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# Motivated Reasoning, Information Avoidance, and Default Bias\*

Katharina Momsen<sup>†</sup>  Sebastian O. Schneider<sup>‡</sup>

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## Abstract

We investigate whether the presence of a default interacts with the willingness of decision-makers to gather, process and consider information. In an online experiment, where about 2,300 participants choose between two compiled charity donation options worth \$100, we vary the availability of information and the presence of a default. Information avoidance, when possible, increases default effects considerably, manifesting a hitherto undocumented channel of the default bias. Moreover, we show that defaults trigger motivated reasoning: In the presence of a default – even if self-selected –, participants consider new information to a lower degree than without a preselected option.

*JEL Classifications:* C90, D64, D83, D91

*Keywords:* Motivated reasoning, information avoidance, defaults, status quo, charitable giving, experiment

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\*The experiment was approved by the IRB of the University of Innsbruck (58/2020) and preregistered with the Open Science Foundation ([osf.io/bhpe9](https://osf.io/bhpe9)), including a pre-analysis plan, and financed by the researchers' own budget. The order of coauthors was randomized using the AEA randomization tool (ID: F\_8VhrTZI011).

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# 1 Introduction

Decision-makers have the tendency not to alter the default – a behavioral feature that has already been documented in the seminal paper by [Samuelson and Zeckhauser \(1988\)](#). This phenomenon can also be observed in everyday life, including the choice of electricity contracts (e.g., [Fowle et al., 2021](#)), retirement savings (e.g., [Carroll et al., 2009](#)), and charitable giving (e.g., [Schulz et al., 2018](#)). In most situations apart from stylized experiments, decision-makers actively need to gather information to be able to make an educated decision. Yet, it has never been investigated whether the presence of a default interacts with the decision-makers’ willingness to gather, process and consider information. As it is now well-documented that decision-makers sometimes engage in motivated reasoning, i.e., they process information in a way that it becomes aligned with their own world view ([Bénabou and Tirole, 2016](#)), or they strategically select information, or even prefer to avoid information about their own health ([Oster et al., 2013](#)), intelligence ([Eil and Rao, 2011](#); [Möbius et al., 2014](#)), or morality ([Dana et al., 2007](#)), for example, this gap is surprising.<sup>1</sup>

Investigating the interaction of defaults with information acquisition, processing and interpretation is important to put results from previous research into perspective (see, e.g., [Blumenstock et al., 2018](#), for a review). Even more importantly perhaps, choice architects would need to think about information provision and presentation, as defaults – desirable or not – may be stickier than previously thought in case of an interaction. In this vein, an interaction must also be considered by regulators to avoid any abusive use.

In this paper, we document and investigate information search and processing together with the degree to which information is finally considered in situations with and without preselected default options. 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 may challenge the default and stress the need to switch. In addition, we alter whether a default exists and, if so, how it is determined – randomly or self-selected based on vague

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<sup>1</sup>See also [Loewenstein \(2006\)](#) and [Golman et al. \(2017\)](#) for further examples.

information. Our experimental approach thus allows us causally to identify motives (not) to gather information and stick to the default option, and, in addition, causally to identify the consequences of the presence of a default on information search, processing, and consideration.

In the experiment, subjects choose between two options that determine how \$100 are split between different charities and organizations. To isolate the mechanism more cleanly, we consider charities to abstract from consequences that might intervene with the subjects' personal payoff. The composition of the options depends on the subjects' preferences elicited at the beginning of the experiment. 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 preferred charity, and the second-largest share to the charity ranked second. In the other option, the order is swapped, making this option look dominated at first sight. The main difference lies in the charities to which smaller shares are donated. Here the (at first sight) dominating option attributes donations to charities that were ranked last, while the other option does not include these charities. As more than 90% of our subjects explicitly opposed donations to the charities they ranked last, learning about these 'bad apples' included in one of the options should thus result in choosing the (at first sight) dominated option. After confronting the subjects with vague information on the options, either one of the options is preselected as default, or no default is selected. We then examine whether and how much information participants collect in the treatment where the composition of the options is initially concealed, and which option they finally choose.

We find that giving participants the option to avoid information increases the default effect by about 20% (or 14-15 percentage points) compared to presenting all information upfront. Controlling for the time spent on processing the information presented, we can explain this effect by the availability of relevant information. We can thus establish information avoidance, as defined by [Golman et al. \(2017\)](#), i.e., as avoidance of information that is known to exist, as an additional and hitherto undocumented channel of the well-known default bias.

Moreover, we find that defaults decrease the degree to which information is considered. In the presence of a default, learning that one option

contains donations to the least-preferred charity has a significantly lower effect on final choices than in the absence of a default. Importantly, the nature of the default does not matter for this result: Whether the default is self-selected or has been randomly allocated makes no difference. This nuance illustrates that the effect in our setting cannot be explained by interpreting the default as a recommendation (as discussed in, e.g., [McKenzie et al., 2006](#); [Blumenstock et al., 2018](#); [Jachimowicz et al., 2019](#)).

We contribute to various strands of literature. Most importantly, our paper is the first to connect two separate strands of literature – the one on motivated reasoning ([Epley and Gilovich, 2016](#); [Bénabou and Tirole, 2016](#); [Gino et al., 2016](#)) through information avoidance ([Golman et al., 2017](#)) and the one on reasons behind default effects ([Dinner et al., 2011](#); [Blumenstock et al., 2018](#)). Using an incentivized experiment, we have identified information avoidance as an additional channel through which defaults affect our decisions. The current evidence on default effects 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 motivated reasoning in general or information avoidance in particular.<sup>2</sup> As most experiments investigate defaults while presenting decision-makers with all information necessary to make an educated choice, our results thus suggest that default effects from experimental decision contexts are severely underestimated, as they abstract from the channel of information avoidance.

Second, our finding that defaults decrease the degree to which information is considered and thus affect final choices speaks to the literature on motivated reasoning ([Bénabou and Tirole, 2016](#)), more precisely to the literature on how decision-makers strategically process information when it is not avoided ([Babcock et al., 1995](#); [Gneezy et al., 2020](#); [Schwardmann et al., 2022](#); [Saccardo and Serra-Garcia, 2020](#)). We thus connect the more general literature on motivated reasoning beyond information avoidance to the one on default effects through yet a different angle, by documenting that motivated reasoning is applied not only with the ultimate goal to protect the decision-makers' egos, but also in contexts that are not relevant to egos, so

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<sup>2</sup>See also [Eidelman and Crandall \(2012\)](#), and [Jachimowicz et al. \(2019\)](#) for additional reviews.

as to comply with the default. In that aspect, our results also complement the literature emerging from psychological research on confirmation bias (e.g., [Charness and Dave, 2017](#)).

Our results are informative for companies and individuals alike. 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 default existed. Companies 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.

Our findings are also policy-relevant in the sense that information provision with respect to contracts that are automatically renewed might need regulation. 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,<sup>3</sup> as this might prevent an abuse of the default effect due to information avoidance, and decrease the scope of information interpretation.

## 2 Experimental Design and Procedures

The main task in our study asked subjects to decide between two options of which each split \$100 among different charities and organizations. These options were tailored to the subjects' preferences. In a between-subjects manner, we varied whether subjects immediately observed all necessary pieces of information (FULLINFORMATION) or if these pieces of information needed to be revealed (HIDDENINFORMATION). In addition, we altered whether one option was preselected as default and, if so, how the default was determined (DEFAULTTREATMENTS).

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<sup>3</sup>See, e.g., [https://en.wikipedia.org/wiki/Annual\\_percentage\\_rate](https://en.wikipedia.org/wiki/Annual_percentage_rate).

## Preference Elicitation

At the beginning of the experiment, we elicited the subjects' preferences about nine charities and organizations<sup>4</sup> by asking them to split hypothetically – without further research – \$100 between these charities and, possibly, a tenth charity of their own choice. The listed 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 the Online Appendix). 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

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<sup>4</sup>In the following, the words 'charities' and 'organizations' are used interchangeably.



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.<sup>5</sup>

### **Construction of Choice Options With and Without ‘Bad Apples’**

Based on the elicited preferences, 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. Additionally including uncontroversial charities in the two options allowed us to adjust the options to our needs.<sup>6</sup>

The uncontroversial organizations were: 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 preferred charity 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 preferred charity.

The crucial differences between the options lay in the smaller donations. The seemingly superior option contained significant donations to two charities to which the subjects explicitly did not allocate any money (‘bad apples’): In total, \$12 were allocated to these organizations, with \$8 to the least-preferred one, to which more than 90% strictly opposed any donation in their name, and 4% of the remaining subjects at least

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<sup>5</sup>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 B in the Online Appendix for details on the attention checks, exclusion rules and behavior of excluded subjects.

<sup>6</sup>We obtain a preference ranking for at least five organizations from the charities listed above; hence, eight uncontroversial organizations are added to the option that excludes the least preferred organizations, and six for the ‘bad apples option’.

declared it their least-preferred option, while donating a positive amount to its counterpart. For the seemingly inferior option, the respective shares were donated to uncontroversial charities.

As the additional donation of \$5 to the preferred charity comes with a donation of \$12 to the ‘bad apples’, 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 option that attributes the largest share to their second-most-preferred charity. We instructed participants that the two options might differ from their input, in order not to create any false expectations (see Figure 2 in the Online Appendix for instructions/screenshots). Additionally, attention checks were implemented to filter out subjects who did not pay attention to the instructions (see Section B in the Online Appendix for details).

### **Provision of Vague Information and Determination 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 appeared to dominate the other, as the largest share was donated to the subject’s most-preferred organization.<sup>7</sup> After presenting these pieces of information, our treatment variation came into play.

In the NODEFAULT condition, subjects were shown the vague 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.<sup>8</sup>

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<sup>7</sup>See Figures 5-7 in the Online Appendix for screenshots.

<sup>8</sup>To be able to assess whether it makes a difference which option was selected as default, we assigned the inferior option with a 66% probability, assuming that this would be by far the most-preferred option in the SELF-SELECTEDDEFAULT condition.

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. If, as in the SELF-SELECTEDDEFAULT condition, subjects feel in a way responsible for the default, they may be more engaged in the decision situation or unwilling to admit that they made an inferior decision. While the former effect may result in an increase in the willingness to gather information and potentially switch, the latter induces subjects to remain ignorant and stick to the default.

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

### **Information Stage: Variation of Information Provision**

In the treatments with all information immediately presented – the FULL-INFORMATION condition –, subjects could directly observe all charities both options consisted of on the page following the one where vague information was provided and defaults, if applicable, were determined. Subjects had all information necessary to make an informed decision. As they could immediately observe that the — at first sight — preferable option included charities they did not wish to support, they could choose their truly preferred option.

In the treatments with information provided on demand – the HIDDEN-INFORMATION condition –, only the charities to which the largest share would be donated were immediately visible on the page following the presentation of vague information and the determination of the default option, if applicable.<sup>9</sup> For each option, subjects had the option to click a button labelled ‘Show Details’ to reveal the names and shares of the other charities contained in the option. When clicked, the button revealed the identity of the charity to which the next-smaller share would be donated, and its re-

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<sup>9</sup>See Figures 8 and 9 in the Online Appendix for screenshots of the decision screens.

spective share in the option. Hence, 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 the two buttons. They could proceed to the next page and make their choice without revealing any new information, after having revealed some, but not all, information, and after revealing the composition of both options.

Note that all subjects – those in the `HIDDENINFORMATION` and those in the `FULLINFORMATION` conditions – could retrieve further information, e.g., the main goals, on the charities and organizations involved in the two options by clicking on their respective slices in the pies (once revealed, if applicable).<sup>10</sup> Moreover, we randomized the side of the screen on which options were shown.

## 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

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<sup>10</sup>The information consisted of the first three sentences (or the first paragraph in case it was shorter than three sentences) on the organizations' Wikipedia pages.

already announced before the information stage (see Figure 10 in the Online Appendix for an example).<sup>11</sup>

## 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., forthcoming). We also elicited time (following, e.g., Falk et al., 2018) and risk preferences (following Schneider et al., 2021) 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. 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.

## 3 Results

In the main text, we present results on subjects in the NODEFAULT condition, and on those who self-selected or were randomly assigned the *inferior option* as their initial (default) choice. More than 90% of those who could self-select their default opted for this option, while in the RANDOM condition it was assigned with a probability of about 2/3. We deem this the natural case, and it enables us to analyze whether the nature of the default – self-selected or randomly assigned – affects results. As we consider the case where the default choice is the superior option of interest as well, we report these – almost always analogous – results in Online Appendix A.

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<sup>11</sup>Switching costs were implemented to investigate if – depending on the default – motivated reasoning could potentially be more powerful than inertia triggered by switching costs.

Being interested in the interplay of default bias and information acquisition and processing, we first investigate whether our default treatments successfully induce a default bias.

To investigate this, we focus on the plain case when all information is provided upfront (FULLINFORMATION). We start with an investigation of the pooled default effect (i.e., RANDOMDEFAULT and SELF-SELECTED-DEFAULT conditions together). We compare the share of subjects choosing the inferior option in presence of a preselected default option to the corresponding share in absence of a preselected default option.

While 49.6% of the subjects choose the inferior option in the absence of a preselected option, 76.0% do so when there is a default – a difference that is significant in a two-sample test of proportions<sup>12</sup> (p-value < 0.001, N = 959).<sup>13</sup> Hence, we can formulate our first result:

**Result 1.** *Defaults matter in our implementation: They induce a default bias, which in our setting reduces choice quality.*

Hence, we find evidence of a strong default effect even when all information is provided upfront. Our default effect of a 26 percentage points increase in the share of subjects choosing the inferior option is within the range of results in [Samuelson and Zeckhauser \(1988\)](#), who report increases in the share of choices of up to 53 percentage points (with a mean of 10 percentage points) due to an option being presented as the default.

In what follows, we study whether and to what extent this default effect is intensified when, in the very same choice setting, information is hidden, but revealable (HIDDENINFORMATION), instead of completely provided upfront (FULLINFORMATION). While the usual driving factors – inertia, anchoring, reference dependence (see, e.g., [Blumenstock et al., 2018](#) or [Eidelman and Crandall, 2012](#)) – will also matter under HIDDENINFORMATION, additional channels come into play: (selective) information search and avoidance, as well as selective information-processing.

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<sup>12</sup>Unless otherwise indicated, all p-values correspond to two-sided hypotheses.

<sup>13</sup>The high share of participants choosing the inferior option might be interpreted as participants not caring about the choice. In that case, however, we would expect 100% choosing the inferior option in presence of a default. Yet, this share is considerably lower. The more appropriate interpretation thus is that choice quality is on average rather low, which is realistic, constant across treatments, and in line with results in, e.g., [Samuelson and Zeckhauser \(1988\)](#).

### 3.1 Information Avoidance as a Driver of Default Effects

Having established that the presence of a preselected (inferior) option induces a default bias, i.e., increases the share of subjects choosing the inferior option, we now investigate how the possibility to avoid information affects this default effect. For now, we thus focus on participants in the DEFAULT conditions to isolate the effect as cleanly as possible.

To investigate the effect of withholding information until actively demanded by the subject in the HIDDENINFORMATION condition (as opposed to always showing all information in the FULLINFORMATION condition), in a first step we pool the two DEFAULT treatments (self-selected and randomly assigned default). In Figure 1a, we compare the stickiness of defaults across conditions varying the availability of information: 76% of subjects kept the preselected (inferior) option even when all information was provided upfront (left-hand side of Figure 1a; note that this explains Result 1). In the HIDDENINFORMATION condition, the share of participants keeping the preselected option even increases to 90%, as illustrated on the right-hand side of Figure 1a.

This difference in the share of subjects who stuck to the preselected option is significant in a two-proportions test (p-value  $< 0.001$ ,  $N = 1158$ ).

**Result 2.** *Giving participants the opportunity to stay ignorant about additional characteristics of the choice options increases the default effect.*

Comparing the nature of the default treatments both under FULLINFORMATION and under revealable, but initially HIDDENINFORMATION yields no statistically significant difference (75.9% (RANDOM) vs. 75.8% (SELF-SELECTED) under FULLINFORMATION ( $N = 576$ ) and 90.7% vs. 89.3% under HIDDENINFORMATION ( $N = 582$ )) for randomly assigned and self-selected default options, respectively.

**Result 3.** *The nature of the default has no influence on the default effect, neither under hidden nor under full information.*

### 3.1.1 Mechanism: The Role of Information Acquisition, Availability and Processing

Can the aggravated default bias in the HIDDENINFORMATION treatment be explained by a reluctance to reveal, and consequently by a lack of critical information? To this end, we now investigate whether the effect can be explained by revelation of a critical amount of information, i.e., whether those who revealed enough information to learn about one or even both ‘bad apples’ in the inferior option switch to the option without the bad apples.<sup>14</sup>

**Information Acquisition and Availability** We first observe that, when information is withheld, but revealable at no cost in the HIDDENINFORMATION condition, the default effect highly depends on information acquisition: Of those who failed to reveal enough information to learn that the preselected default option implies at least one donation that is highly misaligned with their preferences, 98% stick to the default, regardless of whether it is self-selected or randomly assigned.

Turning to those who revealed at least some critical information, 65% stick to the default, with the difference to those failing to reveal this information being significant ( $p < 0.001$ , two-sample test of proportions,  $N = 582$ ). This result also holds for the two default treatments – self-selected and randomly determined – in isolation ( $p < 0.001$  in two-sample tests of proportions in both cases). Just as before, the nature of the default does not make a statistically significant difference here.

Comparing this share of participants, just discussed, who stick to the default after having revealed at least some critical information (65%) to the results above, where all information was provided upfront (76%), we note that those who revealed at least some critical information actively *switch more often* compared to those who have that information provided upfront: The difference is significant in a two-proportions test ( $p < 0.009$ ,  $N = 719$ ). When focusing on those who revealed both ‘bad apples’, this

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<sup>14</sup>When clicking the ‘reveal button’ three times, the first ‘bad apple’ is revealed: Participants learn – given that they process the presented information – that \$8 of the inferior option are donated to an organization that they do not support. After clicking the button four more times (i.e., seven times) the second ‘bad apple’ is revealed, corresponding to a \$4 donation to an unsupported organization.



effect increases only slightly (76% vs. 64%,  $p < 0.007$ ,  $N = 711$ ), as almost all participants (95%) who have revealed the first ‘bad apple’ continue also to reveal the second ‘bad apple’. This suggests that those who actively reveal information process the available information to a higher degree. Here, the nature of the default matters to a certain degree: Analyzing this difference for the two default treatments in isolation only yields a significant difference for those with a self-chosen preselected option in a two-sample test of proportions ( $p\text{-value} < 0.023$ ,  $N = 420$ ;  $p\text{-value}$  for the corresponding test with randomly determined default option: 0.174,  $N = 299$ ). This suggests that information-processing is particularly high among those who actively reveal information after having preselected an option themselves, which suggests that information avoidance as a means of protecting one’s ego plays no role here.

**Information Processing** As we have just seen, having all relevant information available does not necessarily mean processing it, as those who processed the information should actually switch to the option without ‘bad apples’. Indeed, if we proxy information-processing with the time spent on the decision page, we find a significant difference of about 25 more seconds on that page among those who have actively acquired all the relevant information compared to those who had them provided upfront (74.9 vs. 50.3 seconds,  $p\text{-value} < 0.001$ , t-test,  $N = 1408$ ).

Among those who had revealed all relevant information in the HIDDEN-INFORMATION treatment, those with a self-selected default spent about 4 seconds more on the decision page compared to those with a randomly determined default. As the group who had selected their default option themselves also spent about 3 seconds more on the decision page in the FULLINFORMATION condition, compared to those with a randomly determined default, the increase in the time spent on the decision page due to actively revealing information is statistically the same among those two groups (two-sided t-test,  $p\text{-value} = 0.227$  for the logarithm of the time spent; for time spent, it is 0.57).

**Explaining the HIDDENINFORMATION Treatment Effect** Putting these insights together, we can now explain the amplified default effect

caused by the HIDDENINFORMATION treatment: We use a (logistic) regression where we control for information-processing as measured by the (log) time spent on the information and decision page. In Column 1 of Table 1, the increase due to the HIDDENINFORMATION treatment is still significant with an increase of about 15 percentage points, as expressed by the marginal effect in Column 2 (which is very close to the plain difference of about 14 percentage points without any control variables in Figure 1a). We then add an indicator variable for having at least some relevant information available about the unwanted charities in the preselected option in Column 3. This eliminates the increase in the default effect induced by the HIDDENINFORMATION treatment (see Column 4).

We summarize our results on the mechanisms of the HIDDENINFORMATION treatment:

**Result 4.** *Information Avoidance – that is, avoidance of information that participants are aware of and that they can access (Golman et al., 2017) – increases the default effect. This holds independently of the default being randomly determined or self-selected.*

### 3.2 Defaults Affect the Degree to Which Information Is Considered

Keeping the presence of a default fixed, and varying only the availability of information, we have established that information avoidance is a *causal mechanism* of the default bias. We now turn to the reverse direction, keeping the availability of critical information fixed: Does the presence of a default option affect the way available information is processed or the weight it is assigned?

To investigate this, we compare the effect of having (at least some) critical information displayed on choosing the superior option in a logit regression among those with default, and those without a preselected option. Converting the coefficients from the logit regression into marginal effects yields a measure known in the medical literature as “absolute risk reduction”: It expresses how much the risk of choosing the inferior option is reduced by the measure under investigation – in this case, having critical information available, i.e.,  $(SI_{\text{No Info}} - SI_{\text{Info}})$ , where  $SI_{\text{No Info}}$  is the share of

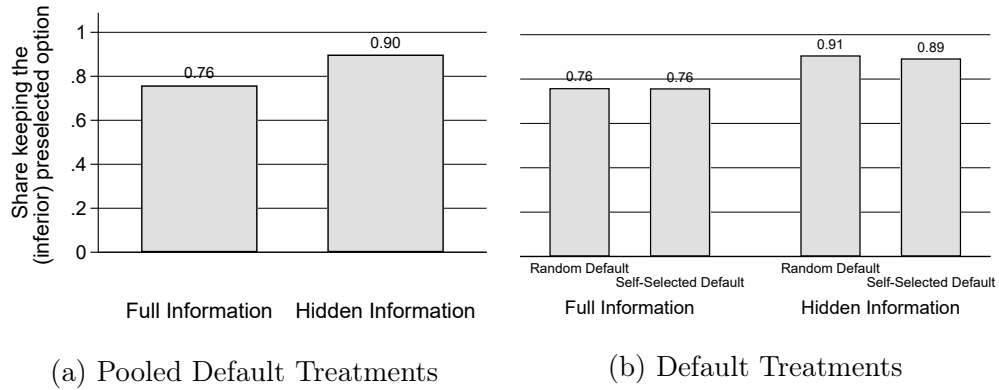


Figure 1: Stickiness of Default Options and Information Availability

Table 1: Hidden Info Treatment: Explaining the Treatment Effect

	Keeping Default Option		Keeping Default Option Controlling for Info	
	Logit	Marginal Effect	Logit	Marginal Effect
Hidden Info Treatment	1.237*** (0.183)	0.147*** (0.021)	0.081 (0.229)	0.009 (0.026)
Time Invested (log)	-1.376*** (0.164)		-1.174*** (0.171)	
Critical Information Available			-2.600*** (0.410)	
N	1129	1129	1129	1129

\*Notes: We also control for risk aversion, impatience, altruism, gender, age and age squared, education, whether or not the study was completed, the hours that have passed since 8am (local time), a time trend, whether or not the session was conducted on a weekend, and the time zone.

subjects choosing the inferior option in absence of critical information, and  $SI_{\text{Info}}$  is the respective share in presence of critical information.

When comparing this risk reduction for participants with and without defaults, it is good practice to consider additionally the different “baseline risks”: Dividing the absolute risk reduction by the baseline risk  $SI_{\text{No Info}}$  yields the “relative risk reduction”. We report these results in Table 2: The first three columns concern all participants from the FULLINFORMATION treatment, plus all those from the HIDDENINFORMATION treatment who revealed at least some critical information. The fourth, fifth and sixth column, in turn, only report results of those in the HIDDENINFORMATION treatment, i.e., focusing on those who have actively revealed information.

We obtain the same result, independently of whether or not we focus on those in the HIDDENINFORMATION treatment, and independently of looking at any two possible ways of inducing a default in our study in isolation (SELF-SELECTED vs. RANDOMLY determined) or pooling them: The coefficients (reported in Columns 1 and 4) differ significantly between those with and those without a preselected default option (see lower panel of Table 2). While those in the NODEFAULT condition are on average 33%-37% more likely to choose the superior option if at least some critical information is displayed (compared to those without at least some critical information, see Columns 2 and 3), this probability is only about 2/3 as big in presence of a DEFAULT option (see Columns 5 and 6). Equivalently, the risk of choosing the inferior option is reduced by 33%-37% by the availability of at least some critical information in absence of a default, whereas this reduction amounts to only 2/3 of this effect in presence of a default; see Columns 2 and 4. In relative terms, the risk of choosing the inferior option is reduced to an even larger extent by the availability of critical information, see Columns 3 and 6. The conclusion stays the same:

**Result 5.** *Defaults decrease the degree to which information is considered, thus trigger motivated reasoning.*

It is noteworthy that the coefficients and shares used for the computation of the risk reductions do not differ between those with a RANDOMLY determined default, and those who SELF-SELECTED their default option.

Table 2: The Value of Information for Decision Making With and Without Default

	Full Info + Hidden Info			Only Hidden Info		
	GLM: Coef.	ARR	RRR	GLM: Coef.	ARR	RRR
Critical Information Available						
(1) -Without Default (=1)	1.672*** (0.202)	0.330*** (0.033)	0.429*** (0.031)	1.856*** (0.296)	0.366*** (0.061)	0.489*** (0.060)
(2) -With Default (=1)	0.438** (0.193)	0.224*** (0.017)	0.247*** (0.018)	0.855*** (0.268)	0.283*** (0.042)	0.336*** (0.041)
Time Invested (log)	0.617*** (0.085)			0.349** (0.169)		
Difference between (1) and (2)	1.234*** (0.000)			1.000*** (0.000)		
Observations	1903	1903	1926	954	954	967

*\*Notes:* This table shows in Columns 1 and 4 the coefficients of a logit regression of choosing the superior option on indicator variables for the presence of a default and whether or not at least some critical information is displayed (main effects reported). In Columns 2, 3, 5 and 6, we report the absolute and relative reduction of the risk of choosing the inferior option that is associated with the availability of information – with and without default. The absolute reduction of this risk 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”. Besides the (log) time spent on the decision and information page (in seconds), we also control for risk aversion, impatience, altruism, gender, age and age squared, education, whether or not the study was completed, the hours that have passed since 8am (local time), a time trend, whether or not the session was conducted on a weekend, and the time zone in the logit regression. Robust standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

This is a clear indicator that this result is not just a consequence of interpreting the default as a recommendation.

We also observe the pattern from Result 5 in the case where the default is the superior option (see Table 3 in the Online Appendix for the analogue to Table 2). In that case, this pattern is caused by individuals *actively switching away* from the preselected option despite having all information available. We see this as a strong indicator that the result is not driven by inertia.

## 4 Conclusion

The influence of defaults on final decisions has been documented in a variety of contexts, ranging from organ donations (Johnson and Goldstein, 2003) and retirement savings (Madrian and Shea, 2001) to the choice of electricity contracts (Fowlie et al., 2021). Several channels have been considered to explain the stickiness of defaults (see, e.g., Blumenstock et al., 2018; Dinner et al., 2011): First, decision-makers may be reluctant to switch because of (perceived) switching costs – their unwillingness to exert effort manifests itself in inertia. Second, defaults may be perceived as implicit recommendations – a phenomenon often referred to as anchoring or endorsement. Finally, decision-makers may interpret the default as a reference point such that switching triggers loss aversion. Acknowledging the importance of these channels, we have identified and presented two new and hitherto undocumented reasons why defaults may be even stickier than previously thought:

Firstly, their effect is stronger when some decision-relevant information is not immediately available, as this triggers information avoidance (Golman et al., 2017). The initial lack of at least some decision-relevant information applies to almost all decision situations outside stylized experiments, suggesting that default effects in everyday life are larger than those measured in experiments.

Secondly, even if decision-makers have enough information, they interpret disadvantageous information about an option differently if it is the default: They exhibit motivated reasoning to reach a conclusion in favor of

the default (Babcock et al., 1995; Gneezy et al., 2020; Schwardmann et al., 2022; Saccardo and Serra-Garcia, 2020).

We are thus the first to combine the hitherto disconnected literature on default effects with the literature on motivated reasoning and information avoidance.

Clearly, the finding that information avoidance increases default effects is closely related to inattention. Since paying attention to the decision problem, including information search, is (cognitively) costly, decision-makers *rationaly* remain inattentive (Sims, 2003; Caplin and Dean, 2015; Gabaix, 2014, 2019) and thus uninformed and inactive. In addition, our results suggest that attention is diverted in a *motivated* way: Defaults induce decision-makers to pay less attention to information that questions the default, even if self-selected. Our findings therefore complement the results in Exley and Kessler (2021), who explore motivated reasoning and rational inattention as driving factors for ignorance.

Overall, our findings imply that choice architects, regulators and management need to take opportunities for motivated reasoning and information avoidance into account when deciding on how to design a decision situation. Defaults need to be set wisely, or communicated with care, as they may be stickier than previously thought. Not only do decision-makers avoid available information that questions their default; they also interpret information differently in the presence of a default. Hence, defaults need to be set carefully – to benefit most from intended default effects and to minimize welfare or productivity losses due to too little switching (or too much switching, if the default contradicts decision-makers’ preferences at first glance, although it would be beneficial, as analyzed in Appendix A). As decision-relevant information is processed less thoroughly when a default is present, it is important that information is presented in a manner that is easy to digest for decision-makers or that information search costs 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 corporate structures. Here, management might be interested in delegating the information search to independent units, and have it triggered automatically.

Defaults are often considered a potent, but rather innocent, nudge in the toolbox of choice architects (Thaler and Sunstein, 2008). Their use is justified not only by their effectiveness and ease in implementation, but also by the fact that they qualify as a measure in line with the concept of *Libertarian Paternalism* (Thaler and Sunstein, 2003). Our results, however, challenge this view by showing that defaults induce decision-makers to consider the available information differently – they lead to a bias in interpreting and applying information. Together with the feature that default effects are even stronger when parts of the decision-relevant information are not immediately available, our results suggest 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.



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## A Online Appendix: Results with Superior Default

In the main text, we have focused on analyzing the case in which the preselected option was the seemingly superior – but actually inferior – default option. A separate analysis for the case when the preselected option is the superior option is needed because the default effect works in the other direction, due to the symmetrical design, but will be reduced rather than aggravated, also due to the symmetrical design, when information is not considered. Relatedly, base levels are different, which would make a comparison of coefficients in logistic regressions misleading.

The study’s interest to investigate the interplay of information avoidance and default effects does not necessarily favor one case over the other. Yet, participants naturally self-selected the seemingly superior option in the vast majority of cases, which we mirrored by a probability of 2/3 of assigning the superior default in the random-default condition. This allows for a meaningful analysis of the nature of the default only in the case considered in the main text. Nevertheless, the symmetrical case with the superior option being the default yields some interesting, complementary insights, which we present here.

We first consider the analogous case to Result 1: When the superior option, i.e., the option that does not contain bad apples, is selected as default, we find that choice quality *increases* significantly compared to the treatment without default. Under full information and in absence of a preselected option, 49.6% of the participants choose the inferior option, while only 18% do so in the presence of a superior default. This difference is large in magnitude and statistically significant in a two-proportions test ( $p < 0.001$ ). Hence, our first result – that defaults have a strong impact on choices – carries over to the situations where the superior option is preselected. Yet, the direction of the effect changes in the expected direction. When the inferior option is preselected, choice quality decreases, while it increases when the option without bad apples is set as default.

To investigate whether the default effect is affected by information provision here, too, we compare the share of subjects who keep the preselected option under full information to the respective share under hidden infor-

mation. In a one-sided test of proportions, we find that subjects are less likely ( $p = 0.027$ ) to keep the (superior) default under hidden (73%) rather than under full information (82%). This finding is driven by uninformed subjects in the HIDDENINFORMATION treatment who do not discover the bad apples in the option to which they willingly switch. These subjects are unwilling to reveal information; yet they are willing to actively give up the preselected choice. Hence, *inertia cannot be the driving factor of the result*. With the superior option as default, giving subjects the opportunity to remain ignorant now *decreases* the default effect and in this case thus also simultaneously *decreases* choice quality – findings that are statistically significant for randomly selected defaults in isolation ( $p = 0.033$ ), but not for self-selected defaults ( $p = 0.420$ ). By design, these effects are symmetrical to our second result where, when the inferior option is preselected, default effects are *stronger*. Yet, either way, choices are worse when information is initially hidden.

Symmetrical behavior to the case where the default is the inferior option can also be observed with respect to information revelation and to keep the default choice: When the default is the superior option, those who reveal are more likely to keep the default than those who do not reveal critical information on the composition of the options: 78% ( $N = 32$ ) of those who reveal vs. 72% ( $N = 155$ ) of those who do not reveal critical information keep the preselected option, although the difference is not significant in this case ( $p = 0.495$ , two-sample test of proportions). Note that, for this result the lack of a statistically significant difference may result from the low number of subjects who were presented the superior choice option in combination with the scant room for increases at this already high base level caused by the default effect.

In contrast to the results in the main text, we do not find a difference beyond information provision between the two information treatments: Those who are immediately informed and those who willingly become informed do not behave differently in this case: 82% ( $N = 185$ ) vs. 78% ( $N = 32$ ), respectively, keep the default option.

We can thus directly state the equivalent to Result 4 in the main text – concluding that information avoidance increases the default effect in case of a seemingly superior default: With a seemingly inferior default, information

avoidance decreases the default effect, and, just as before, it leads to worse decisions. This is thus again perfectly symmetrical to the case with a superior default.

For the sake of completeness, we nevertheless report the analogous finding on information-processing: Those who reveal information spend more time processing information than those who are directly provided with the information (79.7 seconds vs. 50.1 seconds,  $p < 0.001$ , two-sided t-test). Yet, as we have seen above, this increased information-processing does not seem to make a statistically significant difference in the final choice above and beyond the availability of information.

Asking whether defaults influence how we consider information, we compare whether having critical information available without default matters to the same degree that it does with default for making the final decision – this time focusing on the case with superior default. As reported in Table 3, the absolute reduction of the risk of choosing the inferior option due to information availability is lower with superior default than without. Here, it is particularly important to account for the “baseline risk” when comparing the reduction in the risk due to information availability with and without default. After all, in the absence of critical information, due to the documented default effect above, the “baseline risk” to choose the inferior option is much higher without a default than it is with default. However, as can be seen when comparing the relative risk reduction (RRR) in Columns 3 and 6 of Table 3, even when accounting for the “baseline risk”, information is considered more without default than it is with default. This result is thus in line with the finding when the inferior option is set as default, where the default induced participants to consider revealed information less than without default. We note, however, that in the HIDDENINFO treatment, the relative risk reduction due to information availability is lowest in the presence of a superior default, and largest without a default, with the relative risk reduced in the presence of an inferior default in between. The reason for this observation is that individuals switch away from the superior default, and they do so even after having revealed critical information, which reduces the value of information, making the reduction even insignificant. This behavior documents motivated reasoning: Individuals discount

all information that makes the option they perceive as superior appear less beneficent.

Table 3: The Value of Information for Decision-Making With and Without (Superior) Default

	Full Info + Hidden Info			Only Hidden Info		
	GLM: Coef.	ARR	RRR	GLM: Coef.	ARR	RRR
Critical Information Available						
(1) –Without Default (=1)	1.747*** (0.192)	0.356*** (0.032)	0.429*** (0.031)	2.207*** (0.303)	0.455*** (0.059)	0.489*** (0.060)
(2) –With Default (=1)	3.131*** (0.244)	0.075* (0.046)	0.319** (0.130)	3.057*** (0.489)	0.066 (0.085)	0.211 (0.284)
Time Invested (log)	0.134 (0.083)			–0.141 (0.142)		
Difference between (1) and (2)	–1.384*** (0.000)			–0.850* (0.085)		
Observations	1133	1133	1140	569	569	572

\*Notes: This table shows in Columns 1 and 4 the coefficients of a logit regression of choosing the superior option on indicator variables for the presence of a default and whether or not at least some critical information is displayed (main effects reported). In Columns 2, 3, 5 and 6, we report the absolute and relative reduction of the risk of choosing the inferior option that is associated with the availability of information – with and without default. The absolute reduction of this risk is obtained by computing the (average) marginal effect of 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”. Besides the (log) time spent on the decision and information page (in seconds), we also control for risk aversion, impatience, altruism, gender, age and age squared, education, whether or not the study was completed, the hours that have passed since 8am (local time), a time trend, whether or not the session was conducted on a weekend, and the time zone in the logit regression. Robust standard errors in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## B Online Appendix: Attention Checks

The first attention (or bot) check that we implemented allowed subjects to proceed only if the allocation of money to organizations 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 the 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 "honey-pots" 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 that these bots spend, on average, the expected amount of time on, e.g., the instructions page.

## 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 2: 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)	<input type="text" value="0"/>
B: Democrats (political party in the US)	<input type="text" value="0"/>
C: Nuclear Information and Resource Service (anti-nuclear group)	<input type="text" value="0"/>
D: Republicans (political party in the US)	<input type="text" value="0"/>
E: American Nuclear Society (promoting the field of nuclear engineering)	<input type="text" value="0"/>
F: The Non-GMO Project (label for food free of genetically modified organisms)	<input type="text" value="0"/>
G: Biology Fortified, Inc. (promoting genetically modified organisms)	<input type="text" value="0"/>
H: The Nature Conservancy (charitable environmental organization)	<input type="text" value="0"/>
I: The United Nations Children's Fund (UNICEF, social welfare organization)	<input type="text" value="0"/>
J: Other (please provide name and link to donation form)	<input type="text" value="0"/>
Name:	<input type="text"/>
Link:	<input type="text"/>

Sum: \$0.00

Next

Figure 3: 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

The United Nations Children's Fund (UNICEF, social welfare organization)

The Nature Conservancy (charitable environmental organization)

Biology Fortified, Inc. (promoting genetically modified organisms)

American Nuclear Society (promoting the field of nuclear engineering)

Democrats (political party in the US)

### Against a donation?

Yes  No

Yes  No

Yes  No

Yes  No

Yes  No

Next

Figure 4: 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 5: 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 6: 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.

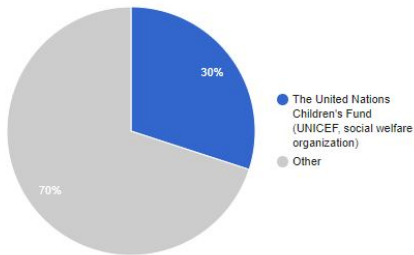
Next

Figure 7: Incomplete Information on Options with Self-Selected Default

## Additional Information on the Options

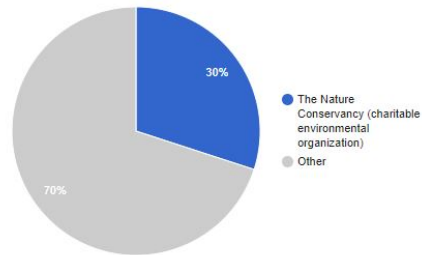
Hover over the legend or a slice of pie to retrieve further information about the respective donation share. Click on a colored slice to retrieve further information about the organization or charity (Source: Wikipedia/Sourcwatch). If the dialog is covered, you can move it by clicking on its title, and dragging it to a free space. If you feel you need more information on the other organizations and charities in a respective option, you can click on the "Show details" button below the option.

Option X



Show Details (X)

Option O



Show Details (O)

Note: Clicking the "Show Details" buttons is **optional**. You can, of course, also proceed without clicking any of the two buttons.

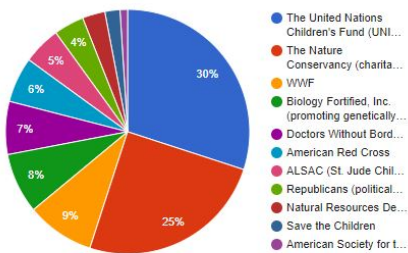
Next

Figure 8: No Default, Hidden Info

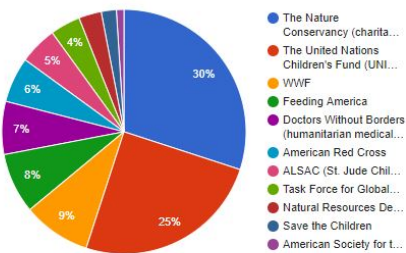
## Additional Information on the Options

Hover over the legend or a slice of pie to retrieve further information about the respective donation share. Click on a colored slice to retrieve further information about the organization or charity (Source: Wikipedia/Sourcwatch). If the dialog is covered, you can move it by clicking on its title, and dragging it to a free space. If you feel you need more information on the other organizations and charities in a respective option, you can click on the "Show details" button below the option.

Option X



Option O



Note: Clicking the "Show Details" buttons is **optional**. You can, of course, also proceed without clicking any of the two buttons.

Next

Figure 9: No Default, Hidden Info Revealed

# Before we proceed...

For your information: To impede possible abuse, we have implemented (technical) protection measures before a selection can be implemented.

We require that participants prove that they are no robots by solving a captcha, similar to this:

Please write the following (lower-case) letters in reverse order.

abcdefg

[Check Captcha](#)

[Next](#)

Figure 10: Letter Task

# Your Decision

Which of the two options would you prefer? Note that we need to make sure you are not a robot before you can make a selection.

Option X

Option O

[Take Necessary Steps](#)

Please write the following (lower-case) letters in reverse order.

bdjymjvl

[Check Captcha](#)

Figure 11: Decision without Default



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2022-05

Katharina Momsen, Sebastian O. Schneider

Motivated Reasoning, Information Avoidance, and Default Bias

**Abstract**

We investigate whether the presence of a default interacts with the willingness of decision-makers to gather, process and consider information. In an online experiment, where about 2,300 participants choose between two compiled charity donation options worth \$100, we vary the availability of information and the presence of a default. Information avoidance, when possible, increases default effects considerably, manifesting a hitherto undocumented channel of the default bias. Moreover, we show that defaults trigger motivated reasoning: In the presence of a default – even if self-selected –, participants consider new information to a lower degree than without a preselected option.

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