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Green by Default: Implications of a default on subsequent behavior

Amendment to Project C: 'Moral Licensing and Defaults' of 'Welfare Effects of Green Default Electricity Contracts'

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Zusammenfassung

Der vorliegende Bericht ist eine Ergänzung des Kapitels ‘Assessing Negative Spillover Effects’ innerhalb des Projektes ‘Green by Default - Welfare Effects of Green Default Electricity Contracts’. Die Ergänzung des Kapitels beschäftigt sich mit der Frage, ob Defaults mit hohen opt-out Kosten Auswirkungen auf Verhaltensweisen haben können, welche nicht in direktem Zusammenhang mit der ursprünglichen Entscheidung stehen. Von besonderem Interesse in der Umweltökonomie sind vor allem negative ‘spillovers’, d.h. ‘nachgelagerte’ Verhaltensweisen, welche anfänglich positive Umwelteffekte in insgesamt negative Effekte umkehren. Unsere Analyse zeigt, dass Defaults, welche nur mit erheblichem Aufwand verändert werden können, keine negativen Auswirkungen auf nachgelagerte Entscheidungen haben. Diese Resultate sind positive Nachrichten für Entscheidungstragende, welche Bedenken hinsichtlich den Auswirkungen auf nachgelagertes Verhalten durch den Einsatz von Defaults haben.

Summary

This report is an amendment of the chapter ‘Assessing Negative Spillover Effects’ within the project ‘Green by Default - Welfare Effects of Green Default Electricity Contracts’. This amendment aims at scrutinizing potential behavioral effects of choice defaults with high costs to opt out, which reach beyond their direct impact on targeted decisions. Of particular interest for policy making in the environmental domain are negative behavioral spillovers, i.e., subsequent behaviors that could diminish positive effects of an initial be-havior. Our analysis shows that defaults that may be changed only with considerable effort, do not interfere with subsequent individual choices. These findings carry a posi-tive message for policy makers fearing adverse consequences from the use of choice defaults.

Résumé

Le présent rapport est un supplément au chapitre intitulé "Assessing Negative Spillover Effects" dans le cadre du projet “Green by Default - Welfare Effects of Green Default Electricity Contracts". Dans ce rapport nous examinons comment des contrats par défaut caractérisés par des coûts de désinscription élevés, qui rendent un changement de contrat difficile, influencent des comportements qui ne sont pas en lien direct avec la décision originale. Les effets de débordement négatifs potentiels (c'est-à-dire les effets négatifs sur les comportements collatéraux) sont particulièrement intéressants pour l'économie de l'environnement, car ils peuvent transformer des effets initialement positifs pour l'environnement en effets globalement négatifs. Notre analyse montre que même les défauts qui ne peuvent être modifiés qu'avec un effort considérable n'interfèrent pas avec des décisions individuelles collatérales. Ces résultats constituent donc un message positif pour les décideurs qui craignent des conséquences négatives de l'utilisation des défauts de choix sur les comportements subséquents.

The initial report is available here:
`https://www.aramis.admin.ch/Texte/?ProjectID=37744&Sprache=en-US`

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Acknowledgments

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Note that this addendum adds new data to 'Project C: Assessing Negative Spillover Effects.' It is therefore *not a completely new written project*, but we complement the initial analysis and results with the additional insights we gained.

Note additionally that this report will not feature an introduction and policy implications for the complete project. These substantive parts have not changed in their meaning and can therefore be found in the initial publication of the project.

1 Project C: Addendum with new data Nudge for Good? Choice Defaults and Spillover Effects[1]

Contextualization and Summary

In addition to the initial Project C, we collect new data in an economic laboratory study to assess whether a well-intended, but costly to opt out of default may lead to negative spillover effects on a related behavior in a subsequent decision, thus undermining the overall effectiveness of a nudge. This type of research is in line with the research program's aim to direct attention to rebound effects of policy interventions. This chapter restates the initial findings amended with the new data that we have collected. In the stylized setting of the laboratory, and while stringently testing for income effects and individual characteristics of participating subjects, we do not find evidence that negative spillovers are triggered by any kind of choice defaults. Project C thus provides an 'all-clear' for the use of choice defaults, even when it is costly for individuals to change from a default to another option, as it seems that such defaults do not impose adverse effects on subsequent behavior.

[1]This project has been published in an earlier version as a working paper (see Ghesla, Grieder, and Schmitz, 2017). The project constitutes of a joint effort of Manuel Grieder, Jan Schmitz and Claus Ghesla. Note that in the table of contents and chapter headers we use 'Assessing Negative Spillover Effects' as a short-title for this project.

1.1 Introduction

Choice defaults appear to be very effective nudges for promoting 'good' causes. For instance, defaults strongly impact individual donation behavior (Altmann, Falk, Heidhues, and Jayaraman, 2014), promote the uptake of green energy contracts (Ebeling and Lotz, 2015), and help increase retirement savings (Choi, Laibson, Madrian, and Metrick, 2003; Cronqvist and Thaler, 2004). Thus, even though there is a lively debate on the ethicality of using defaults as nudges (Bovens, 2009; Desai, 2011; Hausman and Welch, 2010; Sunstein, 2015), their distributional effects (Brown, Farrell, and Weisbenner, 2011; Löfgren, Martinsson, Hennlock, and Sterner, 2012), and whether their use fits the criteria of 'libertarian paternalism' (Carroll, Choi, Laibson, Madrian, and Metrick, 2009; Ghesla, 2017; Keller, Harlam, and Loewenstein, 2011), the effectiveness of default nudges for promoting 'good' causes has generally been taken for granted.

However, for an accurate assessment of the overall effects of default nudges on desired behavior, policy makers should take into account not only the direct impact of default nudges on targeted choices, but also their potential spillover effects[2] on subsequent, related behaviors (see also d'Adda, Capraro, and Tavoni, 2017). In principle, such behavioral spillovers could amplify, eliminate or even reverse the initially positive effects of choice defaults, when judging their impact on the aggregate of relevant behaviors (for overviews see Dolan and Galizzi, 2015; Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014). For instance, if nudging someone into a charitable donation crowds out other pro-social acts in the future, the net effect of the choice default for promoting pro-social behavior is clearly less positive than when no such spillover occurs. In contrast, if the nudge triggers further pro-social behavior in subsequent situations, the effects are even more positive.

What kind of behavioral spillovers may we expect? The existing empirical literature on sequential decision making and how initial behavior affects subsequent behavior points to three distinct possibilities: (i) individuals may behave consistently and in accordance with their prior decisions (e.g., Baca-Motes, Brown, Gneezy, Keenan, and Nelson, 2013; Bea-

[2]Note that in this paper we narrow down the term spillover effects to the effect of an initial behavior on a related *subsequent* behavior. In the literature, the term spillover effect is also used to describe the backfiring of policy instruments because of psychological reactance to a given policy leading to adverse effects on the targeted *initial* behavior (Schultz, Nolan, Cialdini, Goldstein, and Griskevicius, 2007), or to explain so-called rebound effects due to individual adjustments to relative price changes, which are induced by a given policy (Alcott, 2005).

man, Cole, Preston, Klentz, and Steblay, 1983; Brandon, Ferraro, List, Metcalfe, Price, and Rundhammer, 2011; Burger, 1999; Cherry, Crocker, and Shogren, 2003; Cialdini, Trost, and Newsom, 1995; Freedman and Fraser, 1966; Fitzsimons and Shiv, 2001; Grimm and Mengel, 2012; Knez and Camerer, 2000) leading to positive spillovers; (ii) they may license themselves into adverse behavior with respect to their prior decisions (e.g., Conway and Peetz, 2012; Harding and Rapson, 2013; Jacobsen, Kotchen, and Vandenbergh, 2010; Meritt, Effron, and Monin, 2010; Monin and Miller, 2001; Tiefenbeck, Staake, Roth, and Sachs, 2013) leading to negative spillovers; or (iii) they may view related decisions as independent and make sequential decisions in isolation (e.g., similar to when people narrowly bracket choices, see, e.g., Rabin and Weizsäcker, 2009; Read, Loewenstein, and Rabin, 1999), leading to the absence of spillovers. Given the empirical ambiguity of the direction of spillover effects, competing research questions and hypotheses about the spillover effects of pro-social default nudges apply: (i) does pro-social nudging lead to consecutively consistent behavior, thus amplifying the positive initial effect (positive spillover)? Or (ii) do pro-social defaults have adverse effects on subsequent behavior, thus potentially eliminating the initial positive effect (negative spillover)? Or (iii) is there no effect of pro-social default nudges on subsequent, not directly targeted decisions (no spillover)?

The main aim of this paper is thus to investigate experimentally whether and how choice defaults targeted at fostering pro-social behavior affect untreated *subsequent* decisions. For this purpose we ran a laboratory experiment in which we implemented a 'sequential behavior paradigm' design (Mullen and Monin, 2016). In a first stage, subjects played a modified dictator game with a charitable organization as the recipient (Dictator Stage I). In the second stage, subjects played a dictator game with a randomly paired other participant from the same session as the recipient (Dictator Stage II). We implemented two default conditions in Stage I. In both default treatments, subjects were defaulted into being fully pro-social and donating the maximum possible amount to the charity. They needed to opt out if they wanted to do otherwise. Our default treatments differ in the cost (represented by individual effort) subjects had to bear to change the pre-set default donation. In WEAK DEFAULT switching away from the pre-set default required almost no effort and subjects simply had to change a pre-set donation amount. In STRONG DEFAULT changing the pre-set donation amount was more difficult and required participants to exert effort. In this treatment, subjects first needed to complete a real effort task in form of a slider task (see Gill and Prowse, 2018) and could subsequently change the donation.

We varied the strength of the default in our experimental treatments to account for various real-life decision environments that include default nudges. In WEAK DEFAULT, we provided a decision architecture in which it is not costly to switch between options, such as many defaults used in charitable giving (Altmann et al., 2014) or pro-environmental pre-sets (Brown, Johnstone, Hascic, Vong, and Barascud, 2013; Egebark and Ekstroem, 2016). In STRONG DEFAULT, we accommodated the fact that defaults are also widely used in more complex choice environments. Complexity of a choice may come in different facets, such as the time required to reach a decision (Sitzia, Zheng, and Zizzo, 2015), the number of options to choose from (Iyengar, Jiang, and Huberman, 2003) or the experience with a task (Löfgren et al., 2012). Often, defaults in these contexts seem to work (i.e., people stay with the default) because it is laborious for people to make an active choice and to opt out of the default. Therefore, an important question to investigate is whether stronger defaults that make it more costly for decision-makers to opt out, trigger different behavioral spillover effects than weaker defaults where opting out is easy.

Or experimental design benchmarks the two default treatments against a condition in which subjects actively choose their level of pro-social behavior in stage one (NO DEFAULT). Thus, subjects decided how much to give to charity without being confronted with a pre-set donation amount. The subsequent decision in stage two remained the same in all treatments. Subjects played a dictator game with a randomly paired participant from the same session figuring as the recipient. This second decision represents our main dependent variable, as we want to detect whether pro-social behavior induced by a default nudge from the first decision spills over into subsequent decision-making. We further implemented an additional two-tiered control strategy in which a different set of subjects only made decisions in the dictator game of Stage II (Mullen and Monin, 2016). These conditions allow controling for income and altruistic behavior from the charity stage and thus to separate behavioral spillover effects from income effects and altruistic motivations from the initial charity stage.

We find encouraging results. Whereas both default manipulations promoted significantly higher donation levels to charities in the initial decision of Stage I, they did not lead to negative (nor positive) spillovers in the subsequent behavior of Stage II. Hence, in our study, pro-social defaults did not lead to subsequent adverse behavior. These findings hold for both cases when defaults were weak and pre-set donations were relatively costless to change and also when it was costly to switch the default. Moreover, our results do not change when controlling for income effects and altruistic motivations.

These findings carry some positive messages for policy makers and choice architects who aim at organizing contexts in which people make decisions with differently framed choice defaults. Thus, our paper contributes to and aims at connecting the growing literature on the effect of libertarian paternalistic interventions (e.g., Altmann et al., 2014; Ebeling and Lotz, 2015; Thaler and Sunstein, 2003) and behavioral spillovers (e.g., Dolan and Galizzi, 2015; Meritt et al., 2010; Truelove et al., 2014).

To the best of our knowledge, there are only two studies that cover the consequences of nudges on subsequent behavior. First, the study by d'Adda et al. (2017) is most related to our approach as they used a similar design as ours in order to test relevant spillovers of various policy interventions, including a number of typical 'nudges' such as choice defaults and information about social norms, on subsequent behavior. They find that traditional policy interventions in the form of monetary incentives or contractual regulation lead to positive spillovers (mainly because of anchoring effects), whereas nudging interventions had no spillover effects. However, with regard to choice defaults their results remained inconclusive, as their default manipulation did not produce a significant effect on the initial behavior. In our setting, the default effects are statistically significant. This allows us to test the behavioral spillover effects of successful default nudges on subsequent related decisions.

Second, with regards to the effects of defaults on subsequent behavior, de Haan and Linde (de Haan and Linde) present an analysis investigating whether well-intended defaults for an initial behavior reinforce the default effect in a second, related behavior. The authors find that, indeed, being pro-socially defaulted once, people were more likely to follow a pro-social default again if defaults are beneficial to the individual. Our research question differs from de Haan and Linde (de Haan and Linde) with respect to the formalization of the subsequent behavior. While de Haan and Linde (de Haan and Linde) are interested whether well-intended defaults lead to similar default effects over time, we analyze whether a default spills over into an untreated, related behavior.

The remainder of this paper is organized as follows. Section 1.2 presents the experimental design. Section 1.3 presents the study results. Section 1.4 discusses relevant findings and concludes.

1.2 Experimental design, Method and Procedures

To study whether a default in a first *initial* decision affects behavior in an untreated *subsequent* decision we based our experimental design on a 'sequential behavior paradigm', which is typically used to study behavioral spillover effects experimentally (Mullen and Monin, 2016). For both decisions, we implemented dictator games (Forsythe, Horowitz, Savin, and Sefton, 1994; Kahneman, Knetsch, and Thaler, 1986) in order to have two very similar pro-social deeds as an instrument to uncover potential spillover effects of a default in one decision on related subsequent decisions without a default. Specifically, in the first decision subjects played a dictator game paired with a charity as the recipient ('Dictator Stage I'). In the subsequent second decision, subjects played another dictator game in which they were paired with a randomly allotted person in the same laboratory session ('Dictator Stage II'). In both stages, subjects could be either selfish (and keep the money for themselves) or pro-social (and share some of it with the recipient). Importantly, if there are spillover effects, the decision in Dictator Stage II may depend on the behavior in Dictator Stage I and on the presence and strength of a choice default in that stage.

Dictator Stage I Subjects played a dictator game paired with a recipient in form of a charitable organization. They could choose from nine different charities, which served a well-balanced set of purposes, such as charities that deal with environmental and nature conservation, human rights, or health related matters. Thus, we tried to preclude situations in which subjects would have liked to donate, but could not find a suitable charity to do so (Crumpler and Grossman, 2008). Participants received information on each charity by reading a statement of purpose.[3]

[3]These statements were taken from the website of Zewo Foundation, a Swiss institution that certifies charitable organizations with respect to integrity, efficient use of funds, and transparency, see www.zewo.ch/en/

Subjects received information about each charity, which they had to read before they were able to make a choice.[4] Once they had read about all charities, subjects decided to which of the nine charities (only one could be selected) and how much to give. Participants received a total amount of 200 experimental points (ECU) for their choice, of which they kept 100 points as a show-up fee. 100 ECU remained to decide on how much to donate to a charity. Subjects also had the option to donate nothing and keep all experimental points for themselves.

We implemented three treatment variations in Dictator Stage I:

T1 **NO DEFAULT**: Subjects could choose actively if and how much to donate to a charity. Subjects had to actively type the desired amount into an input box. The input box was initially blank.

T2 **WEAK DEFAULT**: We nudged subjects into being fully pro-social and donating the maximum possible amount to a charity by default. The default donation was thus pre-set to the maximum amount subjects could donate (100 ECU). Subjects could change the pre-set amount by actively clicking on a box and entering the desired donation.

T3 **STRONG DEFAULT**: We again nudged subjects into being fully pro-social by setting the default donation to the maximum possible amount that could be donated. In order to change the amount, subjects had to perform a slider task (Gill and Prowse, 2018). Specifically, to change the default donation, subjects had to shift 48 sliders to a value of 50. Only once this task was completed, they could change the donation amount. If they did not complete the slider task, they had to donate the default amount.

We completed the experimental design with a two-tiered control strategy:

C1 **CONTROL INCOME**: Subjects did not participate actively in Dictator Stage I, but received lump-sum payments in addition to their show-up fees. The amounts of these lump-sum payments were derived from the distributions of donation amounts

[4] Appendix 1.C displays the instructions provided to participants and screenshots of the decision screens.

subjects chose in the treatment conditions outlined above. Thus, each donation decision in the NO DEFAULT, WEAK DEFAULT, and STRONG DEFAULT treatments was matched with a lump-sum payment a participant received in the CONTROL INCOME condition. In purely monetary terms, subjects in CONTROL INCOME thus arrived at Dictator Stage II in exactly the same situation as a matched subject from one of the treatments, however without having made a donation decision in Dictator Stage I. Eliminating Dictator Stage I behavior while controlling for any possible income effects provides us with a conservative baseline to which we can compare the Dictator Stage II decisions in our three main treatments.

C2 **CONTROL PASSIVE GIVING**: Subjects received the identical lump-sum payments according to the same procedure as subjects in CONTROL INCOME. Yet, they did participate (to a limited extent) in Dictator Stage I by choosing the charity to which a pre-defined donation was made. By letting subjects choose the charity to which the donation was administered, we made sure that the altruistic utility component, i.e., the individual knowledge that there had been a donation in Dictator Stage I was comparable to subjects' utility in the NO DEFAULT, WEAK DEFAULT, and STRONG DEFAULT treatments.[5] Additionally, as subjects read about the charities in Dictator Stage I in the treatment condition, this condition also controls for any possible priming effects of that task on the subsequent decision in Dictator Stage II.

Dictator Stage II Subjects played a standard dictator game with another participant as the recipient. Each subject was thus paired randomly with another subject in the same session. Both subjects remained completely anonymous with respect to each other and were not able to influence the other participant's decision. To maximize the number of observations, we used a variant of the strategy method (Selten, 1967) and elicited choices for both roles of the dictator and the recipient respectively. Each subject thus decided on the allocation of 200 experimental points between herself and the paired recipient. However, it was common knowledge that only one decision of each pair of subjects was going to be implemented, and that the computer would randomly determine which one. Dicta-

[5] What this condition does not control for is the warm-glow (Andreoni, 1990) stemming from the donation decision in Dictator Stage I. This is intentional, as it is exactly this warm-glow (i.e., the feeling of having done something good) which may affect subjects' decisions in Dictator Stage II (Schmitz, 2018).

tor Stage II was completely identical for subjects in all treatments and control conditions and the decisions made in this stage constitute our main dependent variable. Table 1.1 summarizes the experimental parameters.

Table 1.1: Overview of Experimental Parameters

	Dictator Stage I		Dictator Stage II
	Show-up fee	ECU for decision	ECU for decision
T1 NO DEFAULT	100	100	200
T2 WEAK DEFAULT	100	100	200
T3 STRONG DEFAULT	100	100	200
C1 CONTROL INCOME	100+$\hat{X}$	–	200
C2 CONTROL PASSIVE GIVING	100+$\hat{X}$	fixed: (100-$\hat{X}$)	200

Note.— Subjects in CONTROL INCOME and CONTROL PASSIVE GIVING received a lump-sum payment $\hat{X}$ matching the distribution of the donated amounts in Dictator Stage I in the treatment conditions (see Appendix 1.B for details on the matching procedure). In Dictator Stage II, each subject decided on the allocation of 200 ECU, however, only one decision within each subject pair was implemented. 100 ECU $\equiv$ CHF 10.

Procedures We conducted 23 sessions with a total of 678 participants at the Decision Science Laboratory (DeSciL) at ETH Zurich. We collected data for the NO DEFAULT and WEAK DEFAULT conditions in July and September 2016. The data for the STRONG DEFAULT condition were collected in May and June 2018. It is possible that unobserved changes in the subject pool between 2016 and 2018 could have affected subjects' behavior. However, when we compare the 2016 and the 2018 data of the corresponding control conditions (CONTROL INCOME and CONTROL PASSIVE GIVING), we do not find any significant differences in behavior ($p > .100$ for all comparisons), which is why we pool the data from 2016 and 2018 for the analyses.

In order to obtain the amounts and the distribution of the lump-sum payments ($\hat{X}$) in the control groups, we ran four sessions of NO DEFAULT and WEAK DEFAULT first (in the 2016 wave). Subsequently, we varied treatments and control between sessions[6] and sessions were executed such that treatments and controls were evenly distributed across different times and days. We followed the same procedure for the STRONG DEFAULT

[6] One treatment session was conducted in a within fashion due to unbalanced show up of participants. Results of this single session are not significantly different with respect to the remaining sessions (Kolmogorov-Smirnov test p=0.435 (distribution of giving in Dictator Stage I), p=0.139 (distribution of giving in Dictator Stage II)).

treatment and the corresponding CONTROL INCOME and CONTROL PASSIVE GIVING conditions in the data collection wave in 2018. Thus, we first conducted four sessions in the STRONG DEFAULT treatment to gather information about giving in Dictator Stage I and the income distribution for Dictator Stage II. We computerized the experiment using z-tree (Fischbacher, 2007) and recruited subjects using hroot (Bock, Baetge, and Nicklisch, 2014). The subject pool consisted of students at the University of Zurich and the Swiss Federal Institute of Technology (ETH) in Zurich. An experimental session lasted roughly 50 minutes. All instructions can be found in Appendix 1.C.

At the beginning of a session, subjects were randomly assigned to computer-equipped cubicles. Common rules for participation were read aloud and subjects signed a consent form. They received on-screen instructions for each part of the study (see Appendix 1.C). Subjects knew that the study would consist of several parts, but the contents of each part were not revealed before the respective instructions were provided. In order to ensure comprehension, subjects had to answer control questions before each part. When subjects had comprehension questions, the experimenter answered individually and in private.

Subjects first completed Dictator Stage I (except in CONTROL INCOME). Subsequently, we included a filler task between Dictator Stage I and II. In this task, subjects completed a shortened version of an IQ-test after Cattell (1940). The test was divided into two parts, each part lasting for exactly 90 seconds. The intention of the filler task was to temporally separate Dictator Stage I and II. This separation may be of importance when reviewing the proposed underlying psychological mechanisms of consistency or licensing effects. One line of research argues that individuals store moral credits when behaving 'good', which they then use later on, for instance, to offset a subsequent behavior (Jordan, Mullen, and Murnighan, 2011). Another line of research states that individuals use initial 'good' behavior as a credential to interpret negative subsequent behavior as non-negative (Monin and Miller, 2001). The filler task serves both mechanisms as, on the one hand, it provided sufficient time for subjects to build up moral credits, and on the other hand, it was still short enough so that in the subsequent behavior subjects would remember their initial behavior. Additionally, it limits the potential for demand (Zizzo, 2010) and anchoring effects (see, e.g., d'Adda et al., 2017) and adds to the external validity of the results, as in relevant real-life settings an initial behavior is most likely not followed immediately by a subsequent behavior. After the filler task, subjects proceeded to Dictator Stage II. Upon completion of these tasks, they received feedback on their final payoff and were asked to fill in a supplemental questionnaire. The average payment was approximately CHF 26. Moreover, subjects donated CHF 2,155 to the nine different charities.

1.3 Results

We begin by presenting results for the weak default on giving in Dictator Stage I and the potential spillover effects on giving in Dictator Stage II compared with the no default treatment. We then turn to present results for our strong default treatment in relation to the weak default and no default treatment. Finally, we contrast the findings in the default treatments with behavior in the different control conditions disentangling possible income effects and altruistic motives from spillover effects arising from giving in Dictator Stage I. Table 1.2 provides a descriptive analysis of choices in Dictator Stage I and II across treatment and controls.

Table 1.2: Summary Statistics

		Giving (ECU)	
Treatments	N	Dictator Stage I	Dictator Stage II
NO DEFAULT	129	27.44 (25.38)	35.89 (36.80)
WEAK DEFAULT	129	34.26 (31.47)	39.69 (39.80)
STRONG DEFAULT	128	58.98 (43.82)	40.94 (43.15)
Control Conditions	N		Dictator Stage II
CONTROL INCOME (NO DEFAULT matching)	49	–	39.39 (44.32)
CONTROL INCOME (WEAK DEFAULT matching)	49	–	40.20 (40.59)
CONTROL INCOME (STRONG DEFAULT matching)	50	–	50.80 (42.71)
CONTROL PASSIVE GIVING (NO DEFAULT matching)	46	–	34.57 (39.87)
CONTROL PASSIVE GIVING (WEAK DEFAULT matching)	46	–	43.70 (39.80)
CONTROL PASSIVE GIVING (STRONG DEFAULT matching)	52	–	43.65 (40.44)

Note.— Giving is denoted in ECU. Standard deviations are in parentheses. The data for the six control conditions are split into the respective income matching category, i.e., NO DEFAULT, WEAK DEFAULT, STRONG DEFAULT.

Immediate effect of the weak default Our default manipulation in Dictator Stage I had a significant effect on donation levels. Subjects in WEAK DEFAULT donated on average 25% more than subjects in NO DEFAULT (34.26 experimental points (WEAK DEFAULT) versus 27.44 experimental points (NO DEFAULT)). Thus, as expected, the pro-socially set weak default increased overall donation levels ($t(256) = -1.92$, $p = .028$).[7] Furthermore, subjects in WEAK DEFAULT also had a significantly higher prevalence of choosing exactly the pro-socially set default amount (= 100 ECU) (proportion test, $p = .034$).

The default effect can be further partitioned when considering giving as a two-stage decision process. Subjects first decide whether they want to donate or not. Once decided to donate, they decide on the size of their gift.[8] Our default manipulation did not affect the number of subjects who decided to give nothing in each of the treatments (proportion test, $p = 0.500$). However, it did affect donation levels once subjects decided to give. Comparing only subjects who decided to give a positive amount, donations in WEAK DEFAULT are on average 25% higher than in NO DEFAULT ($t(192) = -2.45$, $p = .008$).

Spillover effect of the weak default In order to assess the direction of potential spillover effects of a weak default we compare giving in Dictator Stage II between the WEAK DEFAULT and NO DEFAULT treatments. Table 1.2 reveals that subjects in both treatments give about one fifth of their endowment to the paired recipient. In the NO DEFAULT treatment, subjects gave 35.89 experimental points or 18% of their endowment. In the WEAK DEFAULT treatment, average giving amounted to 39.69 experimental points or 20% of a subjects' endowment. The difference of less than 4 experimental points is statistically not significant ($t(256) = 0.80$, $p = .427$). We summarize this finding as our first result:

> **RESULT 1:** We find no support for neither positive nor negative spillover effects of a weak pro-social default, that makes it easy to opt out, on non-targeted subsequent behavior.

Immediate effect of the strong default Subjects in STRONG DEFAULT donated on average 72% more than subjects in WEAK DEFAULT (58.98 ECU (STRONG DEFAULT) versus 34.26 ECU (WEAK DEFAULT)) and respectively on average 114% more than in

[7] We report one-sided tests for the default effect as we had a clear and directed ex-ante hypothesis for this effect. For all other effects we had competing hypotheses and thus report two-sided tests.

[8] For instance, Moffatt (2016) deems such an analysis particularly important for Dictator Game data, as the first part (i.e., the decision to donate) characterizes the 'type' of subject, whereas the second part allows assessing choices depending on the type of a person.

NO DEFAULT (27.44 ECU (NO DEFAULT)). Therefore, our stronger default manipulation significantly increased donation levels when compared to these two conditions ($t(255) = -5.20, p < .001$; $t(255) = -7.10, p < .001$). Furthermore, subjects in STRONG DEFAULT were also more likely to donate exactly the default amount when compared WEAK DEFAULT and NO DEFAULT (both proportion tests, $p < .001$).
Our strong default manipulation did not affect the number of subjects who decided to give nothing when compared to the two other conditions (both proportion tests, $p = 0.602$). However, it did affect donation levels once subjects decided to give a positive amount. In STRONG DEFAULT subjects donated on average 67% more than subjects in WEAK DEFAULT ($t(194) = -6.86$, $p < .001$) and respectively on average 109% more than in NO DEFAULT ($t(194) = -9.58$, $p < .001$).

Spillover effect of the strong default Table 1.2 shows that subjects in STRONG DEFAULT gave about one fifth of their endowment to the other recipient. This is very similar to the amounts given by subjects in WEAK DEFAULT and NO DEFAULT. Differences in giving between treatments were not statistically significant ($t(255) = -0.24, p = .810$ WEAK DEFAULT versus STRONG DEFAULT; $t(255) = -1.01, p = 0.314$ NO DEFAULT versus STRONG DEFAULT). We summarize these findings as our second result:

> **RESULT 2:** We find no support for neither positive nor negative spillover effects of a strong pro-social default, that makes it costly to opt out, on non-targeted subsequent behavior.

Figure 1.1 illustrates these findings. Panel A illustrates the statistically significant impact of both defaults on charitable giving in Dictator Stage I (with the STRONG DEFAULT condition adding a significant increase to donation levels compared to the WEAK DEFAULT). Panel B shows that in the untreated Dictator Stage II no spillover of the initial decision can be observed, as we do not find significant differences between the conditions.

Control conditions To put our results to a more conservative test and to ensure the robustness of our findings, we employed a two-tiered control strategy. Comparing choices in NO DEFAULT with choices in the default treatments in Dictator Stage II may omit relevant income effects (as subjects arrived with different amounts of money in Dictator

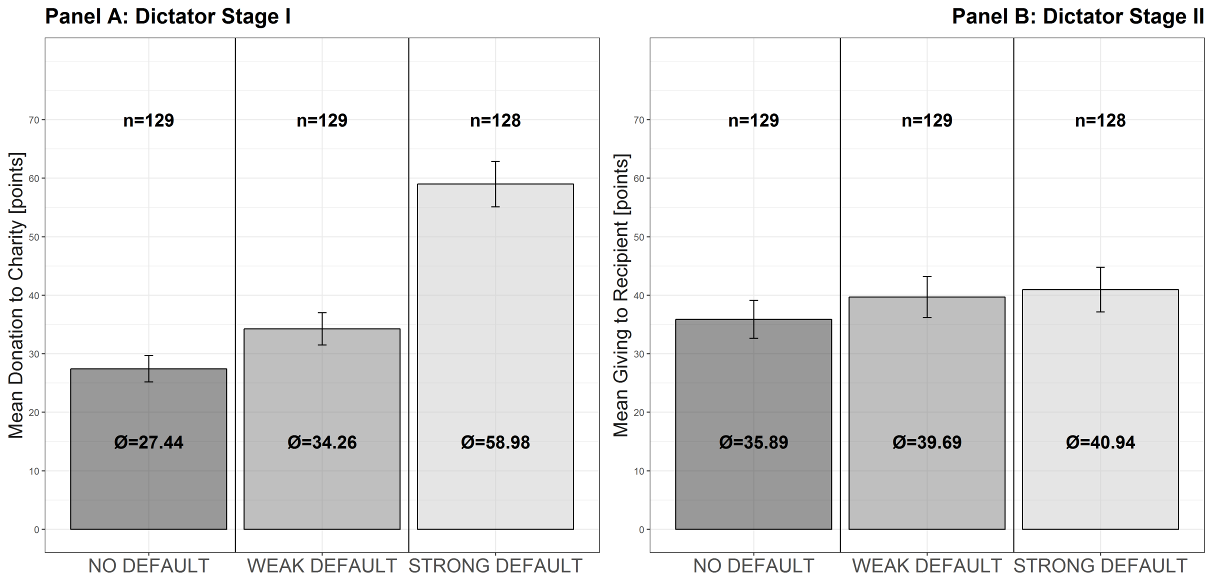

Figure 1.1: Choices in Dictator Stage I and II

Note.— Panel A shows donation decisions (mean points donated to charities) in Dictator Stage I for NO DEFAULT, WEAK DEFAULT and SRONG DEFAULT. Panel B shows mean giving (points given to recipient) in the Dictator Stage II for the three treatment conditions. Error-bars denote plus/minus one standard error of the mean.

Stage II in the default treatments compared with the no default treatment) and motivations of altruism in Dictator Stage I (as subjects donated significantly less in Dictator Stage I in NO DEFAULT compared to WEAK DEFAULT and STRONG DEFAULT). Therefore, we take the choices of subjects in CONTROL INCOME and CONTROL PASSIVE GIVING and compare them to NO DEFAULT and WEAK DEFAULT or STRONG DEFAULT respectively.

Comparing giving in Dictator Stage II across treatments and controls lends further support to results 1 and 2. Subjects' choices in NO DEFAULT and WEAK DEFAULT were not significantly different to those of the matched cases in CONTROL INCOME and CONTROL PASSIVE GIVING (NO DEFAULT vs. CONTROL INCOME: $t(176) = -0.49$, $p = .625$; NO DEFAULT vs. CONTROL PASSIVE GIVING: $t(173) = 0.21$, $p = .838$; WEAK DEFAULT vs. CONTROL INCOME: $t(176) = 0.08$, $p = .939$; WEAK DEFAULT vs. CONTROL PASSIVE GIVING: $t(173) = 0.57$, $p = .567$). Similarly, supporting Result 2, subjects' choices in STRONG DEFAULT were not significantly different to those in CONTROL INCOME or CONTROL PASSIVE GIVING (STRONG DEFAULT vs. CONTROL INCOME: $t(176) = 1.37$, $p = .171$; STRONG DEFAULT vs. CONTROL

PASSIVE GIVING: $t(178) = 0.39$, $p = .697$).[9]

Table 1.3: Regression Models: Giving in Dictator Stage II

DV: Giving to Recipient	OLS	LPM	gamma-GLM
		two-part model	
Intercept	40.805*** (3.958)	0.738*** (0.045)	4.008*** (0.065)
WEAK DEFAULT	2.379 (5.334)	-0.042 (0.062)	0.085 (0.090)
STRONG DEFAULT	-4.361 (5.493)	-0.199*** (0.063)	0.215** (0.106)
CONTROL INCOME	2.709 (5.133)	-0.069 (0.059)	0.168** (0.087)
CONTROL PASSIVE GIVING	-0.051 (5.163)	-0.065 (0.060)	0.096 (0.087)
Income before DG II	-13.599*** (5.102)	-0.218*** (0.059)	-0.052 (0.092)
WEAK DEFAULT x Income before DG II	-6.239 (6.555)	-0.036 (0.076)	-0.067 (0.120)
STRONG DEFAULT x Income before DG II	4.523 (5.901)	0.074 (0.068)	0.070 (0.111)
CONTROL INCOME x Income before DG II	13.735** (6.077)	0.240*** (0.070)	0.023 (0.108)
CONTROL PASSIVE GIVING x Income before DG II	13.342** (6.075)	0.196*** (0.070)	0.074 (0.108)
Observations	678	678	443
R^2	0.047	0.072	-

Note.— $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. Robust standard errors are in parentheses. The dependent variable is giving to the recipient in Dictator Stage II. NO DEFAULT is the omitted treatment captured by the intercepts. "Income before DG II" represents the (mean-centered) monetary income a subject had earned in the experiment when arriving at Dictator Stage II (partly endogenously determined in NO DEFAULT, WEAK DEFAULT and STRONG DEFAULT, exogenously assigned in control treatments). Gamma-GLM estimates are on a log-scale. The two-part model fits the data better than the OLS specification subsuming the complete data. The combined log-likelihood of the two-part model is -2628.207 compared to -3454.219 of the OLS.

Regression analyses The regressions reported in Table 1.3 allow analyzing whether spillover effects differed between the experimental treatments when controlling for potential income effects at the individual level. The variable "Income before DG II" captures the monetary income a subject had earned in the experiment before making the giving decision in Dictator Stage II.[10] The NO DEFAULT treatment is the omitted base category.

[9] For the t-tests reported above, we only considered the exact matches of income for each treatment condition in order to ensure perfect comparability. In the regressions reported in Table 1.3 we use the full data from the control conditions when controlling for possible income effects and can thus increase statistical power.

[10] Note that the main effect coefficients for "Income before DG II" in Table 1.3 do not capture a causal income effect. Because the regressions also contain the interaction terms of this variable with the dummies for the experimental conditions, the coefficients for "Income before DG II" apply to the NO DEFAULT treatment, in which the 'income' (i.e., the money a subject had earned in the experiment before entering Dictator Stage II) was determined by the subject's own donation decision in Dictator

The non-significant treatment dummies for WEAK DEFAULT and STRONG DEFAULT in the OLS regression thus indicate that, on average and compared to the NO DEFAULT treatment, neither a weak nor a strong default in the initial donation decision in Dictator Stage I led to a different spillover effect on the giving decision in Dictator Stage II. There were also no significant differences according to the OLS regressions when comparing WEAK DEFAULT and STRONG DEFAULT to the two control conditions and WEAK DEFAULT and STRONG DEFAULT with each other ($p > .100$ for all post-estimation Wald tests for these comparisons).

As for Dictator Stage I, we can again analyze the data from Dictator Stage II in a two-part model. For this, we used a linear probability model (LPM) to model the binary decision to give any positive amount to the recipient, and subsequently a gamma-GLM to assess how much a subject gave (conditional on giving a positive amount). As the LPM results reported in the corresponding column of Table 1.3 indicate, compared to the NO DEFAULT treatment, the STRONG DEFAULT treatment significantly reduced the number of people who chose to give a positive amount to the recipient in Dictator Stage II. This negative effect is also significant when comparing the STRONG DEFAULT treatment to WEAK DEFAULT ($p = .009$), CONTROL INCOME ($p = .024$), and CONTROL PASSIVE GIVING ($p = .021$) using post-estimation Wald tests. However, those subjects in STRONG DEFAULT who did give something to the recipient, gave more than subjects in NO DEFAULT, thus leading to the non-significantly different giving on average that we found in the OLS regression. Comparing the gamma-GLM coefficient of the dummy for the STRONG DEFAULT treatment to those of the two control conditions and to WEAK DEFAULT, we find that, conditional on giving a positive amount, there were no significant differences in giving across these conditions ($p > .100$ for all post-estimation Wald tests). Thus, in sum, also the regression analyses confirm that, on average, neither the weak nor the strong default in our study caused (negative or positive) spillover effects on subse-

Stage I. Thus, the negative coefficients we find in the regressions are due to self-selection (as participants with a tendency to give little in Dictator Stage I also give little in Dictator Stage II). A causal income effect can be estimated in the CONTROL INCOME condition and corresponds to testing that the sum of the coefficients for "Income before DG II" and the interaction term "CONTROL INCOME x Income before DG II" is different from zero. We do not find evidence for a significant income effect on average giving in Dictator Stage II ($p = .967$ post-estimation Wald test). The corresponding test for the CONTROL PASSIVE GIVING condition reveals that there is also no significant income effect when adding altruistic utility ($p = .938$ post-estimation Wald test).

quent behavior on average. The results from the two-part model provide some additional interesting insights, as the STRONG DEFAULT decreased the number of people willing to give anything in Dictator Stage II. However, this negative effect of the strong default on the propensity to give was compensated by higher giving by those subjects who still decided to give something.

1.4 Discussion and conclusions

In this study, we investigated the potential spillover effects of pro-social defaults on not directly targeted, subsequent behavior. To do so, we contrasted subsequent pro-social behavior when there was no default, an easily changeable "weak" default, and a costly to switch "strong default" implemented to foster an initial pro-social behavior. We tested the potential spillover effects of such choice defaults by applying a two-tiered control strategy taking into account potentially countervailing effects of different income levels and altruistic motivations stemming from the initial behavior.

Our findings are concurrently surprising and encouraging. Although the non-obtrusive, weak default in our study had a significantly positive effect on pro-social giving, it did not cause any problematic effects over time, as behavior did not spillover negatively (nor positively) to the subsequent decision where no default was present anymore. The same was true, for the strong default that made it costly to opt out for participants. The strong default further increased the level of pro-social behavior in the initially targeted decision compared to the weak default, but it also did not lead to adverse effects in the subsequent, non-targeted decision where the default was absent. Both findings are in line with individuals narrowly bracketing their decisions, and it is noteworthy that there are no spillover effects even within relatively short-lived laboratory sessions.

It could be argued that our experimental design, specifically the filler task, may have facilitated narrow bracketing and thus the absence of spillovers. However, we believe this view is not warranted. First, the filler task lasted a maximum of 180 seconds during the conduct of the experiment. Hence, if it is the case that distractions, like filler tasks, are sufficient to eliminate potential spillover effects, it is unlikely that such spillovers are actually relevant in real-life decision making where distractions are presumably frequent and the time that passes between potentially linked decisions is likely to be longer. Second, our experimental design followed the sequential behavior paradigm, where it is common to use filler tasks to ensure sufficient differentiation between initial and subsequent behavior (for instance Gneezy, Imas, Brown, Leif, and Norton, 2012; Sachdeva, Illiev, and Medin, 2009). Thus, we are convinced that the absence of spillover effects in our study is not an artifact of our experimental design.

Therefore, we conclude that based on our data, the use of a choice default – with or without significant costs to opt out – does not seem to influence subsequent behavior. This is an encouraging finding for policy makers wanting to stimulate pro-social behavior via differently designed choice defaults, but fearing subsequent adverse effects.

Nevertheless, much remains to be understood. Subsequent behavior may be due and exposed to a large variety of contextual factors. This study is a first step into the analysis of pro-social defaults and their effects on such behavior. In this study we have started to enhance our understanding of the behavioral mechanisms at play when using different designs of well-intended defaults. Presently, we have no substantiated reasons to believe that pro-socially set defaults impose any undesirable effects on subsequent decision-making.

Appendix

1.A Sample characteristics

Table 1.A.1: Sample characteristics

	NO DEFAULT	WEAK DEFAULT	STRONG DEFAULT	CONTROL INCOME	CONTROL PASSIVE GIVING
N	129	129	128	148	144
Age	23.1	24.0	22.2	22.8	22.6
Income	84%	82%	81%	85%	88%
Education	92%	95%	98%	96%	95%
Extraversion	3.25	3.06	3.19	3.17	3.20
Agreeableness	4.12	4.09	4.12	4.16	3.96
Conscientiousness	3.56	3.59	3.64	3.65	3.69
Neuroticism	2.83	2.57	2.63	2.79	2.69
Intellect	3.73	3.88	3.84	3.86	3.71
Need for Cognition	3.58	3.66	3.62	3.54	3.43
Reactance	3.04	2.89	2.91	2.86	2.89
Regret	3.37	3.21	3.16	3.33	3.35
IQ	8.23	8.37	8.24	8.57	8.32

Note.— Sample characteristics are shown for the five conditions NO DEFAULT, WEAK DEFAULT, STRONG DEFAULT, CONTROL INCOME, and CONTROL PASSIVE GIVING. Age is depicted as mean. Income denotes the share of subjects with a monthly income below CHF 2,000. Education denotes the share of subjects with A-levels or higher. Extraversion, Agreeableness, Conscientiousness, Neuroticism, and Intellect are the five elements of the BIG5-inventory and are elicited on a 5-point Likert-scale. Scores are denoted as means. Need for Cognition is denoted on a 5-point Likert scale. Reactance is also a 5-point Likert scale on the Psychological Reactance Scale. The same metric applies to Regret. IQ is measured with 12 items belonging to the IQ-test after Cattell (1940). IQ is given as a mean score. Contingency tests performed for the complete sample show no signs of significant differences in characteristics across treatment and control conditions.

1.B Matching procedure of income: Control conditions

We apply the same income matching to both control conditions, CONTROL INCOME and CONTROL PASSIVE GIVING. As sessions of NO DEFAULT, WEAK DEFAULT and STRONG DEFAULT were elicited first, it is straightforward to map the distribution and amounts of income kept after Dictator Stage I to the control conditions. In the control conditions, subjects receive an income on top of their participation fee that matches choices in NO DEFAULT, WEAK DEFAULT and STRONG DEFAULT. For instance, if a subject in WEAK DEFAULT decides to donate 10 points to any charity, then the remaining income is 90 points. A subject in CONTROL INCOME/CONTROL PASSIVE GIVING then receives an additional 90 points. Naturally, as we have more subjects in NO DEFAULT, WEAK DEFAULT and STRONG DEFAULT than in either control condition, income matching cannot be executed perfectly. However, the aim of the procedure is to ensure that the distribution of incomes after Dictator Stage I do not differ significantly between treatment and control conditions. This then provides a stable baseline for the comparison of behavior in Dictator Stage II. Table 1.B.1 shows test statistics for the two-sample Kolmogorov-Smirnov-tests for differences in these distributions. The null hypothesis is that the distributions are equal and that the test statistic D is not statistically different from zero.

Table 1.B.1: Kolmogorov-Smirnov test statistics

Distribution Comparison	D	p-value
Complete sample		
NO DEFAULT | CONTROL INCOME	0.068	0.957
NO DEFAULT | CONTROL PASSIVE GIVING	0.078	0.899
WEAK DEFAULT | CONTROL INCOME	0.064	0.978
WEAK DEFAULT | CONTROL PASSIVE GIVING	0.054	0.998
STRONG DEFAULT | CONTROL INCOME	0.026	1.000
STRONG DEFAULT | CONTROL PASSIVE GIVING	0.022	1.000

Note.— Test statistics (D) and p-value for Kolmogorov-Smirnov tests for comparison of income distribution equality among treatment and control conditions. Low values of D suggest that distributions of income do not differ between the corresponding groups. P-values below conventional levels would lead to the rejection of the hypothesis that the underlying distributions are equal.

1.C Experimental Instructions

Note: This set of translated instructions was used for respondents in NO DEFAULT, WEAK DEFAULT and STRONG DEFAULT. Differences in WEAK DEFAULT and respectively STRONG DEFAULT are *italicized*. In CONTROL INCOME, Dictator Stage I was omitted and participants solely received information about their endowment (= participation fee). The remainder of the experimental instructions was identical to NO DEFAULT / WEAK DEFAULT / STRONG DEFAULT. In CONTROL PASSIVE GIVING participants again received information about their endowment (= participation fee). Subsequently, they were presented with the same instructions as in Dictator Stage I. However, they were told that independent of their income an amount between 0 and 100 points would be donated to a charity of their choice.[11] Hence, they could read all the information about the charities and pick one to which the money was donated. Participants were also able to let the computer decide randomly on the choice of a charity. The amount of the donations could not be influenced by the subjects. Subsequent to their decision of choosing a charity, they received feedback about the amount of points that was donated. Afterwards the instructions were identical to those in NO DEFAULT/ WEAK DEFAULT / STRONG DEFAULT.

General Explanations for Participants

Welcome to the experimental laboratory. Today you are taking part in a scientific study, in which you can earn a certain amount of money, which will be handed to you in cash. How much money you earn, is dependent on your decisions and the decisions of other participants. Therefore, please read these instructions carefully.
The set of instructions is for your private use only. Please do not communicate with other participants during the experiment. If you have questions, give a hand signal and the experimenter will come to your desk to answer your questions. Non-observance of this rule will lead to the exclusion of the experiment. During the experiment you will receive information on your computer screen.[12] You take your decisions with keyboard and mouse. Your inputs are completely anonymous. The experimenter knows your identity, however we are not able to relate your decisions with your identity.
Please only use the buttons within the experimental window. With the button 'Continue' and respectively 'Back' you are able to change between the next and the previous page (if possible).
This study consists of **five parts**, in which you receive information and need to make decisions, which may influence your payoff. Your payoff will be calculated in points and converted according to the following rule:

10 Points = 1 Swiss Franc

How much you can earn in each of the parts will be stated in the instructions, which will be shown for each part separately on the screen. At the end of the study, the points you have earned will be converted to Swiss Francs and paid out in cash to you.
The study ends with a short questionnaire. As soon as each participant has completed this questionnaire, the pay-out will be started. You will be called for pay-out by your seat number. Expected processing time for the study is between 45 to 60 minutes.

[11] This procedure follows the instructions by Gneezy et al. (2012) for a 'costless' donation.
[12] Note that these instructions are supplemented with figures from the actual program, as we did not use paper instructions.

Dictator Stage I - NO DEFAULT/WEAK DEFAULT

In this part of the study you receive **200 points**. 100 points thereof are your participation premium, which you can keep with certainty. The other 100 points are at your disposal for your decision in this part of the study. You can thereby decide how to allocate these 100 points (in increments of 10 points) between yourself and a charity. You can keep all points for yourself and give no points to a charity; you can devote all points to a charity and keep no points for yourself; or you can keep a certain amount of points for yourself and pass the remaining points to a charity. The amount of your donation can be specified with in the input field ‘Ihre Spende [in Punkten]’ (Your Donation [in Points]).[13]

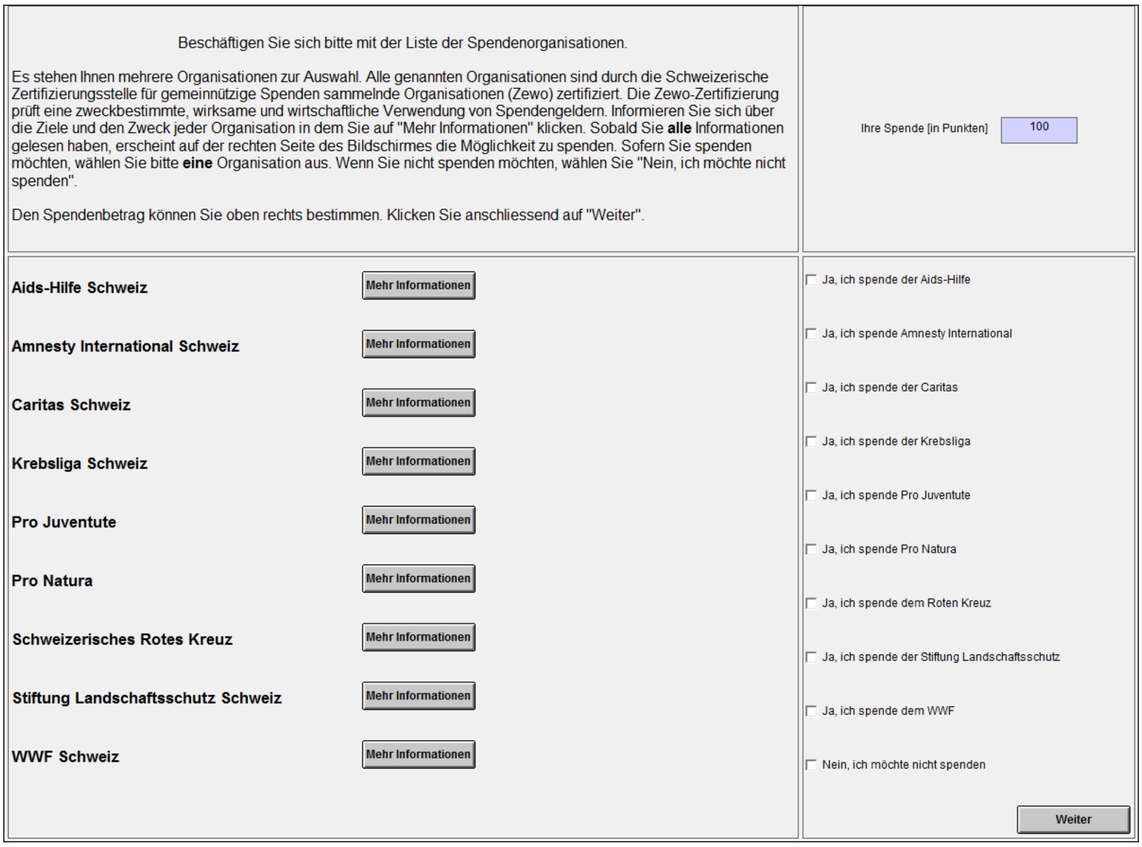

Figure 1.C.1: Sample screen of a decision task in the Dictator Stage I in Experiment I.

Note.— WEAK DEFAULT is simply implemented by pre-specifying the input field to ‘100’. In NO DEFAULT this field initially remains blank.

There are **nine charities** available for selection, which will be described on the left-hand side of the screen. All charities are certified by the ‘Swiss Zewo Foundation’. The ‘Zewo Foundation’ testifies a purposive, effective and economic use of donation money. Inform yourself on the goals and purpose of each charity by clicking on ‘Mehr Informationen’ (More Information). The button ‘Mehr Informationen’ changes its color from red to grey, once you have read the information about a charity.

As soon as you have read the **complete** set of information about each charity, you will be able to select a charity to donate to on the right hand side of the screen. In case that you want to allocate points between yourself and a charity, please select **one** charity. You can

[13]See Figure 1.C.1 for a screen of the decision.

only choose one charity to donate to. *With clicking on "Weiter" (Continue) you donate 100 points to a selected charity. You can specify a different donation amount in the upper right corner (WEAK DEFAULT).* If you do not want to allocate points between yourself and a charity, please choose "Nein, ich möchte nicht spenden." (No, I do not want to donate.)
Please note that points, which you keep for yourself, will be paid out in cash at the end of the study. Points, which you allocate to a charity will be donated by the experimenter to the chosen charity. If you donate, you will receive an official letter by the Chair of Economics at ETH Zurich with your pay-out that the chosen amount will be transfered to the corresponding charity. In order to familiarize yourself with the decision task, please answer the following questions: Person A donates 40 points to a charity.

(1) How many points will person A receive at the end of the study with this decision? (Please note that you will keep 100 of your 200 points with certainty.)
(2) How many points will the charity receive at the end of the study with this decision?

Dictator Stage I - STRONG DEFAULT

In this part of the study you receive **200 points**. 100 points thereof are your participation premium, which you can keep with certainty. The other 100 points are at your disposal for your decision in this part of the study. You can thereby decide how to allocate these 100 points (in increments of 10 points) between yourself and a charity. You can keep all points for yourself and give no points to a charity; you can devote all points to a charity and keep no points for yourself; or you can keep a certain amount of points for yourself and pass the remaining points to a charity. The amount of your donation can be specified with in the input field 'Ihre Spende [in Punkten]' (Your Donation [in Points]).[14]
There are **nine charities** available for selection, which will be described on the left-hand side of the screen. All charities are certified by the 'Swiss Zewo Foundation'. The 'Zewo Foundation' testifies a purposive, effective and economic use of donation money. Inform yourself on the goals and purpose of each charity by clicking on 'Mehr Informationen' (More Information). The button 'Mehr Informationen' changes its color from red to grey, once you have read the information about a charity.
As soon as you have read the **complete** set of information about each charity, you will be able to select a charity to donate to on the right hand side of the screen. In case that you want to allocate points between yourself and a charity, please select **one** charity. You can only choose one charity to donate to. With clicking on "Weiter" (Continue) you donate 100 points to a selected charity. You can specify a different donation amount in the upper right corner. If you do not want to allocate points between yourself and a charity, please choose "Nein, ich möchte nicht spenden." (No, I do not want to donate.)
Once the button in the upper right corner is pressed, subjects receive additional information on how to change the donation amount (STRONG DEFAULT):
To change your donation of 100 points, you need to fulfill a task. The task consists of changing 48 sliders with your mouse. Each slider is initially positioned at 0 and can be moved as far as 100. A number right to the slider indicates its current position. You can readjust the position of each slider as many times as you wish. You have to adjust all sliders to the value 50 - only then you will be able to change the donation amount. If you do not like to fulfill the task, please click on 'Abbrechen' (Cancel).
Please note that points, which you keep for yourself, will be paid out in cash at the end of the study. Points, which you allocate to a charity will be donated by the experimenter

[14]see Figure 1.C.1 for a decision screen

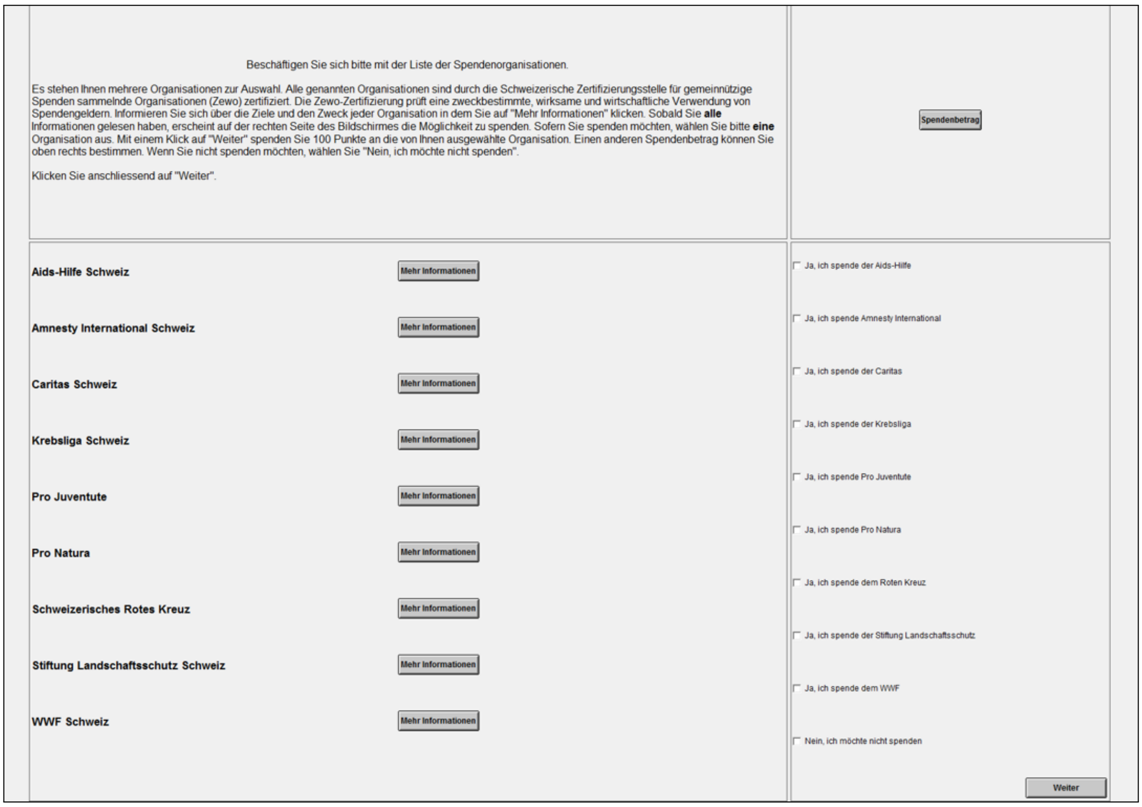

Figure 1.C.2: Sample screen of the decision task in the Dictator Stage I in STRONG DEFAULT.

to the chosen charity. If you donate, you will receive an official letter by the Chair of Economics at ETH Zurich with your pay-out that the chosen amount will be transfered to the corresponding charity. In order to familiarize yourself with the decision task, please answer the following questions: Person A donates 40 points to a charity.

(1) How many points will person A receive at the end of the study with this decision? (Please note that you will keep 100 of your 200 points with certainty.)
(2) How many points will the charity receive at the end of the study with this decision?

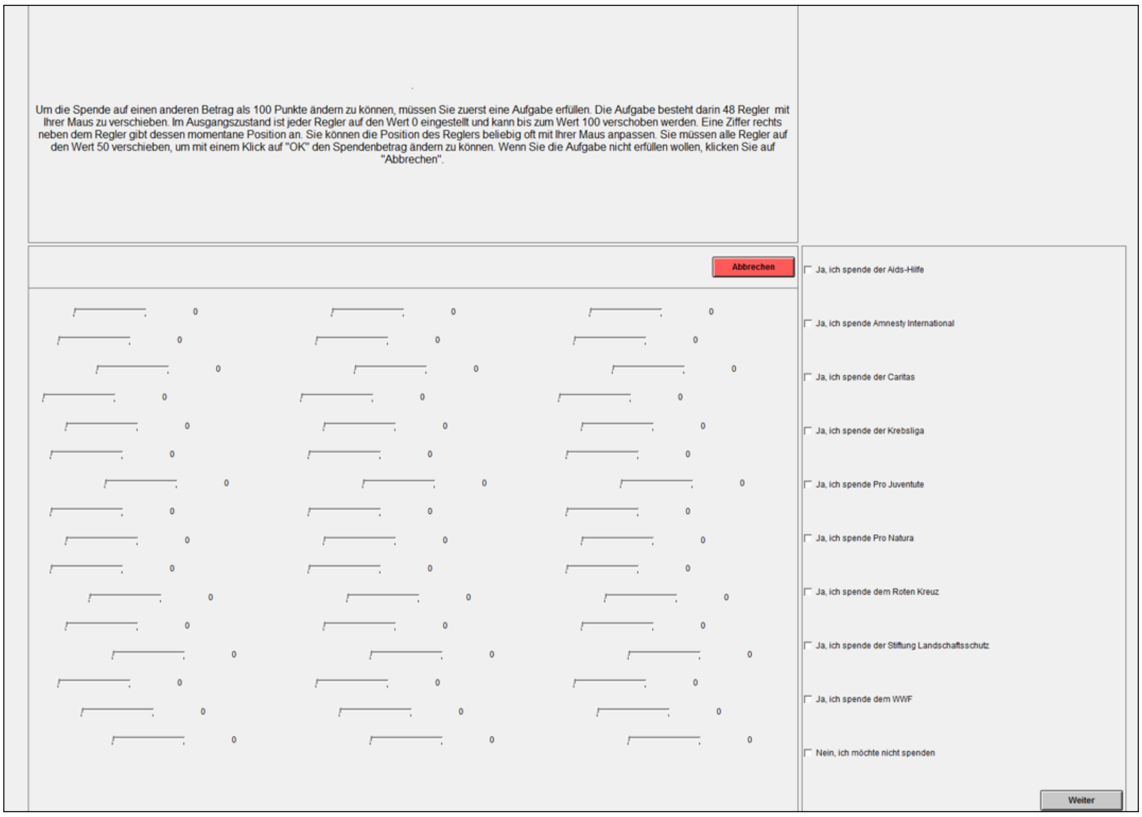

Figure 1.C.3: Sample screen of the slider task in the Dictator Stage I in STRONG DEFAULT.

Filler task: Shortened IQ-test after Cattell (1940)

Note: The IQ-test was divided into two parts, which share exactly the same instructions. In each part, subjects had to identify a subset of four figures. Exemplarily, we show a figure of each subset.

Section 2 consists of a shortened version of an intelligence test. The tests is divided into two parts. For each part you receive further information.
The figure shown below (see Figure 1.C.4 and 1.C.5 for an example in each part) gives you an example of the exercise you have to solve in part one (or two). You have to decide which of the squares on the right hand side follows logically the squares on the left (fits logically into the larger square on the left). You make your choice by clicking on the button below the squares. In this example you should choose ‘c’ (‘b’), because the circles in the squares get smaller from square to square (because it fits exactly with the smaller upper right square).
The test starts as soon as you click the button ‘Start’. You have 1 minute and 30 seconds to answer each part. Probably, the amount of time allowed is not sufficient to answer all questions. Do not let yourself discourage by this. Simply work as correctly and as fast as possible.

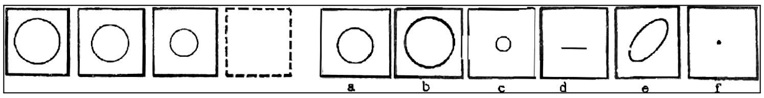

Figure 1.C.4: Sample exercise in part one of the IQ-test

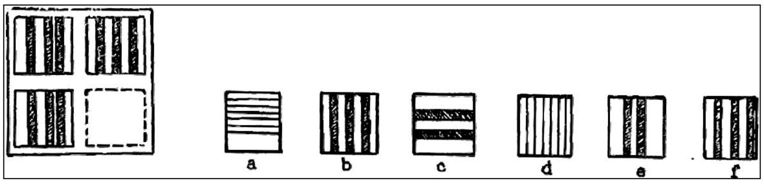

Figure 1.C.5: Sample exercise in part two of the IQ-test

Dictator Stage II

In this section of the study you have to decide on the distribution of **200 points** between yourself and a randomly allocated person in this room. This allocation is anonymous. Neither you nor the other person gets to know your mutual identities during or after the study. You decide in anonymity. Your own decision can not be influenced by the other person.
You have to decide how many points you want to give to the other person. You have 200 points for your decisions. You can keep all points for yourself and give no points to the other person; you can give all points to the other person and keep no points for yourself; or you can keep a certain amount of points (in increments of 10 points) and pass the remaining points to the other person. You can specify the number of points you want to give to the other person in the designated input field.
The other, randomly allocated, person has the same decision task as you and needs to decide how many of the 200 points she or he wants to give to you. **However, only one of these two decisions will be implemented, i.e., the 200 points will be distributed among you and the other person only once.** Which of these two decisions is relevant will be determined randomly by the computer. If the computer (with a probability of 0.5) randomly determines that your decision will be implemented, the other participant will receive the points that you have decided to give to her or him. If the computer (with a probability of 0.5) randomly determines that the decision of the other participant is implemented, you will receive the points that the other participant has decided to give to you. As you are unable to determine whether the computer selects your or the other person's decision, you should carefully consider the decision task.
If your decision is implemented, you will receive the points, which you have kept for yourself and these points will be paid out in cash at the end of the study. If the decision of the other person is implemented, you will receive the points that the other person has given to you, and the other person keeps the remaining points.

In order to familiarize yourself with the decision task, please answer the following questions:
Person A gives 70 points to person B. Person B gives 10 points to person A. The computer implements the decision of person B.

(1) How many points will person A receive at the end of the study with this decision?
(2) How many points will person B receive at the end of the study with this decision?

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