Transparent defaulting: an ethical way of increasing policy compliance

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Introduction

Beyond doubt, vaccines are one of the biggest global health achievements. According to the World Health Organisation (WHO), vaccination helps prevent an estimated two to three million deaths per annum (Bustreo, 2016). In recent years, however, governments worldwide have been forced to take action against the steady return of previously eradicated or controlled diseases, such as measles and rubella. In Europe, more than 116000 cases of measles have been reported in 2019 alone (WHO, 2020). The problem is not restricted to the developing European economies, and it affects countries such as Germany and the United Kingdom. In Germany, almost 9000 cases of measles have been registered for the past 8 years (WHO, 2020), while the cases in the UK approach 8000 for the same period. To counter the issue, the German parliament approved the "Measles Protection Law" (Masernschutzgesetz), which states that as of March 2020 children and staff in communal facilities *must* be vaccinated. Failure to comply will result in fining parents and banning their children from schools and kindergartens. In the UK, however, similar approaches are deemed too restrictive. Leading health experts are concerned with the effectiveness of mandatory vaccination, and point out the negative long term outcomes of excluding children from school (Bedford, 2020). As a cheaper and less paternalistic alternative, academics proposed using default rules in policies aimed at promoting vaccination (Guibilini et al., 2019; Brewer et al., 2017). A default rule goes into effect unless the decision-makers specify otherwise (Brown & Krishna, 2004), and setting such rules has been a major success in increasing vaccination rates (Chapman, Li, Colby, & Yoon, 2010) and decreasing vaccine refusals (Opel & Omer, 2015).

Despite its effectiveness, the use of default options was met with considerable criticism on ethical grounds due to their non-transparent nature (Smith, Goldstein & Johnson, 2013). Arguably,

the targeted population has little awareness of the impact defaults have on their decisions (Hansen & Jespersen, 2013), which can be interpreted as a threat to their autonomy. At the same time, transparency has been discussed as possibly reducing the effectiveness of policy interventions in general, including defaulting (Bovens, 2009). Supposedly, once the targeted individuals realise their choices have been influenced, they can retaliate and opt-out, due to manifestations of psychological reactance (Brehm, 1966; Brehm & Brehm, 1981), or general willingness to preserve their autonomy (Ryan & Deci, 2000).

Naturally, policy makers would want the best of both worlds: a behavioural intervention, which reliably increases policy compliance, and does so in an ethical way. Is that at all possible, however? The following dissertation aims to answer this question, and to demonstrate that transparency can even boost the effectiveness of default nudges. Further, I highlight the discrete forms of transparency which can benefit default compliance. Last, I demonstrate that combining defaulting and transparency has a stronger positive effect on compliance than using each technique in isolation.

Overview

This cumulative dissertation is based on the following three empirical papers:

1. Paunov, Y., Wänke, M., & Vogel, T. (2019a). Transparency effects on policy compliance: disclosing how defaults work can enhance their effectiveness. *Behavioural Public Policy, 3*(2), 187-208. https://doi.org/10.1017/bpp.2018.40

2. Paunov, Y., Wänke, M., & Vogel, T. (2019b). Ethical defaults: which transparency components can increase the effectiveness of default nudges?. *Social Influence, 14*(3-4), 104-116. https://doi.org/10.1080/15534510.2019.1675755

3. Paunov, Y., Wänke, M., & Vogel, T. (in press). Combining defaults and transparency information to increase policy compliance. *Social Psychology*.

The papers are presented in Appendix I in their manuscript form.

The dissertation body is organized into three sections – a theoretical frame section, an empirical summary section, and a general discussion section. The first section contains the synthesis and analysis of the literature, relevant for the dissertation project. There, the essential theoretical concepts such as nudging, defaulting, and transparency are introduced, followed by a summary and discussion of the current literature on transparent defaulting. After that, the research questions, which represent the core of the dissertation, are formalized. The empirical summary section follows, aiming to answer these questions by presenting evidence from the experimental studies included in the dissertation project. Last, the general discussion section derives the theoretical and practical implications of the empirical findings, and provides directions for future research.

Theoretical frame

Nudging

One of the corner stones of democracy is the shared belief that the very purpose of political organization is to ensure the well-being of society's members. This purpose is achieved through provision and maintenance of the greater good, defined as an amalgamation of common social interests and facilities (Rawls, 1971). While interpretations of the construct differ (Hussain, 2018), there is a universal agreement that policy-makers and citizens alike are expected to act in a way, which promotes it. The citizens dedicate a stake of their freedom and resources to elected representatives, which in turn use their power to materialize and preserve the greater good via instituting various laws and policies.

Traditionally, compliance to such policies is assured through campaigning, incentivisation, and, in some cases, penalization for non-compliance. Often, however, these approaches are either expensive (Capraro et al., 2018), or too paternalistic and restrictive (Thaler & Sunstein, 2008). As an alternative, Thaler & Sunstein (2008) proposed an innovative approach avoiding steep costs and strict regulations. Integrating insights from social psychology and dual-process theories (Evans, 1984, Chaiken & Trope, 1999), the authors introduced a set of behavioral interventions aimed at promoting various prosocial and self-beneficial behaviors. These interventions, or "nudges", make use of subtle changes in the decision environment in order to guide people's actions. While a precise operational definition of nudging is still missing, (Marteau, Ogilvie, Roland, Suhrcke, & Kelly, 2011) ten years of implementation have cemented the nudging approach as the social influence tool of choice for governments and private agents alike.

At its core, nudging reflects a rather appealing governance approach, which Thaler and Sunstein (2008) label "libertarian paternalism". Its libertarian aspect is related to the claim, that nudging policies do not forbid or penalize non-compliant behavior. Instead, they aim to influence the way choices are presented to the targeted population, an approach branded as "choice architecture". Nudging is also paternalistic to the extent that it ascribes legitimacy to the actions of certain influence agents, or "choice architects", in their intent to influence people's behavior. In this sense, nudges are described as both liberty-preserving and paternalistic, representing a sort of "soft" and "self-conscious" (Thaler & Sunsten, 2008) effort on behalf of governments and private parties to guide people's choices for their own good.

While such philosophy can be rather questionable under close scrutiny[1], there is little doubt that most nudges appeal to the general public. As Jung and Mellers (2016) demonstrated, Americans consider them necessary and effective, and express general support for a variety of nudge-based policies. Data from most European countries reveal a similar pattern (Reisch, Sunstein, & Gwozdz, 2017). A few examples of successful nudges: when a number of UK municipalities decided to paint green footsteps leading to their communal trash bins, they measured a staggering reduction in littering of 19.5% (KBT Report, 2015). In the US, re-arranging food items in school cafeterias made healthy food choices easier to access, and increased sales of healthier food by 18% while simultaneously decreased unhealthy food consumption by 28% (Hanks, Just, & Wansink, 2012). Implementing intentions by proactively choosing a date and time for an influenza shot increased vaccination rates in a number of US companies with 4.2% (Milkman,

[1] In the sense that it potentially legitimizes policy interventions from private agents, which may not have people's best interests in mind. In contrast, governmental institutions receive explicit mandate to implement policies in order to sustain the greater good.

Beshears, Choi, Laibson, & Madrian, 2011). In short, the examples of successful nudges are as numerous as the problems they address. The next section presents the most effective nudging approach: the use of default options.

Defaults

No other nudge has received as much scientific and public attention as defaulting. A default is "*the choice alternative a consumer receives if he/she does not explicitly specify otherwise*" (Brown & Krishna, 2004, p.529). In the general case, default nudges are characterized by a decision situation in which one of the choice options is pre-selected, but the decision-maker retains the possibility to actively choose another alternative (i.e., to opt out). A large body of research (for reviews see Willis, 2013, and Szaszi et al., 2018) demonstrates that people exhibit clear preferences for the default option, thus making defaulting the most effective nudging strategy (Hummel & Maedche, 2019). Defaults have particularly strong impact in cases when the decision-makers perceive the default option as the recommended one (McKenzie, Liersch, & Finkelstein, 2006), or when the pre-selected behavior is altruistic (Everett et al., 2014). Default effects are powerful[2], and default interventions have been successfully implemented by private and governmental institutions as means of increasing policy compliance. Perhaps the most clear-cut demonstration of how effective those interventions are is reported by Johnson and Goldstein (2003). The authors present data from a number of European countries, which differ in their organ donation policies. If German or Dutch citizens want to be organ donors, they have to proactively register and carry an organ donor pass with them at all times. However, in countries like Austria and Hungary donation consent

[2] Hummel & Maedche (2019) report a relative effect size of 78% for default nudges. Data were extracted from 21 studies and 62 separate effects. The authors define relative effect size as the percentage change between the dependent variable of the treatment group and the control group.

is automatically presumed, while one retains the freedom to opt-out by filling in a specific legal form. With a simple flip of the status quo, courtiers with opt-out donation policies boasted a staggering 99% average compliance (Johnson & Goldstein, 2003).

Defaulting is also effective when it comes to environmental preservation. In several lab experiments and natural studies Pichert and Katsikopoulos (2008) demonstrated that people's choices of electricity providers (green/from renewable energy sources vs. grey/from coal plants) are dependent on the option, which is presented as the default. As a result of the default intervention, the village of Schönau in Baden-Württemberg is now entirely powered by "green" energy sources since 2007. In addition, approximately 94% of all customers of a large South German energy provider also "switched" to renewable power once the option became the status quo.

The Default Effect

The research interest in defaulting matches the popularity of default nudges, and the literature reflects several schools of thought regarding the cause of the default effect. The first category of explanations (labelled further "cognitive bias account") interprets the effect as a result of various decision-making biases. One interpretation from this category points out to loss aversion (Kahneman & Tversky, 1984;1991) and the endowment effect (Kahneman, Knetsch, & Thaler, 2011) as potential antecedents of the default effect. It has been theorized that people stick with defaulted options because they view them as their own, and giving them up is perceived as a loss (Kahneman, Knetsch, & Thaler, 1991). Another bias-based explanation is that people are more likely to blame themselves and experience regret about a poor outcome when they change a default, than when the outcome is caused by remaining with it (Willis, 2013). Hence, people are drawn to

remain with the pre-selection, a manifestation of a phenomenon, known as the omission bias (Ritov & Baron, 1990).

A different perspective (addressed further as "communication account") regards defaults as a form of social interaction between policy makers and the targeted population. As in most other communication settings, a communicator's choice of message carries information about her/his attitudes toward a given choice option (Sher & McKenzie, 2006). In fact, McKenzie et al. (2006) demonstrated that when defaulted, the targeted individuals were able to recognize that the pre-selected choice option is the preferred one, and that the experimenters wanted them to pick that option. Consequently, the participants exhibited a clear preference for that option. Therefore, the authors concluded that setting up a default *per se* was perceived as a form of an implicit recommendation, which in turn resulted in compliant behavior.

Overall, the abovementioned perspectives provide an insight as to why defaults are a very successful way of influencing choice. However, both the cognitive bias and communication accounts are vulnerable to criticism, which has triggered a considerable discussion about the implementation of default nudges. The next section introduces that discussion.

Defaults and transparency: Ethicality debate

The use of defaults has raised ethical concerns related to their supposedly covert nature. To clarify, default nudges are presumably "working in a way that the citizen in the situation cannot reconstruct either the intention or the means by which behavioral change is pursued" (Hansen & Jespersen, 2013, p. 17). This account holds true if one takes the cognitive bias account as a probable cause for the default effect: both the omission bias and the endowment effect are processes, over which people have no conscious control (Tom, Nelson, Srzentic, & King, 2007; Ritov & Baron,

1990). Hence, some researchers argued that defaulting is unethical, and default-based policies would limit people's autonomy and ability to exercise informed choice (Smith, Goldstein & Johnson, 2013).

However, others claim that all nudges, including defaults, inherently contain a necessary degree of transparency in their nature (Bovens, 2009). One can assume that any given nudge consists of tangible environmental cues, such as the arrangement of food items in the cafeteria, or the green steps leading to trashcans. These cues or features realize the choice architecture, and are *potentially* recognizable by the targeted individuals. Therefore, they introduce a necessary degree of *in principle* transparency (Bovens, 2009) to the given nudge. In default interventions, this would mean that the targeted individuals have the theoretical ability to acknowledge the pre-selection, i.e. to become aware of its presence in the choice environment. In fact, if one assumes that the default effect is driven by implicit recommendations (McKenzie et al., 2006), it follows that people do recognize the influence attempt. It is this realization is enables them to make inferences about the default-setter's preferences and intentions in the first place, before they can conclude that the default is recommended. Therefore, it does not seem prudent to consider defaults as entirely non-transparent.

Nevertheless, one can argue that potential transparency and implicit endorsement are necessary, but not sufficient conditions for the ethical justification of defaults. First, the theoretical possibility to acknowledge the default does not automatically translate to actual awareness. Whether one becomes aware would depend on a variety of situational and personal factors, such as her/his watchfulness (Ivancovic & Engelen, 2019), or the degree to which the pre-selection is apparent (Schmidt, 2017). Some default nudges can be pretty hard to detect: A classic example is

the introduction of default plate sizes to control snacking and unhealthy food consumption. A number of field experiments (for a meta-analysis see Cadario & Chandron, 2018) demonstrate that setting downsized plates as the default leads to reduced calorie intake and less snacking. In such cases, the autonomy of those who fail to recognize the pre-selection is still diminished, since they remain completely unaware of the influence on their choice.

Second, while it is true that the implicit recommendation account presupposes awareness of the pre-selection, the inferences people make once they become aware can vary greatly. If the targeted individuals infer that the default option is not only recommended, but also the best one (Smith, Goldstein, & Johnson, 2003), or the socially accepted one (Sunstein & Thaler, 2003), their autonomy can still be affected by non-deliberate mechanisms, such as normative pressure (Everett et al., 2014) or the imitation heuristic (Henrich et al., 2001).

In sum, there is evidence that while defaulting is not completely non-transparent (under certain pre-conditions), it is also not autonomy preserving. Hence, a number of researchers called for an increase in transparency when nudging with defaults, especially on the governmental level (Sunstein, 2015; Ivancovic & Engelen, 2019).

Defaults and transparency: Effectiveness debate

In turn, however, the call for transparency sparked another debate about the nature of the relationship between transparency and the effectiveness of defaults and nudges in general. According to some researchers, there is a tradeoff between transparency and effectiveness when nudging interventions are implemented. Bovens (2009) speculated that most nudges should become increasingly ineffective the more people realize they are being nudged: “the more actual…transparency we demand, the less effective these techniques are” (p. 13). Hence, despite

that Bovens considered the nudging approach transparent *in principle*, the common lack of *de facto* transparency in the implementation of most nudges deemed nudged decisions non-autonomous. Therefore, he speculated that even if people comply with a given nudge, they would engage in behaviors that are inconsistent with their initial decision once they become aware of the influence on their choice. Thus, the author concluded that nudges, such as defaults, work "better in the dark" and could lose their effectiveness when transparency is introduced.

Prominent psychological theorizing seems to support such a position. In general, people strive for self-determination (Deci, 1975; Ryan & Deci, 2000) and resent limitations to their freedom of choice. In order to reinstall that freedom, they could deliberately resist choosing a defaulted option, and would rather endorse alternative ones, a manifestation of retaliatory behavior known as psychological reactance (Brehm, 1966; 1981).

However, if one considers defaults a form of a social interaction, concerns about a tradeoff between transparency and effectiveness do not seem as justified. Adopting this perspective, one would predict that a proactive disclosure of the default strategy may not harm its effectiveness, but can even give it a boost. One reason would be that a disclosure can greatly simplify the decision situation for the targeted individuals. As discussed previously, people can comply with a default nudge since they recognize a pre-selected option as the recommended one. However, that is merely one of the conclusions they reach when defaulted, and it is unlikely to be the first one. Like most social interactions, the default nudge is a communication setting, in which people make a number of pragmatic inferences in order to make sense of the situation and satisfy their needs to convey meaning (Wänke, 2007). Since a direct message from the default-setter is missing, they first need to infer that the pre-selection *is* the message. Only after that, they can start making further inferences about the message's meaning, the intent behind the pre-selection, etc. A proactive

disclosure of the default, however, could make the policy maker's intent clear, and would also clarify which behaviour is expected from the targeted individuals. Therefore, adding accurate transparency information to the default setting could potentially reduce the number of pragmatic inferences and cognitive processing needed to reach a decision, and lessen the uncertainty in the decision making process. In addition, a proactive disclosure can also give the default a further boost by transforming the implicit recommendation into an explicit one: Explicit recommendations are a well-known motivator of choice behaviour (e.g. O'Keefe, 1997, Ansari, Essegaier, & Kohli, 2000; Kinney et al., 1998).

Moreover, making default nudges transparent could also benefit compliance by reducing the negative experiences people have when defaulted. In general, people express greater support for nudges, which allow them to reach autonomous decision (e.g enhanced calorie labelling, Reisch & Sunstein, 2016). In contrast, the public views defaults less favorably and perceives them as more autonomy threatening than other nudges (Jung & Mellers, 2016). This could be because people have specific expectations about the quantity and quality of information that should be provided in a given communicative attempt (Grice, 1989). When such expectations are not met, people can assume that the communicator withholds information, and tries to deceive them (Information Manipulation Theory, McCornack et al., 1992). Thus, once people become aware of the choice architecture but receive no information on it, they could feel deceived and retaliate (Brehm, 1966; 1981). A proactive transparency disclosure would eliminate such negative feelings, therefore boosting the effectiveness of the default.

Lastly, a disclosure may also trigger positive inferences about the communicator. Specifically, transparent disclosures can foster the perception that the communicator is fair (Steffel, Williams & Pogacar, 2016) and sincere (Paunov et al., 2019a), which is an integral part of the

communicator's trustworthiness (Grimmelikhuijsen & Meijer, 2012) and credibility (Eisend, 2006). A number of findings from communication and persuasion research show that both constructs are strongly and positively related to the persuasiveness of influence attempts (Priester & Petty, 2003; Hovland & Weiss, 1951; Hovland, Janis, & Kelley, 1953). Therefore, one might even expect an increased rather than reduced compliance when the default setter is transparent about the influence attempt.

Overall, there are sufficient reasons to assume that introducing transparency to classical default nudges may not reduce their effectiveness, as predicted by Bovens (2009). On the contrary, interpreting defaults as a social interaction suggests that transparency can even boost the default impact. With the competing theoretical predictions in mind, the next section introduces the current literature on transparent defaulting in detail.

Defaults and Transparency: Empirical findings

Given the extent of the transparency-effectiveness debate, the empirical evidence on the topic is surprisingly scarce. The few existing experimental studies do not support the negative theoretical predictions, outlined earlier, but produced mainly null-effects (Bruns et al., 2018; Loewenstein et al., 2015, Steffel, Williams, & Pogacar, 2016). To begin with, Loewenstein and colleagues (2015) asked their participants to make a hypothetical end-of-life decision (a choice between staying on life support or a painless passage). One or the other alternative was pre-selected between subjects. Orthogonally, the researchers varied whether the participants were informed of the default before or after making the decision. The results indicated no significant information effect on the participants' choice. However, the authors state that the null finding may reflect the respondents' desire for decision consistency (Falk and Zimmerman, 2013), since the default and transparency effects were isolated after all participants were asked to make the same decision twice:

once with a pre-selected alternative, and the second time imagining that they had not been defaulted previously. In addition, the disclosure offered by the experimenters contained very little information, related to the default, and was more informative of the experimental design, than of the purpose or the presence of the pre-selection. Yet, in a set of studies Steffel, Williams, and Pogacar (2016) did not find significant effects of transparency in several defaults either. In their first study, the participants imagined they were about to join a fictitious social network, which employed an opt-out privacy policy. The network defaulted the participants to either share their personal details, or not. In a transparency condition, the participants received information about the default's purpose and behavioral means. However, the disclosure was provided in such a way, that it was not clear whether it came from the policy endorser (i.e. the social network), or from the experimenters. This creates a methodological issue, since the participants could have inferred that the network attempts to hide the default policy, and the experimenters disclosed the attempt, thus negating potential positive transparency effects. Likewise, Bruns et al. (2018) studied default effects on donation behavior and did not find an advantage of transparency. The participants imagined they donated a percentage of their remuneration for completing a task. However, default donations were set to a very costly option (80% of the money earned), hence possibly diminishing a positive transparency effect by setting a default option outside the participants' personal latitude of acceptance.

To summarize, the use of defaulting has received a lot of attention due to its effectiveness and the ethical discussion it provoked. While positions on the ethicality of default nudges differ, there seems to be a consensus that decisions influenced by defaulting are not entirely autonomous (Tom, Nelson, Srzentic, & King, 2007; Hansen & Jespersen, 2013). However, the recommendations to make defaults more transparent (Sunstein, 2015; Ivancovic & Engelen, 2019)

triggered concerns regarding a possible negative effect on the effectiveness of default interventions. Yet, a social communication approach suggest that default compliance can benefit from transparent disclosures. With opposing theoretical predictions and a limited literature pool, an in-depth exploration of the role of transparency in default nudging is still due. The current dissertation aims to fill that gap and answer the following research questions:

a) What is the effect of proactive transparency disclosures on the effectiveness of default nudges?
b) Which are the transparency components *within* a disclosure, which can bring about an effect on default compliance?
c) If transparent defaults are effective, would the effect stem from providing decision-relevant information per se, or a combination of both defaulting *and* information provision is needed?

Answering these questions can help clarify some of the ethical and pragmatic uncertainties related to the implementation of default nudges, and contribute to the ongoing transparency-effectiveness debate. Ultimately, finding a transparency effect on default compliance can empower both scientists and choice architects to employ a more ethical and autonomy – preserving version of the classical default nudge. In order to achieve that, a series of experimental studies were conducted within the framework of the present dissertation, which addressed the abovementioned research questions directly. A summary of the experimental setups and empirical findings per question is reported next. Since the findings come from co-authored papers, I will use the pronoun "we" throughout most of the following section. The full texts of the respective articles are attached in Appendix IV.

Empirical Summary

First and foremost, we needed to establish a working definition of transparency in order to systematically measure its effect on default compliance. Combining the insights of Bovens (2009), Hansen & Jespersen (2013), and Sunstein (2015), we identified three major transparency components, which would situate the construct within a communicative defaulting paradigm. The first component represents our belief that a social interaction between default setter and addressee can take place only after the *presence* of the pre-selection has been recognized in the decision environment. The second component represents the pragmatic inferences people make about the *purpose* of the pre-selection, once they become aware of its presence. These can be about the behavioral outcome, targeted by the endorser (e.g. inferring that the default is set because the policy maker them to choose the pre-selected option), or about the end goal of the pre-selection (e.g. inferring that a default donation value is set to increase contributions towards a given charitable cause). The last component represents the peoples' beliefs about the general effect of defaulting (i.e. how the pre-selected value *per se* influences their choice). Combining all we defined default transparency as *an objective intervention characteristic, stemming from a full endorser disclosure of the default's presence, purpose, and behavioral means.* Formulated this way, the definition also reflected our belief that transparency has to be introduced *proactively* to a default nudge (in the form of a disclosure), so that it can produce an effect on compliance.

To isolate that effect, we conducted three studies, which allowed for a comparison between a classic default nudge and an intervention, in which the endorser proactively disclosed information about the pre-selected option (Paunov, Wänke, & Vogel, 2019a). In the first two studies, we also tested our assumption that an effect on compliance can be achieved *if* this information is disclosed

proactively. There, the effects of defaulting and transparent defaulting were explored in a hypothetical scenario, which also included a third, non-transparent-but-aware condition. In it, the participants imagined they retrieve the transparency information from memory (remembering they read an article about defaulting), and not from the policy endorser. Beyond hypothetical scenarios, a third study tested the effect of transparency versus non-disclosure for actual choices.

In the first two studies the participants imagined they log in to check their new online study program, when they notice that some of their electives are already pre-selected. In a transparency condition, the "university administration" admitted they pre-selected the courses, and that they did it to ensure they choose them. In addition, the administrators disclosed the way defaults affect behavior in general, namely that people tend to stick with pre-selections when choosing (for the full text of all scenarios, see Appendixes for Manuscript 1, A, p. 63). In a non-transparent-aware condition the participants were asked to imagine they recall the same information, and that based on it they infer that the university wanted them to choose the preselected courses. A classic default nudge condition obtained a baseline. Endorser trustworthiness was explored as a mediator in the first study, and feelings of being deceived in the second.

In both studies, a proactive disclosure of a default's presence, purpose and behavioral means significantly increased policy compliance. Moreover, the positive transparency effect was present *only* when the disclosure came from the policy endorser (i.e. the university administration). The data also suggested that people may feel deceived when defaulted and the endorser does not make that transparent. This was the case independent of whether the participants retrieved information about the defaults purpose and functioning from memory, or received no information. A proactive disclosure from the policy endorser completely mitigated the negative affect.

A third study replicated the findings with actual choices, where the positive transparency effect was even stronger: the default setting more than doubled choices for a particular decision option, and making the default transparent quadrupled them.

Despite the promising results, however, we knew little about the robustness of the effect and the impact of the separate transparency components on compliance. More specifically, it remained unclear whether one needed to disclose all components to achieve the desired effect, or a single one was sufficient to deliver it. In theory, each component had the potential to bring about a positive compliance effect on its own, except for the disclosure of the default's general effect (i.e. that people tend to stick with pre-selected options). There, we hypothesized that the outcome could go both ways: If the participants interpret the disclosure as an act of sincerity, than they would be more willing to comply (Steffel, Williams, & Pogacar, 2016; Paunov, Wänke, & Vogel, 2019a), but if they infer that the nature of defaulting is restricting their decision autonomy, then they would retaliate (Brehm, 1966). Therefore, in our next experiment (Paunov, Wänke, & Vogel, 2019b), we varied whether people were informed about *what* the default is intended to achieve (disclosure of target behavior), *why* the endorser wants people to choose the defaulted option (disclosure of purpose), and *how* defaults affect behavior in general (disclosure of general effect) in separate conditions. The full text of the respective disclosures is available in Manuscript 2, Table 1 (p. 84).

In addition, we wanted to replicate the effect in a more challenging social dilemma setting, where choosing the default option went against the self-interest of the participants. Such a setup represented traditional defaulting more closely, since default nudges are often aimed at promoting altruistic behaviors (Hummer, & Maedche, 2019). Therefore, we employed the services of a paid respondent panel, and informed the participants in all default conditions that choosing the pre-

selected option will presumably require them to spend more time working on a task, than they will be getting paid for. In reality, everyone worked on a task compatible with their endowment.

The results re-affirmed that transparent defaults can be more effective than the conventional default nudge. Moreover, the effect survived in a setting, where the participants believed that choosing the default option meant sacrificing personal resources for the sake of contributing to a prosocial goal (for the participant instructions, see the Supplementary Material for Manuscript 2, page 97).

Beyond previous data, we demonstrated that disclosing the default's purpose, both in terms of the desired target behavior, and as clarifying the end goal of the default, had a positive effect on compliance. Informing the participants about how defaults work in general did not increase choices of the default option.

Despite replicating the effect and identifying the responsible transparency elements, a closer look at the evidence demanded further scrutiny. In particular, we were still to determine whether the compliance increase was due to the combined influence of the default *and* the disclosed transparency information, or due to providing decision-relevant information *per se.* On the one hand, persuasion research shows the effects from two information cues can add up (*additivity hypothesis,* Bohner et al., 1995; Maheswaran & Chaiken, 1991) to exert influence on a decision's outcome. Therefore, both the implicit recommendation from the default itself and the transparency information might have been necessary to increase the effectiveness of the influence attempt. However, evidence from the same vein of research demonstrates that more diagnostic information can attenuate the effect of less diagnostic cues on choice (*attenuation hypothesis,* Chaiken & Maheswaran, 1994). Hence, the default may even become obsolete if superseded by the informational value of the provided transparency information (Keller, Harlam, Lowenstein, &

Volpp, 2011). If that was the case, we could have obtained a compliance boost by simply informing the participants which option is most desirable, even in the absence of a default value.

To address the issue, we introduced an informed free-choice condition to our transparent defaulting paradigm (Manuscript 3, p. 100). The results showed the transparent default nudge does not owe its effectiveness to the mere presence of decision-relevant information. Instead, we demonstrated that both defaulting and information provision contribute to the boost in compliance, in an additive fashion. The effects were statistically independent (Paunov, Wänke, & Vogel, in press).

This experiment concluded the empirical phase of the dissertation project. The next section presents a summary of the findings and their theoretical and practical implications. The limitations of our approach and directions for future research are discussed.

Discussion

With conflicting theoretical predictions and scarce empirical evidence, we set out to explore the effect from introducing transparency to conventional default nudges. Our main goals were to demonstrate the effect, and identify the transparency components responsible for it. Further, we wanted to specify whether it originated from the combined influence of defaulting and transparency information, or from merely informing people about their choice options.

In accordance with previous research (Steffel, Williams, & Pogacar, 2016, Bruns et al., 2018), we found no evidence of a negative transparency effect on default compliance. On the contrary, we were the first to report that proactive transparency can increase the effectiveness of default nudges. Across all experiments within the dissertation project, transparent default conditions were almost twice as effective as mere defaults. Notably, this was the case for hypothetical as well as for actual choices. Therefore, our findings lend support to the general call for transparency in nudging interventions (Sunstein, 2015), and indicate that an ethical and effective interpretation of the conventional default nudge could be a viable asset for policymaking.

Note, that it is indeed the act of revealing one's intentions as a policymaker, and not people's awareness per se that drives the positive transparency effect on compliance. The effect is present only when the policy endorser proactively discloses the transparency information, but not when awareness comes from other sources. In the latter instance, participants can feel deceived. In our paradigm, a full endorser disclosure eliminated the negative affect. In a broader perspective, this finding lends support to the proactive transparency perspective in governance (Darbishire, 2010): It suggests that policy disclosures can be a promising alternative the current approach to

transparent policy making, where mere access to information is provided, and it is up for the targeted individuals to "inform" themselves about the specific policy.

Further, we showed that not every transparency disclosure can bring about the positive transparency effect. Disclosing which target behavior was desired, as well as sharing the default's purpose increased compliance in comparison to a classic default nudge. As to our knowledge, this was the first report of an actual beneficial effect of behavioral information in default nudging. Previous research has related such disclosures to increased theoretical support for similar nudges (Felsen et al., 2013), but not to actual choice behavior. Informing the participants of the defaults' general effect produced no effect on compliance. Possibly, two processes cancelled each other out: while participants in this condition perceived the endorser as fair, they also rated the argument strength of the disclosure as lowest from all conditions. Such an interpretation might explain the lack of reactance manifestations in previous research (Steffel, Williams, & Pogacar, 2016; Bruns et al., 2018).

In addition, we managed to demonstrate that the transparent default nudge does not owe its effectiveness to the mere presence of transparency information. It appears that both defaulting and information provision contribute to a positive effect on compliance in an additive manner. In this sense, our results provide empirical support in favor of combining nudging with more traditional interventions, as recommended in previous literature (Ölander & Thøgersen, 2014).

Lastly, our findings suggest that the positive impact of transparency persists in cases, when choosing the pre-selected option implies a tradeoff between personal gain and promoting an altruistic goal. The participants in our last experiments chose to stay with the default at their own expense, believing that doing so will help us reach our goals. Therefore, it appears that transparent defaulting is applicable to the most common default settings, where such a tradeoff is present- to

policies aimed at increasing charitable donations at the expense of personal gain (Fiala & Noussair, 2017) or contributing to environmental protection (Bruns et al., 2018). In a broader sense, the results also substantiate the notion that the success of defaulting in altruistic settings has normative moral underpinnings (Everett et al., 2015).

While our findings advocate transparent defaulting as an effective tool for increasing policy compliance, there are several limitations to their generalizability. Based on previous research (Bruns et al., 2018), we assume that the effect would not persist across settings, in which the costs for the targeted individuals exceed their personal latitude of acceptance. If the default value is set too high, people tend to ignore the content and valence of simultaneously presented information (Goswami & Urminski, 2016). In a transparent default setting, high default costs could be interpreted as a more informative cue, thus attenuating the effect of the transparency information. Whether that is indeed the case, however, remains an open empirical question. Future research could address the issue via setting up orthogonal experimental conditions with default amount and transparency information varied between subjects.

Second, we limit our predictions to settings where the disclosed purpose of a default is related to promoting a greater good such as increasing donations or protecting the environment. Previous research does not report a positive transparency effect if the defaults' intent serves the default-setters´ strict self-interest (Steffel, Williams, & Pogacar, 2016), or goes against firmly-held convictions of the targeted population (Tannenbaum, Fox & Rogers, 2014).

Next, despite the evidence that the effects of information provision and defaulting are statistically independent, it does not automatically follow that they represent different underlying processes. For instance, people can still make the same inference in both cases, e.g. to ascribe positive qualities to the default setter either because the pre-selection promotes a pro-social

behavior, or because the endorser is perceived as more sincere when information is present. Future research needs to conduct a more systematic investigation of the mechanisms behind the effects of mere defaulting and transparent default nudges.

One of the ways to do so would be to approach defaults from a different angle. Note that in our research, we adopted a communication perspective, which interprets defaulting as a form of social interaction between default setter and recipient. However, other explanations for the default effect can provide an alternative to interpreting the positive impact of transparency. A classic one is the status quo effect, an umbrella term for a number of decision making biases, which cause inertia in situations of choice (Samuelson & Zeckhauser, 1988). According to this perspective, people stick with defaults partially because of the transaction costs of opting out. These costs can be objective (having to fill in a lengthy opt-out form) or psychological (e.g. anticipated decision regret). In some of our experiments (Manuscripts 2 and 3), the transparency disclosure suggests that switching from the default means deciding not to help the experimenters. This would render opting out selfish, and choosing it could arguably elicit feelings of guilt (Lindsay, 2005). Anticipating such a negative affect would represent an additional cost to opting out, thus making the default option even more desirable. Future research can explore this possibility by manipulating the transparency information, for instance by varying its altruistic component between subjects, and measuring rates of anticipated guilt.

Another theoretical perspective also seems promising for future research. The Query Theory of Value Construction (Johnson, Häubl, & Keinan, 2007) regards choice preferences as the result of dynamically constructed internal questions, or queries. These queries are executed in series, one after the other, and their order and content influence people's decisions. Queries, which come to mind first, are weighted more heavily than the next, and their content usually reflects the

advantages of the status quo. Hence, when a certain choice option is defaulted, people are more likely to retrieve its positives first, and more often than its negatives (Dinner et al., 2011). This in turn skews preferences in favor of the default. Within the paradigm, the positive transparency effect could be explained by the salience of the proactive disclosure, and the content of the information in it. In our setup, the disclosure is presented simultaneously with the choice options, and it largely consists of relevant information about the pre-selection. Arguably, this should make the transparency information salient enough to be reflected by the queries, which are on top of the consideration order. In this way, the transparency information can weigh choice even more in favor of the default. One can test the assumption by subjecting participants to a query listing procedure, and measure differences in the order and content of the decision queries between a transparency group and a conventional default control.

Conclusion

Despite several shortcomings, we present strong evidence in favor of transparent defaulting: an ethical and effective alternative to the conventional default nudge. While it is clear that the positive transparency effect requires further investigation, it is also evident that proactive disclosures can be the way forward when nudging with defaults. At the very minimum, our research shows that the policymakers should not shun from being transparent when influencing others. In the words of the famous English politician Sir Edwin Sandys: "Honesty is (still) the best policy" (1599).

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Appendix I

Empirical publications in manuscript form. All final and definitive versions on record are available online or in paper form from the respective publishers and are fully referenced before each manuscript.

Manuscript 1:

Paunov, Y., Wänke, M., & Vogel, T. (2019a). Transparency effects on policy compliance: disclosing how defaults work can enhance their effectiveness. *Behavioural Public Policy*, *3*(2), 187-208. https://doi.org/10.1017/bpp.2018.40

Abstract

From an ethical standpoint, transparency is an essential requirement in public policy making. Ideally policy makers are transparent and actively disclose the presence, purpose and means of a decision aid. From a practical view, however, transparency has been discussed as reducing the effectiveness of decision aids. In the present paper, we elaborate on how transparency affects the effectiveness of defaults. In three experiments, we manipulated whether the endorser was transparent about the default or not, and assessed participants' decisions to opt-out or comply. Throughout experiments, we find that proactive transparency reduced opt-out rates as compared to a non-transparent default condition. Moreover, proactive disclosure of a default reduced opt-out rates as compared to informed control groups, where participants imagine they retrieve the default-related information by themselves (Experiments 1 & 2). The results further indicate that the lack of proactive disclosure may lead the targets to perceive the endorser as less sincere and to feel deceived, which in turn hinders the effectiveness of the default. In general, our findings lend support to the proactive transparency paradigm in governance, and show that a default-based policy can be transparent and effective at the same time.

Rooted in the popular Nudging paradigm, defaulting quickly became a hot discussion topic in both the academic and public domains. Default interventions are characterized by a decision situation, in which one of the choice options is pre-selected, but the decision maker retains the possibility to actively choose another alternative, i.e. to opt out. In general, people tend to stick to the preselected option, making defaulting an effective strategy for influencing choice. Defaults are particularly effective in cases, when the decision makers perceive the default option as the recommended one (McKenzie, Liersch, and Finkelstein, 2006) or when they postpone the decision for a later time (for a comprehensive review, see Willis, 2012).

Default effects are powerful, and default interventions have been successfully applied by various private and governmental institutions as means of increasing policy compliance. Famous examples include the organ donation policy default by Johnson and Goldstein (2003), the automatic enrollment "Save More Tomorrow" program by Thaler and Benartzi (2004), and Allcott and Mullainathan's energy saving defaults (2010). The success of defaults in policy making seems so prominent, that their implementation has even been endorsed in an executive order by US president Barack Obama (2015, Sep. 15), in which he encourages "giving particular consideration to the selection and setting of default options" (Sec.1, (b), iii).

The use of defaults however, has also raised concerns about their transparency. On one hand, one might conceive of default implementation as crossing the line between choice enhancement and manipulation. The target of a default intervention is unaware of the influence intent and the means by which the behavioral change is pursued (Hansen and Jespersen, 2013). Therefore, some have argued that decisions made under such non-transparent conditions are not fully autonomous, and are thus limiting people's capability of exercising informed choice (Smith,

Goldstein and Johnson, 2012). In response, an increase in transparency was recommended (Sunstein, 2015).

On the other hand, researchers have also contemplated a possible tradeoff between the degree of transparency of such interventions, and their effectiveness. Luc Bovens (2009) also considered decisions influenced by nudges as non-autonomous, and speculated that when people become aware of having been influenced in their choice, they would engage in behaviors that are inconsistent with their initial decision. Therefore, he made the theoretical argument that non-transparent nudges, such as defaults, "work better in the dark" and become increasingly ineffective as transparency is introduced. Hence, one can argue that making default interventions transparent may be detrimental to their effectiveness.

It goes without saying that both ethicality and effectiveness are of crucial importance in policy making. But are the two really in contradiction to each other? According to prominent psychological theorizing, people strive for self-determination (Deci, 1975; Ryan and Deci, 2000) and resent limitations to their freedom of choice (Brehm, 1966). In order to reinstall that freedom, they would deliberately resist choosing the default option and rather endorse options that are not defaulted. Thus, disclosing the influence attempt may easily decrease its impact.

However, predictions in the opposite direction are also viable. As McKenzie, Liersch, and Finkelstein (2006) demonstrate, defaults are perceived as a form of implicit recommendation. The pre-selection of an option communicates that the policy maker actually endorses that option. A transparent communicator – one who voluntarily discloses the default – further emphasizes that she considers a particular option to be the best one. Doing so, the communicator transforms the implicit recommendation into an explicit one, and can thus give the default a further boost: explicit

expert advice is known to have a profound effect on informing people's behaviour (e.g. Kinney et al., 1998).

Moreover, a full endorser disclosure might communicate that the endorser does not want to trick the target populace into a certain behaviour (i.e. to choose the pre-selected option), but to help people make an informed choice instead. Such disclosure might further account for a positive effect on policy compliance: targets may form the impression that the endorser is honest and trustworthy. Previous research from political science shows that transparency is positively associated with trustworthiness, and also affects policy satisfaction (Park and Blenkinsopp, 2011) and citizen compliance (Tyler, 1990, 1998). Research from social psychology shows that a disclosed intent can be perceived as working against a communicator's interest, which further boosts perceptions of the communicator's credibility and makes her appeals more persuasive (Walster, Aronson, and Abrahams, 1966). Put together, there is sufficient theoretical argumentation to doubt a negative influence of disclosure and to even assume that a voluntary disclosure by the endorser might actually boost rather than undermine the effect of defaults.

Given that default interventions gained much prominence in policy settings (e.g. Johnson & Goldstein, 2003; Thaler & Benartzi, 2014), the empirical evidence on the topic is surprisingly limited. The few existing experimental studies do not support either perspective, but produced mainly nul-effects (Bruns et al., 2018; Loewenstein et al., 2015, Steffel, Williams, & Pogacar, 2016). For instance, Loewenstein and colleagues (2015) asked participants to make a hypothetical end-of-life decision (a choice between prolonging life at all cost or a set of comforting measures, ensuring a smooth end-of life passage). One or the other alternative was defaulted between subjects. Orthogonal to the variation of the default, the researchers varied if participants were informed of the default before or after making the decision. To isolate default and transparency effects, all

participants were asked to make the same decision again, this time imagining the default had not been previously present. The results indicated no significant effect of transparency on the participants' choice. As the authors state, however, the finding may possibly reflect carry-over effects, stemming from participants' desire for decision consistency (Falk and Zimmerman, 2013). Yet, in a set of studies Steffel, Williams, and Pogacar (2016) did not find significant effects of transparency in several defaults either. Importantly for the present analysis, one of their studies actually resembled an institutional policy setting (Study 1a). The participants had to imagine they were about to join a fictitious social network, which defaulted them to either share their personal details, or not. In some conditions participants received information about the default's purpose and behavioral means. Notably however, the information was provided in such a way, that it was not clear whether the research participants attributed its disclosure to the policy endorser (i.e. the social network), thus representing proactive transparency, or to the experimenters.

With opposing theoretical predictions and scarce empirical evidence, we believe that the effect of transparency on default effectiveness is worth exploring. More specifically, research in a policy setting can benefit from a direct comparison between an uninformed control and an explicit endorser disclosure condition. For the purposes of the present research, we define default transparency as an objective policy characteristic, stemming from a full endorser disclosure of the default's presence, purpose, and behavioral means.

Lastly, we believe that prior designs leave the role of non-disclosure partially unexplored. In reality, lack of disclosure does not mean that the decision makers remain oblivious of the intervention's presence or purpose. When information is not readily available, people remain capable of retrieving it and drawing an inference to reach a decision. Therefore, we first test the effects of transparency and non-disclosure in a hypothetical scenario that also includes a condition,

in which the participants imagine they retrieve the default-related information by themselves. Finally, a third study tests the effect of transparency versus non-disclosure on compliance for actual choices.

Study 1

The first study compared the default option opt-out rate following full endorser disclosure with an uninformed control group. A second control condition was run, in which the participants received the same information as in the transparent condition, but imagined they had retrieved it by themselves (awareness condition). This setting enabled us to control for the amount of presented information, keeping it equivalent to the transparent condition.

Given people's tendency to strive for self-determination (Deci and Ryan, 2000), one would predict higher opt-out rates in both the transparent condition and the awareness condition, compared to the uninformed control group. In fact, defaults may be viewed as particularly paternalistic if endorsers reveal their intent.

Alternatively, a communication perspective holds that transparency evokes inferences about the endorser as fair and trustworthy. Such conception would predict lower opt-out rates in the transparent condition, compared to the two non-disclosure conditions. Both accounts point to the subjective feeling of trusting the endorser versus suspecting the endorser of manipulation. Therefore, we measured the extent to which participants perceived the endorser as trustworthy.

Method

Participants and design

The required sample size for a planned power of 80 per cent (two-tailed, $\alpha = .05$, $Pr(Y = 1 |X = 1)$ $H0 = .5$) was calculated with G*Power (Faul, Erdfelder, Buchner, and Lang, 2009) using odds ratios (ORs). With no previous data on the effect of explicit endorser disclosure, we assumed

a probability of opt out under transparency Pr(Y = 1 |X = 1) H0 = .33. This translates into an OR = .492, rendering the required sample size to 264 participants.

Two hundred and ninety-two English-speaking participants were recruited via an international online respondent service (210 male, 80 female, 2 unclassified; mean age 33.6 years, (*SD*=9.6)). Each participants was endowed with .30 USD for participation in the study.

All participants were given a choice scenario where a default was set. Participants were randomly assigned to the following three conditions: a transparent condition where the policy endorser disclosed setting the default in order to influence participants´ choice, a control group which received no further information (non-transparent condition), and a control group that received the same information about the default as in the transparent condition, but the participants imagined they retrieved said information by themselves (non-transparent aware condition).

Stimulus material

The participants were presented with a hypothetical scenario online. They were asked to imagine that they had recently enrolled in a university. Upon seeing the course program, they noticed that some of their electives had already been pre-selected, but they could change them by filling in a paper form and delivering it to the university administration. In the non-transparent condition, the participants received no further information. In the transparent condition, the university administration notified the participants of the pre-selection. In this condition the university also explained how defaults are a means to influence people to make a particular choice, and that this was the reason why the university preselected the electives it wanted the students to choose. In the non-transparent aware condition, the participants were asked to imagine having recently read an article that described how defaults are a means to influence people to make a particular choice.

Based on this article they would infer that the university wanted them to choose the preselected courses. The full text of the scenarios is available in Appendix A.

Measures

The main dependent variable was the proportion of people not choosing the default option (opt-out rate), coded 0 for staying with it, and 1 for opting out.

In order to measure the feeling of being able to trust the endorser versus feeling manipulated we used an adapted version of the Trust in government scale, adopted from Grimmelikhuijsen and Meijer (2012). The questionnaire explores trustworthiness as a multidimensional construct, and has three subscales measuring perceived endorser honesty, benevolence, and competence. The participants rated their agreement with 15 statements on a 5 point Likert scale, ranging from 1=”strongly disagree to 5= “strongly agree”. Sample items are: “The university administration is professional“ (competence), “The university administration is genuinely interested in the students’ well-being” (benevolence), “The university administration approaches students in a sincere way” (honesty). In order to ensure that the participants had read all relevant stimulus information, they were asked to briefly describe the scenario in an open format. Finally, the participants were debriefed and thanked in written form.

Results and discussion

Based on the responses to the control question, we excluded twenty-nine participants for not complying with the instruction (responded with “did not read”, “don’t know”, etc.), five for copy-pasting random instruction text, and twelve for responding incomprehensively in a language, different than English.

Table 1 shows that the full disclosure by the endorser reduces the opt-out rate in comparison to the two non-transparent conditions. While roughly every second participant chose to opt out in the non-transparent conditions, the opt-out rate dropped to 37.6% when the endorser was transparent about the default.

Table 1

Proportion of participants deciding to opt out per condition in Study 1

Condition	transparent	non-transparent	non-transparent aware
Optout	37.6%(32/85)	51.2%(41/80)	53.0% (43/81)

Note: Participants opt-out to stay with default option ratio in parentheses.

To check whether the reduction is significant, we ran a number of binomial logistic regressions. The binary decision (stay = 0; opt out = 1) served as our criterion. This criterion was predicted from two Helmert-contrasts. The first Helmert contrast accounted for differences between the non-transparent control conditions (non-transparent = non-transparent aware = -.33) and the transparent condition (transparent = .67), thus indicating how transparency affects opt out rates. The second Helmert-contrast accounted for differences between the two non-transparent groups (transparent = 0, non-transparent = -,50, non-transparent aware = .50). It indicates whether mere awareness leads to more or less opt out decisions as compared to non-transparent control group.

The analysis yielded the following effects. The first contrast was significant, b = -.726 (SE = .285), Wald-χ2(1) = 6.487, p = .011. Thus, the participants in the transparent condition were significantly less likely to opt out than those in the non-transparent conditions, in support of the prediction of positive effect of transparency on policy compliance.

Second, awareness of the default strategy alone did not seem to increase or decrease opt-out rates in comparison to the non-transparent control group, as is evident from the non-significance of the second Helmert-contrast, b = -.089 (SE = .338), Wald-χ2(1) = .070, p = .792. A summary of the regression coefficients summary is presented in Table 2.

Table 2

Summary of Logistic regression Analysis for the effects of transparency and awareness on opt-out rates

Predictors	B	SE	Wald	df	Exp(B)
Transparency	-0.726*	0.285	6.487	1	0.484
Awareness	-0.089	0.338	0.070	1	0.915
Constant	0,024	0.136	0.032	1	1.025

Note: The predictors are Helmert contrast-coded. Transparency compares participants from the transparent condition versus participants from the other two conditions. Awareness compares participants from the non-transparent condition against those from the non-transparent aware condition.

*p<.05

As a next step, we explored the role of trustworthiness. When we regressed trustworthiness scores on the same two contrasts, the coefficient for transparency (vs. the other conditions) was not significant, b = .117 (SE = .079), p = .138. Accordingly, we did not find any evidence for an indirect effect of disclosure on opt out decisions via trustworthiness, b = -.662, SE = .057, 95% CI[-.233, .010] (5000 replicates), although trustworthiness was an independent predictor of opt-out rates, b = -.378 (SE = .161), Wald-χ2(1) = 5.75, p = .016, indicating that the higher the perceived trustworthiness of the endorser, the lower the participant opt-out rates were across conditions.

In summary, the results confirmed the prediction that institutions can increase policy compliance by making default interventions transparent. Notably, the effect appears only when the endorser explicitly discloses the default's presence, purpose and behavioral means. Participants

who knew of the possible effect of defaults, but had not been directly informed by the endorser, showed no increase or decrease in compliance.

Despite the clear pattern regarding the impact of transparency on policy compliance, we did not find support for the explanatory role of trustworthiness. Though the effect of our manipulation on trustworthiness pointed in the expected direction, this effect did not reach conventional levels of significance. Before rejecting the explanation, however, it appears worthwhile scrutinizing the scale properties with regard to both reliability and construct validity. Indeed, a closer look at the trustworthiness scale showed that the item structure did not replicate the original one, rendering the aggregation into subscales as doubtful. Looking at the individual item level, it became apparent that the participants in the non-transparent conditions agreed less that the university administration approached them in a sincere way (M = 3.55, SD = .82), than those in the transparent condition (M = 3.80, SD=.75); t = -2.28, p = .023. This suggests that without explicit disclosure defaults might indeed be interpreted as an act of insincerity. In turn, this could elicit a feeling of being deceived and account for the lower compliance rates amongst the participants in the non-transparent conditions. Insights from the field of communication seem to back up such an assumption. In general, people have specific expectations about the provision of information (Grice, 1989, McCornack, 1992). These include expectations regarding the amount of information that should be provided in a communicative attempt. When such expectations are not met, people can assume that the communicator withholds information, which can be interpreted as deception (McCornack et al., 1992). When people think they are deceived, a range of negative reactions, from disappointment to outrage, are typical (Gordon and Miller, 2000).

Attending to these considerations, we ran a replication study, in which we replaced the trustworthiness scale with a specific measure of the subjective feeling of being deceived. Thus, we aim to capture its role as a potential mediator of the transparency effect.

Study 2

Study 2 aimed at replicating the findings from Study 1. The design and materials were to the same as those of Study 1, except for small changes on measurement level. Specifically, the more general trustworthiness measure was replaced with an explicit measure of feeling deceived. In order to improve the external validity of our results, we conducted the experiment with a German speaking sample.

Method

Participants

We extrapolated the required number of participants based on the effect size from the first study. An observed odds ratio of 0.484 and a probability of opt out under transparency of 0.376 rendered a required sample of 294 participants.

Two hundred and eighty-five participants (225 females, 58 males, 2 unclassified) were recruited online via a popular social network. The mean age of the participants was 24.9 years (SD = 6.03). After participation, respondents indicated whether they want to be entered in a raffle for two online shopping vouchers in total value of 30€ (EUR). After the data collection period expired, two participants were selected at random to receive the vouchers.

Measures

The presented scenarios and disclosure information were identical to those in Study 1. After the decision to change the electives or stay with the pre-selected ones, the participants were presented with a self-constructed scale intended to measure the subjective feeling of being deceived. It consisted of three positively phrased items, (e.g "Thinking of my interaction with the university administration at Albington, I believe they approached me in a sincere way"), and four negatively phrased items (e.g. "Thinking of my interaction with the university administration at Albington, I believe they tried to trick me"). For all items see Appendix C. Participants indicated their agreement with the statements on a 7-point rating scale (1 = "not at all"; 7 = "most definitely"). The resulting scale had a very high internal consistency (Cronbach's $\alpha = .89$).

Finally, we assessed whether the participants had read and understood the stimulus materials. For an objective and reliable measure of stimulus comprehension, we used a multiple choice test instead of an open text format. For a complete list of questions per condition, see Appendix B. Correct responses on all questions were required to include a participant in the analysis.

Results and discussion

Forty-eight participants were excluded for providing wrong answers to one or more items of the stimulus material attention check. As can be seen in Table 3, the resulting pattern replicated the findings of Study 1. Again, disclosure decreased opt out rates, whereas the two non-disclosure conditions did not differ from each other.

Table 3

Proportion of participants deciding to opt out per condition in Study 2

Condition	Transparent	non-transparent	non-transparent aware
Optout	65%(54/83)	80%(60/75)	75.9% (60/79)

Note: Participants opt-out to stay with default option ratio in parentheses.

The significance of the pattern was tested by means of a binomial logistic regression. The binary decision (stay = 0; opt out = 1) was predicted from the same two Helmert contrasts, used in our previous study. The first contrast accounted for differences between the non-transparent groups (non-transparent and non-transparent aware = -.33) and the transparency group (transparent = .67), indicating how transparency affects opt-out rates. The second Helmert-contrast (adjusted for group size) accounted for differences between the two non-transparent groups (non-transparent = -.55, transparent = 0, non-transparent aware = .45). This coefficient indicated whether default awareness leads to more or less opt-out decisions as compared to the non-transparent control group.

In line with the results from our first experiment, the participants in the transparency condition were significantly less likely to opt out, b = -.634 (SE = .301), Wald-χ2(1) = 4.43, p = .035, than those in the non-transparent conditions. As expected, the second planned contrast (non-transparent versus non-transparent aware condition) revealed no significant differences between groups, b = -.236 (SE = .391), Wald-χ2(1) = .366, p = .545. Logistic regression coefficients summary is presented in Table 4.

Table 4

Summary of Logistic regression Analysis for the effects of transparency and awareness on opt-out rates in Study 2

Predictor	*B*	*S.E.*	*Wald*	*df*	*Exp(B)*
Transparency	-,634*	,301	4,43	1	,530
Awareness	-,236	,391	,366	1	,789
Constant	1,308	,150	47,7	1	2,823

Note: The predictors are weighted Helmert contrasts. Transparency compares participants from the transparent condition versus participants from the other two conditions. Awareness compares participants from the non-transparent condition against those from the non-transparent aware condition.
*p< .05.

As demonstrated in Table 5, the participants' scores of feeling deceived were lowest in the transparent condition, whereas they were almost identical in the non-transparent conditions.

Table 5

Mean scores of feeling deceived per condition in Study 2

Condition	transparent	non-transparent	non-transparent aware
Feeling deceived	3.01 (-1.23)	3.35 (-1.2)	3.44 (-1.13)

Note: Standard deviations in parentheses.

To check the significance of the pattern, we regressed the participants' deception scores on the two Helmert contrasts. As expected, the participants in the transparent condition felt significantly less deceived and manipulated than those in the non-transparent groups, b = -.332, SE = .138, p = .017, 95%CI [-.604; -.061]. Participants who imagined to have discerned all default information by themselves did not feel more or less manipulated than those in the other non-transparent condition, b = .077, SE = .163, p = .63, 95% CI [-.244; .398].

Next, we tested whether the subjective feeling of being deceived mediates the effect of transparency on policy compliance. For that purpose, we used four binomial regression models and the SPSS PROCESS macro (Hayes, 2013). The first model indicated that transparency was significantly negatively related to opt-out, b = -.634, SE =.312, p = .032, 95% CI [-1.261; -.0421]. The second demonstrated that transparency led to a decrease of the subjective feeling of being deceived, b = -.332, SE =.137, p = .017, 95% CI [-.604; -.061]. In turn, feeling deceived accounted for an increase in opt-out rates, b = .464, SE =. 166, p = .005, 95% CI [.138; .791]. Lastly, when the subjected feeling of being deceived and transparency were pooled together in a regression model, the relationship between transparency and opt-out rates was no longer significant, b = -.497, SE = .310, p = .108, 95% CI [-1.106; .111]. Bootstrap confidence intervals derived from 5000 replicates also demonstrated that the indirect effect coefficient was significant, b = -.154, SE = .082, 95% CI [-.362; -.033], which supported the hypothesis that the relationship between transparency and opt-out rates is mediated by the subjective feeling of being deceived.

As previously, the results show that institutions can increase policy compliance by making default manipulations transparent. Importantly, disclosure by an endorser safe-guards against people becoming suspicious or feeling manipulated, which in turn makes them more likely to comply with the default.

Despite the clear evidence for a beneficial effect of transparency on default compliance, limitations may arise from the fact that the choices in our setup were hypothetical. Although using scenarios is a common approach to assessing transparency effects (Loewenstein et al., 2015, Steffel, Williams, and Pogacar, 2016), it cannot be guaranteed that effects for hypothetical choices would be observed for actual choices, as well. In fact, recent research in a different hypothetical setting showed that transparency may lead to more positive attitudes towards the default-setter, but

these effects do not necessarily transfer to default compliance (Steffel, Williams and Pogacar, 2016). In order to test whether transparency effects on compliance rates are evident in actual choice behavior, we conducted a third experimental study.

Study 3

As in the previous studies, the moderating role of transparency on default compliance was tested in a university decision context, yet with actual choices. Participants chose between several studies in which they could volunteer to participate. The studies were described in terms of their different durations. Analogously to the previous studies, we realized a transparent and a non-transparent default condition, in both of which the middle study duration (8 to10 minutes) was pre-selected. Different from the previous studies, we also ran a free choice (non-default) condition in order to isolate a default effect and possible participant preferences for the middle study duration.

Participants & Design

Participants

A convenience sample of one hundred and seventy-nine participants (131 females, 47 males, 1 unclassified) was obtained via a popular social network. The mean age of the participants was 25.8 years (SD=7.03). They were randomly assigned to one of the three between-participant conditions (transparent default vs. non-transparent default vs. free choice). A subsequent sensitivity analysis revealed that given $N = 179$, $1\text{-}ß = 0.8$, $\alpha = .05$, and an observed $Pr(Y = 1 |X = 1) H0 = .46$ our study was sufficiently sensitive to detect a minimum effect size (and odds ratio) of 2.132.

After participation, respondents indicated whether they wanted to be entered in a raffle for two online shopping vouchers in total value of 20€ (EUR). After the data collection period expired, two participants were selected at random to receive the vouchers.

Materials & Procedure

On a social media platform an invitation to participate in psychological research was posted and the respective link was provided. Participants who clicked on the link were informed that there were several studies from which they could choose, and that the studies differed in content and duration. Only the duration was provided for each study without further content description. Participants were also informed that the reward for their participation was independent of the duration of the study they chose to complete. The following five choice options were listed: "< 5 minutes", "5 to 8 minutes"; "8 to 10 minutes"; "10 to 12 minutes"; and "more than 12 minutes". In both default conditions, the middle "8 to 10 minutes" study duration category was pre-selected. In the transparent condition, the pre-selection was accompanied by a notification explaining the purpose and the behavioral means of the pre-selection (the full text of the stimulus material per condition is available in Appendix D). In a free choice condition none of the options was pre-selected. In order to preserve the participants' time resources, no actual study was administered. Upon indicating their choice, all respondents were redirected to a page, where they were thoroughly debriefed about the procedure and purpose of the experiment, and could participate in the prize raffle.

Results and Discussion

Descriptive results are reported in Table 6. In the transparent condition, 46.6% of the participants chose to stay with the default (vs. 53.4% who opted out), while in the non-transparent default condition only 27.1%% chose this option, but 72.9% opted out. For a comparison standard, the crucial option was selected by only 11.3% of the participants in the free choice condition.

Table 6

Percentages of decisions to stay versus opt out from designated option per condition in Study 3

Condition	transparent default	non-transparent default	free choice
Stay	46,6%(27/58)	27,1%(16/59)	11,3% (7/62)
Opt-out	54,3% (41/58)	72,9% (43/59)	88,7% (55/62)

Note: Proportion of participants who chose the option pre-selected in the default conditions upper row) vs. proportion of participants who chose an alternative option (lower row).

For a test of significance, choices (coded 0 = stay/middle option; 1 = opt out/other option) were predicted from two Helmert contrasts in a binomial logistic regression. The contrasts indicated whether there was a default or not (transparent default and non-transparent default = .33; free choice = -.67) and whether the default was transparent or not (transparent default = .50, free choice = 0, non-transparent default = -.50). Coefficient summary is available in Table 7.

Table 7

Summary of logistic regression analysis for the effects of default and transparency on choice in Study 3

Predictor	*B*	*S.E.*	*Wald*	*df*	*Exp(B)*
Default	1.498**	0.446	11.231	1	4.472
Transparency	0.85*	0.393	4.664	1	2.34
Constant	-1.058	0.186	32.022	1	0.347

Note: The predictors are Helmert contrasts. Default compares participants from the default conditions versus participants from the free choice condition. Transparency compares participants from the non-transparent default condition against those from the transparent default condition.

**p<.01

*p< .05

The first contrast was significant, b = 1.498 (SE = 0.446), Wald-$\chi 2(1)$ = 11.23, p = .001, showing that defaults systematically increase the choice of the pre-selected option over a free choice format. Pertinent to our research question, transparency significantly increased the

proportion of participants choosing the defaulted option as compared to the non-transparent default condition: b = 0,850 (SE = 0.393), Wald-$\chi 2(1)$ = 32.02, p = .031. Thus, even in case of actual choices, making a default transparent increased its efficiency. It should be noted that the respective option was not highly favored a priori. The default setting more than doubled choices for this option, and making the defaulting strategy transparent quadrupled it.

General Discussion

Across three experiments, we demonstrated that that the full disclosure of a default's presence, purpose and behavioral means increased policy compliance. Notably, this was the case for hypothetical as well as for actual choices. Such finding supports the call for transparency in nudging interventions (Sunstein, 2015), showing that a transparent default may still be effective.

Our data also suggest that to some extent, people may feel deceived when a default is presented, no matter whether they manage to retrieve all default-related information or not. A full disclosure by the endorser is a possible remedy against such inferences. In a broader sense, such finding lends support to the proactive disclosure perspective in governance (Darbishire, 2010), and shows that proactive transparency can be a beneficial tool for increasing policy compliance in cases, where the traditional (a.k.a. reactive) open access approaches fail.

While these findings advocate transparency as a tool to increase default efficacy, there are some limitations to their generalizability. First, transparency may not always yield the inference that the endorser is fair. For instance, we would expect that disclosure effects depend on whether the disclosure is perceived as being voluntarily made. If one perceives the disclosure to be given by obligation, for example due to a specific regulation which requires it, the introduction of transparency may lose its advantage. Second, transparency effects should depend on whether the

purpose is compatible with the receiver's interest. That is, receivers might be willing to comply with defaults that serve themselves or an institution they are willing to support. Disclosing that the default serves the default-setters vested self-interests at the cost of the receiver may yield smaller if not detrimental effects (cf. Steffel, Williams, and Pogacar, 2016). Likewise, transparency may not increase default compliance if it discloses inacceptable costs. Bruns et al. (2018) studied default effects on donation behavior and did not find an advantage of transparency. Different from the present study, however, defaults in Bruns et al. (2018) were set to a very costly option (i.e. donate 80% of the money earned). Arguably, transparency affects the willingness to comply, but only within the personal latitude of acceptance. Taken together, these considerations might imply that transparency is most beneficial for defaults nudging people to the better – which is a basic requirement in the Nudging paradigm.

However, this does not exclude the possibility of a detrimental effect of endorser disclosure on the effectiveness of non-transparent nudges in general (cf. Bovens, 2009). Behavioral interventions which directly influence the attributes of our choice alternatives (e.g., decreasing portions or plate sizes in cafeterias) are much more difficult to detect, and can thus be perceived as more intrusive when disclosed. Perhaps such interventions would still work best in the dark, but this stands to be seen.

Conclusion

In summary, the present research adds to the available literature on default effects, and demonstrates that default-based policies can be both ethical and effective at the same time. Doing so, it contributes to the ongoing ethicality-effectiveness debate, showing that at least in some situations, one can have best of both worlds: an ethical and effective behavioral intervention.

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Appendix A

Stimulus Material Studies 1 and 2 per condition.

Non-transparent condition:

Please read the text below carefully and answer the question honestly. There is no right or wrong answer, just pick the answer you want. Once you are ready, please click the "Next" button.

Shortly after finishing your bachelor studies, you find a suitable master's program at another university. Not before long, you get accepted and move to campus. As soon as you get your online university credentials, you browse through the courses, included in the program. You notice that besides being enrolled in the mandatory courses, you have also been registered for a few elective ones.

The electives you are registered for vary in content and length.

Underneath the elective course descriptions you see the following information:

Please note, that you can change the pre-selected electives to other alternatives. In order to do so, you can visit the administration department and file a change form (available in hard copy only).

What do you do next?

- Stay with the pre-selected courses
- Change the pre-selected courses to the other alternatives

Transparent condition:

Please read the text below carefully and answer the question honestly. There is no right or wrong answer, just pick the answer you want. Once you are ready, please click the "Next" button.

Shortly after finishing your bachelor studies, you find a suitable master's program at another university. Not before long, you get accepted and move to campus. As soon as you get your online university credentials, you browse through the courses, included in the program. You notice that besides being enrolled in the mandatory courses, you have also been registered for a few elective ones.

The electives you are registered for vary in content and length.

Underneath the elective course descriptions you see the following information:

> Please note, that you can change the pre-selected electives to other alternatives. In order to do so, you can visit the administration department and file a change form (available in hard copy only).
>
> 
>
>
> Please be advised, that our enrollment method is based on the scientific findings of Johnson and Goldstein (2013). The authors show that when people face a decision, they would often stay with the option, which is pre-selected. Therefore, we have pre-selected the elective courses for you, since we would like to direct you towards choosing them.

What do you do next?

- ○ Stay with the pre-selected courses
- ○ Change the pre-selected courses to the other alternatives

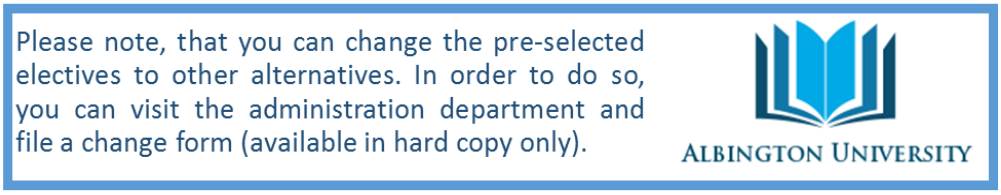
Non-transparent aware condition:

Please read the text below carefully and answer the question honestly. There is no right or wrong answer, just pick the answer you want. Once you are ready, please click the "Next" button.

Shortly after finishing your bachelor studies, you find a suitable master's program at another university. Not before long, you get accepted and move to campus. As soon as you get your online university credentials, you browse through the courses, included in the program. You notice that besides being enrolled in the mandatory courses, you have also been registered for a few elective ones.

The electives you are registered for vary in content and length.

Underneath the elective course descriptions you see the following information:



You remember that you recently stumbled over a scientific article by Johnson and Goldstein (2013). The authors showed that when people face a decision, they would often stay with the option which is pre-selected. Thus, you infer that the university administration has pre-selected the elective courses for you, since they want to direct you towards choosing them.

What do you do next?

- Change the pre-selected courses to the other alternatives
- Stay with the pre-selected courses

Appendix B

Control questions study 2 (per condition).

Non-transparent condition

Please, answer the following questions about the situation you were confronted with in the beginning:

In the presented situation, we asked you to imagine, that...

-you were a teacher in a foreign university

-you recently paid a high tuition fee in a university

-you enrolled in a master's program

When you had a look at the courses you needed to follow...

-you had to choose all your elective courses by yourself

-some of the elective courses were already chosen for you

-some of the elective courses were past deadline for enrollment

A university disclaimer underneath the course descriptions stated that...

-you can change the preselected electives after you hand in a change form in person

-you cannot change the preselected electives

-you can change the preselected electives online

Non-transparent aware condition: the three questions from the non-transparent condition plus*:*

After you read about the enrollment procedure at Albington, you remembered about an article, which explains

-that when people face a decision, they will often stay with the option, which is preselected for them

-that when people face a decision, they will often select the option, which is best for them

-that when people face a decision, they will often postpone their choice for a later time

Based on the information in the article you remembered reading, you inferred that

-the university administration preselected the elective courses for you, since they wanted you to make an active choice

-the university administration preselected the elective courses for you by mistake

-the university administration preselected the elective courses for you, since they wanted you to stick with these courses

Transparent condition: the three questions from the non-transparent condition plus*:*

The university administration at Albington posted a disclaimer, which contained information about their enrollment policy. What was their enrollment policy based on?

-on the fact, that when people face a decision, they will often stay with the option, which is preselected for them

-on the fact, that when people face a decision, they will often select the best option for them

-on the fact, that when people face a decision, they will often postpone their decision for a later time

Appendix C

Subjective feeling of being deceived measure (Chronbach's α= .89)

Participants indicated their agreement with seven statements on a seven-point Likert scale, anchored from 1(not at all) to 7 (most definitely). Three of the items were positively worded and their scores consequently reversed.

Thinking of my interaction with the university administration at Albington, I believe they…

Were open with me…….not at all (1) most definitely (7)

Tried to trick me…… not at all (1) most definitely (7)

Approached me in a sincere way……not at all (1) most definitely (7)

Made an attempt to swindle me…… ….not at all (1) most definitely (7)

Were trying to mislead me……..not at all (1) most definitely (7)

Were honest with me……. not at all (1) most definitely (7)

Appendix D

Stimulus material Study 3 per condition.

Transparent default condition

Welcome to the experimental database of the Department of Economic and Consumer Psychology

We have a large selection of studies that you can work on. The studies have different content and duration.

Please select a category of studies from the options below. The categories are ordered according to the duration of the studies they contain. Once you have selected a category, you will be randomly assigned a study of the respective duration.

Please note the following: based on the results of Johnson und Goldstein (2003) we know that in decision situations, people often stick with a choice option, which is preselected for them. Therefore, we have preselected a category for you, since we would want you to choose a study from this category.

Once you have completed the study, you can enter a raffle to win two 10 € Amazon vouchers. Please note that you will only receive one entry, no matter if you have completed a long or a short study.

- ○ **Category A** (< 5 min)
- ○ **Category B** (5 – 8 min)
- ◉ **Category C** (8 – 10 min)
- ○ **Category D** (10 – 12 min)
- ○ **Category E** (> 12 min)

Default condition

Welcome to the experimental database of the Department of Economic and Consumer Psychology

We have a large selection of studies that you can work on. The studies have different content and duration.

Please select a category of studies from the options below. The categories are ordered according to the duration of the studies they contain. Once you have selected a category, you will be randomly assigned a study of the respective duration.

Once you have completed the study, you can enter a raffle to win two 10 € Amazon vouchers. Please note that you will only receive one entry, no matter if you have completed a long or a short study.

- ○ **Category A** (< 5 min)
- ○ **Category B** (5 – 8 min)
- ◉ **Category C** (8 – 10 min)
- ○ **Category D** (10 – 12 min)
- ○ **Category E** (> 12 min)

Free choice condition

Welcome to the experimental database of the Department of Economic and Consumer Psychology

We have a large selection of studies that you can work on. The studies have different content and duration.

Please select a category of studies from the options below. The categories are ordered according to the duration of the studies they contain. Once you have selected a category, you will be randomly assigned a study of the respective duration.

Once you have completed the study, you can enter a raffle to win two 10 € Amazon vouchers. Please note that you will only receive one entry, no matter if you have completed a long or a short study.

- **Category A** (< 5 min)
- **Category B** (5 – 8 min)
- **Category C** (8 – 10 min)
- **Category D** (10 – 12 min)
- **Category E** (> 12 min)

Manuscript 2:

Paunov, Y., Wänke, M., & Vogel, T. (2019b). Ethical defaults: which transparency components can increase the effectiveness of default nudges?. *Social Influence*, *14*(3-4), 104-116. https://doi.org/10.1080/15534510.2019.1675755

Abstract

Default options have been successfully utilized in influencing behavior across multiple domains. Recent empirical evidence advocated the induction of transparency to default interventions as an effective tool for increasing policy compliance (Paunov, Wänke & Vogel, 2018). However, the roles of the different transparency components in achieving the effect remain unexplored.

In an experimental study, we measured the effects of three different transparency disclosures on default effectiveness. The default's target behavior, the default's purpose, and the way defaults work were disclosed in separate conditions. Our results show that transparency significantly increases compliance to the default nudge. In addition, we provide an insight as to which transparency components are most effective in boosting the default effect.

Influencing others has long been a central theme in social psychology (for reviews see Cialdini & Goldstein, 2004; Pratkanis, 2007; van der Pligt & Vliek, 2016). Over time, many influence and persuasion theories have been proposed (Eagly & Chaiken, 1993; Crano & Prislin, 2008; Vogel & Wänke, 2016) and numerous influence techniques have been researched and applied (Cialdini, 2016; Goldstein, Martin & Cialdini, 2008). Recent years saw a new interest in the topic as well as a perspective shift when Thaler and Sunstein (2008) propagated "nudging" people by engineering the choice environment in a manner that presumably facilitates pro-social and self-beneficial behavior. While a precise operational definition of nudging is still lacking (Marteau, Ogilvie, Ronald, Suhrcke, & Kelly, 2011), nudgers, also known as choice architects, have successfully implemented nudge-based interventions in institutional and private policies across multiple domains.

One of the most effective means of nudging is the use of default options. Typically, the decision makers are presented with an array of choice options, one of which is pre-selected. However, they retain the possibility to actively choose another alternative, i.e. to opt-out from the default. Generally, people tend to stick to the preselected option, thus making defaults an effective strategy for influencing choice. Default-based interventions have been successful in promoting pro-social behavior in a wide range of settings, including organ donation decisions (Johnson & Goldstein, 2003), retirement savings (Thaler & Benartzi, 2004), and energy conservation (Allcott & Mullainathan, 2010). At first glance, defaults seem to capitalize on people's inertia, which makes them stick to the pre-selected option, but as elaborated later, defaults also involve a social component (McKenzie, Liersch, and Finkelstein, 2006).

The implementation of defaults, however, has also raised concerns about the degree to which default-based interventions restrict peoples' freedom of choice. As the target of a default

intervention is unaware of the influence attempt and the way it brings about the desired behavioral change (Hansen & Jespersen, 2013), some researchers have argued that defaults limit people's autonomy and their ability to exercise informed choice (Smith, Goldstein & Johnson, 2013). In line with this notion, Jung and Mellers (2016) demonstrated that defaults were viewed less favorably and were perceived as more autonomy threatening than other nudges. Thus, an ethical perspective calls for transparency in default interventions.

Yet, researchers have also expressed concerns that transparency might harm the effectiveness of default nudges. Meta-analytic evidence by Wood and Quinn (2003) indicates that being forewarned about an upcoming attitude influence appeal makes people bolster their attitudes as a form of a defensive response. Consequently, such defensive position may prompt retaliation against a given choice architecture, provided that the invigorated attitudes go against it. Therefore, Krijnen, Tannenbaum, and Fox (2017) speculated that once an influence attempt is disclosed, people can actively choose to oppose the promoted course of action. Bovens (2009) also speculated that once people became aware that there is an attempt to influence their choice, they can counteract it by engaging in behaviors that are inconsistent with the purpose of the intervention and/or their initial decision. Hence, he argued that non-transparent nudges, such as defaults, work "better in the dark" and should become largely ineffective as transparency is introduced. Although Bovens did not test his assumptions, reactance theory (Brehm, 1966) and people's strife for self-determination (Deci, 1975; Ryan & Deci, 2000) would make similar, even stronger predictions, linking resistance to the mere presence of an influence attempt.

On the other hand, social psychological perspectives challenge the considerations regarding ethicality and transparency. In most social interactions, a communicator's choice of message type conveys information about her/his attitudes towards a given choice option to the other party (Sher

& McKenzie, 2006). If one is to construe the default setting as a form of a social interaction between the default setter and the targeted population, then one can assume that setting the default in itself can communicate information about the default setters' preferences and intentions. In fact, McKenzie and colleagues (2006) demonstrated that when defaulted, the targeted individuals were able to recognise that a particular choice option is made easier to adopt, and that the default-setters wanted them to choose that option. Therefore, the authors concluded that the default setting *per se* was perceived as a form of an implicit recommendation, which was sufficient to trigger a desirable response. Moreover, insights from persuasion research (Persuasion Knowledge Model; Friestad & Wright, 1994; Kirmani & Campbell, 2009) show that people are not only capable of recognizing the influence agent's intent, but can also construe beliefs about her or his strategies and tactics, synthesizing those in a form of unique persuasion knowledge. In this sense, it is not entirely prudent to think of defaults as completely non-transparent and unethical. Even when no additional information is conveyed, people seem to recognize the default as an influence attempt and extrapolate the default's setter's attitudes and intentions, thus (at least partially) retaining their ability to make an informed choice.

Adopting such a perspective, one would predict that a disclosure of the default strategy may not harm the effectiveness of a default as Bovens (2009) predicted, but might even boost its impact. Since people are capable of recognizing defaults as implicit influence attempts, a transparent communicator – one who proactively discloses the default setting– can transform the implicit recommendation into an explicit one, and can thus give the default a further boost: Explicit recommendations are known to have a strong effect on informing people's behaviour (e.g. O'Keefe, 1997; Ansari, Essegaier, & Kohli, 2000; Kinney et al., 1998). Moreover, an explication may not only foster the addressee`s confidence in which behaviour is desired, but could also trigger

positive inferences about the communicator. Specifically, transparent disclosures can foster the perception that the communicator is fair (Steffel, Williams & Pogacar, 2016) and sincere (Paunov et al., 2018), which is an integral part of the communicator's credibility (Eisend, 2006). A plethora of findings from communication and persuasion research show that source credibility has a strong persuasive impact (e.g. Hovland & Weiss, 1951; Hovland, Janis, & Kelley, 1953; for a meta-analysis see Wilson and Sherrell, 1993). Therefore, one might even expect an increased rather than reduced compliance when the default setter is transparent about the influence attempt.

The empirical evidence on transparency is mixed, but so far the respective research has found no evidence of a negative impact on default effectiveness (Loewenstein et al., 2015; Steffel et al., 2016; Bruns et al., 2018). Instead, a recent experiment by Paunov, Wänke, and Vogel (2018) demonstrated that transparency can actually increase the effectiveness of several default nudges. Building on the theoretical insights of McKenzie et al. (2006) and Hansen and Jespersen (2013), Paunov and colleagues took an eclectic approach to conceptualizing transparency. They defined it as an objective intervention characteristic, whereby the endorser fully discloses the default's presence, its purpose, and its general effect. The authors' main reasoning was that by proactively installing transparency, the policy makers would communicate that they do not intend to trick people into the desired behavior, but to help them make an informed choice instead. Across three experimental studies, the endorser´s proactive disclosure of the default's presence, purpose, and general effect significantly increased compliance in comparison to free choice and a traditional default condition. However, while this transparency induction proved successful in eliciting the desired choice, the individual role of each transparency component remains unclear. Does one really need to disclose all three components, or is a single one sufficient to increase compliance? Is one component more effective than another in bringing the desired behavioral change? The

present research provides a more systematic test of different transparency disclosures, namely the what, the why and the how. More specifically, we vary whether people are explicitly informed (a) *what* the default is intended to achieve (disclosure of target behavior), (b) *why* the endorser wants people to choose the defaulted option (disclosure of purpose), and (c) *how* defaults affect behavior in general (disclosure of general effect).

In principle, each category has the potential to deliver a positive effect on its own. First, one can assume that clarifying the default target behavior may trigger an increase in compliance simply because it makes the respective behavior more obvious and salient. Generally, people react positively to the presence of exact behavioral information, and express more support for nudges, which provide it (Felsen et al., 2013).

Second, disclosing the reason why the default should be chosen can provide people with a valid justification for complying. Compliance and willingness to cooperate increase significantly when a request (Langer et al., 1978; Bohm & Hendricks, 2010) or an influence attempt (Becker, 1978) is accompanied by higher levels of justification. In addition, disclosing the reason behind the default can be especially beneficial, if it represents a strong argument in favor of complying: A number of findings from persuasion research reveal a positive link between the strength of the arguments, which constitute a given influence attempt, and its persuasiveness (Chaiken, 1980; Petty & Cacioppo, 1979). Therefore, disclosing the default's purpose can not only help people justify complying, but can also contribute to the persuasiveness of the disclosure, provided that it presents a strong argument in favor of the pre-selection.

Lastly, a proactive disclosure of the default's influence on people's decision-making may also have a positive effect on compliance via creating the perception that the endorser approaches

the targeted population in a sincere way (Steffel et al., 2016; Paunov, et al., 2018). Put together, either transparency component may be the sole cause of an increase in compliance.

However, there is also the possibility that in isolation, certain transparency components may fail to produce a positive effect or could even be detrimental to the default's effectiveness. Prominent psychological theorizing asserts that people strive for self-determination (Deci, 1975; Ryan & Deci, 2000) and resent limitations to their freedom of choice (Brehm, 1966). Presumably, an explicit disclosure of the default's general effect is most likely to trigger opposition. Informing people that they generally tend to stay with a defaulted option, once such is set, can render it especially salient that their choice is not entirely autonomous.

In sum, despite the recent evidence that an eclectic disclosure of all transparency components can increase the effectiveness of a default nudge (Paunov et al., 2018), little is known about the robustness of the effect and the exact impact of the separate transparency components. The present research aims to fill these gaps. First, we intend to replicate the positive effect of transparency on default compliance, introducing a more challenging social dilemma setting, where choosing the default option goes against the strict self-interest of the participants. Second, we aim to disentangle the effects of the different transparency constituents via setting up separate experimental conditions for the respective disclosures.

The present research

An online experiment was conducted, which explored the effects of transparency on default nudge compliance in the context of devoting time (personal cost) to promote participation in scientific research. Compliance rates across five experimental conditions were compared between participants. A free-choice condition obtained a baseline. In a second condition a conventional default was set. Three further conditions each realized one type of disclosure as described

previously: a target behavior disclosure condition ("what"), a disclosure of purpose condition ("why"), and a general effect disclosure condition ("how"). The effectiveness of the default intervention was isolated by comparing the free-choice condition against the four default conditions, where we predicted a positive effect of defaulting on willingness to participate in scientific research. The main effect of transparency was derived from contrasting the conventional default condition against the three transparent default conditions. In congruence with Paunov et al. (2018), we predicted a positive effect of transparency on default effectiveness.

As discussed previously, each type of disclosure has the potential to bring about the desired behavioral change either in isolation, or in a combination with other disclosures. Based on our theoretical reasoning, we assume that disclosing the defaults purpose, or providing information about the expected participant behavior will increase compliance with the default. Given the argument that disclosing the default's general effect can trigger resistance (Bovens, 2009; Brehm, 1966), we expect that such disclosure is less likely to produce a positive effect, and can even hinder the default's effectiveness. Lastly, in an explorative manner, we assessed the participants' scores on two variables: disclosure argument strength and perceived endorser deceptiveness. As theorized previously, differences between the strength of the arguments in the respective disclosures can provide an indication as to how justifiable compliance is, or how persuasive the disclosures are. Perceived endorser deceptiveness, on the other hand, has been previously associated with compliance in Paunov et al., (2018) experiments, and may help explain the main transparency effect on compliance.

Method

Participants and design

The required sample size for a planned power of 80 per cent (1-*ß* = 0.8, two-tailed, α= .05), was calculated with G*Power (Faul, Erdfelder, Buchner, and Lang, 2009) using odds ratios (ORs). The expected effect size was extrapolated based on the results of Paunov et al. (2018). An odds ratio of 2.132 and a probability of opting out under transparency Pr(Y = 1 |X = 1) H0 = .46 rendered a required sample of 228 participants. Conservatively, 311 English-speaking participants were recruited via an online respondent panel (198 females (64, 3%), 109 males (35,4%), 1 unspecified (0,3%), mean age 33.5 years, (*SD*=11,3)), and were randomly assigned to the five experimental conditions. Each participant was paid £ 0.45 for participating in the study.

Materials and procedure

Respondents who decided to take part in our research were informed that there were several studies they could choose from, and that the studies differed in content and duration. Only the duration was provided for each study without any content description. The following five choice options were listed: "< 3 minutes", "3 to 5 minutes"; "5 to 7 minutes"; "7 to 9 minutes"; and "more than 9 minutes". Across all five conditions and simultaneously with the presentation of the choice options, the participants had been informed that they would be paid for doing a 5 minute study, independent of the duration of the study they chose. Therefore, choosing the first option provided the opportunity to spend less than 5 minutes (thus maximizing the participant's profit), while the second allowed for spending a maximum of 5 minutes in order to break even without violating the contract. Because we wanted to default an option that was not attractive a priori, in all four default conditions, the "5 to 7 minutes" study duration category was pre-selected. A choice of this option meant that the participants accepted higher personal costs than necessary, for the sake of supporting

scientific research. In the three transparent default conditions, the pre-selection was accompanied by a disclosure, which clarified either the expected participant behavior (target behavior disclosure), the purpose of the default (default purpose disclosure), or the way in which defaults affect behavior (general effect disclosure). In the non-transparent default condition, the "5 to 7 minutes" option was pre-selected, unaccompanied by transparency information. In the free choice condition none of the choice options was pre-selected. The respective conditions and disclosures are presented in Table 1. The exact wording of our general instruction is provided in part 1 of the Supplementary Material (SM).

After indicating their choice, the participants filled in an exploratory post-questionnaire, which measured scores on two constructs: disclosure argument strength and perceived endorser deceptiveness. Disclosure argument strength was assessed across the transparent default conditions to test which disclosure(s) represented a stronger argument in favor of the pre-selection. The perceived endorser deceptiveness measure was administered across all conditions to probe for a possible explanation of the positive transparency effect. After that, the participants responded to several multiple-choice questions, designed to capture the extent to which they had read and understood the stimulus information. Upon providing some demographic information, the participants were redirected to an unrelated task, which took approximately 3 minutes to complete. Finally, all respondents were thoroughly debriefed about the purpose of the experiment, re-affirmed their agreement to submit data, and received a code to redeem their endowment.

Table 1. Overview of the experimental conditions and disclosures.

Condition	Disclosure
Default & target behavior disclosure	Please note the following: we would want you to choose Category C (5-7 min). Therefore, we have preselected this category.
Default & purpose disclosure	Please note the following: with choosing Category C (5-7 min), you guarantee that we will be able to accomplish our research objectives. Therefore, we have preselected this category.
Default & general effect disclosure	Please note the following: we know that in decision situations, people often stick with a choice option which is preselected for them. Therefore, we have preselected Category C (5-7 min).
Conventional default	none
Free choice	none

Measures

The main dependent variable was choice of the target option, coded 0 for not choosing it/opting out, and 1 for choosing it/staying with the default. After making their choice, the participants were presented with the exploratory post-choice questionnaire, where six items (α = .89) captured the degree to which the participants felt deceived by the endorser (e.g. "When I consider how the choice of categories was presented to me, I think that the experimenters tried to manipulate me."). In the transparency conditions, seven additional items (α =.91) measured how strong the arguments for setting up the default in the respective disclosures were (e.g. "The argument, which the experimenters made for pre-selecting a category for me, was compelling."). Agreement with all statements was indicated on a seven-point rating scale (1 ="not at all"; 7="most definitely"). For all items, see part 2 of the SM.

Lastly, we presented the participants with several multiple-choice questions designed to capture the extent, to which they had paid attention to the stimulus information. A full list of the respective questions per condition is available in part 3 of the SM. In accordance with the procedure

employed by Paunov et al., (2018), only participants who had answered all control questions correctly were included in the analysis.

Results and Discussion

Forty-six participants were excluded for providing wrong answers to one or more items of the stimulus attention check, leaving a sample of *N*=265 valid cases. Running all following analyses with all participants included yields the same conclusions. The main descriptive results are reported in Table 2. Across the transparent conditions, an average of 76.3 % of the participants chose to stay with the default (vs. 23.7% who opted out), while in the conventional default condition, only 49.1% chose this option, but 50.9% opted out. For comparison, the crucial option was selected by only 22.2 % of the respondents in the free-choice condition.

For a test of significance, choices (coded 1 = stay/choose target option; 0 = opt out/choose other option) were predicted from two Helmert contrasts in a binomial logistic regression. The contrasts indicated whether there was a default or not (free choice = – 0.50, transparent default conditions and conventional default condition = 0.125), and whether the default was transparent or not (transparent defaults = 0.167, free choice = 0, conventional default = – 0.50). A summary of the regression is available in Table 3.

Table 2
Proportion of participants choosing the target option per condition.

Condition	Default & target behavior disclosure	Default & purpose disclosure	Default & general effect disclosure	Conventional default	Free choice
Choice of target option	83.6% (46/55)	85.4% (41/48)	60.4% (32/53)	49.1% (27/55)	22.2% (12/54)

Note: Participants' choice of the target option versus choice of another option ratios are in parentheses.

Table 3
Summary of logistic regression analysis for the effects of default and transparency on choice of target option.

Predictor	B	S.E.	Wald	Df	Exp(B)
Default	3.391**	0.58	34.128	1	29,696
Transparency	1.806**	0.493	13.413	1	6.086
Constant	0.443	0.141	9.827	1	1.557

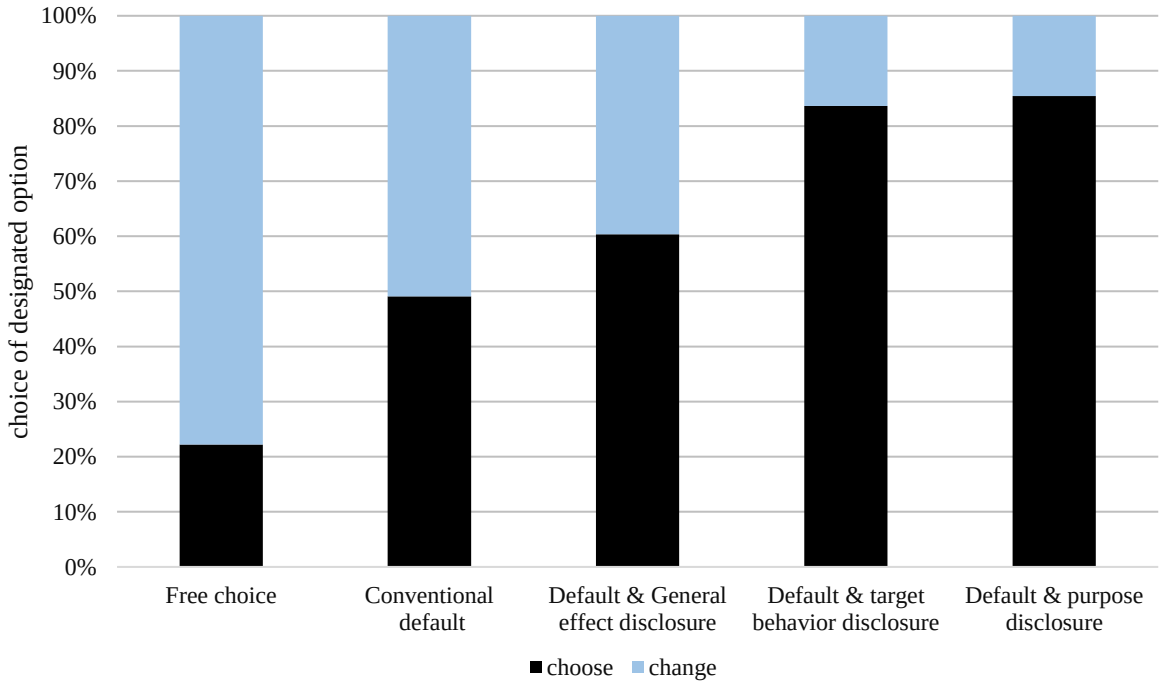
Note: The predictors are Helmert contrasts. Default compares participants from the free choice condition versus participants from the default conditions. Transparency compares participants from the transparent default conditions against the conventional default condition.
**p< .001.

The first contrast was significant, b = 3.39 (SE = 0.58), Wald-$\chi 2(1)$ =34.12, p < .001, showing that the default systematically increased choices of the target option over a free-choice format. Pertinent to our research question, transparency significantly increased the proportion of participants choosing the defaulted option as compared to the conventional default condition: b = 1.81 (SE = 0.49), Wald-$\chi 2(1)$ = 13.41, p < .001. Therefore, even in cases when the desired behavior comes with the possibility of a personal loss, making a default explicitly transparent increased its effectiveness. Notably, the respective choice option was not favored *a priori,* as evident from the free choice condition. The default setting doubled the choices for that option, and making the default explicitly transparent more than tripled it[3].

[3] In addition to the analysis on binary compliance decisions, we also ran an ordinary least squared regression with time spent as the criterion variable. For this purpose, the time categories were transformed to a continuous time scale (< 3 minutes" = 2, "3 to 5 minutes" = 4; "5 to 7 minutes" = 6; "7 to 9 minutes" = 8; and "more than 9 minutes" = 10), and regressed on the two Helmert contrasts. Besides the significant intercept, b = 5.00, SE = .11, t = 44.52, p < .001, the first coefficient was significant, b = 2.07, SE = .45, t = 4.64, p < .001, indicating that people in the default condition were willing to spend more time on research than people in the free-choice condition. The second coefficient, b = .92, SE = .43, t = 2.13, p = .034, was significant, too. Thus, participants accepted to spent more time when the default was made transparent than when it was not. Further pairwise comparisons revealed that disclosing the default's general effect, M = 4.75, SD = 1.93, yielded similar donations as the mere default, M = 4.80, SD = 2.09, t(106) = .12, p = .91. However, clarifying the target behavior, M = 5.56, SD = 1.42), t(108) = 2.24, p = .027, and disclosing the default's purpose, M = 5.95, SD = 1.27), t(101) = 3.33, p = .001, both significantly increased the amount of time spent on research as compared to the mere-default condition. Overall, the findings from the continuous regression model complement the main analysis, showing that transparency does not only increase default compliance, but also increases donation quantity. However, this interpretation has to be met with certain

Next, in an exploratory manner, we compared the effect of each transparency component against the conventional default condition. When faced with a mere default, 49.1% of the participants stayed with the pre-selected option. As illustrated in Figure 1, compliance rates were higher in the general effect disclosure condition (60,4%, $X^2(1, N = 108) = 1.39$, $p = .239$), the target behavior disclosure condition (83.6%, $X^2(1, N = 110) = 14.70$, $p < .001$), and the default purpose disclosure condition (85.4%, $X^2(1, N = 103) = 15.07$, $p < .001$), but only the latter two conditions were significantly different from the conventional default group. Therefore, disclosing the way in which defaults affect behavior in general was not detrimental to default effectiveness as could be assumed based on self-determination and reactance theories. Yet, it had no significant positive effect either. In congruence with our predictions, disclosing the purpose of the default or the expected target behavior was sufficient to bring about a positive effect on default effectiveness.

Figure 1. Participant choice of target option per condition



caution, since the transformation of a categorical dependent measure to a continuous one assumes equidistance of the categories.

Next, we assessed whether compliance behavior was related to participants' perceptions of endorser deceptiveness. While higher perceived deceptiveness was negatively correlated with choosing the default option (r = -.144, n = 265, p =.019), the deceptiveness scores did not differ significantly between conditions (F(4, 260) = .917, p = .455), suggesting that perceived deceptiveness was not responsible for the transparency effect.

Moreover, across the transparency conditions we assessed the strength of the argument for setting up the default. Perceived argument strength was positively correlated with compliance (r = .208, n = 180, p = .005) so that stronger arguments were associated with higher compliance rates. Participant scores were highest in the default purpose condition (M = 5.14, SD = 1.18), followed by the general effect (M = 4.69, SD = .94) and goal behavior (M = 4.07, SD = 1.5) disclosure conditions (F(2, 153) = 10.582, p = .001). In sum, people's compliance to a transparent default nudge was positively related to the strength of the arguments provided for setting up the intervention, and disclosing the defaults' purpose was considered the strongest argument.

General discussion

The results confirmed that introducing transparency to a conventional default nudge can increase its effectiveness. Replicating the findings of Paunov et al. (2018) not only substantiates a previously isolated finding in the literature, but also supports a proactive approach to transparency in nudging, re-affirming that (within limitations) a transparent nudge can be more effective than a conventional one. After all, transparency is one of the guiding principles of the nudging paradigm (Thaler & Sunstein, 2008), enabling people to scrutinize the implemented forms of choice architecture (Sunstein, 2015).

In addition, we demonstrated that that the role of transparency in defaults is more complex than expected from previous (null) findings (Loewenstein et al.,2015; Steffel et al.,2016). Going beyond the data available so far, we demonstrated which transparency disclosures can bring about the effect. In line with our predictions, disclosing the purpose of the default had a positive effect on compliance. This finding is in line with previous research on request justification (Langer, 1978), and provides a possible link to the persuasiveness of the disclosure, which was judged to contain the strongest argument in favor of the pre-selection.

We also showed that disclosing which target behavior is desired increased compliance in comparison to a classic default nudge. While previous research links the provision of exact behavioral information to increased theoretical support for similar nudges (Felsen et al., 2013), as to our knowledge, our findings are the first to relate the disclosure of target behavior to an increase in actual compliance with defaults.

Further, informing the participants of the defaults' general effect was neither detrimental, nor beneficial for compliance. Possibly, two effects cancelled each other out, resulting in a null-effect: while on the one hand participants in this condition perceived the endorser as relatively fair, they rated the argument strength of the disclosure as lowest from all conditions. A similar combination of positive and negative effects of transparency may help explaining why previous research on transparent defaults (Steffel et al., 2016; Bruns et al., 2018) did not find an increase in compliance, or in manifestations of psychological reactance upon disclosure.

Our results also lend support to the generalizability and robustness of the effect. First, we demonstrated that the positive impact of transparency persists even in cases, when the pre-selected option implies a possible personal loss (namely spending more time working than one is paid for). Second, instead of the research volunteer sample used by Paunov et al. (2018), we employed the

services of a commercial panel. Research shows that the majority of panel workers are motivated by money (Paolacci, Chandler, & Ipeirotis, 2010), and the quantity of their participation is usually a function of payment (Litman et al., 2015). Nevertheless, the participants in our experiment chose to stay with the default at their own expense, especially so when the default's purpose or target behavior was disclosed.

While our findings advocate transparency as a tool to increase default effectiveness, there are several limitations to their generalizability. First, a disclosure of the default's purpose may not always benefit default participation. For instance, if the default purpose is perceived to be at odds with strong behavioral guides, such as social norms or moral mandates (Seiler, 2015), its disclosure can fail to produce the desired effect.

Second, if the purpose of a given default is interpreted as serving the default-setters´ vested self-interests instead of being benevolent (Steffel, Williams, and Pogacar, 2016), no positive effects are expected.

Likewise, transparency may not increase compliance if the default contribution comes with inacceptable costs. As Bruns et al. (2018) demonstrated, default effects on donation rates did not benefit from transparency, when the contribution was set to a very costly option. It seems that transparency affects the willingness to comply, but only within a given acceptance threshold. Whether that is indeed the case, remains an open empirical question.

Another possible direction for future research is related to the mechanisms behind the individual disclosure contributions to compliance. Here, we provided some preliminary insight regarding the roles of endorser deceptiveness and disclosure argument strength. However, since these measures were not the main focus of our research, the evidence is exploratory and

correlational. Therefore, further and more systematic investigation is pending, where, for instance, the variables are manipulated experimentally.

Conclusion

The present research re-affirms the positive effect of transparency on the effectiveness of default nudges. Our findings also provide an insight as to which transparency components are worth disclosing in a default setting. Importantly, we show that an ethical and theory-driven interpretation of the classic default nudge can increase compliance even in cases, where the desired behavior comes at a personal cost.

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Supplementary Material

1. *General instructions (all conditions). In the free-choice condition, none of the choice options was pre-selected. Across the default conditions, category C (5-7) min. was defaulted, as shown in the example below. In the transparent default conditions, the same category was pre-selected, and a transparency disclosure was provided. The full text of the transparency disclosures per condition is available in Table 1 from the main body of the paper.*

Welcome to the experimental database of the Department of Economic Psychology

We have a large selection of studies you can work on. The studies have different content and duration.

Please select a category of studies from the following options. The categories are ordered by the duration of the studies they contain. Once you have selected a category, a study of that duration will be randomly assigned to you.

Placeholder for transparency disclosure text per condition

Please note that you will receive the agreed payment (for 5 min.), no matter if you complete a longer or a shorter study.

- ☐ Category A (< 3 min)
- ☐ Category B (3 – 5 min)
- ☑ Category C (5 – 7 min)
- ☐ Category D (7 – 9 min)
- ☐ Category E (> 9 min)

2. *Full list of items in the perceived endorser deceptiveness and perceived argument strength measures. All statements within the respective measure were presented in a random order. Agreement with all statements was indicated on a seven-point rating scale (1 =“not at all”; 7=“most definitely”)*

2.1 *Perceived endorser deceptiveness*

When I consider how the choice of categories was presented to me, I think that the experimenters...

...were open with me.
...were trying to mislead me.
...approached me in a sincere way.
...were honest with me.
...tried to manipulate me.
...tried to trick me.

2.2 *Perceived argument strength*

The argument, which the experimenters made for pre-selecting a category for me, ...

...was convincing.
...was compelling.
...was cogent.
...was defendable.
...was logically sound.
...gave me a good reason to choose the preselected option.

3. *Full list of attention check items per condition (* marks the correct answer).*

<u>In all conditions:</u>

On the page, where you had to choose a study category...

...*one of the categories was already preselected[1].

...the categories were ordered by the topic of the studies they contain.

...none of these options is correct.

...I don’t know.

[1] This answer was correct in all default conditions, and incorrect in the free choice condition. There, the answer “…none of these options was correct” was the right one.

Default & purpose disclosure condition

On the page where you had to choose a study category, one of the categories was already preselected. We preselected category C (5-7 min.) for you, because...

...*with the choice of this category you guarantee, that we will accomplish our research objectives.

...the choice of this category would increase your payment.

...this category contained the most exciting studies.

...I don’t know.

Default & target behavior disclosure condition

On the page where you had to choose a study category, one of the categories was already preselected. We preselected category C (5-7 min.) for you, because.....the choice of this category would increase your payment.

...this category contained the most exciting studies.

...*we wanted you to choose this category.

...I don’t know.

Default & general effect disclosure condition

On the page where you had to choose a study category, one of the categories was already preselected. We preselected category C (5-7 min.) for you, because...

...the choice of this category would increase your payment.

...*we know that in decision situations, people often stick with a choice option which is pre-selected for them.

...this category contained the most exciting studies.

...I don’t know.

Manuscript 3:

Paunov, Y., Wänke, M., & Vogel, T. (in press). Combining defaults and transparency information to increase policy compliance. *Social Psychology*.

Abstract

Combining the strengths of defaults and transparency information is a potentially powerful way to induce policy compliance. Despite negative theoretical predictions, a recent line of research revealed that default nudges may become more effective if people are informed why they should exhibit the targeted behavior. Yet, it is an open empirical question whether the increase in compliance came from setting a default and consequently disclosing it, or the provided information was sufficient to deliver the effect on its own. Results from an online experiment indicate that both defaulting and transparency information exert a statistically independent effect on compliance, with highest compliance rates observed in the combined condition. Practical and theoretical implications are discussed.

One of the corner stones of social organization is the shared belief that its very purpose is to ensure the well-being of society's members. This purpose is achieved through peoples' ability to exhibit prosocial behavior, i.e. to act in a way that benefits others (Hinde & Groebel, 1991), often at a cost to the individual (Irwin, 2009). However, people are also innately motivated to maximize their own utility (De Cremer, Seelenberg & Murningham, 2013). Therefore, the traditional approaches to promoting pro-social behavior seek to either incentivize it, or to penalize free-riding (Capraro, Jagfeld, Klein, Mul, & van de Pol, 2019). Consequently, such approaches can be either costly, or too restrictive.

As an alternative, Thaler and Sunstein (2008) introduced a set of behavioral interventions, or "nudges", as a cheap and less paternalistic way of promoting various pro-social and pro-self behaviors. They defined nudging as "…any aspect of the choice architecture, that alters people's choices in a predictable way without forbidding any options or significantly changing their economic incentives" (Thaler & Sunstein, 2008, p. 6). In their simplest form, nudges engineer the choice environment so that choosing the desired option becomes easier. This is primarily done by targeting effortless and intuitive thought processes (Thaler & Sunstein, 2008; Kahneman, 2011), which do not involve reflection (Hansen & Jespersen, 2013). To illustrate, a set of human eyes next to charity boxes increases donations by creating a sensation of being observed (Kelsey, Vaish, & Grossmann, 2018), and video primes of pristine nature increase pro-environmental attitudes (Bimonte et al, 2019).

However, nudges can also involve more deliberate, reflective thought processes (Thaler & Sunstein, 2008; Hansen & Jespersen, 2013). These interventions manipulate the information coupled with the desired choice option, and arguably require more processing effort to work (Prestwich, Kenworthy, & Conner, 2017). For instance, communicating social comparisons for

energy use reduces energy consumption (Allcott, 2011), and asking people to imagine "the right thing to do" increases charity donations to humanitarian organizations (Capraro et al., 2019).

Perhaps the most effective nudging technique is the use of defaults. Default nudges establish a setup, in which certain decision options are pre-selected, and come into effect if people do not take active steps to change them (Sunstein & Reisch, 2014). Arguably, defaults share characteristics of both nudging approaches: Some researchers state that defaults work because of automatic processes such as the status quo bias (Samuelson & Zeckhauser, 1988) and loss aversion (Tversky & Kahneman, 1991), while others claim that people comply because they reflect and infer that the pre-selected option is the recommended one (McKenzie, Liersch & Finkelstein, 2006). In any case, default nudges are effective in inducing compliance to pro-social policies, with prominent examples involving organ donation (Johnson & Goldstein, 2003) and retirement savings (Thaler & Benartzi, 2004).

However, researchers have argued that defaults restrict people's autonomy and ability to exercise informed choice (Smith, Goldstein & Johnson, 2013), since the targeted individuals are largely unaware of the goal of the intervention (Hansen & Jespersen, 2013). Consequently, a call for transparency when implementing default nudges emerged (Sunstein, 2015).

Yet there is concern that providing information about defaults would diminish their effect on compliance, and could even have adverse effects. Bovens (2009) theorized that once people receive information about a given nudge, they can react with behaviors that are inconsistent with the goal of the intervention. Reactance theory (Brehm, 1966) and the self-determination approach (Deci, 1975; Ryan & Deci, 2000) would even assume that the mere presence of an influence attempt might elicit resistance.

The empirical evidence on the matter is limited, but so far the available literature reports no negative impact of combining transparency and defaults (Loewenstein et al., 2015; Steffel et al., 2016; Bruns et al., 2018). Instead, a recent line of research by Paunov, Wänke, & Vogel (2019a; 2019b) demonstrated that providing transparency information about a default was not only not detrimental, but even increased the effectiveness of the influence attempt. Across a number of studies, the authors asked their respondents to decide how much time they wanted to spend on participating in an experiment. Telling them that one option was preselected because choosing this option would be best for the researchers (informed default) almost tripled compliance in comparison to a conventional default intervention. This substantial boost was explained by adopting a socio-psychological perspective, which treats defaults as a form of social communication between default-setter and recipient. As suggested by McKenzie and colleagues (2006), the targets of a default intervention are likely to infer that the pre-selected option is implicitly recommended by the default setter. In this vein, Paunov et al. (2019a) reasoned that a providing information about the default may boost its impact by transforming the implicit recommendation into an explicit one. Moreover, laying open one´s intentions also signals that one is sincere and is to be trusted.

However, a closer look at the evidence reveals that the sizable effect of the informed default nudge might require further investigation. In particular, it remains unclear whether the high compliance increase in the informed default condition was due to the combined influence of both information and defaulting, or because of the information alone. Since the information provided for the default should work similarly to a persuasive argument, both propositions are possible. On the one hand, it has been shown that in a given decision situation, the effects from two information cues can add up (*additivity hypothesis,* Bohner et al., 1995; Maheswaran & Chaiken, 1991) to exert

influence on the decision's outcome. Thus, both the implicit recommendation from the default and the information about its purpose may contribute to the effectiveness of the influence attempt. Accordingly, the decision cues (defaulted option and information) could exert two independent main effects on compliance.

On the other hand, it is known that more diagnostic information can attenuate the effect of the less diagnostic cues on choice (*attenuation hypothesis*, Chaiken & Maheswaran, 1994). Pertinent to this perspective, the default in a combined nudge may be seen as relatively uninformative, and can even become obsolete when compared to the informational value of the provided information (Keller, Harlam, Lowenstein, & Volpp, 2011). In that case, one should be able to achieve a substantial compliance boost by simply informing the participants which option is most desirable, even when no default is set.

In sum, it appears that a clean test of the effectiveness of informed defaulting is still pending. To address the issue, the present research adds a condition in which no default is set, but the same information as in the informed default condition is provided. This allows to disentangle the effect of the information from the combined effect of informed defaulting.

The present research

An online experiment was conducted, which explored the effects of providing information and defaulting in the context of devoting time to scientific research. Compliance rates across four experimental conditions were compared between participants. The setup was largely similar to the one, used by Paunov and colleagues (2019b), except for the introduction of a separate information provision condition (for a detailed description of the stimulus material and experimental procedure, see Method). Since in our setup both decision cues (defaulted option and information) work in the

same direction, we predicted positive main effects of defaulting (H1) and information (H2) on compliance, in accordance with the additivity hypothesis (Bohner et al., 1995; Chaiken & Maheswaran, 1994). As a consequence, we predicted that the combination of defaulting and information will yield the highest compliance across conditions.

Method

Participants and design

The required sample size for a planned power of 1-*ß* = 0.8 (two-tailed, α= .05), was calculated with G*Power (Faul, Erdfelder, Buchner, & Lang, 2009) using odds ratios (ORs). The expected effect sizes were extrapolated from previous research on informed defaults (Paunov et al., 2019a; 2019b). An odds ratio of 2.132 and a probability of opting out under information provision Pr(Y = 1 |X = 1) H0 = .46 rendered a required sample of 228 participants. Two hundred and fifty-six (256) English-speaking participants were recruited via an online respondent panel (163 females (63.7%), 93 males (36.3%), mean age 37.4 years, (*SD*=12)), and were randomly assigned to four experimental conditions in a 2(default: present/absent) X 2 (Information: provided/not provided) between-participants design. Each respondent was paid £ 0.45 for taking part in the study.

Materials and procedure

Respondents who decided to take part in our research were informed that there were several studies from which they could choose, and that the offered studies differed in content and duration. Only the duration was provided for each study without any content description to avoid unrelated topic preferences. The following five choice options were listed: "< 3 minutes", "3 to 5 minutes"; "5 to 7 minutes"; "7 to 9 minutes"; and "more than 9 minutes". Across all conditions and simultaneously with the presentation of the choice options, the participants were informed that they

would be paid for doing a 5 minute study, independent of the duration of the study they chose. Therefore, choosing the "< 3 minutes" represented the opportunity to spend less than 5 minutes (thus maximizing the respondents' profit), while the second allowed for spending a maximum of 5 minutes in order to break even without violating the contract. Across two default conditions, the "5 to 7 minutes" option was pre-selected. A choice of this option meant that the participants accepted personal costs for the sake of supporting scientific research, a representation of pro-social behavior. In the conventional default condition, the "5 to 7 minutes" option was pre-selected, unaccompanied by information. In the informed default condition, the pre-selection was accompanied by a disclosure, which clarified the consequences of sticking with the default: "Please note the following: with choosing Category C (5-7 min), you guarantee that we will be able to accomplish our research objectives". The same information was presented in an information provision condition, where none of the choice options was pre-selected. A forth, free choice condition obtained a baseline. Upon making their choice and providing some demographic information, the participants were redirected to an unrelated task, which took approximately 5 minutes to complete. Finally, all respondents were thoroughly debriefed about the purpose of the experiment, re-affirmed their agreement to submit data, and received a code to redeem their payment.

Results and Discussion

The main descriptive results per cell are reported in Table 1. Specifically, compliance was lowest in the free choice condition, where only 16.9% of the respondents chose the target option. Participant compliance was higher when a default was set but no information was given (64.1%), and when no default was set but information was provided (68.3%). Pertinent to our research question, the highest compliance rate was observed for the group, which received both default and

information treatments (91%). There, the participants chose the target option significantly more than those in the information provision condition ($X^2(1, N = 127) = 10.37$, $p = .001$) and the conventional default condition ($X^2(1, N = 131) = 13.82$, $p < .001$).

To test the pattern, the main dependent measure (coded 1 for stay/choose target option, and 0 for opt out out/choose a different option) was predicted from two effect-coded contrasts in a binomial logistic regression. The contrasts indicated whether there was a default or not (default conditions = 1, free choice conditions = -1), and whether information was provided or not (information conditions= 1, no information conditions = -1). Table 2 provides a summary of the results. The first contrast was significant, $b = 1.025$ (SE = 1.65), Wald-$\chi2(1) = 38.45$, $p < .001$, indicating that the default increased compliance, in accordance with our first hypothesis. In congruence with our second prediction, the second contrast was also highly significant, $b = .930$ (SE = 1.65), Wald-$\chi2(1) = 31.625$, $p < .001$, showing that the provision of information increased choices for the target option. The effects from both treatments appeared statistically independent from one another, as indicated by the non-significant interaction term, $b = -.155$ (SE = 1.65), Wald-$\chi2(1) = .877$, $p = .349$. Therefore, at least on the statistical level, the effectiveness of informed defaults seems to reflect the additivity of both factors.

Table 1

Percentage of participants choosing the target option as a function of a 2(default: present/absent) X 2(information provided/not provided) between participants design.

	Default		
Information	Present	Absent	Total
Provided	91% (61/67)	68.3% (41/60)	80.3% (102/127)
Not provided	64.1% (41/64)	16.9% (11/65)	40.3% (52/129)
Total	77.8% (102/131)	40.8% (52/125)	

Note: Participants' choice of the target option versus choice of another option ratios are in parentheses.

Table 2
Summary of logistic regression analysis for the effects of default and information provision on choice of target option.

Predictor	B	S.E.	Wald	Df	OR	95% CI
Default	1.025**	0.165	38.457	1	2.788	[2.01, 3.85]
Information	.930**	0.165	31.625	1	2.534	[1.83, 3.50]
Default x Information	-.155	0.165	0.877	1	0.857	[0.62, 1.18]
Constant	0.519*	0.165	9.846	1	1.68	

Note: OR = odds ratio; CI = confidence interval; the predictors are effect coded variables. Default compares participants from the free choice conditions versus participants from the default conditions. Information compares participants from the informed conditions against the uninformed conditions. Default x Information represents the interaction term.
**p< .001, *p<.05

General Discussion

The results confirmed that a combination of defaulting and informing about the default can be a powerful tool for inducing compliance. Our findings contribute to the current state of nudging research in several ways: First, they demonstrate that the positive impact of informed defaults persists in cases, when choosing the pre-selected option implies a tradeoff between personal gain and a pro-social goal. Therefore, it appears that informed defaulting is applicable to the most common default settings, where such a tradeoff is present - to policies aimed at increasing charitable donations (Fiala & Noussair, 2017) or contributing to environmental protection (Pichert & Katsikopoulos, 2008).

Beyond previous data, we showed that the informed default nudge does not owe its effectiveness to the mere presence of transparency information. Instead, we find evidence that both defaulting and the information contribute to the sizable increase in compliance, in a rather additive fashion. In this sense, the results help dissuade previous concerns that combining both approaches can have an adverse effect on compliance (Bovens, 2009). With that, we hope that our findings may encourage policy makers to employ both techniques when nudging pro-social behavior.

While the results advocate the combination of defaulting and information provision as an effective way of increasing compliance, there are several limitations to their generalizability.

First, we limit our predictions to settings, where choosing the default benefits others. Defaults aimed at self-beneficial behaviors (such as healthy eating or exercising, for instance) should gain less from information about the consequences of complying. That is because people would presumably know this information already (Payne1, Jones, & Harris, 2004), and highlighting it is less likely to trigger a substantial increase in compliance.

Second, despite the evidence that the effects of defaulting and informing are statistically independent, it does not follow that the processes in all groups are the same: for instance, participants in the combined group could still make sense of the joint presence of both in their own way (e.g. considering that the defaulter is more sincere when information is present). Future research needs to conduct further and more systematic investigation of the mechanisms behind the effects in all conditions. For instance, by assessing whether the experimental conditions affect people's inferences regarding the appropriateness of the target behavior, on the one hand, and their perceptions of the policy maker on the other (Paunov et al., 2019a).

Conclusion

The present research demonstrates that the positive effect of transparent defaults stems from both defaulting and explaining the default. Importantly, we present initial proof for the building blocks of a nudge, which constitutes a transparent and effective pro-social intervention.

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