignorance and non-processing of information. Put differently, while the documented effects cannot be driven by inertia or laziness regarding switching behavior, we find that defaults cause inertia regarding information acquisition and processing.

Rational Inattention Clearly, the finding that the presence of a preselection induces decision-makers to remain ignorant is closely related to rational inattention. Since paying attention to the decision problem is cognitively costly, decision-makers *rationally* remain inattentive if they assume their attention is not needed (Sims, 2003; Caplin and Dean, 2015; Gabaix, 2014, 2019). As a consequence, they stay uninformed and eventually remain inactive.

It might seem rational for the decision-makers to remain inattentive in the information stage in our setting in two cases: First, if they had perceived the default option randomly selected by the computer as a recommendation, and second, if they had been convinced that they had obtained sufficient information to assess which option was in line with

[23] Presenting the 'Bad-Apples-Option' as default increases the share of subjects choosing the 'Bad-Apples-Option' by 26 percentage points, while a default on the 'No-Bad-Apples-Option' reduces the share by 32 percentage points. Both differences are statistically significant in a two-sample test of proportions. Samuelson and Zeckhauser (1988) report increases in the share of choices of up to 53 percentage points due to an option being presented as the default.

their preferences, having seen only vague information about the choice options in Stage 2. While both might have been the case for some, it cannot explain the general pattern that we find, as we now explain.

We can exclude the first 'rational-inattention-story' (defaults perceived as recommendation trigger rational inattention) as it cannot explain why decision-makers who self-select their preselected option show the same behavior as those for whom the computer randomly selected the default option. Recall that in Stage 2, before the 'information stage' (Stage 3), we present the exact same information regarding the two options in the different treatment conditions – with the difference that participants in the SELF-SELECTEDDEFAULT condition decided for one option, and those in the NODEFAULT condition did not.

Regarding the second 'rational-inattention story' (decision-makers are convinced already in Stage 2 that they know enough to make an informed decision and disregard any information in Stage 3), we first note that this must have been the same for all treatments, unless the presence of a default option, even if self-selected, created such a conviction in a rational way. As the information provided were exactly the same in all treatments, there is no scope for rational inattention that would selectively only apply to the DEFAULT conditions.

Nevertheless, it seems that the latter explanation has some substance, even if it *cannot be described as rational inattention* to information in Stage 3. We observe that subjects in the DEFAULT conditions spend significantly more time in Stage 2 (where defaults are allocated) compared to those in the NODEFAULT condition (about 5-6 seconds on average, $p < 0.01$ in both negative binomial and OLS regression with the specifications used throughout in this paper). There is no significant difference between the SELF-SELECTED and RANDOMDEFAULT treatments in this regard. Hence, the DEFAULT conditions increase and possibly deplete attention in Stage 2, resulting in reduced attention in Stage 3. However, we argue that it cannot be described as rational inattention; rather, we interpret it as 'directed inattention'.

In that light, our results can be seen as complementary to earlier results on rational inattention, as they suggest that (in)attention, rational or not, might be diverted in a *systematic* way: The presence of a preselection induces decision-makers to process information on the available choice options to a lower degree than without a preselection. They discount or even discard information that questions the superiority of the option they perceive as more beneficent. Thus, our findings complement the findings in Exley and Kessler (2023), who clarify that, while strategic information avoidance and rational inattention both are driving factors behind ignorance, they differ conceptually, and can be disentangled. The behavior we have documented here – which could be called 'directed inattention' – is a driver of ignorance, but it is distinct from rational inattention.

5 Conclusion

In this study, we provide first evidence on the interaction of defaults regarding choice options (or preselected options, more generally) with information acquisition and processing, by combining the hitherto disconnected literature on default or status quo effects with the literature on information avoidance and ignorance.

Acknowledging the importance of previously documented channels for default effects (e.g., inertia, reference point dependence, or anchoring, see Blumenstock et al., 2018, or Dinner et al., 2011, for reviews), we have been able to show the existence of a novel reason why defaults and preselected options in general may have a larger impact on choices than previously thought.

Firstly, when some decision-relevant information is not immediately available, the presence of a preselected option, for example in the form of a default nudge, induces ignorance. Participants reveal information to a significantly lower degree if a preselected option is available – no matter if it is self-chosen, or whether it has been randomly allocated (and communicated so). This effect can thus not be explained by participants seeing the preselected option as a recommendation (see, e.g., Altmann et al., 2022). Since an initial lack of at least some decision-relevant information applies to almost all decision situations outside stylized experiments, our results suggest that the effects manifested in real-world settings that are caused by one option being preselected, e.g., default nudges or previous choices determined earlier in time, may even be larger than those measured in experiments, and not necessarily in the best interest of decision-makers. This finding provides complementary evidence to some recent, rather critical studies on nudges, for example DellaVigna and Linos (2022), which questions the effect sizes of nudges.

Secondly, even if decision-makers have obtained enough information, they account for the information about an option to a different degree if one option is preselected: They are more likely to reach a conclusion in favor of the option that is perceived as superior when the default has been allocated (Babcock et al., 1995; Gneezy et al., 2020; Schwardmann et al., 2022; Saccardo and Serra-Garcia, 2023; Exley, 2020). Hence, the quality of their choices suffers as they fail to take critical information into account.

Overall, our findings imply that choice architects, regulators and management need to take opportunities for information avoidance and ignorance into account when considering the design of a decision situation. Not only do decision-makers remain more ignorant with respect to information about their choice options in the presence of a preselected option; they also process information differently. Hence, if employed, defaults need to be set *very* carefully – to ensure that decision-makers, not choice architects, benefit from intended default effects and to minimize welfare or productivity losses due to uninformed decisions.

As decision-relevant information is processed less thoroughly when a preselected option is present, it is important that information is presented in a manner that is easy to digest for decision-makers or that hurdles for information search are minimized. Here, our insights might inform regulation to protect consumers, for example by requiring the provision of easy-to-access and easy-to-compare information about contracts that are automatically renewed year after year. This recommendation also applies to public and private administrative structures more generally, including, for example, public procurement, or the choice of a supplier. Here, legislation and management might be interested in delegating the information search to independent units, and have it triggered automatically.

Finally, our results challenge the view that defaults are in line with the concept of Libertarian Paternalism (Thaler and Sunstein, 2003) by showing that they induce decision-makers to reveal less information and to consider the available information differently – they lead to a bias in interpreting and applying information. Our results show that defaults may be less of an innocent policy and marketing tool than previously thought, and regulators should thus be made well aware of that. Moreover, our findings point out that default nudges easily become sludges (Thaler, 2018) in multiple ways: They may not only be designed to benefit the choice architect instead of the decision-maker; they may also affect the decision-maker's willingness to make an informed and educated decision, thereby decreasing choice quality, and eventually welfare, significantly.

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A Online Appendix: Bot Filter and Attention Checks

The first attention (or bot) check that we implemented allowed subjects to proceed only if the allocation of money to organizations in Stage 1 summed up to \$100, while allocating at most \$40 to a single charity. Once this criterion was met, we checked whether the allocations were sufficiently reasonable: Subjects who allocated money to both organizations of a pair with conflicting goals were made aware of this, and were given the possibility to revise their allocation (note that this did not apply to the political parties, where donations were allowed to both of them). Those who repeatedly allocated money to both organizations in a pair with conflicting goals or even to all four controversial organizations, i.e., both organizations in both pairs with conflicting goals, were excluded from the study.

Not giving any money to either organization in a pair of controversial organizations was considered as a sign of indifference, which was of course no reason for exclusion. In light of the many alternatives that are uncontroversial, and of the participants' limited budget, we argue that the case is rather unlikely in which participants consciously contribute positive amounts to both organizations in a pair of these four organizations pursuing conflicting goals because they are indifferent between the two organizations. In addition, for those participants who do not have any preferences about the organizations in our study, not even the controversial ones, we cannot be sure how to interpret their choices made in later stages of the experiment, which is why they have to be excluded.

In total, 430 participants were not allowed to participate in our study due to failing this second attention check. This check worked as intended: All of the excluded participants allocated money to all four controversial organizations; they allocated money to 8.7 organizations on average (of nine possible ones) compared to 3.3 among the non-excluded participants, and never gave a reasoning for their choice. The amounts they allocated have less variation expressed by a median standard deviation of \$6, compared to \$15 among the non-excluded, with the comparisons being significant in t-tests (p-value < 0.001 in each case).

Using so-called 'honeypots' on this page additionally allows us to identify bots: naming the input fields for custom charities such that bots would fill them in a revealing manner (such as 'name' or 'URL' where a bot would enter a forename, or try to submit the task's URL). Accordingly, 68 participants (about 16% of those who failed the attention check) were identified as bots. However, it is worth noting – especially for researchers interested in online data collection – that these bots spend, on average, *the expected amount of time on, e.g., the instructions page*, that is: A time-based exclusion criteria would not have been successful in filtering out these bots.

B Online Appendix: Additional Results

Table 6: Summary Statistics: Control Variables

	Mean	s.d.	Min	Max	Obs.
Age Information					
Age	42.1	12.9	18	89	2289
Age squared (de-meaned)	165.7	207.6	0	2202	2289
Gender					
Female	0.51	0.50	0	1	2289
Male	0.48	0.50	0	1	2289
Not disclosed	0.01	0.08	0	1	2289
Other	0.00	0.07	0	1	2289
Education					
Professional or tertiary education degree	0.71	0.45	0	1	2289
Started tert. edu., no degree, below 25	0.02	0.13	0	1	2289
Started tert. edu., no degree, above 25	0.17	0.38	0	1	2289
Sec. edu. degree or less	0.10	0.30	0	1	2289
Preferences					
Risk Aversion	0.8	1.0	-2.3	2.5	2275
Impatience	116.7	15.4	100	140	2285
Altruism	7.9	2.4	0	10	2289
Individual Session Information					
Hours since 8am (local time)	8.8	2.7	1.1	19.2	2300
Session completed on a weekend	0.14	0.35	0	1	2300
Time investment: Among Fastest 25%	0.24	0.43	0	1	2300

Table 7: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by The Number of Clicks to Reveal New Information – Negative Binomial Regressions, All Coefficients

	Pooled	Preselected Option		Allocation of Preselection	
		Bad-Apples-Option	No-Bad-Apples-Option	RandomDefault	Self-Selected Default
With Preselection	−0.25***	−0.24**	−0.28**	−0.22*	−0.27**
	(0.10)	(0.10)	(0.14)	(0.11)	(0.11)
Age Information					
Age	−0.03***	−0.03***	−0.03***	−0.03***	−0.03***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Age squared (de-meaned)	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Gender					
Male	0.33***	0.37***	0.33**	0.34***	0.35***
	(0.09)	(0.10)	(0.13)	(0.11)	(0.11)
Not disclosed	0.01	0.06	−0.22	−0.26	0.17
	(0.51)	(0.56)	(0.64)	(0.65)	(0.56)
Other	0.38	0.40		−19.64***	0.69***
	(0.35)	(0.35)		(1.04)	(0.26)
Education					
Started tert. edu., no degree, below 25	−0.19	−0.13	0.23	−0.33	0.42
	(0.54)	(0.65)	(0.65)	(0.68)	(0.63)
Started tert. edu., no degree, above 25	−0.10	−0.24*	−0.11	−0.17	−0.23
	(0.14)	(0.13)	(0.19)	(0.16)	(0.15)
Sec. edu. degree or less	−0.07	−0.03	0.03	0.05	−0.05
	(0.15)	(0.15)	(0.21)	(0.18)	(0.17)
Preferences					
Risk Aversion	0.02	0.08	0.00	0.03	0.06
	(0.05)	(0.05)	(0.07)	(0.07)	(0.06)
Impatience	−0.00	−0.00	0.00	−0.00	−0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Altruism	0.02	0.03	0.00	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Individual Session Information					
Hours since 8am (local time)	0.01	−0.00	0.01	0.01	−0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Session completed on a weekend	0.09	0.07	−0.04	−0.01	0.04
	(0.14)	(0.15)	(0.20)	(0.17)	(0.17)
Time investment: Among Fastest 25%	−1.87***	−1.93***	−1.76***	−1.75***	−2.00***
	(0.14)	(0.14)	(0.18)	(0.17)	(0.14)
Constant	3.10***	3.27***	2.57***	2.90***	3.13***
	(0.47)	(0.50)	(0.66)	(0.60)	(0.55)
Model: Dispersion (Poisson: $\ln(\alpha) = -\infty$)					
$\ln(\alpha)$	0.63***	0.62***	0.64***	0.65***	0.60***
	(0.05)	(0.05)	(0.07)	(0.06)	(0.06)
N	1145	958	571	767	762

Notes: This table shows the coefficients from negative binomial regressions of the number of clicks to reveal new information on indicator variables for the presence of a preselected option (HiddenInformation treatment). In Column 1, we report the results from all participants in the HiddenInformation treatment and pool both ways of determining a preselected option (RandomDefault or Self-SelectedDefault) as well as both possible preselections ('Bad-Apples-Option' and 'No-Bad-Apples-Option'). In Columns 2 and 3, we look at the two possible preselected options in isolation ('Bad-Apples-Option' and 'No-Bad-Apples-Option', respectively), and in Columns 4 and 5, we investigate the ways of allocating a preselected option in isolation (RandomDefault and Self-SelectedDefault, respectively). Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Table 3 in the main text for average marginal (treatment) effects of being confronted with a preselection and Table 8 for results from OLS regressions.
Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by The Number of Clicks to Reveal New Information – OLS Regressions

	Pooled	Preselected Option		Allocation of Preselection	
		Bad-Apples-Option	No-Bad-Apples-Option	RandomDefault	Self-Selected Default
With Preselection	−0.90**	−0.84*	−1.20*	−0.97*	−0.84
	(0.46)	(0.49)	(0.62)	(0.52)	(0.53)
Age Information					
Age	−0.15***	−0.15***	−0.10***	−0.14***	−0.13***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Age squared (de-meaned)	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Gender					
Male	1.80***	1.99***	1.90***	1.70***	2.13***
	(0.43)	(0.47)	(0.64)	(0.54)	(0.54)
Not disclosed	−0.39	−0.30	−2.23	−2.14	−0.30
	(2.71)	(3.07)	(2.69)	(2.46)	(3.49)
Other	3.26	3.36		−5.98***	5.89*
	(3.41)	(3.35)		(1.28)	(3.40)
Education					
Started tert. edu., no degree, below 25	−3.00*	−3.29	−1.19	−3.96*	−0.29
	(1.78)	(2.03)	(2.66)	(2.06)	(2.58)
Started tert. edu., no degree, above 25	−0.90*	−1.27**	−1.00	−1.01	−1.37**
	(0.54)	(0.60)	(0.76)	(0.66)	(0.67)
Sec. edu. degree or less	−0.14	0.07	0.29	0.32	0.07
	(0.69)	(0.77)	(1.10)	(0.90)	(0.89)
Preferences					
Risk Aversion	−0.01	0.13	−0.08	−0.07	0.15
	(0.21)	(0.24)	(0.30)	(0.27)	(0.25)
Impatience	−0.00	−0.00	0.00	−0.00	−0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Altruism	0.12	0.13	0.01	0.10	0.07
	(0.08)	(0.09)	(0.12)	(0.10)	(0.10)
Individual Session Information					
Hours since 8am (local time)	−0.05	−0.06	−0.12	−0.06	−0.12
	(0.08)	(0.09)	(0.11)	(0.10)	(0.11)
Session completed on a weekend	0.09	−0.07	−0.32	−0.15	−0.13
	(0.70)	(0.74)	(1.07)	(0.84)	(0.88)
Time investment: Among Fastest 25%	−6.07***	−6.27***	−5.73***	−5.74***	−6.47***
	(0.37)	(0.41)	(0.53)	(0.46)	(0.45)
Constant	12.70***	12.48***	11.41***	12.07***	12.74***
	(2.24)	(2.53)	(3.17)	(2.74)	(2.84)
N	1145	958	571	767	762

Notes: This table shows the coefficients from OLS regressions of the number of clicks to reveal new information on indicator variables for the presence of a preselected option (HiddenInformation treatment). In Column 1, we report the results from all participants in the HiddenInformation treatment and pool both ways of determining a preselected option (RandomDefault or Self-SelectedDefault) as well as both possible preselections ('Bad-Apples-Option' and 'No-Bad-Apples-Option'). In Columns 2 and 3, we look at the two possible preselected options in isolation ('Bad-Apples-Option' and 'No-Bad-Apples-Option', respectively), and in Columns 4 and 5, we investigate the ways of allocating a preselected option in isolation (RandomDefault and Self-SelectedDefault, rspectively). Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Table 7 above for results from negative binomial regressions, and Table 3 in the main text for resulting average marginal (treatment) effects of being confronted with a preselection.
Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by The Number of Clicks to Reveal New Information – Negative Binomial Regressions, Minimal Specification

	Pooled		Preselected Option				Allocation of Preselection			
			Bad-Apples-Option		No-Bad-Apples-Option		RandomDefault		Self-Selected Default	
	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME
With Preselection	−0.19**	−1.02**	−0.16*	−0.89*	−0.28**	−1.46*	−0.17	−0.90	−0.21**	−1.17**
	(0.10)	(0.51)	(0.10)	(0.54)	(0.14)	(0.75)	(0.11)	(0.60)	(0.10)	(0.55)
Individual Session Information										
Hours since 8am (local time)	0.01	0.07	0.01	0.04	0.01	0.03	0.01	0.07	−0.00	−0.01
	(0.02)	(0.10)	(0.02)	(0.11)	(0.02)	(0.13)	(0.02)	(0.12)	(0.02)	(0.12)
Session completed on a weekend	0.07	0.38	0.04	0.21	−0.08	−0.40	0.05	0.25	−0.04	−0.23
	(0.14)	(0.77)	(0.15)	(0.84)	(0.19)	(0.99)	(0.18)	(0.97)	(0.15)	(0.81)
Time investment: Among Fastest 25%	−1.67***	−8.87***	−1.65***	−9.02***	−1.54***	−8.18***	−1.50***	−8.04***	−1.73***	−9.51***
	(0.15)	(0.89)	(0.17)	(1.01)	(0.20)	(1.19)	(0.18)	(1.07)	(0.17)	(1.07)
Constant	1.89***		1.93***		1.94***		1.85***		2.04***	
	(0.17)		(0.19)		(0.23)		(0.20)		(0.20)	
Model: Dispersion (Poisson: $\ln(\alpha) = -\infty$)										
$\ln(\alpha)$	0.73***		0.74***		0.71***		0.74***		0.71***	
	(0.05)		(0.05)		(0.07)		(0.06)		(0.06)	
N	1155	1155	968	968	573	573	773	773	768	768

Notes: This table shows the coefficients from negative binomial regressions of the number of clicks to reveal new information on indicator variables for the presence of a preselected option (HiddenInformation treatment). In Column 1, we report the results from all participants in the HiddenInformation treatment and pool both ways of determining a preselected option (RandomDefault or Self-SelectedDefault) as well as both possible preselections ('Bad-Apples-Option' and 'No-Bad-Apples-Option'). In Columns 2 and 3, we look at the two possible preselected options in isolation ('Bad-Apples-Option' and 'No-Bad-Apples-Option', respectively), and in Columns 4 and 5, we investigate the ways of allocating a preselected option in isolation (RandomDefault and Self-SelectedDefault, rspectively). In the minimal specification, we control for whether or not the completion time was among the fastest 25%, the session was completed on a weekend, and for the time the session was completed. See Table 3 in the main text for average marginal (treatment) effects of being confronted with a preselection (using the full set of control variables) and Table 8 for results from OLS regressions (using the full set of control variables).
Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by Different Outcomes – Negative Binomial Regressions, All Coefficients

	No. of Clicks	Revealing New Info (=1)	Revealing Critical Info (=1)	Seconds on Page
With Preselection	−0.252***	−0.315**	−0.316*	−0.077***
	(0.097)	(0.156)	(0.162)	(0.029)
Age Information				
Age	−0.033***	−0.044***	−0.048***	0.006***
	(0.004)	(0.007)	(0.007)	(0.001)
Age squared (de-meaned)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Gender				
Male	0.327***	0.562***	0.628***	−0.040
	(0.093)	(0.151)	(0.156)	(0.026)
Not disclosed	0.010	−0.399	−0.304	−0.194
	(0.511)	(0.921)	(0.944)	(0.152)
Other	0.385	1.595*	0.723	0.023
	(0.345)	(0.852)	(0.835)	(0.138)
Education				
Started tert. edu., no degree, below 25	−0.190	−1.083*	−1.076*	−0.009
	(0.541)	(0.630)	(0.645)	(0.118)
Started tert. edu., no degree, above 25	−0.103	−0.447**	−0.492**	−0.020
	(0.137)	(0.206)	(0.217)	(0.033)
Sec. edu. degree or less	−0.071	0.124	0.024	0.032
	(0.148)	(0.250)	(0.262)	(0.045)
Preferences				
Risk Aversion	0.024	0.004	−0.012	0.005
	(0.052)	(0.077)	(0.081)	(0.013)
Impatience	−0.002	−0.000	−0.001	−0.001
	(0.003)	(0.005)	(0.005)	(0.001)
Altruism	0.024	0.041	0.038	0.018***
	(0.019)	(0.031)	(0.033)	(0.006)
Individual Session Information				
Hours since 8am (local time)	0.012	−0.013	−0.026	0.012**
	(0.019)	(0.030)	(0.032)	(0.005)
Session completed on a weekend	0.094	0.071	0.028	0.077*
	(0.141)	(0.243)	(0.254)	(0.041)
Time investment: Among Fastest 25%	−1.874***	−2.380***	−2.714***	−1.499***
	(0.139)	(0.278)	(0.321)	(0.025)
Constant	3.096***	0.893	1.111	3.715***
	(0.473)	(0.760)	(0.800)	(0.136)
Model: Dispersion (Poisson: $\ln(\alpha) = -\infty$)				
$\ln(\alpha)$	0.631***			−1.371***
	(0.051)			(0.035)
N	1145	1145	1145	2275

Notes: This table shows the coefficients of indicator variables for the presence of a preselected option when regressing different measures for information search behavior on these indicator variables for all subjects in the HiddenInformation treatment in Columns 1 to 3, and all subjects in the HiddenInformation and the FullInformation treatments in Column 4 (that is, pooling the possible default options, and pooling possible ways of allocating a preselected option, i.e., the RandomDefault and Self-SelectedDefault conditions). In Column 1, the measure of information search behavior is the number of clicks, as in Tables 3, 7, and 8 and the coefficient results from a negative binomial regression. In Columns 2 and 3, the measures of information search behavior are binary, and indicate whether or not any information additional to the information shown in Stage 2 (when defaults are allocated) or whether critical information (that is, information on a bad apple) has been revealed, respectively. These coefficients result from logit regressions. In Column 4, information search is measured by the number of seconds spent on the information page. As this number is skewed and discrete, we apply a negative binomial regression, from which the reported coefficient results. Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Table 4 in the main text for average marginal (treatment) effects of being confronted with a preselection, and Table 11 for results from OLS regressions. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by Different Outcomes – OLS Regressions

	No. of Clicks	Revealing New Info (=1)	Revealing Critical Info (=1)	Seconds on Page
With Preselection	−0.903**	−0.055**	−0.052**	−4.365**
	(0.458)	(0.027)	(0.026)	(1.713)
Age Information				
Age	−0.146***	−0.007***	−0.008***	0.345***
	(0.021)	(0.001)	(0.001)	(0.072)
Age squared (de-meaned)	0.001	0.000	0.000	0.012*
	(0.001)	(0.000)	(0.000)	(0.006)
Gender				
Male	1.797***	0.093***	0.097***	−1.698
	(0.430)	(0.025)	(0.024)	(1.525)
Not disclosed	−0.387	−0.074	−0.055	−6.852
	(2.712)	(0.147)	(0.148)	(6.724)
Other	3.261	0.320*	0.134	0.299
	(3.408)	(0.181)	(0.186)	(6.028)
Education				
Started tert. edu., no degree, below 25	−2.997*	−0.180*	−0.167*	−1.103
	(1.776)	(0.099)	(0.099)	(6.064)
Started tert. edu., no degree, above 25	−0.904*	−0.069**	−0.070**	−1.819
	(0.544)	(0.031)	(0.030)	(1.923)
Sec. edu. degree or less	−0.136	0.020	0.004	1.597
	(0.690)	(0.041)	(0.040)	(2.650)
Preferences				
Risk Aversion	−0.006	0.000	−0.002	0.298
	(0.212)	(0.012)	(0.012)	(0.690)
Impatience	−0.005	−0.000	−0.000	−0.017
	(0.014)	(0.001)	(0.001)	(0.048)
Altruism	0.119	0.006	0.005	0.767***
	(0.079)	(0.005)	(0.004)	(0.288)
Individual Session Information				
Hours since 8am (local time)	−0.050	−0.002	−0.004	0.762**
	(0.084)	(0.005)	(0.005)	(0.310)
Session completed on a weekend	0.090	0.014	0.007	4.625*
	(0.702)	(0.042)	(0.041)	(2.455)
Time investment: Among Fastest 25%	−6.070***	−0.308***	−0.307***	−44.953***
	(0.373)	(0.023)	(0.022)	(1.075)
Constant	12.697***	0.613***	0.627***	36.303***
	(2.236)	(0.127)	(0.125)	(7.643)
N	1145	1145	1145	2275

Notes: This table shows the coefficients of indicator variables for the presence of a preselected option when regressing different measures for information search behavior on these indicator variables using OLS regressions for all subjects in the HiddenInformation treatment in Columns 1 to 3, and all subjects in the HiddenInformation and the FullInformation treatments in Column 4 (that is, pooling the possible default options, and pooling possible ways of allocating a preselected option, i.e., the RandomDefault and Self-SelectedDefault conditions). In Column 1, the measure of information search behavior is the number of clicks, as in Tables 3, 7, and 8. In Columns 2 and 3, the measures of information search behavior are binary, and indicate whether or not any information additional to the information shown in Stage 2 (when preselected options are allocated) or whether critical information (that is, information on a bad apple) has been revealed, respectively. In Column 4, information search is measured by the number of seconds spent on the information page. See Table 10 above for results from GLM regressions, and Table 4 in the main text for resulting average marginal (treatment) effects of being confronted with a preselection. Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*).
Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: The Effect of the Presence of a Preselected Option on Information Search Behavior Measured by Different Outcomes – Negative Binomial Regressions, Minimal Specification

	No. of Clicks		Revealing New Info (=1)		Revealing Critical Info (=1)		Seconds on Page	
	Coeff.	AME	Coeff.	AME	Coeff.	AME	Coeff.	AME
With Preselection	−0.192**	−1.020**	−0.240	−0.043*	−0.246	−0.042	−0.078***	−3.863***
	(0.095)	(0.512)	(0.146)	(0.026)	(0.150)	(0.025)	(0.029)	(1.468)
Individual Session Information								
Hours since 8am (local time)	0.013	0.067	−0.008	−0.001	−0.022	−0.004	0.011**	0.549**
	(0.018)	(0.096)	(0.028)	(0.005)	(0.030)	(0.005)	(0.005)	(0.249)
Session completed on a weekend	0.071	0.379	0.091	0.016	0.049	0.008	0.078*	3.866*
	(0.145)	(0.769)	(0.228)	(0.041)	(0.235)	(0.040)	(0.041)	(2.044)
Time investment: Among Fastest 25%	−1.668***	−8.868***	−2.070***	−0.370***	−2.370***	−0.401***	−1.570***	−77.752***
	(0.149)	(0.891)	(0.268)	(0.045)	(0.317)	(0.052)	(0.024)	(2.113)
Constant	1.891***		−0.513*		−0.481		4.057***	
	(0.174)		(0.288)		(0.300)		(0.053)	
Model: Dispersion (Poisson: $\ln(\alpha) = -\infty$)								
$\ln(\alpha)$	0.733***						−1.323***	
	(0.047)						(0.035)	
N	1155	1155	1155	1155	1155	1155	2300	2300

Notes: This table shows the coefficients of indicator variables for the presence of a preselected option when regressing different measures for information search behavior on these indicator variables for all subjects in the HiddenInformation treatment in Columns 1 to 3, and all subjects in the HiddenInformation and the FullInformation treatments in Column 4 (that is, pooling the possible default options, and pooling possible ways of allocating a preselected option, i.e., the RandomDefault and Self-SelectedDefault conditions). In Column 1, the measure of information search behavior is the number of clicks, as in Tables 3, 7, and 8, and the coefficient results from a negative binomial regression. In Columns 2 and 3, the measures of information search behavior are binary, and indicate whether or not any information additional to the information shown in Stage 2 (when defaults are allocated) or whether critical information (that is, information on a bad apple) has been revealed, respectively. These coefficients result from logit regressions. In Column 4, information search is measured by the number of seconds spent on the information page. As this number is skewed and discrete, we apply a negative binomial regression, from which the reported coefficient results. In the minimal specification, we control for whether or not the completion time was among the fastest 25%, the session was completed on a weekend, and for the time the session was completed. See Table 4 in the main text for average marginal (treatment) effects of being confronted with a preselection (using the full set of control variables), and Table 11 for results from OLS regressions (using the full set of control variables). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: The Value of Information for Decision-Making With and Without Default – All Coefficients

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.774***	0.364***	0.529***	2.158***	0.442***	0.528***
	(0.189)	(0.031)	(0.085)	(0.277)	(0.054)	(0.103)
(2) –With Preselection (=1)	1.235***	0.183***	0.286***	1.614***	0.260***	0.323***
	(0.178)	(0.023)	(0.054)	(0.244)	(0.044)	(0.073)
Age Information						
Age	−0.003			−0.002		
	(0.005)			(0.007)		
Age squared (de-meaned)	−0.000			0.000		
	(0.000)			(0.000)		
Gender						
Male	−0.053			−0.111		
	(0.095)			(0.147)		
Not disclosed	−0.145			−0.223		
	(0.596)			(0.849)		
Other	0.941			−0.614		
	(0.662)			(0.963)		
Education						
Started tert. edu., no degree, below 25	−0.695*			−0.232		
	(0.422)			(0.658)		
Started tert. edu., no degree, above 25	−0.001			0.115		
	(0.125)			(0.185)		
Sec. edu. degree or less	−0.286*			−0.609**		
	(0.165)			(0.282)		
Preferences						
Risk Aversion	−0.038			−0.022		
	(0.048)			(0.073)		
Impatience	−0.004			−0.000		
	(0.003)			(0.005)		
Altruism	−0.009			−0.011		
	(0.020)			(0.029)		
Individual Session Information						
Hours since 8am (local time)	0.010			0.023		
	(0.017)			(0.028)		
Session completed on a weekend	−0.094			−0.686***		
	(0.131)			(0.259)		
Time investment: Among Fastest 25%	−0.176			0.409**		
	(0.119)			(0.192)		
Constant	−0.928*			−1.672**		
	(0.515)			(0.772)		
Difference between (1) and (2)	0.540***	0.181***	0.242***	0.545**	0.182***	0.205**
	(0.000)	(0.000)	(0.000)	(0.036)	(0.006)	(0.017)
Observations	2273	2273	2273	1144	1144	1144

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with and without default. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without preselected option. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of criticial information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Tables 14 to 17 for results when restricting the sample to the RandomDefault, the Self-SelectedDefault, or those who were facing the 'No-Bad-Apples-Option' or the 'Bad-Apples-Option' or as default, respectively.

Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: The Value of Information for Decision-Making With and Without Default – Randomly Allocated Defaults Only (RandomDefault Condition)

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.800***	0.367***	0.560***	2.177***	0.438***	0.529***
	(0.190)	(0.031)	(0.114)	(0.287)	(0.055)	(0.122)
(2) –With (Random) Preselection (=1)	1.493***	0.158***	0.304***	1.815***	0.223***	0.310***
	(0.190)	(0.035)	(0.091)	(0.296)	(0.061)	(0.107)
Age Information						
Age	−0.002			−0.002		
	(0.005)			(0.009)		
Age squared (de-meaned)	−0.000			0.000		
	(0.000)			(0.001)		
Gender						
Male	−0.061			−0.089		
	(0.114)			(0.175)		
Not disclosed	0.229			−0.029		
	(0.590)			(0.805)		
Other	1.066			0.000		
	(1.280)			(.)		
Education						
Started tert. edu., no degree, below 25	−0.619			−0.440		
	(0.482)			(0.674)		
Started tert. edu., no degree, above 25	−0.045			0.025		
	(0.147)			(0.216)		
Sec. edu. degree or less	−0.499**			−0.997***		
	(0.202)			(0.361)		
Preferences						
Risk Aversion	−0.043			−0.027		
	(0.056)			(0.086)		
Impatience	−0.004			0.001		
	(0.004)			(0.006)		
Altruism	−0.010			−0.023		
	(0.024)			(0.036)		
Individual Session Information						
Hours since 8am (local time)	0.002			0.023		
	(0.020)			(0.032)		
Session completed on a weekend	−0.260*			−1.229***		
	(0.154)			(0.327)		
Time investment: Among Fastest 25%	−0.190			0.355		
	(0.140)			(0.225)		
Constant	−0.852			−1.715*		
	(0.606)			(0.896)		
Difference between (1) and (2)	0.307**	0.208***	0.256***	0.362	0.216***	0.219**
	(0.020)	(0.000)	(0.001)	(0.249)	(0.007)	(0.047)
Observations	1526	1526	1526	765	765	766

Notes: This table shows in Columns 1 and 4 the coefficients of a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with and without (randomly allocated) default. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without (randomly allocated) default. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of critical information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained by computing the (average) marginal effect of the respective coefficient resulting from the logistic regression. The relative risk reduction is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Tables 15 to 17 for results when restricting the sample to the Self-SelectedDefault, or those who were facing the 'No-Bad-Apples-Option' or the 'Bad-Apples-Option' as default, respectively.
Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: The Value of Information for Decision-Making With and Without Default – Self-Selected Defaults Only (Self-SelectedDefault condition)

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.761***	0.361***	0.530***	2.114***	0.437***	0.497***
	(0.190)	(0.032)	(0.099)	(0.280)	(0.055)	(0.104)
(2) –With (Self-Chosen) Preselection (=1)	0.956***	0.208***	0.289***	1.399***	0.295***	0.319***
	(0.192)	(0.029)	(0.059)	(0.300)	(0.060)	(0.081)
Age Information						
Age	−0.003			−0.006		
	(0.006)			(0.009)		
Age squared (de-meaned)	−0.000			−0.000		
	(0.000)			(0.001)		
Gender						
Male	−0.062			−0.154		
	(0.122)			(0.195)		
Not disclosed	−0.379			0.166		
	(0.809)			(0.893)		
Other	1.355*			−0.220		
	(0.822)			(1.075)		
Education						
Started tert. edu., no degree, below 25	−0.899*			−0.812		
	(0.525)			(0.953)		
Started tert. edu., no degree, above 25	0.019			0.198		
	(0.159)			(0.246)		
Sec. edu. degree or less	−0.200			−0.289		
	(0.213)			(0.356)		
Preferences						
Risk Aversion	−0.105*			−0.075		
	(0.061)			(0.098)		
Impatience	−0.005			0.004		
	(0.004)			(0.006)		
Altruism	−0.010			−0.012		
	(0.026)			(0.038)		
Individual Session Information						
Hours since 8am (local time)	0.013			0.002		
	(0.021)			(0.036)		
Session completed on a weekend	0.050			−0.204		
	(0.168)			(0.324)		
Time investment: Among Fastest 25%	−0.282*			0.325		
	(0.155)			(0.261)		
Constant	−0.797			−1.833*		
	(0.643)			(1.003)		
Difference between (1) and (2)	0.806***	0.153***	0.240***	0.715**	0.142*	0.178**
	(0.000)	(0.000)	(0.001)	(0.018)	(0.067)	(0.050)
Observations	1511	1511	1511	761	761	761

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with and without (self-chosen) default. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without (self-chosen) default. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of critical information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Tables 14 to 17 for results when restricting the sample to the RandomDefault, the Self-SelectedDefault, or those who were facing the 'No-Bad-Apples-Option' or the 'Bad-Apples-Option' as default, respectively.

Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: The Value of Information for Decision-Making With and Without Default – Default is the No-Bad-Apples-Option

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.791***	0.366***	0.494***	2.180***	0.448***	0.491***
	(0.190)	(0.031)	(0.099)	(0.289)	(0.056)	(0.114)
(2) –With No-Bad-Apple-Preselection (=1)	3.157***	0.083*	0.488	3.165***	0.084	0.255
	(0.240)	(0.045)	(0.438)	(0.472)	(0.078)	(0.269)
Age Information						
Age	0.005			0.008		
	(0.007)			(0.011)		
Age squared (de-meaned)	−0.000			−0.000		
	(0.000)			(0.001)		
Gender						
Male	−0.010			−0.000		
	(0.140)			(0.210)		
Not disclosed	0.058			−0.024		
	(0.988)			(1.239)		
Other	0.000					
	(.)					
Education						
Started tert. edu., no degree, below 25	−0.684			−0.201		
	(0.546)			(0.714)		
Started tert. edu., no degree, above 25	−0.024			0.062		
	(0.179)			(0.271)		
Sec. edu. degree or less	−0.373			−0.529		
	(0.228)			(0.368)		
Preferences						
Risk Aversion	−0.070			−0.011		
	(0.065)			(0.100)		
Impatience	−0.004			0.002		
	(0.005)			(0.007)		
Altruism	−0.003			−0.001		
	(0.032)			(0.046)		
Individual Session Information						
Hours since 8am (local time)	−0.005			−0.021		
	(0.023)			(0.037)		
Session completed on a weekend	−0.052			−0.591*		
	(0.187)			(0.351)		
Time investment: Among Fastest 25%	0.079			0.607**		
	(0.166)			(0.263)		
Constant	−1.188			−2.116**		
	(0.724)			(1.031)		
Difference between (1) and (2)	−1.366***	0.282***	0.006	−0.985**	0.364***	0.236
	(0.000)	(0.000)	(0.986)	(0.041)	(0.000)	(0.340)
Observations	1133	1133	1134	570	570	570

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with preselected 'No-Bad-Apples-Option' and without any preselection. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without preselected 'No-Bad-Apples-Option'. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of critical information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Tables 14 to 17 for results when restricting the sample to the RandomDefault, the Self-SelectedDefault, or those who were facing the 'Bad-Apples-Option' or the 'No-Bad-Apples-Option' as default, respectively.

Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 17: The Value of Information for Decision-Making With and Without Default – Default is the Bad-Apples-Option

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.756***	0.354***	0.649***	2.083***	0.421***	0.576***
	(0.195)	(0.032)	(0.120)	(0.286)	(0.056)	(0.112)
(2) –With Bad-Apple-Preselection (=1)	0.547***	0.233***	0.333***	1.160***	0.336***	0.389***
	(0.188)	(0.017)	(0.052)	(0.271)	(0.046)	(0.070)
Age Information						
Age	−0.011*			−0.016		
	(0.006)			(0.011)		
Age squared (de-meaned)	−0.000			−0.000		
	(0.000)			(0.001)		
Gender						
Male	−0.037			−0.102		
	(0.121)			(0.210)		
Not disclosed	−0.083			0.011		
	(0.664)			(0.883)		
Other	2.326***			0.326		
	(0.828)			(1.093)		
Education						
Started tert. edu., no degree, below 25	−1.225**			−1.787*		
	(0.500)			(1.018)		
Started tert. edu., no degree, above 25	−0.037			0.242		
	(0.158)			(0.272)		
Sec. edu. degree or less	−0.324			−0.752*		
	(0.218)			(0.411)		
Preferences						
Risk Aversion	−0.066			−0.021		
	(0.061)			(0.101)		
Impatience	−0.009**			−0.002		
	(0.004)			(0.006)		
Altruism	0.005			−0.012		
	(0.026)			(0.045)		
Individual Session Information						
Hours since 8am (local time)	−0.011			−0.014		
	(0.021)			(0.039)		
Session completed on a weekend	−0.116			−0.537*		
	(0.164)			(0.321)		
Time investment: Among Fastest 25%	−0.733***			0.173		
	(0.157)			(0.308)		
Constant	0.254			−0.481		
	(0.629)			(1.062)		
Difference between (1) and (2)	1.209***	0.122***	0.316***	0.922***	0.085	0.187**
	(0.000)	(0.001)	(0.000)	(0.001)	(0.210)	(0.043)
Observations	1903	1903	1903	957	957	957

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with preselected 'Bad-Apples-Option' and without any preselection. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without preselected 'Bad-Apples-Option'. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of critical information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". Omitted categories are Female (for *Gender*) and having completed a professional or tertiary education degree (for *Education*). See Tables 14 to 16 for results when restricting the sample to the RandomDefault, the Self-SelectedDefault, or those who were facing the 'No-Bad-Apples-Option' as default, respectively.
Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 18: The Value of Information for Decision-Making With and Without Default – Minimal Specification

	FullInformation + HiddenInformation			Only HiddenInformation		
	Logit Coefficient	ARR (AME)	RRR	Logit Coefficient	ARR (AME)	RRR
Critical Information Available						
(1) –Without Preselection (=1)	1.760***	0.362***	0.425***	2.120***	0.438***	0.490***
	(0.188)	(0.031)	(0.034)	(0.271)	(0.054)	(0.062)
(2) –With Preselection (=1)	1.234***	0.185***	0.230***	1.559***	0.252***	0.293***
	(0.177)	(0.023)	(0.027)	(0.234)	(0.042)	(0.050)
Individual Session Information						
Hours since 8am (local time)	0.010			0.017		
	(0.017)			(0.029)		
Session completed on a weekend	−0.107			−0.689***		
	(0.129)			(0.256)		
Time investment: Among Fastest 25%	−0.169			0.423**		
	(0.113)			(0.177)		
Constant	−1.720***			−1.943***		
	(0.233)			(0.328)		
Difference between (1) and (2)	0.526***	0.177***	0.195***	0.561**	0.185***	0.197***
	(0.000)	(0.000)	(0.000)	(0.029)	(0.006)	(0.008)
Observations	2298	2298	2298	1154	1154	1154

Notes: This table shows in Columns 1 and 4 the coefficients from a logit regression of choosing the 'No-Bad-Apples-Option' on an indicator for the display of at least some critical information, i.e., information on unwanted charities in the 'Bad-Apples-Option' (main effects reported) – with and without default. In Columns 2 and 5, we report corresponding (average) marginal effects (AME), which express the increase in the likelihood to choose the 'No-Bad-Apples-Option' (in percentage points) that is associated with the availability of information – with and without preselected option. Following the medical literature, this increase (in percentage points, i.e., in absolute terms) in the likelihood to choose the 'No-Bad-Apples-Option' can equivalently be interpreted as the reduction of the 'risk' (in absolute terms) to choose the 'Bad-Apples-Option', which is referred to as absolute risk reduction (ARR). Considering the share choosing the 'Bad-Apples-Option' in absence of criticial information makes it possible to express the AME/ARR in relative terms, a measure known as relative risk reduction (RRR), which is presented in Columns 3 and 6. Following the definition in Footnote 20, the RRR is obtained via OLS regression for estimation of the respective shares, which are subsequently used for computation of the relative risk reduction with standard errors resulting from the "delta method". In the minimal specification, we control for whether or not the completion time was among the fastest 25%, the session was completed on a weekend, and for the time the session was completed. See Table 13 for results when using the full set of control variables.
Robust standard errors (rows 1 to 3) and p-values (row 4) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

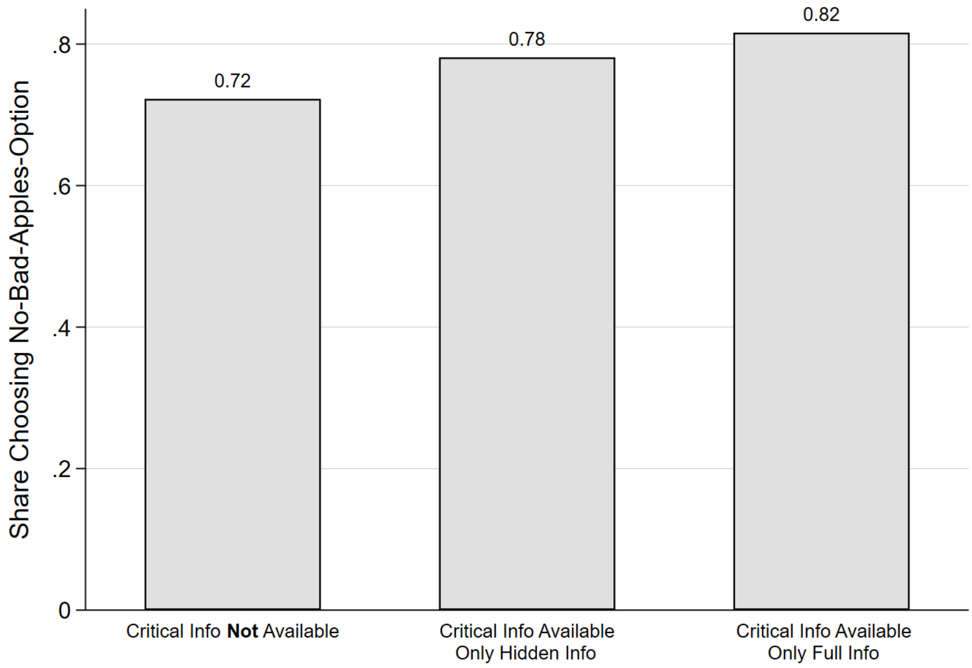
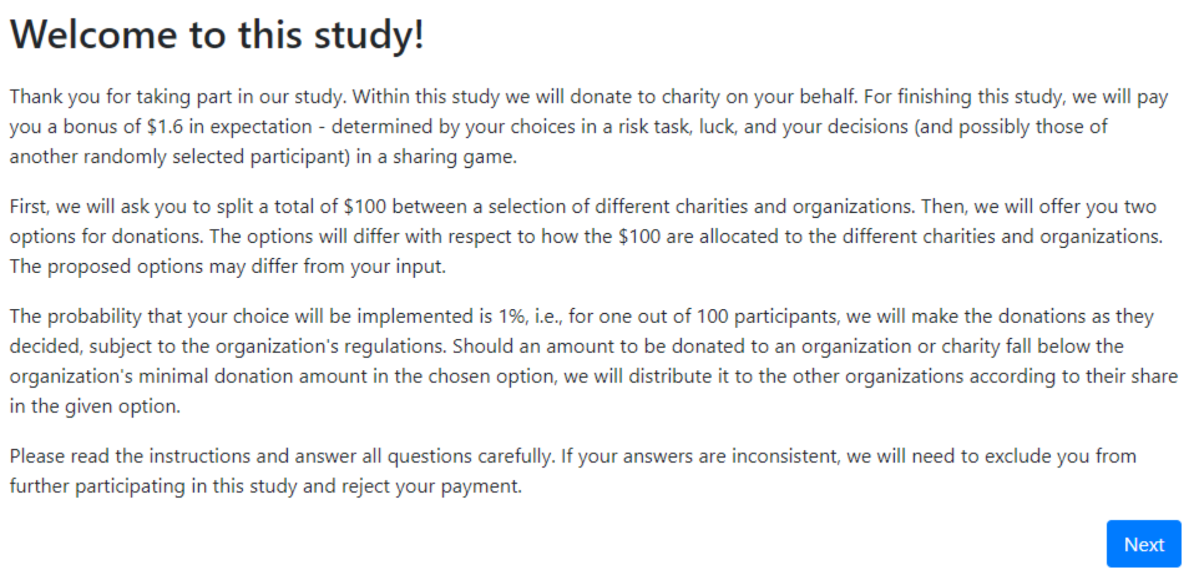


Figure 2: Choosing the 'No-Bad-Apples-Option' When It Has Been Set as Default (Randomly Allocated or Self-Selected) – With and Without Critical Information Displayed: Inertia (Not Switching) Would Be Expected With Critical Information Available, but Active Switching is Observed for About 20% of Participants.

C Online Appendix: Screenshots


Welcome to this study!

Thank you for taking part in our study. Within this study we will donate to charity on your behalf. For finishing this study, we will pay you a bonus of $1.6 in expectation - determined by your choices in a risk task, luck, and your decisions (and possibly those of another randomly selected participant) in a sharing game.

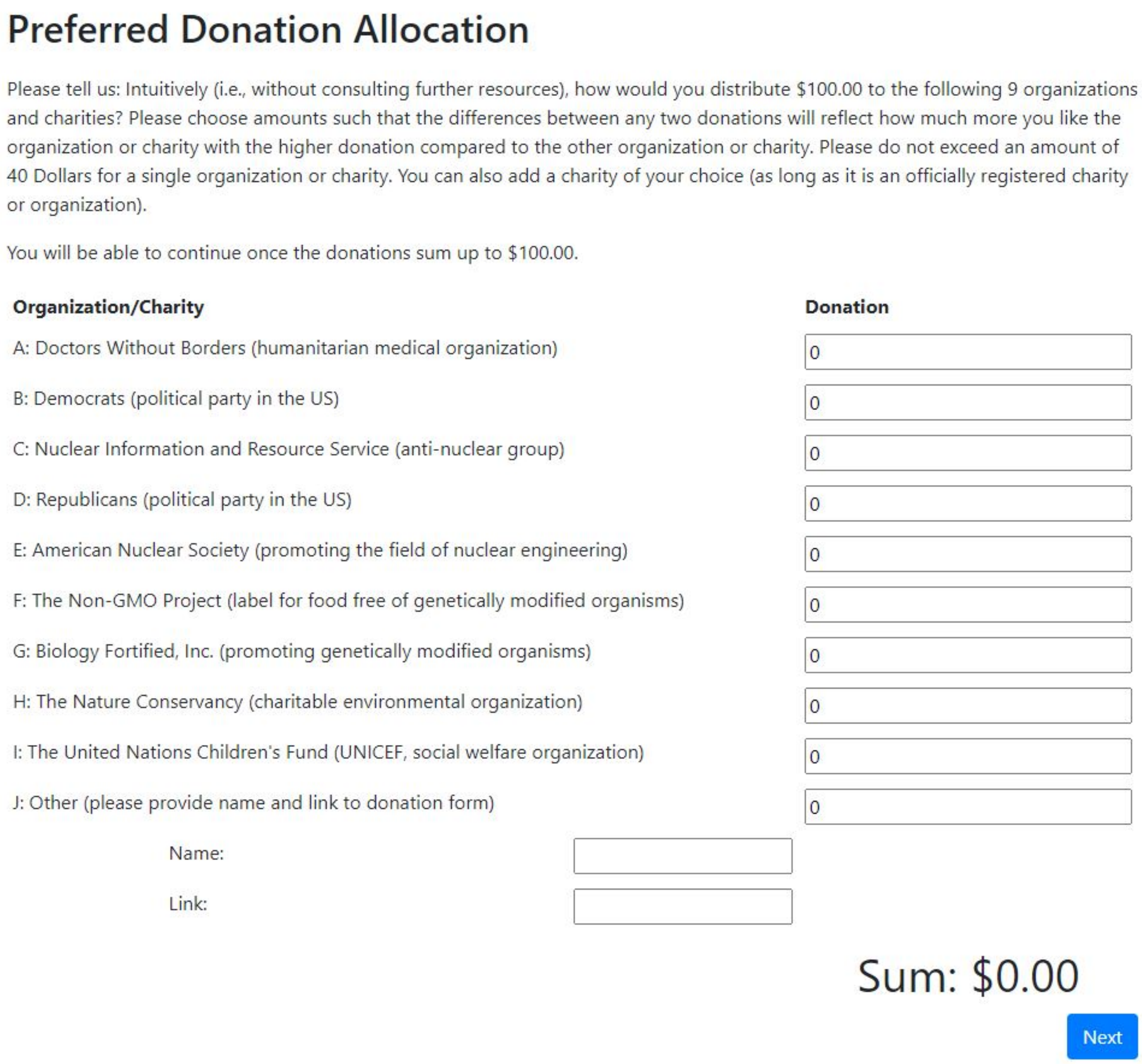
First, we will ask you to split a total of $100 between a selection of different charities and organizations. Then, we will offer you two options for donations. The options will differ with respect to how the $100 are allocated to the different charities and organizations. The proposed options may differ from your input.

The probability that your choice will be implemented is 1%, i.e., for one out of 100 participants, we will make the donations as they decided, subject to the organization's regulations. Should an amount to be donated to an organization or charity fall below the organization's minimal donation amount in the chosen option, we will distribute it to the other organizations according to their share in the given option.

Please read the instructions and answer all questions carefully. If your answers are inconsistent, we will need to exclude you from further participating in this study and reject your payment.

Next

Figure 3: Instructions



Preferred Donation Allocation

Please tell us: Intuitively (i.e., without consulting further resources), how would you distribute $100.00 to the following 9 organizations and charities? Please choose amounts such that the differences between any two donations will reflect how much more you like the organization or charity with the higher donation compared to the other organization or charity. Please do not exceed an amount of 40 Dollars for a single organization or charity. You can also add a charity of your choice (as long as it is an officially registered charity or organization).

You will be able to continue once the donations sum up to $100.00.

Organization/Charity	**Donation**
A: Doctors Without Borders (humanitarian medical organization)	0
B: Democrats (political party in the US)	0
C: Nuclear Information and Resource Service (anti-nuclear group)	0
D: Republicans (political party in the US)	0
E: American Nuclear Society (promoting the field of nuclear engineering)	0
F: The Non-GMO Project (label for food free of genetically modified organisms)	0
G: Biology Fortified, Inc. (promoting genetically modified organisms)	0
H: The Nature Conservancy (charitable environmental organization)	0
I: The United Nations Children's Fund (UNICEF, social welfare organization)	0
J: Other (please provide name and link to donation form)	0

Name:

Link:

Sum: $0.00

Next

Figure 4: Donation Allocation

Preferred Donation Allocation: Some more details

Please let us know: Which charity/organization do you support the most?

Moreover, you stated that you would donate $0.00 to some charities or organization(s).

Which of these do you support the least?

Are you against donating money to any of these charities on your behalf?

Charity	Against a donation?
The United Nations Children's Fund (UNICEF, social welfare organization)	○ Yes ○ No
The Nature Conservancy (charitable environmental organization)	○ Yes ○ No
Biology Fortified, Inc. (promoting genetically modified organisms)	○ Yes ○ No
American Nuclear Society (promoting the field of nuclear engineering)	○ Yes ○ No
Democrats (political party in the US)	○ Yes ○ No

Next

Figure 5: Additional Information on Preferences with Respect to Charities

Thanks! Please consider the following options

Thank you very much! From now on, there won't be any consistency check anymore, it's just about how you personally would decide.

As indicated at the beginning, we will donate money to charities and organizations on your behalf, if your choice is selected for implementation. Every participant can decide between two options for donations, which amount to $100.00 each.

Here are the two options:

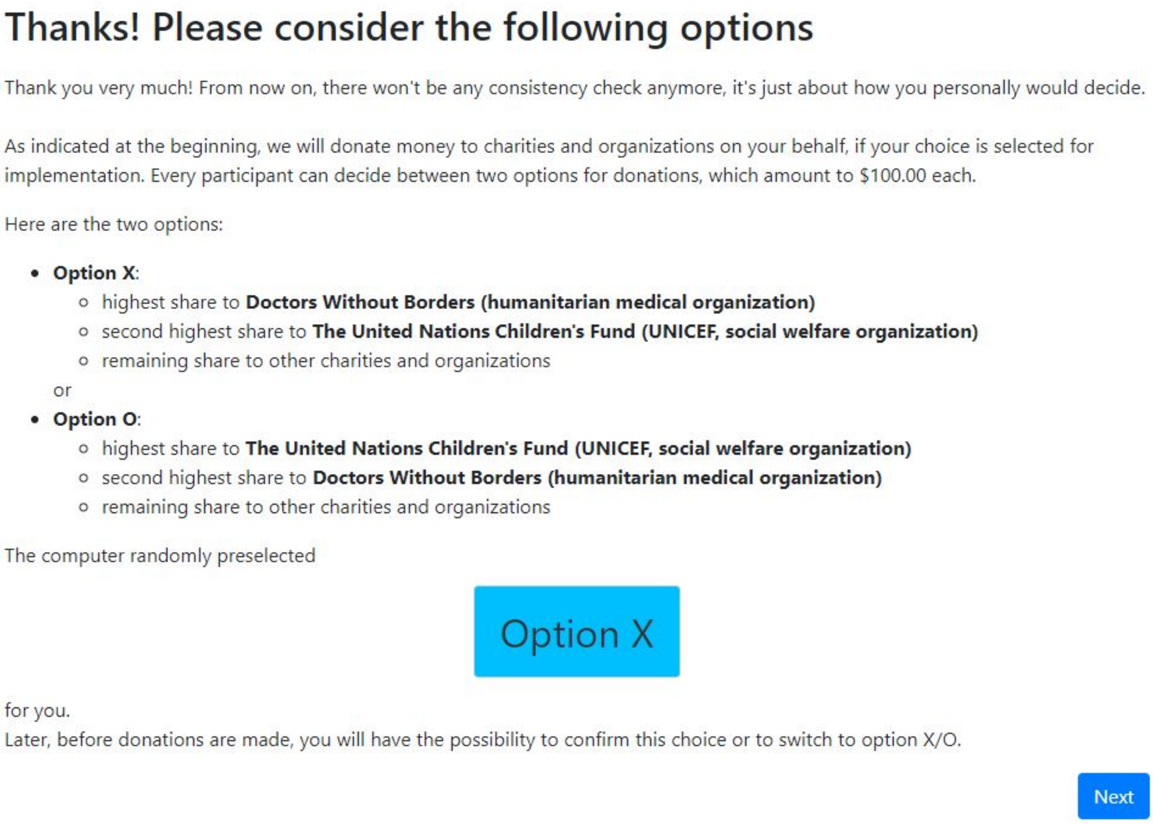
- **Option X**:
  - highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - second highest share to **The Nature Conservancy (charitable environmental organization)**
  - remaining share to other charities and organizations

  or
- **Option O**:
  - highest share to **The Nature Conservancy (charitable environmental organization)**
  - second highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - remaining share to other charities and organizations

Later, before donations are made, you will have the possibility to make your choice.

Next

Figure 6: Incomplete Information on Options without Default



Thanks! Please consider the following options

Thank you very much! From now on, there won't be any consistency check anymore, it's just about how you personally would decide.

As indicated at the beginning, we will donate money to charities and organizations on your behalf, if your choice is selected for implementation. Every participant can decide between two options for donations, which amount to $100.00 each.

Here are the two options:

- **Option X**:
  - highest share to **Doctors Without Borders (humanitarian medical organization)**
  - second highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - remaining share to other charities and organizations

  or
- **Option O**:
  - highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - second highest share to **Doctors Without Borders (humanitarian medical organization)**
  - remaining share to other charities and organizations

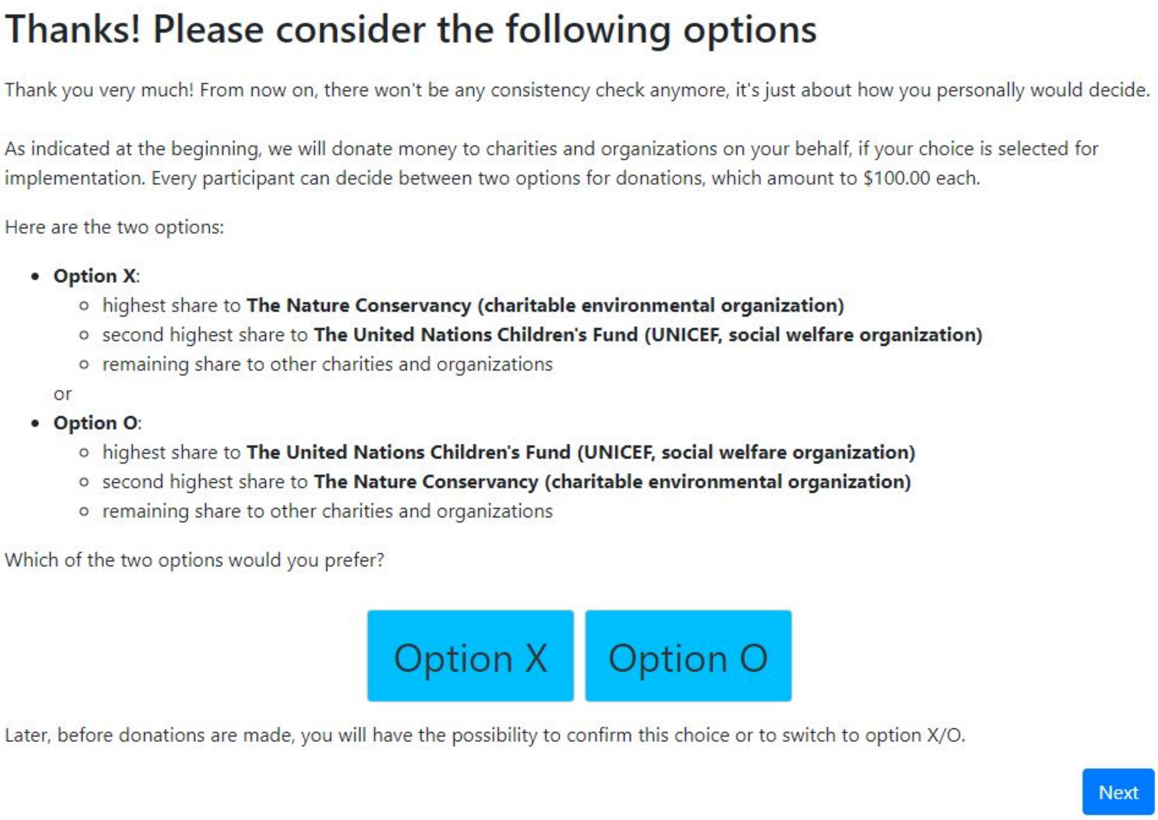
The computer randomly preselected

Option X

for you.
Later, before donations are made, you will have the possibility to confirm this choice or to switch to option X/O.

Next

Figure 7: Incomplete Information on Options with Randomly Selected Default



Thanks! Please consider the following options

Thank you very much! From now on, there won't be any consistency check anymore, it's just about how you personally would decide.

As indicated at the beginning, we will donate money to charities and organizations on your behalf, if your choice is selected for implementation. Every participant can decide between two options for donations, which amount to $100.00 each.

Here are the two options:

- **Option X**:
  - highest share to **The Nature Conservancy (charitable environmental organization)**
  - second highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - remaining share to other charities and organizations

  or
- **Option O**:
  - highest share to **The United Nations Children's Fund (UNICEF, social welfare organization)**
  - second highest share to **The Nature Conservancy (charitable environmental organization)**
  - remaining share to other charities and organizations

Which of the two options would you prefer?

Option X Option O

Later, before donations are made, you will have the possibility to confirm this choice or to switch to option X/O.

Next

Figure 8: Incomplete Information on Options with Self-Selected Default

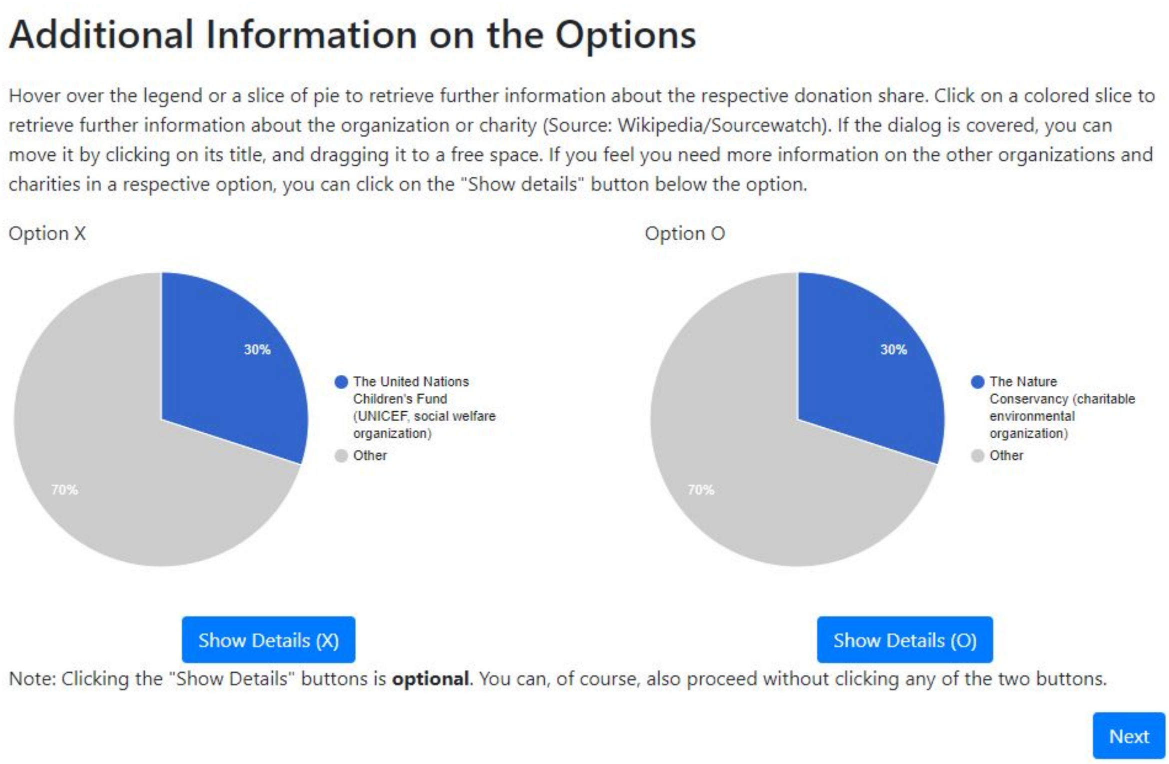


Figure 9: No Default, Hidden Info

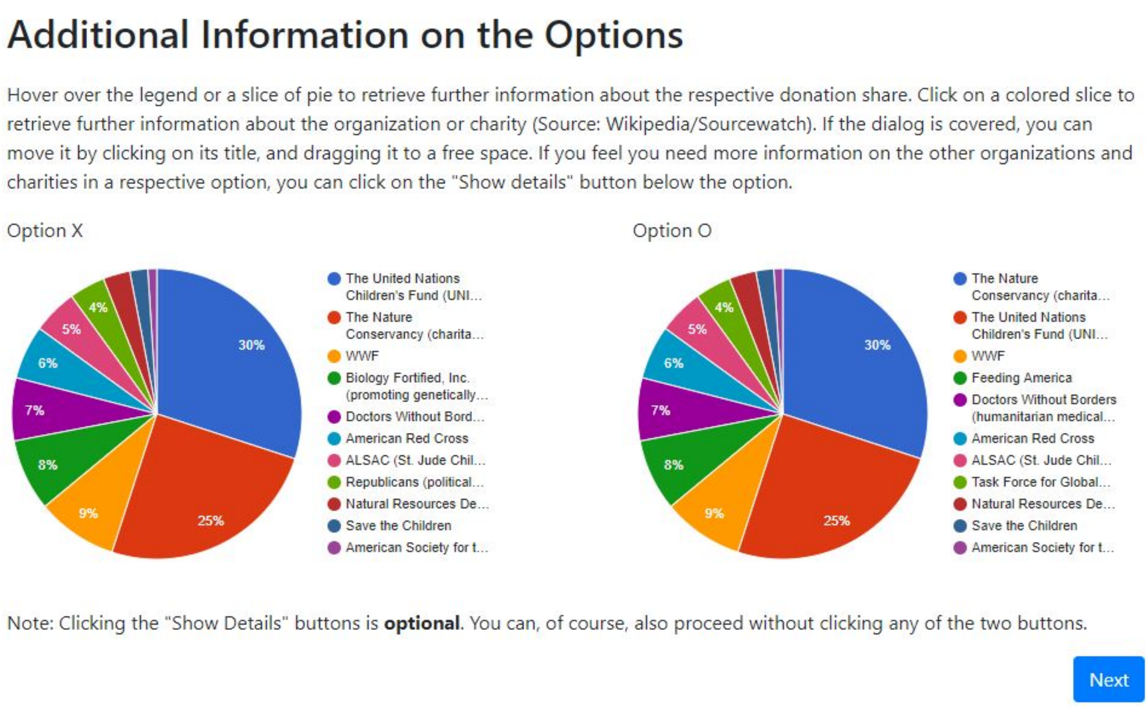


Figure 10: No Default, Hidden Info Revealed

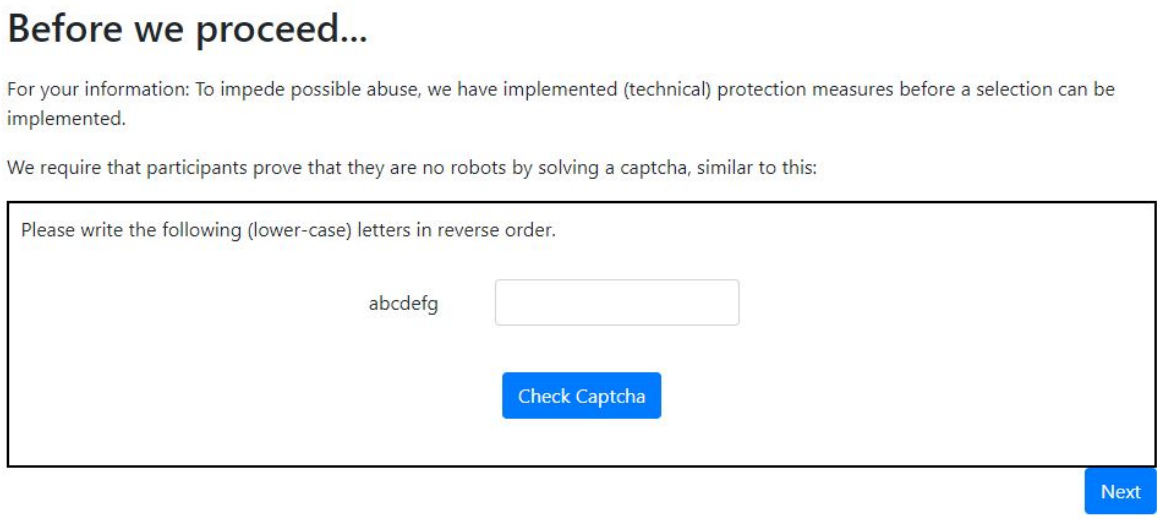


Figure 11: Letter Task

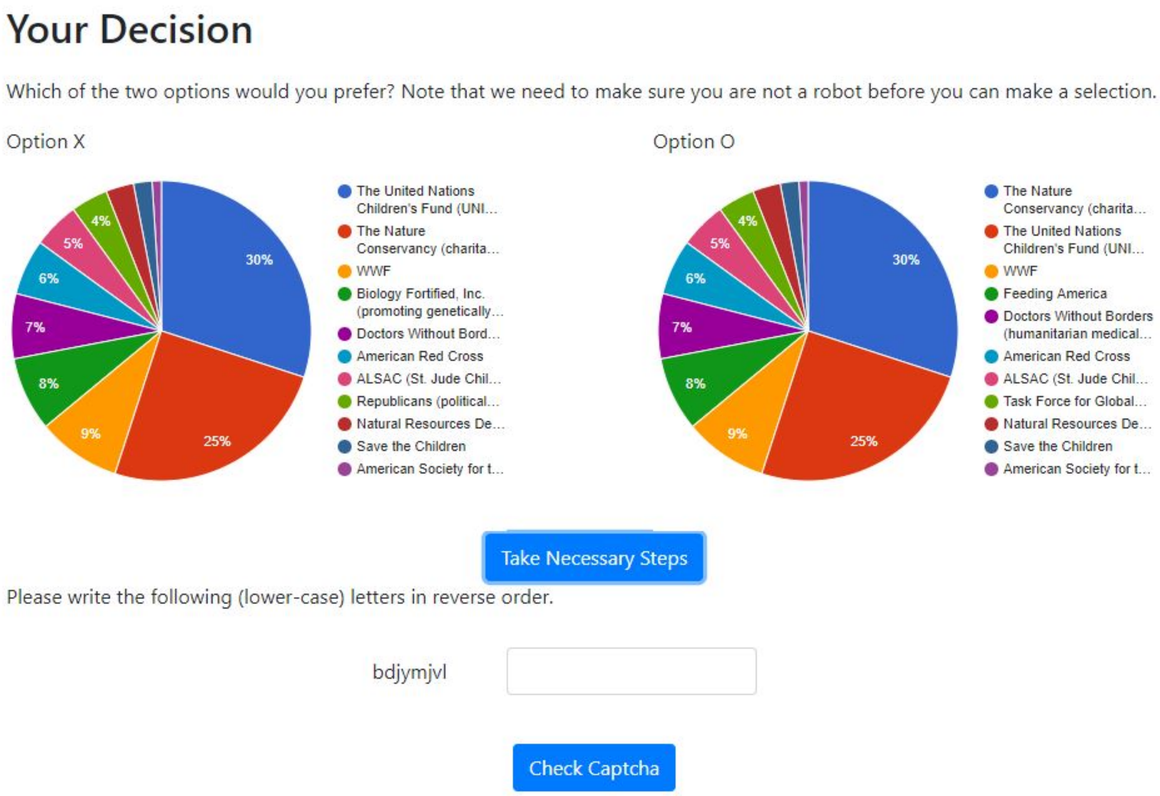


Figure 12: Decision without Default (Full Info/Hidden Info Revealed)

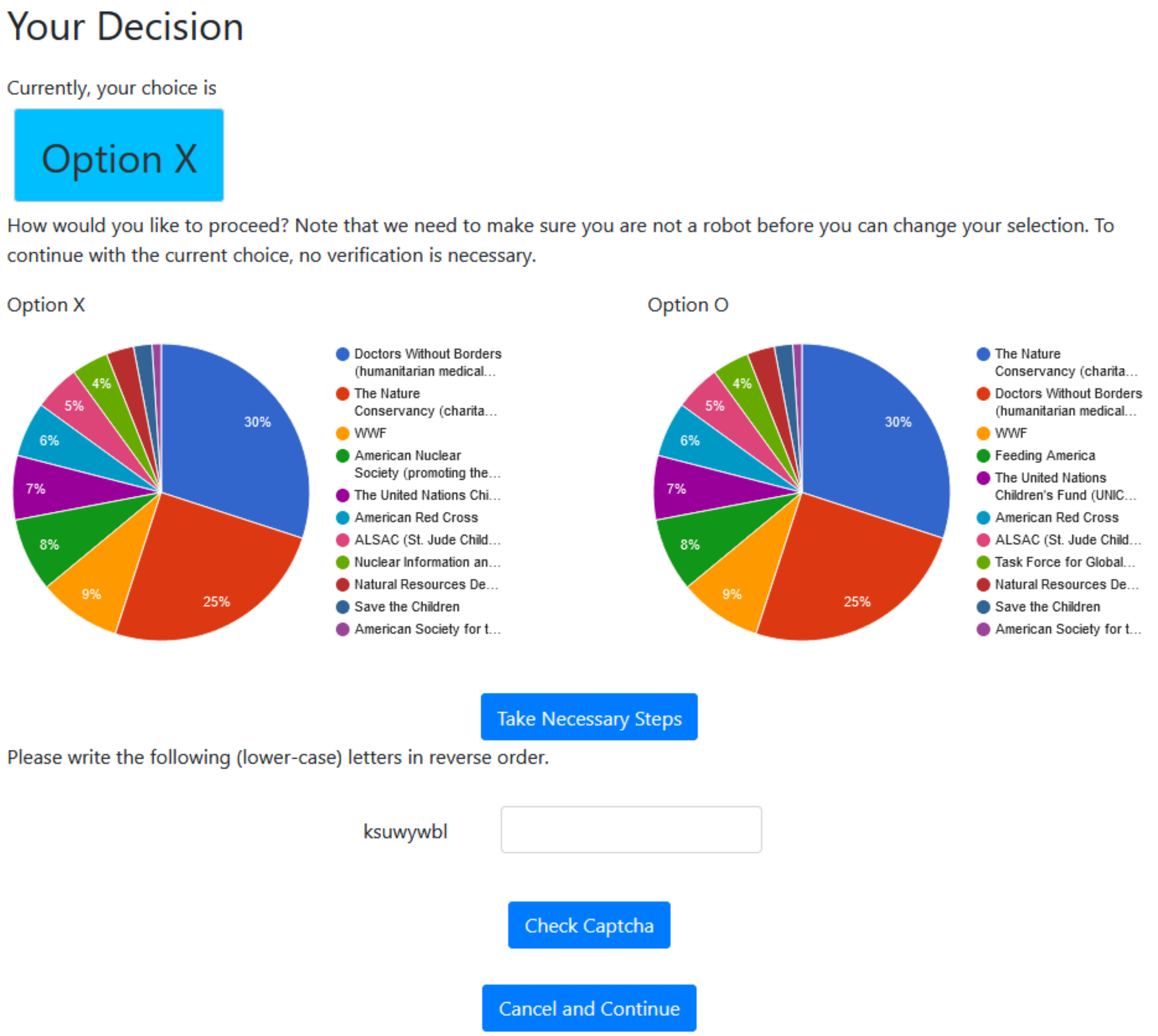


Figure 13: Decision with Default (Full Info/Hidden Info Revealed)