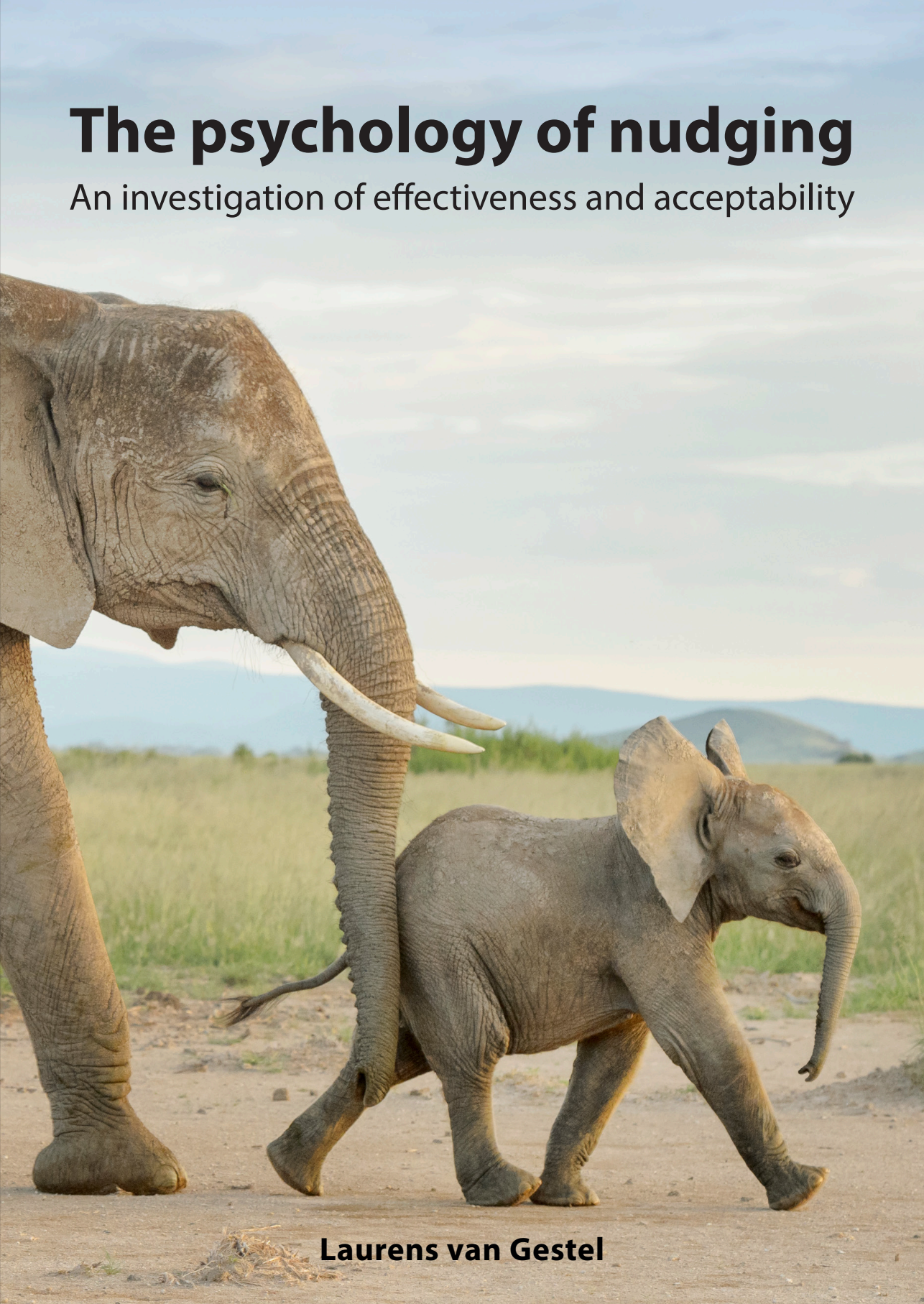
The psychology of nudging

An investigation of effectiveness and acceptability



Laurens van Gestel

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The psychology of nudging
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De psychologie van nudging
Onderzoek naar effectiviteit en aanvaardbaarheid

(met een samenvatting in het Nederlands)

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Chapter 1

General introduction

This dissertation aims to examine when and for whom nudges can be an effective and acceptable means of stimulating desired behavior. Since the introduction of the term 'nudge', the intervention has become increasingly popular both in scientific research and public policy. Nudges offer to be a novel and supplemental policy tool that goes with the grain of human behavior rather than combating all of its complexities. Scientific research thus far has established promising results regarding effectiveness and acceptability, but less is currently known about the conditions under which these nudges are effective and acceptable. The current dissertation aims to contribute to a robust science of nudging research by investigating when and for whom nudges are effective and acceptable. In doing so, the specific focus throughout the dissertation will lie on elements related to effort and motivation, as these factors have diverse and important implications for the theory, practice and ethics of nudging.

The term 'nudge' was introduced to the mainstream public by behavioral economist Richard Thaler and legal scholar Cass Sunstein in their 2008 bestseller 'Nudge: Improving decisions about health, wealth, and happiness'. The book summarized decades of research on human information processing and behavior and made a compelling case for the strategic use of those behavioral insights in public policy as a supplement to existing tools for influencing citizen behavior. In doing so, the authors heavily relied on research on dual-process theories and sketched a view on humans as fallible, with limited attention and willpower. And, exactly because people are not always willing or capable of making deliberate choices, it was explained that people can benefit from a choice architecture that incorporates this knowledge for the better. Although the insights were not necessarily novel from a psychological science perspective, the book was important in creating awareness of those insights among policy makers. Consequently, nudging sparked a lot of interest among researchers from various disciplines and catalyzed the installation of so-called behavioral insight units that apply behavioral insights in policies across the globe.

This introductory chapter serves as a broad overview of the current knowledge and evidence-base of nudging so as to set the stage for the research questions that stand central in this dissertation. I will first define what nudges are and describe their underlying principles. Next, I will elaborate on the current evidence-base for nudging interventions in terms of general effectiveness and acceptability, after which I will outline the need to consider the specific conditions under which nudges are effective and acceptable. The final part of this chapter is dedicated to describing the specific research questions in this dissertation and to providing an overview of the empirical chapters that address these research questions.

Definition and Underlying Principles

A nudge was originally defined by Thaler and Sunstein as 'any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid. Nudges are not mandates' (Thaler &

Sunstein, 2008, p. 6). Paradigmatic examples of nudges include the opt-out default for organ donation (e.g., Johnson & Goldstein, 2003), social norm for energy usage (e.g., Allcott, 2011), and rearrangement of healthy food options to the cash register desk (e.g., Kroese, Marchiori, & De Ridder, 2015). Other types of interventions such as persuasive communication (Stiff & Mongeau, 2016), taxation (e.g., sugar tax; Chaloupka, Powell, & Warner, 2019), or legislation (e.g., smoking ban; Meyers, Neuberger, & He, 2009) do not count as nudges, as they either provide more information without making behavior easier or restrict choice. Since the introduction of the term nudge, its definition has been extensively debated and refinements have been suggested to describe the intentionality of steering behavior and the extent to which nudges make use of biases and heuristics (e.g., Hansen & Jespersen, 2013; Hausman & Welch, 2010; Marchiori, Adriaanse, & De Ridder, 2017). In the current dissertation, I follow previously established definitions and describe nudges as strategic alterations in the immediate choice environment (i.e., choice architecture) that (are intended to) stimulate certain behavior (Thaler & Sunstein, 2008). In agreement with many scholars, I tend to view nudging as an umbrella term as nudges come in all sorts and shapes. What all these different types of nudges have in common though is that they guide human interaction with the environment by capitalizing on behavioral insights into human decision making. To understand how nudges effectively use cues in the physical and/or social environment, we first need to explicate dual-process theories.

The nudge approach recognizes that people do not always have the capacity or willingness to deliberate upon their decisions, and nudges thereby aim to stimulate desired behavior by incorporating insights from more automatic processes like heuristics (so-called 'rules of thumb', such as when we follow the majority or act in line with authority figures), impulses, or habits. As such, nudging has its roots in dual-process accounts of human reasoning and behavior that originated in the 1970s and 1980s (Kahneman, 2011; Thaler & Sunstein, 2008). A host of different dual-process theories exist and, as originally introduced, they all largely overlap in their terminology and underlying assumptions (for an overview see Evans, 2008). While nudging was originally based on dual *systems* with a list of features belonging to either of the two systems, I rather adhere to a view on human processing in terms of two *types* of thinking (Type 1 and Type 2) with defining features and typical correlates.[1] The main defining difference between the two types of reasoning lies in the engagement of working memory. Type 1 processes place a minimal burden on working memory, while Type 2 processes do require working memory engagement. Further typical correlates of Type 1 processes are that they are often fast, automatic, and

1. I purposefully avoid the frequently used terms System 1 and System 2, to avoid misconceptions about assumed evolutionary distinctions (where System 1 is a supposedly primitive system and System 2 is an evolutionary advanced system) and to avoid misconceptions about the two systems being located in specific locations in the brain (see also Evans & Stanovich, 2013).

effortless, while typical correlates of Type 2 processes are that they are slow, controlled, and effortful (for an overview see Evans & Stanovich, 2013). While Type 1 reasoning originally tended to be put in a bad light and be blamed for erroneous decisions as a result from unwanted habits or ill-considered impulses, current theorizing recognizes the adaptive nature of Type 1 processes. That is, Type 1 processes are efficient and save effort and the use of heuristics often leads to accurate judgments (Gigerenzer & Gaissmaier, 2011). Moreover, Type 2 processes are no longer seen as infallible either, as evidenced by research on motivated reasoning (Kunda, 1990) and self-licensing (De Witt Huberts, Evers, & De Ridder, 2012).

Different views exist about the interaction between the two types of reasoning, but the *default-interventionist* perspective has traditionally been supported by most scientists (e.g., Kahneman, 2011; Evans & Stanovich, 2013). According to this perspective, when faced with a novel problem, Type 1 processes are often prompted quickly and effortlessly by default, while the more effortful Type 2 processes can intervene if need be. Much discussion continues to exist about the validity of dual-process typologies as an approximation of the human cognitive architecture. For example, it has been argued that there is little systematic research demonstrating that features from the same type of reasoning align better together than with features from the other type of reasoning (i.e., the alignment problem; Melnikoff & Bargh, 2018). In other words, a particular feature of Type 1 processing (e.g., nonconsciousness) is not necessarily more likely to occur with another typical correlate of Type 1 processing (unintentionality) than with a correlate of Type 2 processing (intentionality). Think, for example, about a skilled piano player who plays the instrument with intentionality but without the need for conscious monitoring of every individual action, contradicting the idea that typical correlates of a particular type of reasoning align well. Moreover, empirical evidence seems to question the time-course assumption of the default-interventionist perspective that an initial false response is consequently overridden by a well-elaborated correct response. That is, even though there is a heuristic which cues a false response in a reasoning problem, the initial response based on automatic processes can often be correct without the need to override the response at a later stage (i.e., the time-course problem; Bago & De Neys, 2017). Finally, some scholars argue for the existence of a single process account with continuous qualitative differences instead of a dual-process account with absolute differences between the two types of reasoning (Kruglanski & Gigerenzer, 2011; Osman, 2004). Perhaps slightly ironically, some also question the usefulness of the discussions about the dual-processing accounts (De Neys, 2021), while yet again others emphasize the potential value of dual-processing for behaviorally informed public policy (Chater, 2018).

Regardless of which specific human cognitive architecture one assumes, nudges deviate from neoclassical economic theory that views mankind as rational beings that maximize their choice outcomes based on all relevant information available to them (Jolls, Sunstein, & Thaler, 1998). Rather, nudging is based on the idea that people are boundedly

rational (Gigerenzer & Selten, 2002; Simon, 1955). In other words, people do not always outweigh all the pros and cons or undertake a full cost-benefit analysis, but rather base decisions on what is deemed satisfactory given the complexity of the problem and the available cognitive resources. That is not to say that people continuously make completely biased and erroneous decisions (i.e., "Bounded rationality is not irrationality", Simon, 1985, p. 297), but rather that the perfect rationality assumption of neoclassical economics does not adequately map onto human processing and behavior. Building further on these insights, nudges acknowledge that much of our daily behavior is based on rather automatic processes like heuristics (Gigerenzer & Gaissmaier, 2011; Gilovich, Griffin, & Kahneman, 2002), impulses (Strack & Deutsch, 2004), and habits (Wood & Rünger, 2016). The view of humans as boundedly rational beings thus results in an additional set of behavioral change interventions that in principle does not necessarily rely on effortful processing in order to be effective. In other words, rather than combating all the difficulties that arise when trying to change behavior via effortful routes, nudges are thought to go with the grain of human behavior and befriend the effortless processes that guide much of our decisions and behavior.

Effectiveness

Now that we briefly discussed the definition of nudging and its underlying principles, it is time to outline the evidence-base in terms of effectiveness and acceptability. Nudges received increasing interest from scientists and policy makers since the publication of some highly effective interventions, such as the Save More Tomorrow program (Thaler & Benartzi, 2004) and the opt-out system for organ donation (Johnson & Goldstein, 2003). Following the establishment of the Behavioural Insights Team in 2009 in the United Kingdom, other countries (e.g., the United States and Germany) and international institutions (e.g., European Union, World Bank, and OECD) as well as private companies have become actively engaged in using behavioral insights in public policy. The seemingly sheer simplicity of these interventions combined with the high impact made a clear and illuminating case for the relevance of applying behavioral insights in public policy. Moreover, the cost-effectiveness of these interventions often compared favorably compared to other types of policy instruments (Benartzi et al., 2017; Wagner, Montoy, Drabo, & Dow, 2020). But now that we are a few years on the road, what do systematic reviews and meta-analyses tell us about the effectiveness of nudges in stimulating desired behavior?

By now several systematic reviews and meta-analyses have been conducted. Some have focused on outlining the scope of the field (e.g., Hummel & Maedche, 2019; Szaszi, Palinkas, Palfi, Szollosi, & Aczel, 2018), while others were more narrowly focused on particular behaviors (e.g., healthy eating; Cadario & Chandon, 2020) or particular types of nudges (e.g., defaults; Jachimowicz, Duncan, Weber, & Johnson, 2019). One of the main take away messages of almost all of these reviews is that nudges do show promise in effectively changing behavior for the better, but that current evidence is based on studies

that are limited in quantity and quality, that there are large differences between studies in demonstrating effectiveness, and that little is known about the hypothesized working mechanisms and boundary conditions of nudges (Arno & Thomas, 2016; Broers, De Breucker, Van den Broucke, & Luminet, 2017; Bucher et al., 2016; Hummel & Maedche, 2019; Jachimowicz et al., 2020; Szaszi et al., 2018).[2] In fact, a systematic scoping review of the domain-general field of nudges revealed that only a small portion of experimental studies is dedicated to identifying boundary conditions or mechanisms, while the majority of the studies is focused on establishing effects of particular nudges in particular settings (Szaszi et al., 2018).

Hummel and Maedche (2019) aimed to quantify the effects of nudges on behavior and focused on a range of different types of nudges as well as on different behavioral domains. Their analysis showed that 62% of the published effects were significant, and the overall median relative effect size was 21% (39% for the significant effects only), which means that the desired behavior was performed 21% more when participants were nudged compared to control groups. The overall effectiveness on an individual level thus suggests that, although nudges should not be seen as a panacea, they can overall affect individual behavior in a meaningful way. Especially given that it is relatively easy to increase the reach at a rather low cost (cf. De Ridder et al., 2020), these effects on an individual level can add up and create profound effects on a population level (Cialdini, Martin, & Goldstein, 2015). Yet, the review also showed that there are large discrepancies between studies and types of nudges. To illustrate, nudges that relied on precommitment or reminders yielded much smaller effects than default nudges, which were the most effective from a list of 10 different types of nudges (median effect size of 50%). It is thus important to keep in mind that nudging remains an umbrella term and that effects that are found for a specific type of nudge do not necessarily uphold for other nudges. A recent meta-analysis that was purely focused on defaults confirmed that defaults can have a considerable effect on desired behavior. This analysis revealed a medium effect size of $d = 0.68$, but with substantial variation between studies (Jachimowicz et al., 2019). As a prototypical example of nudges, defaults can thus instigate meaningful behavior change on an individual level, but care should be taken in implementing defaults as the effectiveness is not guaranteed due to potential moderators or mechanisms of which currently little is known.

Most studies on nudging desired behavior are currently conducted in the realm of health behavior (Hummel & Maedche, 2020; Szaszi et al., 2018). In this field, several meta-

2. Just like in meta-analyses on other phenomena, there is concern about possible publication bias (Hummel & Maedche, 2019). Analyses to detect publication bias (e.g., trim-and-fill method or p-curve analysis) tend to reveal that there is little publication bias in the literature on nudging effectiveness (Cadario & Chandon, 2020). Surprisingly, results by Jachimowicz and colleagues (2019) even show that, if anything, large effect sizes are underreported. Nevertheless, reported effect sizes should be interpreted with caution.

analyses and systematic reviews have been conducted as well, and they generally reveal small to medium individual-level effects of nudges (e.g., Arno & Thomas, 2016; Broers et al., 2017; Bucher et al., 2016; Cadario & Chandon, 2020; Wilson, Buckley, Buckley, & Bogomolova, 2016). Cadario and Chandon (2020) conducted a thorough meta-analysis of nudges stimulating healthy food choices in the field. The analysis revealed that, after controlling for behavioral, population, and study characteristics, these nudges yielded an average small effect of $d = 0.23$, corresponding with a daily decrease of 124 kcal in energy intake. Yet, as the review points out, little is known about how nudges differentially impact different individuals. In line with this, almost all of these reviews signal a large heterogeneity between studies (e.g., Arno & Thomas, 2016; Broers et al., 2017; Jachimowicz et al., 2019) and thus point towards a need to further scrutinize potential boundary conditions and opportunities for creating impact. Going back to the notion of bounded rationality – which suggests that people satisfice given the complexity of the problem and the availability of cognitive resources – little is known about the effectiveness of nudges exactly under those circumstances when more effort is required due to the complexity of the choice or the involvement of cognitive resources. It is important to know whether nudges live up to their promise in those situations, as it has been suggested that nudges make choice easier without relying on complex cognitive processes, but with hardly any empirical foundation for such claims. Similarly, empirical research thus far has put little attention on addressing whether those who are motivated to perform the nudged behavior are indeed the ones who benefit from a nudge, which is important to know for understanding whether nudges effectively reach the intended target population.

Acceptability

Next to evaluations of effectiveness, acceptability as judged by laypeople has also received increasing attention in nudging research. According to the field of implementation science, public acceptability is a core element for the systematic uptake of evidence-based practices in real-world contexts (Luszczynska, Lobczowska, & Horodyska, 2020), and a lack of acceptability has long been a hindrance to successful implementation of novel interventions (Proctor et al., 2011). Systematic reviews or meta-analyses on acceptability of nudges are yet to be conducted, but evidence from both hypothetical survey or vignette studies (e.g., Evers, Marchiori, Junghans, Cremers, & De Ridder, 2018; Junghans, Cheung, & De Ridder, 2015; Reisch & Sunstein, 2016; Reynolds, Archer, Pilling, Kenny, Hollands, & Marteau, 2019; Sunstein, Reisch, & Rauber, 2018) as well as field studies with actually implemented nudges (e.g., Hansen, Schilling, & Malthesen, 2019; Kroese et al., 2015; Van Gestel, Kroese, & De Ridder, 2018; Venema, Kroese, & De Ridder, 2018) is accumulating that nudges are generally seen as acceptable by the public. However, just like with studies on nudge effectiveness, currently little is known about when and for whom nudges are an acceptable means of steering behavior. In order to successfully implement nudges with meaningful impact, it is important to have a better understanding of the conditions under which nudges are judged acceptable by those that are targeted. To illustrate, it is largely

unclear whether those who want to perform the behavior and/or those who could benefit from additional aid are indeed those who are more open to being nudged.

Apart from laypeople's judgments of acceptability, there remains debate about the legitimacy of nudges as judged by ethicists and philosophers. Much of the debate originates from the incorrectly assumed view of Type 1 reasoning as distinctly erroneous and the concerns mostly revolve around elements of manipulation and autonomous decision making. It has, for example, been suggested that nudges are paternalism in disguise (Hansen & Jespersen, 2013) and that they work best 'in the dark' (Bovens, 2009). Consequently, it has been argued that nudges restrict people's autonomy to reach their own decisions, because nudges are thought to exploit cognitive biases and heuristics, thereby leaving people with little to no control to evaluate and deliberate for themselves (Hausman & Welch, 2010). Yet, it should be said that many of these criticisms are based on theoretical assumptions of nudges, while the empirical study of the processes that underlie nudge effects is still in its infancy. Nevertheless, current evidence based on the existing empirical insights suggests that these concerns could perhaps be alleviated (De Ridder, Kroese, & Van Gestel, in press). For example, it has been shown that transparency information does not decrease nudge effectiveness (Bruns, Kantorowica-Reznichenko, Klement, Jonsson, & Rahali, 2018; Wachner, Adriaanse, & De Ridder, 2020), countering the idea that nudges work best in the dark. It goes without saying that empirical studies on public acceptability of nudges should not be seen as a substitute of normative debates about the legitimacy of nudges, but they do nevertheless provide valuable empirical data to further fuel these discussions. Moreover, given the discrepancy between criticisms from scholars and judgments from laypeople, it is important to gain a finer understanding of when and for whom nudges are an acceptable means of stimulating desired behavior.

Understanding When and for Whom Nudges are Effective and Acceptable

Nudges can thus be effective in stimulating certain behavior, they are generally well-accepted by the public, and often compare favorably to other interventions in terms of cost-effectiveness. But these findings are mostly based on rather general observations or meta-analytic averages of effectiveness and acceptability, while large differences between types of studies, types of nudges, and types of behaviors may occur. As a matter of fact, the reviews unveil that there is a void in nudging research that addresses mechanisms and boundary conditions. Besides the observation of the existence of this hiatus, several scholars have argued for the relevance of addressing questions related to conditions of effectiveness and acceptability (e.g., De Ridder et al., 2020; De Ridder et al., in press; Marchiori et al., 2017; Marteau, Fletcher, Hollands, & Munafò, 2020; Szaszi et al., 2018). Many important issues could be identified to study further, but two of the most fundamental issues with diverse implications for the theory, practice, and ethics of nudges relate to issues of effort and motivation. For example, little is known about how the complexity of the decision, the availability of cognitive resources, and motivation of the decision maker impact nudge effectiveness and acceptability. Thus, in order to understand when and

for whom nudges are effective and acceptable, it is time to start identifying boundary conditions and opportunities for successful implementation, and to systematically map the conditions of 'Nudgeability' (i.e., aspects of susceptibility to being nudged). Ultimately this should also provide better insights in which people are most likely to benefit from the implementation of nudges: those who could need aid and/or those who want aid.

Many of the critical questions that stand out in terms of effectiveness and acceptability are in principle questions that are related to the psychology of man. While the origin of nudging was heavily based on decades of research in psychology, the central role of psychological science in nudging research somehow diminished over time as the popularity of nudging in public policy grew. Already in 2017 it was claimed that there was a need to 'put the psychology back in nudging' in order to address the most pressing questions (Marchiori et al., 2017). Psychological factors that reveal boundary conditions, underlying mechanisms, and downstream effects on objective behaviors (e.g., compensatory behavior) and subjective evaluations (e.g., feelings of autonomy) are thus required in order to advance the field. I believe that in order to evaluate the full potential of nudges as an effective and acceptable behavior change tool, and in order to advance theory and practice, we indeed need to put back the psychology in nudging research and identify when and for whom nudges are effective and acceptable. Consequently, these new insights may aid in building a robust science of nudging that is theoretically sound and more efficacious in its implementation.

The Present Dissertation

This dissertation aims to offer new insights into when and for whom nudges are an effective and acceptable means of stimulating desired behavior and thereby aims to contribute both to the practice and theoretical basis of nudging. In order to address the question *when nudges are effective*, the present dissertation focuses on elements of the choice set (Chapter 2) and on dual-processing (Chapter 3). In order to address the question *for whom nudges are effective*, the present dissertation focuses on human motivation (Chapter 4). Finally, in order to answer the question *for whom nudges are acceptable*, the present dissertation focuses on self-regulatory capacity and motivation (Chapter 5). As alluded to before, throughout the dissertation the focus will lie on elements relating to effort (complexity of the choice set, dual-processing, self-regulatory capacity) and motivation (autonomous and controlled motivation). Throughout the dissertation, several types of nudges will be included (e.g., defaults and rearrangement of choice options) across several types of behaviors, mostly related to health and sustainability. The main dependent variables of interest are effectiveness (Chapters 2, 3, and 4) and acceptability (Chapter 5).

Before outlining the separate chapters, there are a few general remarks that are in place to mention. First of all, note that throughout this dissertation I tend to use the word effectiveness to describe an effective (nudge) manipulation. Most of the studies were conducted in rather controlled circumstances, so in some fields the word efficacy may

better cover the intended meaning.

Second, I value to mention that the current dissertation heavily relies on previous studies and uses (operationalizations of) nudges that have been shown to be effective in earlier work. In order to address the main research question when and for whom nudges are effective and acceptable, I deliberately decided to build on previous work that showed that the nudges under consideration do indeed yield the intended effects. Consequently, the current dissertation perhaps could lead to an overestimation of the general effectiveness of nudges, as I presorted effective nudges (with substantial variation) from previous work in order to be able to address the more fundamental aspects that are considered in the current dissertation.

Third, I also value to mention that most of the studies in this dissertation were preregistered (Chapters 3, 4, and 5) and that we applied power analysis in all chapters. Recently, preregistration has become the norm in psychological science (e.g., Nosek, Ebersole, DeHaven, & Mellor, 2018; Nosek et al., 2019), but a systematic scoping review on nudging from 2018 revealed that none of the included studies were preregistered and only 7% applied power analyses (Szaszi et al., 2018). I am aware that this practice is rapidly changing in nudging research as well, and believe that this contributes to the quality of nudging research as a mature and open part of science.

Finally, please note that all chapters in this dissertation – including the introduction and general discussion chapters – were written in such a way that they can be read independently and in any given order. Chapters 2 to 5 were written as separate articles for scientific journals. Therefore, the content of the chapters may overlap to some extent.

Overview of Chapters

Chapter 2 starts with the observation that much of the literature on nudging effectiveness is based on rather simple, often dichotomous, choices, whereas in reality people often face a more complex world with multiple alternatives that may require more effort to reach a decision. Two field experiments focusing on overt behavior were conducted to establish whether a nudge remained effective in these more complex choice situations with multiple alternatives in the immediate surroundings of the targeted option. In order to study this, we used a choice set that existed of either three or nine different options. Inspired by research on choice sets and consumer behavior we used different pieces of chocolate and we used a proximity nudge that placed the targeted option closest to the participant to stimulate the choice for that option. Using both frequentist and Bayesian statistics, we showed in both studies that the proximity nudge remained effective when more alternative options were present, suggesting that nudges can remain effective in stimulating certain behavior in more complex situations. We incorporated these insights in the remaining chapters of this dissertation and mostly included more than one alternative option in the choice sets in our studies.

Chapter 3 reports on a Registered Report that included two online experiments that investigated the hypothesized working mechanisms of nudges as making use of effortless

processes. In both studies, we used a default nudge that stimulated the choice for green amenities. In Study 1, we manipulated cognitive load with a dot memorization task, and thereby inhibited Type 2 processing. This study showed that the default effect was statistically equivalent under low and high load, which suggests that the default effect is not strengthened or weakened when people are bound to resort to Type 1 processes. In Study 2, we manipulated deliberation with specific instructions, and thereby stimulated Type 2 processing. This study showed that deliberation significantly decreased the number of amenities chosen, but that deliberation did not moderate the default effect, suggesting that default effects are not necessarily impregnable as deliberation may operate in parallel and impact choice simultaneously. Together, these studies imply that default nudges are not dependent on elaborate effortful processing in order to be effective, but that deliberation can in parallel lead to different choice outcomes.

Chapter 4 reports on three online experiments that focused on the role of motivation in nudges' effectiveness. We used three different types of defaults in three different behavioral domains and investigated how defaults and different types of motivation affect choice outcomes. In doing so, we focused on the potentially moderating role of motivation as well as on the possible incremental value of nudges on top of existing motivation. Across three studies focusing on three different behavioral domains (health, sustainability, and prosocial behavior) we showed that the defaults were effective in promoting desired behavior, and that measures of motivation strength and autonomous motivation had additional positive main effects. We did not find evidence that controlled motivation affected behavioral outcomes, nor did we observe any significant interaction effects. Moreover, we consistently found that the default nudges indeed had incremental value on top of existing motivation. Together, these studies imply that both defaults and motivation affect behavior, such that the default sets the anchor from which people can adjust according to the type and strength of their motivation.

In **Chapter 5**, we ran an online vignette study to investigate how public acceptability of nudges relates to aspects of self-regulation capacity and motivation. Across three different types of nudges (default, portion size, and rearrangement) and across two behavioral domains (healthy eating and sustainable eating), we showed that autonomous motivation was a robust predictor of nudge acceptability, but that elements of self-regulation capacity had little impact on ratings of acceptability. Altogether, this study showed that those who want to perform the desired behavior are more likely to accept it, but that capacity to do so does not play a role in predicting acceptability.

In the final chapter of this dissertation, **Chapter 6**, I summarize the main findings of this dissertation in light of the main aim of the dissertation, outline the most important theoretical and practical implications, reflect on the limitations of the empirical work, and pave the road for future research.

Chapter 2

Beyond discrete choices – Investigating the effectiveness of a proximity nudge with multiple alternative options

This chapter is based on:
Van Gestel, L. C., Adriaanse, M. A., & De Ridder, D. T. D. (2020). Beyond discrete choices Investigating the effectiveness of a proximity nudge with multiple alternative options. *Frontiers in Psychology*, *11*. doi: 10.3389/fpsyg.2020.01211

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LG, MA, and DR contributed to the conception and design of the study. LG performed the data analysis and wrote a first draft of the manuscript. MA and DR provided substantial comments and suggestions for improvement.

Abstract

Nudges are defined as small adjustments in the choice architecture that stimulate desirable behavior. Nudging techniques can be used as a promising policy tool, but research has hardly systematically taken into account the complexity of the situation in which nudges have been implemented. In the current studies, we investigated the effectiveness of a proximity nudge on food choice in a realistic situation with multiple options in the immediate surroundings of the target option. In two studies, we presented participants from a community sample with an assortment of either three or nine different types of chocolate. For half of the participants, the target chocolate was placed most proximally on the table. Across two studies, we demonstrated that the proximity nudge was effective in stimulating the choice for a specific piece of chocolate in a simple and more complex situation. Results were further qualified by Bayesian analyses, which revealed most support for the hypothesis that the proximity effect existed in both the conditions with three and nine options, regardless of the number of options in the choice set. Results imply that the proximity effect can remain robust in realistic situations that include multiple options in the immediate environment to choose from.

In recent years, more and more research has been devoted to interventions called nudges: small adjustments in the choice architecture that stimulate a specific choice option, without forbidding alternative options or financially interfering with them (Thaler & Sunstein, 2008). While many studies have shown promising results, surprisingly little attention has been paid to the hypothesized working mechanisms or boundary conditions of their effectiveness. A recent scoping review revealed that the majority of published studies aimed to demonstrate the effectiveness of nudges in a particular setting (Szaszi, Palinkas, Palfi, Szollosi, & Aczel, 2018). Oftentimes, these settings are rather simplified settings in which a decision maker has to decide between choosing one out of two alternatives. For example, in the realm of health behavior, studies have focused on the likelihood of consumption of one available snack (Maas, De Ridder, De Vet, & De Wit, 2012) or on consumption of one out of two alternatives (Privitera & Zuraikat, 2014). In other behavioral fields, such as sustainable behavior, similar simplified settings have been used to study the effectiveness of nudges, such that participants are given the choice between one green and one grey energy plan (Vetter & Kutzner, 2016). In reality, however, people often face a complex choice of choosing between multiple products for consumption or multiple energy plans. This is reflected in field studies on nudging (Cadario & Chandon, 2020), where there are multiple alternative options in the wider environment, but a systematic comparison of nudge effectiveness with multiple alternative options in the immediate environment is missing. Therefore, in order to move the field forward, we suggest that more systematic research is needed where nudges are studied in more realistic, complex situations that involve multiple options.

People in Western societies have an ever-increasing number of options to choose from in a wide variety of settings. To illustrate, many supermarkets have increased their assortments such that consumers nowadays have up to an entire isle with different varieties of cereal, supposedly enabling them to choose their most preferred option. Yet, many studies have documented limitations to human decision making in terms of being unable to consider a wide variety of options (Schwartz, 2004). Investigating the effectiveness of nudges in complex contexts that involve a multitude of options is thus important as these types of choices are the choices that people struggle with particularly, and which they encounter increasingly often. Therefore, in the current set of studies, we aim to investigate whether the effectiveness of a nudge is dependent on the number of alternatives to choose from. We do so by investigating the effectiveness of a proximity nudge that employs the distance of the desired alternative as a nudge to encourage a specific choice.

Nudging and Choice Architecture Interventions

Nudges are defined as small adjustments in the choice architecture that stimulate a specific choice. They make strategic use of biases or rules of thumb that guide people's behavior, such as the status quo bias (Samuelson & Zeckhauser, 1988) or the social proof heuristic (Cialdini, 2009). Inherent to the definition of nudges is that alternative options

remain readily available and are not forbidden or financially made less attractive. Nudging as such is thought to be an umbrella term for a wide variety of interventions that alter cues in the physical and/or social environment to promote a choice that is deemed desirable by the choice architect. Hence, many different types of nudges have been applied across various behavioral fields. Prototypical examples range from defaults that create an opt-out system as opposed to an opt-in system (e.g., Johnson & Goldstein, 2003; Madrian & Shea, 2001; Pichert & Katsikopoulos, 2008) to making desirable behavior more salient (e.g., Wilson, Buckley, Buckley, & Bogomolova, 2016) or placing the desirable option more proximally (Hunter, Hollands, Couturier, & Marteau, 2018; Maas et al., 2012). These and other types of nudges have been applied in a wide variety of domains, including health, sustainable and prosocial behavior.

Although there are notable differences between these nudging interventions, they all share the principle of making it easier to perform the desirable behavior, which is the cornerstone of the idea behind nudging. Building on research on human decision making and information processing that highlights two systems of thinking (intuitive vs. reflective; see Evans, 2008, for an overview), nudges are assumed to align with automatic processes (Marteau, Ogilvie, Roland, Suhrcke, & Kelly, 2011). That is, nudges aim to steer behavior without taxing cognitive resources by making strategic use of automatic tendencies. This implies that nudging interventions are in principle not dependent on cognitive capacity in order to be effective. Preliminary evidence seems to be in line with this proposition, as it has been shown that increasing the distance towards a bowl filled with M&Ms decreases the likelihood of taking any snacks, regardless of cognitive resources (trait or state; Hunter et al., 2018). Similarly, in a study investigating the use of sugar tongs as a means of increasing the effort needed to acquire a certain snack, it was found that the effect on intake is not dependent on the availability of cognitive resources (state; Brunner, 2013). At the same time, there is suggestive evidence indicating that nudges are more effective in the crucial circumstances when deliberate reasoning is inhibited due to reduced self-control capacity (Salmon, Fennis, De Ridder, Adriaanse, & De Vet, 2014). Nevertheless, these studies altogether seem to imply that nudges capitalize on effortless processes involved in the decision-making process.

Proximity

One of the most typically studied nudges in the domain of food choice relies on the proximity effect: the phenomenon that people are most likely to choose an option that is presented most proximally to them. It has, for example, been shown that placing an unhealthy snack further away from a participant decreases both the likelihood of consuming that snack as well as the amount of consumption (Maas et al., 2012). The effect on likelihood of consumption was recently replicated amongst participants who did not move the bowl filled with snacks such that the distance manipulation remained intact (Hunter et al., 2018). A recent review of positional influences further revealed a positive effect in 16 out of 18 studies (Bucher et al., 2016). Yet, when more options are on offer,

results thus far seem mixed. One study provided suggestive evidence for the existence of a proximity effect in a so-called competitive food environment, where people were offered the choice between two snack options that differed in healthiness (Privitera & Zuraikat, 2014). In this study, it was shown that, even though participants reported a higher liking for the unhealthy snack (popcorn) than for the healthy snack (apple slices), participants consumed most of the snack that was placed most proximally to them. A recent experiment which employed a similar design with one healthy and one unhealthy snack, however, revealed different results. This study showed that the likelihood of consuming the unhealthy snack (M&Ms) was affected by its own proximity such that it was more often consumed when placed proximally. The likelihood of consuming the healthy snack (raisins), however, was not affected by its own proximity nor by the proximity of the unhealthy snack (Hunter, Hollands, Pilling, & Marteau, 2019). Altogether, these studies point out that there is a need to study the proximity effect in more complex situations.

In this light, it is striking to observe that the current evidence base of nudging effectiveness lacks a systematic approach in regard to the number of options in the immediate environment. On the one hand, nudges have been studied extensively in field studies where there are numerous options in the wider environment to choose from (Cadario & Chandon, 2020). To illustrate, a recent field study on the effectiveness of a repositioning nudge was conducted in a kiosk that included up to 179 food products (Van Gestel, Kroese, & De Ridder, 2018). These types of field studies have been conducted in a wide variety of settings with varying levels of complexity in the wider environment, but a systematic investigation of the possible impact that the level of complexity may have is missing. On the other hand, experimental research is mostly limited to studies on binary choices of either or not choosing, or of choosing a desirable (e.g., sustainable, healthy, etc.) option over an undesirable (e.g., unsustainable, unhealthy, etc.) option. To illustrate, studies on proximity have typically investigated the likelihood of consumption of one snack when there is only one snack on offer (Hunter et al., 2018; Maas et al., 2012) or consumption of one out of two options (Hunter et al., 2019; Privitera & Zuraikat, 2014). These experimental studies thus often lack a more realistic complex situation with multiple alternative options in the immediate proximity of the target option. Therefore, to advance the knowledge base and effective implementation of the nudges in daily practice, it is essential to acquire a further understanding of the conditions under which the effectiveness remains existent. In particular, a stronger focus on the number and type of alternatives is called for, since there may be aspects inherent to the choice set at hand that complicate the decision-making process.

Choice Sets

Research in behavioral judgment and decision making has highlighted the importance of considering the wider choice context in nudging research. Research on choice overload, for example, has suggested that, even though people tend to cherish the idea of freedom of choice, the more options there are to choose from, the more complicated the decision

becomes. Consequently, decision outcomes may not always be optimal and may reflect subjectively or objectively suboptimal decisions (Schwartz, 2004). And while the necessary precedents as well as the behavioral and affective consequences of choice overload are debated upon (Chernev, Böckenholt, & Goodman, 2015; McShane & Böckenholt, 2018; Scheibehenne, Greifeneder, & Todd, 2010), the phenomenon at least illustrates the importance of considering the number and type of alternative options when stimulating one particular option. In fact, for the study of nudges, it is important to consider the wider choice architecture when introducing one small adjustment to it. Moreover, given that nudges are thought to capitalize on effortless processes, it is important to examine whether the effects remain robust in exactly those circumstances that require more effort to reach a decision. Our goal in the current studies is not to manipulate choice overload as such, but rather to systematically investigate whether the number of options in a choice set impact nudge effectiveness.

The Current Studies

In the current studies, we aim to systematically investigate whether the proximity effect on the likelihood of choosing a certain option for consumption remains robust in situations where there are more alternatives to choose from. Having more options to choose from increases the complexity of the decision and may spread attention over these alternatives, thereby making it a more challenging environment for the nudge to stimulate desirable behavior. Yet, it is often argued that nudges capitalize on effortless processes, which could imply that nudges remain effective in exactly these more challenging situations where more effort is called for in order to reach a decision. In two studies, we let participants pick a piece of chocolate out of an assortment of either three or nine alternatives with equal utility. For half of the participants, we restructured the assortment structure so that the target chocolate was placed more proximally to the participant. Across these studies, we focused on the likelihood of choosing a particular option for consumption, rather than the likelihood of consumption or the amount of consumption. Inspired by research on choice overload, we also measured subjective difficulty and experienced doubt while making the choice. No a priori hypotheses were formulated for these measures.

Study 1

Method

Participants and design. Study 1 was conducted as part of an alumni event of Utrecht University. In total, one hundred thirty-four (85 women, 48 men, for 1 participant gender was not recorded) participants, with an average age of 41.86 (*SD* = 18.22), participated in the study. The study used an experimental 2 (number of options: 3 vs. 9) X 2 (nudge: absent vs. present) between-subjects design. The main dependent variable was whether or not participants chose the target chocolate (which was positioned most proximally in the nudge conditions).

Procedure. Upon arrival at the alumni event, people were invited to partake in the study. If participants indicated to be willing to participate in the study, they received a

questionnaire which was color coded with a red, blue, green, or yellow header in order to randomize participants over the four conditions. After having filled out the questionnaire, participants could hand it in at the location with the matching color code (i.e., participants who filled out a questionnaire with a green header were requested to hand it in at the table with a green label). When participants handed in their questionnaire, they were informed that they could choose a piece of chocolate as a reward, after which we would ask them a few more questions about their choice. The presentation of these chocolates differed between the four conditions such that there were either three or nine options and such that one of the options was placed most proximally or not. After having picked a piece of chocolate, participants were asked to fill out a follow-up questionnaire, which, upon completion, was stapled to the original questionnaire by one of the experimenters.

Materials.

Primary questionnaire. Participants initially received the primary questionnaire, which was a Dutch version of the Rational Experiential Inventory – Short Form (Norris, Pacini, & Epstein, 1998; Witteman, Van den Bercken, Claes, & Godoy, 2009). This questionnaire was used for exploratory purposes, which were beyond the purpose of the current study, and simultaneously served as the starting point of the study.

Choice of chocolates. Participants were ostensibly rewarded for filling out the primary questionnaire with a piece of chocolate of their choice. In line with previous studies on choice overload (e.g., Iyengar & Lepper, 2000), we took care in controlling for prior preferences. The chocolates used in the current study were Quality Street chocolates, which are relatively unfamiliar amongst the Dutch population. We chose these chocolates as our choice set because there is a large enough variety of options. Thereby, the choice was reduced to a decision between different chocolates of different shapes and colors. All chocolates were sorted and each type was presented in a separate bowl. Throughout the study, careful attention was paid to the number of chocolates presented in each bowl, and the bowls were frequently refilled so as not to install an implicit norm indicating a popular piece of chocolate (Prinsen, De Ridder, & De Vet, 2013).

The number of options as well as the presentation of these options differed in the four conditions. In the conditions with three options, participants could choose between three alternatives presented in three separate bowls, while in the conditions with nine options, participants could choose between nine alternatives presented in nine separate bowls. The three types of chocolates that were used in the conditions with three options were different from the nine types of chocolates that were used in the conditions with nine options. Within those conditions, the target chocolate remained the same. In the conditions without the nudge, the options were presented as three in a row, or as a matrix of three by three. In the conditions with the nudge, one of the options was presented more proximally. Importantly, although the bowl filled with the target chocolate was placed more proximally, all alternative options remained within arm's reach (see Figure 2.1 for a graphic overview of the set-up).

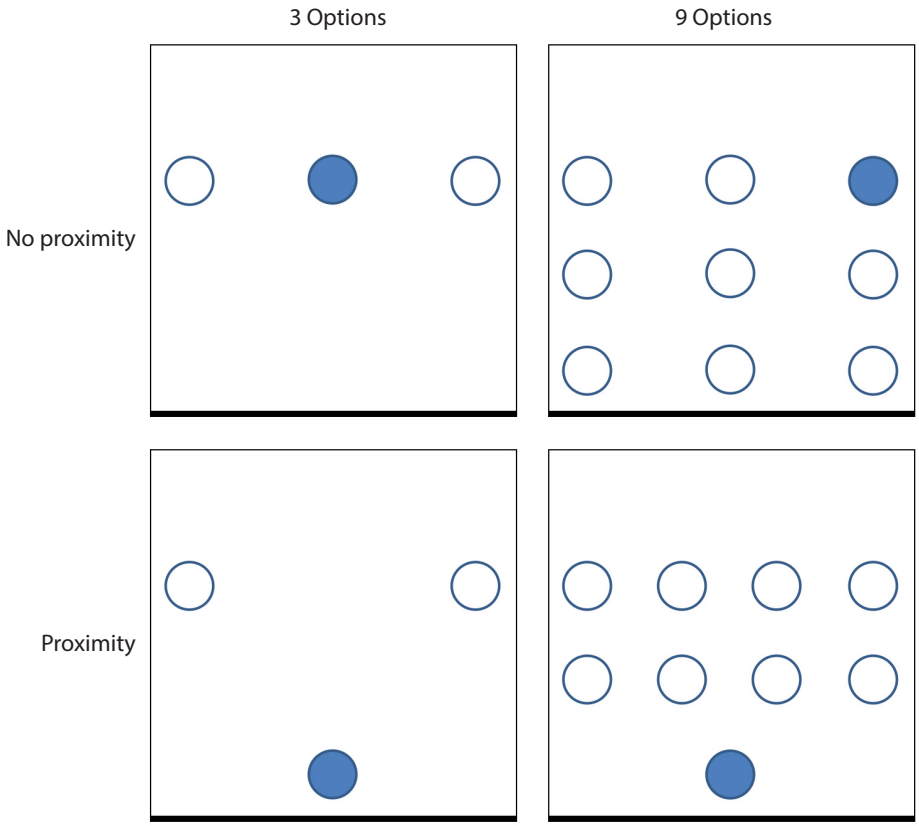

Figure 2.1. Schematic overview of the set-up in Study 1. The squares represent a table, and the thicker lines at the bottom represent the side of the table from which participants would approach the set-up. The circles represent bowls with chocolate, and the filled circle represents the bowl with the target chocolate.

Follow-up questionnaire. The follow-up questionnaire, which was administered after participants had chosen their piece of chocolate, contained the main variables of interest. First, participants were presented with pictures of the chocolates, and were asked to encircle the chocolate they had just chosen. Next, we asked participants questions about their experience with the decision-making process, focusing on the subjective difficulty of the choice ('How difficult was it to choose from the different options?'), satisfaction with the choice ('How satisfied are you with your choice?'),[1] and experienced doubt (To

1. During the study, it was observed that some participants tasted the chocolate before filling out the question on satisfaction, while others did not. Therefore, results are not truly indicative of the satisfaction with the decision process, so results will not be reported in the results section. No significant results were found for this dependent variable.

what extent did you doubt your choice?'). These questions were asked on 7-point Likert scales, ranging from 1 (*not at all*) to 7 (*very much*). Next, participants were asked if they were familiar with Quality Street chocolates (*yes*/*no*), and whether they had an a priori preference (*yes*/*no*). Finally, participants reported their age and gender.

Data analysis plan. We used both frequentist and Bayesian statistics in R. The main frequentist analysis was a stepwise logistic regression on the dependent variable whether or not participants had chosen the target chocolate. In the first block, only a main effect of the nudge was investigated. In the second block, a main effect of the number of options was added. In the third and final block, the interaction effect of the proximity nudge and number of options was added. Follow-up analyses consisted of ANOVAs with subjective difficulty and experienced doubt as dependent variables.

Besides, we conducted a logistic regression with Bayesian statistics using the R package Bain (Gu, Mulder, & Hoijtink, 2018). By conducting these analyses, we were able to account for base rate likelihoods (33% chance of choosing a particular piece of chocolate if presented with three options vs. 11% chance of choosing a particular piece of chocolate if presented with nine options). We did this by multiplying the observed probabilities of choosing the target chocolate in the conditions with three options with a weight of 3, while multiplying the observed probabilities of choosing the target chocolate in the conditions with nine options a weight of 9. Moreover, we used these additional analyses to evaluate the evidential base for three separate, informative, hypotheses. The statistical hypotheses that were evaluated and compared were: 1) The nudge is ineffective no matter the number of options and there is no interaction effect; 2) The nudge is effective in both the condition with three and nine options, but there is no interaction effect; 3) The nudge is effective in both the condition with three and nine options, and there is an interaction effect such that the nudge is more effective in the condition with nine options than in the condition with three options.

Results

Descriptives. Table 2.1 presents the descriptive statistics of all variables under study.

Randomization check. Across the four conditions, participants did not differ from each other in age ($F < 1$, $p = .993$) or gender ($\chi^2(3) = 4.27$, $p = .234$), indicating successful randomization of participants. Exactly half of the participants indicated to be familiar with Quality Street chocolates. A large majority of 113 participants indicated to have had no prior preference when selecting a piece of chocolate.

Main analyses. In order to analyze the main research question, a logistic regression model was built using the stepwise method. In step 1, the main effect of the nudge was added. This model provided significant model fit, $\chi^2(1) = 5.42$, $p = .020$, and revealed a main effect of the nudge, $b = 0.85$ ($SE = 0.37$), $p = .021$, $OR = 2.33$, *95% CI* [1.14, 4.87], implying that participants were more than twice as likely to choose the target chocolate when this option was positioned proximally than when it was not positioned proximally. In step 2, the main effect of the number of options was added. Adding this independent

Table 2.1

Descriptive statistics for the sample characteristics and main variables of interest by condition for Study 1

Characteristics	Group				All participants
	3 options, no proximity	3 options proximity	9 options, no proximity	9 options, proximity	
n	37	34	34	29	134
Age (*M*(*SD*))	41.65 (19.61)	42.62 (18.57)	41.32 (17.12)	41.86 (18.16)	41.86 (18.22)
Gender (%(*n*))					
Male	45.95 (17)	41.18 (14)	23.53 (8)	31.03 (9)	35.82 (48)
Female	54.05 (20)	58.82 (20)	73.53 (25)	68.97 (20)	63.43 (85)
NA	0.00 (0)	0.00 (0)	2.94 (1)	0.00 (0)	0.75 (1)
Difficulty (*M*(*SD*))	2.38 (1.55)	2.59 (1.48)	2.97 (1.71)	3.10 (1.74)	2.74 (1.63)
Doubt (*M*(*SD*))	2.78 (1.69)	2.65 (1.69)	2.94 (1.65)	2.79 (1.42)	2.79 (1.61)
Satisfaction (*M*(*SD*))	5.03 (1.69)	5.12 (1.30)	5.09 (1.63)	5.24 (1.64)	5.11 (1.56)
Familiar (%(*n*))					
Yes	64.86 (24)	50.00 (17)	44.12 (15)	37.93 (11)	50.00 (67)
No	35.14 (13)	50.00 (17)	55.88 (19)	62.07 (18)	50.00 (67)
Preference (%(*n*))					
Yes	10.81 (4)	2.94 (1)	23.53 (8)	27.59 (8)	15.67 (21)
No	89.19 (33)	97.06 (33)	76.47 (26)	72.41 (21)	84.33 (113)
Target chocolate chosen (%(*n*))	37.84 (14)	64.71 (22)	14.71 (5)	24.14 (7)	35.82 (48)

variable significantly improved model fit, $\chi^2(1) = 15.38$, $p < .001$, so that the model again fitted the data well, $\chi^2(2) = 20.80$, $p < .001$. Adding the number of options to the model did not change significance for the main effect of the nudge, $b = 0.93$ ($SE = 0.39$), $p = .019$, $OR = 2.53$, *95% CI* [1.18, 5.57]. The model also revealed a main effect of the number of options, $b = -1.53$ ($SE = 0.41$), $p < .001$, $OR = .22$, *95% CI* [0.09, 0.47], indicating that participants were about 4.5 times more likely to choose the target chocolate when offered three options than when offered nine options. This ratio was to be expected given differing base rates. In step 3, the interaction effect of the nudge and the number of options was added. Adding this interaction effect did not improve model fit, $\chi^2(1) = 0.36$, $p = .550$. In line with that, the interaction effect did not prove to be significant, $b = -0.50$ ($SE = 0.82$), $p = .548$, $OR = 0.61$, *95% CI* [0.12, 3.12].

Excluding those who indicated to have had a prior preference ($n = 21$) did not change the pattern of results, although the significant main effect of the nudge became marginally significant ($p = .056$ in step 1, $p = .068$ in step 2). Similarly, including age and gender as covariates did not change the pattern or significance of the main results, nor did these covariates have a significant effect on the likelihood of choosing the target chocolate.

Bayesian analyses. Bayesian analyses revealed relatively little evidence for the first hypothesis stating the absence of any nudge or interaction effect compared with its complement, BF_{1c} = 2.68. Contrary, relative to its complement, there seems to be strong evidence for the effect of the nudge in both the condition with three and nine options, BF_{2c} = 15.75. Finally, relative to its complement, there seems to be some positive evidence of both an effective nudge and an interaction effect with the number of options, BF_{3c} = 4.44. Relative to each other, positive evidence for the second evaluated hypothesis – that the nudge is effective in both the condition with three and nine options and that this effect is not moderated by the number of options – was found, BF_{21} = 5.87 and BF_{23} = 5.85.

Follow-up analyses. Follow-up analyses revealed no main effect of the nudge (p = .543) on subjective difficulty, nor an interaction effect with the number of options (p = .891). However, results did reveal a marginally significant main effect of the number of options on subjective difficulty, $F(1, 130) = 3.89$, $p = .051$, $\eta_p^2 = .03$, such that participants in the condition with three options found it less difficult (M = 2.48, SD = 1.51) than participants in the condition with nine options (M = 3.03, SD = 1.71) to reach a decision. Regarding experienced doubt, no main or interaction effects were found (all ps > .591).

Discussion

Study 1 revealed suggestive evidence for the effectiveness of the nudge in both the condition with three and nine options. In this study, we found strongest evidence for the hypothesis that this effect is present regardless of the number of options to choose from. Follow-up analyses suggested that it was easier for participants to choose a piece of chocolate when presented with few rather than a lot of alternatives, but this effect was only marginally significant. Overall, Study 1 revealed interesting trends but was rather underpowered. Therefore, we decided to conceptually replicate Study 1 with a larger sample and an improved set-up of the experiment.

Study 2

Method

Participants and design. Study 2 was conducted at the Dutch National Health Fair. Based on the results of Study 1, the required sample size was calculated from analytical expressions for the relation between power and sample size in Moerbeek and Maas (2005), while ignoring the multilevel data structure. For this sample size calculation, we used the odds ratio for the main effect of the nudge in Study 1 (i.e., OR = 2.53) and the main effect of the number of options (i.e., OR = 0.22). These expressions assume equal sample sizes in each of the four groups of the factorial design. As these sample sizes varied slightly in Study 1, the power is likely to be overestimated by a few percent. The analysis revealed that, in order to replicate a statistically significant main effect of the nudge with 80% power, at least one hundred and ninety-four participants needed to be recruited. An additional sensitivity analysis further revealed that with four hundred and ten participants we would be able to achieve 80% power with effect sizes that would deviate with 25% from those found in Study 1 (i.e., a 25% smaller odds ratio for the main effect of the nudge

(*OR* = 1.90), and a 25% larger odds ratio for the main effect of the number of options (*OR* = 0.16)).

In total, we had four days of data collection which resulted in four hundred twelve (353 women, 58 men, for 1 participant gender was not recorded) participants, with an average age of 44.63 (*SD* = 17.56). The study used a quasi-experimental 2 (number of options: 3 vs. 9) X 2 (nudge: absent vs. present) between-subjects design. The main dependent variable was whether or not participants chose the target chocolate (which was positioned most proximally in the nudge conditions).

Procedure. The National Health Fair lasted four days. Each day was divided in a morning session and an afternoon session and in each of the sessions one of the conditions was set up. The order in which the conditions were taking place was counterbalanced, so that each condition was run in one morning session and one afternoon session, as well as once during a workday and once during a day in the weekend.

Participants were welcomed at the stand and were asked if they wanted to participate in a study on food choices. After giving informed consent, the study started with an unrelated questionnaire for which they would be rewarded with a piece of chocolate. Just as in Study 1, participants were provided with a follow-up questionnaire in which we asked questions about the chocolate they had just chosen. After having filled out all questionnaires, participants were thanked for their participation.

Materials.

Primary questionnaire. At the start of the study, participants received a bogus questionnaire that we used to enforce the cover story that the chocolate was a reward.

Choice of chocolates. Participants were ostensibly rewarded for filling out the primary questionnaire with a piece of chocolate of their choice. We used the same chocolates as in Study 1. Study 2 largely resembled Study 1 in the design, but a different set-up of the presentation of the chocolates was used in the conditions with nine options. First, in the condition without the nudge, the bowl with the target chocolate was placed centrally rather than in the corner. Second, in the condition with the proximity nudge, a similar kind of triangular structure as in the condition with three options was used (see Figure 2.2 for a graphic overview of the set-up).

Follow-up questionnaire. We used the same follow-up questionnaire as in Study 1.[2]

Data analysis plan. We used the same data analysis plan as in Study 1 and again conducted both frequentist and Bayesian analyses, this time in a more confirmatory manner.

2. Again, results for satisfaction will not be reported in the results section for the same reason as in Study 1. However, in Study 2 results indicated a main effect of the number of options on satisfaction, $F(1, 402) = 9.07$, $p = .003$, $\eta_p^2 = .02$, such that participants were more satisfied when offered nine options ($M = 4.99$, $SD = 1.78$) than when offered three options ($M = 4.44$, $SD = 1.95$).

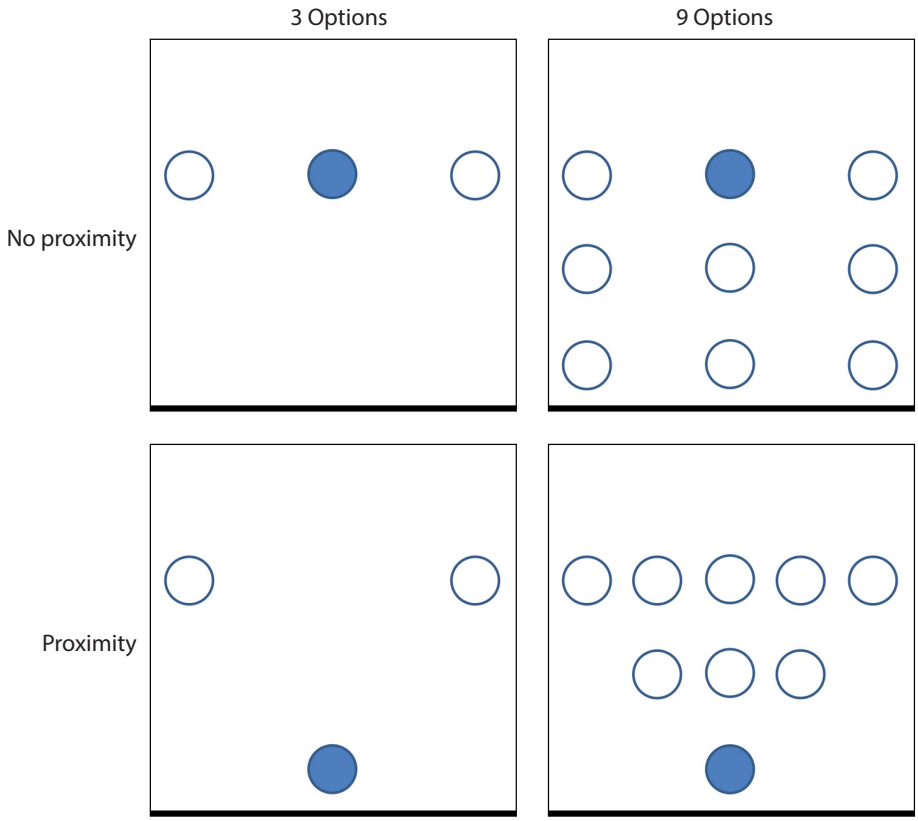

Figure 2.2. Schematic overview of the set-up in Study 2. The squares represent a table, and the thicker lines at the bottom represent the side of the table from which participants would approach the set-up. The circles represent bowls with chocolate, and the filled circle represents the bowl with the target chocolate.

Results

Descriptives. Table 2.2 presents the descriptive statistics of all variables under study.

Randomization check. Across the four conditions, participants did not differ from each other in age ($F < 1$, $p = .766$) or gender ($\chi^2(3) = 0.36$, $p = .949$), indicating successful randomization of participants. Over half of the participants ($n = 232$) indicated to be familiar with Quality Street chocolates. A large majority of 325 participants indicated to have had no prior preference when selecting a piece of chocolate.

Main analyses. In order to analyze the main research question, a logistic regression model was built using the stepwise method. In step 1, the main effect of the nudge was added. This model provided significant model fit, $\chi^2(1) = 12.19$, $p < .001$, and revealed a main effect of the nudge, $b = 0.74$ ($SE = 0.22$), $p < .001$, $OR = 2.11$, *95% CI* [1.38, 3.23], implying that participants were more than twice as likely to choose the target chocolate

Table 2.2

Descriptive statistics for the sample characteristics and main variables of interest by condition for Study 2

Characteristics	Group				All participants
	3 options, no proximity	3 options proximity	9 options, no proximity	9 options, proximity	
n	83	95	124	110	412
Age (*M*(*SD*))	45.95 (18.70)	43.96 (18.37)	43.66 (17.14)	45.32 (16.53)	44.63 (17.56)
Gender (%(*n*))					
Male	14.46 (12)	13.68 (13)	15.32 (19)	12.73 (14)	14.08 (58)
Female	84.34 (70)	86.32 (82)	84.68 (105)	87.27 (96)	85.68 (353)
NA	1.20 (1)	0.00 (0)	0.00 (0)	0.00 (0)	0.24 (1)
Difficulty (*M*(*SD*))	2.89 (1.74)	2.40 (1.67)	2.50 (1.57)	2.82 (1.89)	2.64 (1.72)
Doubt (*M*(*SD*))	3.04 (1.76)	2.52 (1.62)	2.69 (1.76)	2.79 (1.78)	2.75 (1.74)
Satisfaction (*M*(*SD*))	4.22 (2.04)	4.68 (1.82)	4.93 (1.85)	5.02 (1.73)	4.75 (1.90)
Familiar (%(*n*))					
Yes	55.42 (46)	58.95 (56)	54.84 (68)	56.36 (62)	42.48 (175)
No	42.17 (35)	38.95 (37)	44.35 (55)	43.64 (48)	56.31 (232)
NA	2.41 (2)	2.11 (2)	0.81 (1)	0.00 (0)	1.21 (5)
Preference (%(*n*))					
Yes	16.87 (14)	18.95 (18)	22.58 (28)	20.91 (23)	20.15 (83)
No	81.93 (68)	82.05 (77)	75.81 (94)	78.18 (86)	78.88 (325)
NA	1.20 (1)	0.00 (0)	1.61 (2)	0.91 (1)	0.97 (4)
Target chocolate chosen (%(*n*))	38.55 (32)	54.74 (52)	14.52 (18)	28.18 (31)	32.28 (133)

when this option was positioned proximally than when it was not positioned proximally. In step 2, the main effect of the number of options was added. Adding this independent variable significantly improved model fit, $\chi^2(1) = 29.57$, $p < .001$, so that the model again fitted the data well, $\chi^2(2) = 41.76$, $p < .001$. Adding the number of options to the model did not change significance for the main effect of the nudge, $b = 0.72$ ($SE = 0.22$), $p = .001$, $OR = 2.06$, *95% CI* [1.33, 3.21]. The model also revealed a main effect of the number of options, $b = -1.19$ ($SE = 0.22$), $p < .001$, $OR = 0.31$, *95% CI* [0.20, 0.47], indicating that participants were more than three times more likely to choose the target chocolate when offered three options than when offered nine options. Again, this ratio was to be expected given differing base rates. In step 3, the interaction effect of the nudge and the number of options was added. Adding this interaction effect did not improve model fit, $\chi^2(1) = 0.19$, $p = .665$. In line with that, the interaction effect did not prove to be significant, $b = 0.19$ ($SE = 0.45$), $p = .665$, $OR = 1.22$, *95% CI* [0.50, 2.96].

Excluding those who had indicated to have had a prior preference ($n = 83$) did not

change the pattern or significance of the main results. Similarly, including age and gender as covariates did not change the pattern or significance of the main results, nor did these covariates have a significant effect on the likelihood of choosing the target chocolate.

Bayesian analyses. Bayesian analyses revealed little evidence for the first hypothesis stating the absence of any nudge or interaction effect compared with its complement, BF_{1c} = 0.69. Contrary, relative to its complement, there seems to be relatively strong evidence for the hypothesis stating that the nudge is effective in both the condition with three and nine options, BF_{2c} = 11.00. Finally, relative to its complement, there seems to be strong evidence of both an effective nudge and an interaction effect with the number of options, BF_{3c} = 41.41.

Relative to each other, results showed strong positive evidence for the second evaluated hypothesis – that the nudge is effective in both the condition with three and nine options and that this effect is not moderated by the number of options – relative to the first evaluated hypothesis which specified the absence of a nudge and interaction effect, BF_{21} = 16.03. Similarly, results showed relatively strong positive evidence for the third evaluated hypothesis – that the nudge is effective in both the condition with three and nine options and that this effect is moderated by the number of options – relative to the first evaluated hypothesis, BF_{31} = 6.98. Lastly, there seems to be slightly more support for the second hypothesis in comparison to the third hypothesis, but the Bayes factor could not substantially differentiate between the two, BF_{23} = 2.30. Altogether, the data imply that the proximity nudge was effective in both conditions, with a remaining possibility that the nudge effect was most pronounced in the condition with nine options.

Follow-up analyses. Follow-up analyses revealed no main effect of the nudge (p = .559) or number of options (p = .876) on subjective difficulty, but did reveal a significant interaction effect, $F(1, 408) = 5.64$, $p = .018$, $\eta_p^2 = .014$. Post-hoc comparisons revealed that in the condition with three options, participants experienced the decision as easier when the nudge was present ($M = 2.39$, $SD = 1.66$) than when the nudge was absent ($M = 2.89$, $SD = 1.74$), $p = .049$. In the condition with nine options, there was no difference between the two groups in subjective difficulty ($p = .175$). Regarding experienced doubt, there was no main effect of the nudge ($p = .194$) nor a main effect of the number of options (p = .891). There was, however, a marginally significant interaction effect, $F(1, 404) = 3.10$, p = .079, $\eta_p^2 = .01$. Post-hoc comparisons revealed that in the condition with three options, participants experienced less doubt when the nudge was present ($M = 2.51$, $SD = 1.62$) than when the nudge was absent ($M = 3.04$, $SD = 1.76$), $p = .043$. In the condition with nine options, there was no difference between the two groups in subjective difficulty ($p = .727$). Altogether, given the range of p-values, these exploratory analyses did not reveal robust evidence for any effects on subjective difficulty or experienced doubt.

Discussion

Study 2 replicated the main results of Study 1 and revealed strong evidence for the effectiveness of the proximity nudge on the likelihood of choosing the target chocolate

regardless the number of options involved. Follow-up analyses provided suggestive trends indicating decreased difficulty and experienced doubt due to the nudge in the condition with three options, but did not find a similar trend in the condition with nine options.

General Discussion

Over the last decade, research on nudging as a promising novel technique for promoting desirable behavior has emerged. Considerable efforts have been made in establishing the evidence base of certain nudges in certain settings, but a systematic investigation of the effectiveness of nudges in settings that vary in complexity has largely been missing. While research in behavioral judgment and decision making has highlighted the importance of taking the characteristics of a choice set into account (Schwartz, 2004), this has so far not been embedded in research on nudging and the proximity effect. On the one hand, field studies have investigated the effectiveness of several nudges in wider environments that contain numerous options (Cadario & Chandon, 2020), but lack a systematic interpretation of the complexity of that environment. On the other hand, most empirical research on nudging has been dedicated to investigations of choosing one particular option or not (Hunter et al., 2018; Maas et al., 2012), or of choosing that option over another option (Hunter et al., 2019; Privitera & Zuraikat, 2014). Yet, it is important to advance the field by creating a thorough understanding of what works, but also of when and under what circumstances these nudges work in more complex, realistic, settings with alternative options in the immediate surroundings. In the current set of studies, we therefore made a systematic comparison of the effectiveness of the proximity nudge with choice sets consisting of three or nine alternatives.

Across two studies with community samples, we found support for the effectiveness of the proximity nudge on food choice in a real-life simple choice context as well as a real-life complex choice context involving multiple options. Results showed that participants were more than twice as likely to select the target chocolate when the bowl was placed proximally to the participant, thus revealing strong support for the effectiveness of a proximity nudge. Frequentist statistics further revealed that this effect of the proximity nudge was not moderated by the number of options in the choice set. In both studies, strongest evidence was found for the hypothesis that the nudge is effective in both the condition with three and nine options, without interaction with the number of options in the choice set. However, in Study 2, some support was also found for the possibility that the effect is even more pronounced in the condition with nine options. Follow-up analyses revealed suggestive trends regarding post-decision evaluations of experienced doubt and subjective difficulty, but did not reveal robust effects across the two studies.

In the current studies, we used choice sets that consisted of either three or nine options as a first examination of the robustness of the proximity effect on food choice with differing number of options. Results demonstrated that proximity remained effective in stimulating the selection of a specific option across these choice sets, implying that

nudges can remain effective behavior change tools in more complex situations involving a multitude of alternative options. This is important for the ecological validity of studies on nudging, since people nowadays face an ever-increasing number of options to choose from in a wide variety of settings. Besides, research on behavioral judgment and decision making has revealed that aspects of the choice set, such as the size of the set or complexity of it, may affect behavior (Schwartz, 2004). Therefore, in order to move the field forward, we suggest that future studies should investigate the effects of different nudges in more realistic and complex situations, while systematically taking into account the number of options in the immediate environment of the decision maker.

In the current studies, we did not find a moderating role of the number of options on the effectiveness of nudging on food choice. Given the current results, we can thus conclude that the proximity effect remains effective if there are less than ten options in a choice set. However, it is important to highlight that in relation to research on choice overload, having nine options in a choice set may still be on the lower side of the spectrum (Chernev et al., 2015; Reutskaja, Lindner, Nagel, Andersen, & Camerer, 2018), and may in fact be about the right number of options for some consumer products (Shah & Wolford, 2007). It remains to be determined whether the effect of nudges is also unaffected by the number of options when the size of the choice set increases further beyond the nine options presented in the current studies. In addition, the chocolates used in the current studies differed from each other in small and rather trivial dimensions, such as color of the wrapper and shape of the chocolate. The difference in taste between the different chocolates was unknown to many of the participants. Yet, choice complexity not only increases with the number of options, but also with the number of attributes that belong to each option (Chernev et al., 2015). To illustrate, the choice for a specific electronic device over another device can be based on a combination of specifications such as battery life, memory and processor speed, thereby complicating the decision. Therefore, while the current operationalization presented participants to a more challenging situation than in other studies of the proximity effect, it is important to stress that still it is not indicative of extremely complex decisions. Future research is required to test whether the level of complexity of choices moderates the effectiveness of nudging interventions in more extreme situations. In line with research on choice overload (Chernev et al., 2015), we would expect the effect to of the nudge to become even more pronounced in these highly complex situations.

A major limitation of the current studies was that it was not feasible to vary the target chocolate across participants. Yet, it should also be noted that all options in the current experiment had the same utility. Apart from subjective preferences, which we tried to rule out as much as possible, none of the options was objectively superior or inferior to the other in order to simplify experimental control. This also implies that, in the current set of studies, we were strictly speaking not 'nudging for good', but rather stimulating the selection of one alternative over a variety of others. Whether or not the intention of the nudge aligned with the nudgee's goals, and how this altogether may impact the

effectiveness of the nudge, was therefore beyond the scope of the current studies. The results of the current studies therefore imply that proximity can be effective in stimulating a specific option of a choice set with the same utility, but care should be taken into translating these findings, especially in regard to stimulation of desirable behavior. For example, in the context of health promotion, it will be relevant to examine whether proximity can effectively nudge the selection of healthy food over a variety of unhealthier alternatives.

Future research could dive deeper in the effectiveness of nudges in the face of an overload of options. An intriguing possibility remains that nudging may in fact be especially effective in promoting specific behavior in such circumstances. Not only is there room for improvement considering that people may make suboptimal decisions in such circumstances (Schwartz, 2004), there may also be something inherent to nudging that prevails in such circumstances: when people do not have the motivation or capacity to make optimal decisions. Moreover, nudges are most effective in the absence of clear preferences (Venema, 2020), and choice overload is most likely to occur when there is no dominant option for which one has a preference (Chernev et al., 2015). An interesting possibility is thus that nudges are most effective when choice situations become very complex as in people having no clear preferences because of being confronted with (too) many options. In such situations, nudges may facilitate decision making and guide decision makers effectively through this complex situation. Therefore, future research should investigate the effectiveness of nudging in more complicated situations.

Moreover, in the current studies we explored effects of the proximity nudge on subjective experiences during the decision-making process. Future research can delve deeper into this by examining affective states prior to, during, and after making a decision. As nudges are intended to make the desirable option the easier choice, it would be interesting to examine whether this proposition is reflected in affective states as experienced by the decision maker. Moreover, better insights into the working mechanisms and boundary conditions of nudges and the proximity effect are required, not only for scientific progress, but also for practical and effective implementation in daily life. Current evidence seems to suggest that the proximity effect is driven by a decrease in perceived (physical) effort to obtain a particular option on offer, rather than by an increase in salience of that proximal option (Maas et al., 2012). Besides, it has been shown that the sight of proximal food activates eating-related cognitions and motor responses, thereby allowing for immediate interaction with the presented food (Junghans, Evers, & De Ridder, 2013). The present studies add that this effortless route to the most proximal option is not affected by the number of alternative options. However, a coherent or conclusive explanation for the proximity effect is still required. Overall, the findings of the current studies suggest that the proximity effect can effectively steer food choice in a realistic and complex situation, regardless of whether the choice set is small or moderate in size.

Chapter 3

Do nudges make use of automatic processing? Unraveling the effects of a default nudge under Type 1 and Type 2 processing

This chapter is based on:
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LG, MA, and DR conceptualized the research idea. LG developed the research design and wrote the Stage 1 manuscript in consultation with MA and DR. LG collected, analyzed, and interpreted the data and wrote the Stage 2 manuscript. MA and DR provided critical feedback on the manuscript.

Abstract

Nudges have become increasingly popular among policy makers as a tool to stimulate desirable behavior for individuals or society. One of the most prevailing assumptions of nudges is that they make use of automatic processing. Yet, this assumption has received little attention in experimental research. In two preregistered and high-powered studies, we investigated this hypothesized working mechanism by using a nudge that has most typically been described as a Type I nudge: defaults. In both studies, we used a scenario in which participants could choose from a list of green amenities, which were either preselected (opt-out condition) or not (opt-in condition). In Study 1, we investigated the effectiveness of this default nudge under Type 1 processing by manipulating cognitive load. In Study 2, we investigated its effectiveness under Type 2 processing by explicitly instructing half of the participants to deliberate upon their choice. Both studies revealed strong and robust evidence for the default effect. Study 1 further revealed that this default effect was statistically equivalent under cognitive load. Study 2 revealed that the default effect was not attenuated when participants deliberated upon their decision, but instead showed a main effect of deliberation. Together, this implies that default nudges are not dependent on elaborate processing in order to be effective, but that deliberation can in parallel lead to different choice outcomes.

Over the past decade, public policy makers have embraced choice architecture interventions as a means of stimulating desirable behavior for individuals or society at large. These interventions are oftentimes called nudges, which are defined as simple changes in the choice architecture that alter behavior in a predictable way without forbidding or interfering financially with any of the options (Thaler & Sunstein, 2008). This term, nudging, comprises a wide variety of interventions that share the idea of making the desirable behavior the easy option. Early nudging studies showed promising results on behavioral outcomes and revealed additional benefits of nudges as a policy tool such as ease of implementation and cost-effectiveness (Benartzi et al., 2017). The widespread implementation of these nudges is a novel development, but the behavioral principles on which they are based or not necessarily new. In fact, the idea of nudging is based on decades of research in psychology and human judgment and decision making (e.g., Evans, 2008; Kahneman, 2003). As such, nudges challenge rational choice theory and are rather based on the idea of bounded rationality (Kahneman, 2003; Simon, 1955). That is, nudges are thought to shape the environment in such a way that it supports individuals in making desirable decisions without taxing cognitive resources. For that reason, nudges have received a lot of positive reactions as a novel public policy tool, but also negative criticism regarding the ethics of nudging (e.g., Hausman & Welch, 2010).

Thus far, a large majority of the studies on nudging has investigated the effects of nudges on several behavioral outcomes, but few studies have focused on the working mechanisms or boundary conditions of nudges (Szaszi, Palinkas, Palfi, Szollosi, & Aczel, 2018), highlighting the need to investigate when and why nudges work. One of the pressing issues is that it is not clear to what extent nudges operate through automatic processes. It is often claimed that nudges take advantage of automatic processes by employing the very heuristics and biases that are often blamed as the reason for suboptimal decisions. By making strategic use of these 'flaws' in decision making, it is argued that nudges can stimulate desirable behavior. While the assumed automatic nature of the processes involved in nudging initially has been proposed as the foundation of all nudging interventions, experimental research on this premise remains rather scarce. Consequently, it remains unclear to what extent nudges take advantage of automatic processes, and what this means in practice.

In the present studies, we aim to experimentally study this basic premise of nudging by investigating the underlying cognitive mechanism of one of the nudges that has most consistently been described as relying on automatic processes: defaults. Previous research has investigated some of the hypothesized working mechanisms of defaults (e.g., Dinner, Johnson, Goldstein, & Liu, 2011; McKenzie, Liersch, & Finkelstein, 2006), but research on the more fundamental question of whether defaults take advantage of automatic processes is still rather scarce. In two studies, we aim to investigate the effectiveness of a default nudge under circumstances in which deliberation is either inhibited (by a cognitive load manipulation; Study 1) or stimulated (with instructions; Study 2). We first

review the literature on dual process theories and nudging in general, before focusing on defaults as the focal nudge in these studies.

Dual process models

The theoretical framework on which the concept of nudging was built is the dual system framework (Kahneman, 2011; Thaler & Sunstein, 2008). According to this, and some related frameworks (for an overview, see Evans (2008)), human judgment and decision making originates from two distinct cognitive systems: System 1 and System 2. System 1 is commonly described as automatic, heuristic-based, fast and frugal, and has typically been given responsibility for biased or erroneous decisions. System 2 is commonly described as deliberate, analytical, slow and effortful (Evans & Stanovich, 2013; Kahneman, 2011). System 2 processes typically demand working memory capacity, while System 1 processes demand fewer cognitive investments (De Neys, 2006; Evans & Stanovich, 2013). Given that people are boundedly rational and thus not always willing or able to invest cognitive effort, they often resort to System 1 processes. The idea behind nudging is to befriend these processes, rather than to fight them, in order to help people make more desirable decisions through making this desirable decision the easy decision.

Yet, current theorizing on dual processing has become more nuanced. As such, current theorizing speaks of two *types* of reasoning, with defining features and typical correlates, rather than two systems of reasoning, in which each system is governed by a list of features. Also, Type 1 processes are no longer seen as responsible for errors and biases (Bago & De Neys, 2017; Evans & Stanovich, 2013), but represent adaptive reasoning in their own right (Gigerenzer & Gaissmaier, 2011). There continues to be debate about the dual system frameworks (e.g., Melnikoff & Bargh, 2018), but most theorists would nevertheless agree that a distinction can be drawn between processes that require working memory resources (Type 2 processes) and processes that do not require working memory resources (Type 1 processes). Generally, it is thought that people act intuitively (Type 1) by default, and that reflective reasoning (Type 2) can intervene if people are willing and able to invest cognitive effort. This view has been referred to as the default-interventionist framework (Evans & Stanovich, 2013).

For nudging research, however, this revamped conceptualization of dual process theories bears important implications. As nudges are thought to take advantage of automatic processes, the question becomes what this exactly implies. One possibility is that under Type 1 processing, nudges become more effective. That is, as nudges take advantage of the heuristics and biases that are correlated with Type 1 processing, they should be more effective in exactly those circumstances where people are most likely to use these processes. Therefore, inhibiting Type 2 reasoning, for example by time pressure (Evans & Curtis-Holmes, 2005) or concurrent working memory load (Bago & De Neys, 2017), could in theory increase the effectiveness of nudges. Another possibility, however, is that the effectiveness of nudges is unaffected by inhibition of Type 2 processes. This would be in line with the idea that people are cognitive misers (Fiske & Taylor, 2013) and that Type

2 only intervenes when people are able and motivated to invest cognitive effort. That is, people generally rely on Type 1 processing and inhibiting Type 2 processing would not change that. Consequently, nudges could remain equally effective in those circumstances.

Another intriguing question is what happens to the effectiveness of nudges when Type 2 processing is stimulated. After all, the mere availability of cognitive resources does not necessarily imply engagement of Type 2 processes (Thompson, Turner, & Pennycook, 2011). These Type 2 processes are oftentimes stimulated with instructions to deliberate on a decision, for example by instructing participants to reason deductively (Evans, Handley, Neilens, & Over, 2010) or to provide reasons for choosing a particular option (e.g., Dijkstra, Van der Pligt, & Van Kleef, 2013; Horstmann, Ahlgrimm, & Glöckner, 2009). Research on biases has revealed that people are less biased in their reasoning skills when instructed to think deductively, provided that they have sufficient cognitive capacity to do so (Evans et al., 2010). Exposing someone to a nudge while instructing them to think carefully may thus render the nudge less effective. In order to investigate the effectiveness of nudging under Type 1 and Type 2 processing, we will make use of a nudge that has most consistently been put forward as relying on Type 1 processes: defaults.

Defaults

A default is an option that is preselected, such that, in the absence of an active decision, the decision maker will stick with the preselected option. Which option is set as the default can have considerable effects for the option that is chosen most frequently. Typically, in an opt-out system (with a default) frequencies are considerably higher than in an opt-in system (without a default). Defaults are commonly embedded in binary decisions (e.g., being an organ donor or not, or having a green or grey energy plan), but can also be used for more continuous measures (e.g., the amount of money donated, or the number of green amenities selected). While some nudges (e.g., reminders) can impact behavior via Type 2 processes, defaults are generally seen as the most prototypical example of nudging (e.g., Thaler & Sunstein, 2008) and have most consistently been classified as Type I (Hansen & Jespersen, 2013) or non-educative (e.g., Sunstein, 2016) nudges.

Examples abound of default effects across many behavioral domains such as sustainable behavior (Pichert & Katsikopoulos, 2008; Vetter & Kutzner, 2016) and financial behavior (Madrian & Shea, 2001). The most illustrative difference between having an opt-in and an opt-out system was revealed by Johnson and Goldstein (2003) who showed a dramatic difference in the proportion of citizens registered as organ donors. While those countries that adhered to an opt-in system had consent rates ranging from 4.25% to 27.5%, countries that adhered to an opt-out system had consent rates ranging from 85.9% to 99.98%. Whether this difference in consent rates as a matter of fact translates in higher donation rates has been debated, but the difference compellingly illustrates the power that defaults can have on behavior.

A recent meta-analysis of default effects (Jachimowicz, Duncan, Weber, & Johnson, 2019) further revealed that defaults can have a considerable influence on behavior, while

also noting substantial variation in effect-sizes. This meta-analysis showed that defaults have a considerable effect on behavior with an average medium to large effect size of $d = 0.68$, meaning that the likelihood of choosing a particular option is 0.68 *SD*s higher when this is set as the default compared to an opt-in situation. Yet, the authors also revealed significant heterogeneity in effect sizes, suggesting possible moderation. Regarding study characteristics, it was revealed that default effects are larger in consumer domains (than in non-consumer domains) and weaker for decisions pertaining to sustainable behavior (as opposed to decisions not pertaining to sustainable behavior).

Recently, more and more research has been devoted to studying the underlying mechanisms of defaults. To date, three major, but not mutually exclusive, underlying mechanisms of this default effect have been put forward (Jachimowicz et al., 2019; Johnson & Goldstein, 2003). First, it has been argued that defaults are effective because it requires effort, either physical or cognitive, to override the status quo. Research on the status quo bias (Samuelson & Zeckhauser, 1988) has demonstrated that people tend to disproportionally stick with the status quo. Consequently, choice architects can strategically create a status quo by setting a default option from which people can opt-out. Changing away from that default often involves physical or cognitive effort, and people may not be motivated enough to invest the required amount of effort into the decision in order to opt-out (Fiske & Taylor, 2013). Second, it has been shown that defaults imply endorsement by the choice architect (McKenzie et al., 2006). According to this explanation, the decision maker infers a recommendation from the choice architect and may decide to follow up on this recommendation. A series of studies by McKenzie and colleagues (2006) revealed that policy makers tend to leak their own preferences in setting the default option, and that decision makers tend to infer an implicit recommendation from the choice architect to choose the option that has been set as the default. Third, it has been suggested that changing away from the default is evaluated in terms of losing something already endowed (Dinner et al., 2011; Park, Jun, & Macinnis, 2000; Tversky & Kahneman, 1981). This explanation suggests a valuation shift such that the default option is valued relatively more merely because the decision maker envisions to own this option (Dinner et al., 2011).

While all three explanations of the default effect seem plausible and have received experimental support, thus far no coherent and robust explanation for the default effect has been found. It has been reasoned that all three explanations could explain default effects, depending on the situation and the type of default (Dinner et al., 2011). The previously alluded to meta-analysis by Jachimowicz and colleagues (2019) revealed moderation of the default effect by both endorsement and endowment, but not by effort, but caution should be taken in interpreting these results as these hypothesized mechanisms were coded afterwards rather than integral aspects of the studies included in the analysis.

Besides, there is little empirical research regarding the more fundamental question

of whether defaults make use of automatic processing. As far as we are aware of, there is currently only one published paper that explored Type 1 processing in relation to default effects (Gärtner, 2018). This study showed that there is no additional effect of time pressure on prosocial decisions when participants were presented with a default in a dictator game. Similarly, as far as we know, there is only one published paper that investigated Type 2 processing in relation to default effects (Steffel, Williams, & Pogacar, 2016). In a set of studies, these authors showed that the effect of a transparent default is attenuated when participants are instructed to articulate their preferences before choosing. In a follow-up study, it was shown that forcing participants to take extra time to choose did not attenuate the default effect, but simply having more time does not necessarily guarantee deliberation (Horstmann, Hausmann, & Ryf, 2010; Payne, Samper, Bettman, & Luce, 2008). Interestingly, this paper revealed that the default effect was attenuated because the manipulation to articulate preferences resulted in more balanced reasoning. In line with these results, we hypothesize that deliberation instructions will attenuate the default effect.

The current studies

To investigate the effectiveness of a default nudge under Type 1 and Type 2 processing, we conducted two studies. In the first study, we aim to load working memory capacity by a commonly used cognitive load manipulation, thereby inhibiting Type 2 processing among half of the participants. In the second study, we aim to stimulate Type 2 processing, by instructing half of the participants to think deliberately about their decision. We aim to manipulate these processing types concurrently with a task in which participants are asked to choose from a list of green amenities. For half of the participants, all options will be preselected as the default such that participants can opt-out if they want, while for the other half of the participants none of the options will be preselected such that participants can opt-in if they want. We chose this default, as previous research has demonstrated large overall effect sizes with sufficient variation between participants for it to be sensitive to other manipulations (Steffel et al., 2016).

Across both studies, we hypothesize that more green amenities will be selected if these are set as the default. In line with the default-interventionist perspective and in line with the study by Gärtner (2018), we further expect that this effect is not affected by cognitive load, such that the default is effective in both the condition with low and high load (Study 1). Further, we expect that deliberation instructions will activate Type 2 processes and will lead to a reasoned decision. Consequently, in line with previous research (Steffel et al., 2016), we expect that people are willing to deviate from the status quo. Thus, we expect that giving instructions to deliberate on the decision attenuates the default effect (Study 2). In both studies, we will explore for downstream effects on satisfaction, as this may be an important proxy for future behavior (Wirtz, Kruger, Scollon, & Diener, 2003).

Study 1

Study 1 aimed to investigate the effectiveness of a default nudge on sustainable behavior under Type 1 processing. Therefore, we subjected participants to a scenario in which they had just rented a newly constructed apartment. Participants were shown a list of green amenities in either an opt-in (no default) or opt-out (default) format and were asked to choose the amenities that they wanted to have. We aimed to inhibit Type 2 processing by manipulating cognitive load. Therefore, half of the participants had to remember a simple pattern of 4 dots, while the other half had to remember a highly difficult pattern of 5 dots. We expected to find a significant main effect of the default, and no interaction effect with cognitive load.

Method

Participants and design. The smallest effect size observed in Steffel and colleagues (2016) for the main effect of the default on the number of green amenities chosen was $\eta_p^2 =$ .37. Using this most conservative effect size for the main effect in G*Power (Faul, Erdfelder, Buchner, & Lang, 2009), and assuming power of 80% and a significance level of .05, we would require a total sample size of 16 participants to replicate this main effect. As we aimed to demonstrate that cognitive load did not affect the effectiveness of the default, we resorted to equivalence testing. We defined $d = -0.3$ as the lower bound and $d = 0.3$ as the upper bound, as these bounds approximately correspond with a meaningful effect of selecting 1 amenity more or less in the studies by Steffel and colleagues (2016). Using the TOSTER package in R (Lakens, 2017) with these bounds, and using 80% power and a significance level of .05, we would require a sample size of 191 participants per condition. In order to guarantee enough power for our proposed analyses with subsamples, we oversampled by 10%, resulting in a final sample size of 840 participants (507 female, 329 male, 4 Other/Rather not specify; $M_{age} = 34.17$, $SD_{age} = 13.64$).

Participants were recruited from Prolific Academic and we included adult participants with a UK nationality. In order to ensure quality of the data, we set approval rates to 95%. Participants were rewarded with £0.60 for their participation. We used a 2 (choice format: opt-in vs. opt-out) x 2 (cognitive load: low load vs. high load) between-subjects design with the number of green amenities chosen as dependent variable.

Procedure. The study was run on Gorilla (Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2020), a platform for online experiments. After having signed the informed consent, we administered a so-called lifestyle questionnaire which included items related to the motivation to behave sustainably. Next, participants were shown a pattern of dots and were asked to remember it and to reproduce it after having completed the main part of the study. Next, participants read a scenario adapted from Steffel and colleagues (2016), in which participants had to choose from a list of green amenities with either an opt-in or an opt-out format. After having chosen the amenities, participants were asked to reproduce the pattern of dots by clicking on the location of the dots in the matrix. After that, we asked four questions about the difficulty of remembering the pattern of

dots. Next, we measured satisfaction with their choice as well as demographics (age and gender). Finally, we queried for suspicion of the goal of the study, before debriefing the participants.

Materials.

Cognitive load task. We used the dot memorization task, a task in which participants have to memorize a pattern of dots that is presented in a matrix. This is a secondary load task that burdens cognitive resources and thereby reduces the possibility of Type 2 engagement (Bago & De Neys, 2017; Miyake, Friedman, Rettinger, Shah, & Hegarty, 2001). Consequently, several studies have shown that this manipulation affects performance on reasoning tasks, independent of individual differences in working memory capacity (e.g., De Neys, 2006). The task has successfully been used in online studies before (Bago & De Neys, 2017). To further enhance the strength of the manipulation, we will use the 'extra high load' manipulation from Johnson, Tubau, and De Neys (2016) as our high load condition.

For this task, all participants were first shown an empty 4x4 matrix. Next, for 1600 ms a pattern of dots appeared, and participants were asked to remember the pattern of dots and to reproduce it later. In the low load condition participants were shown a simple 1-piece pattern of 4 dots (i.e., the dots were placed in a straight vertical line), while in the high load condition participants were shown a complicated 4-piece pattern of 5 dots, meaning that only 2 of those 5 dots were adjacent to each other (Bethell-Fox & Shepard, 1988; Johnson et al., 2016; see Figure 3.1 for the patterns). After having completed the main part of the study, participants were shown an empty matrix again and were asked to reproduce the pattern of dots by clicking on the correct cells within the matrix. We measured the number and percentage of correctly localized dots in the matrix. After having filled in the matrix, we informed participants that they could forget about the pattern of dots. In order to see whether participants wrote down the pattern of dots, we asked them to reproduce the pattern once more at the very end of the study and asked them to honestly indicate if they had written down the pattern of dots.

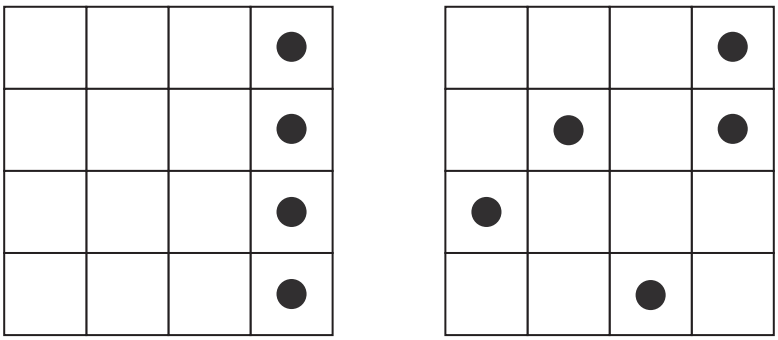

Figure 3.1. Dot patterns as presented in the dot memorization task in the low load condition (left) and high load condition (right).

Scenario. We used an adapted version of the scenario used by Steffel and colleagues (2016), in which participants were instructed to imagine that they had just rented a newly constructed apartment. Before signing the contract with the landlord, participants were offered a list of 14 optional green amenities. We updated the list of amenities so that it was suited for our UK sample and in line with current standards (see Supplementary Materials for the list of amenities). In the opt-in condition, participants were shown the list of amenities, with none of them preselected. They were told that none of the amenities were currently included in the rent, but that they could choose to select the amenities of their liking for an additional monthly price, ranging from £2 to £8 for each amenity chosen. In the opt-out condition, participants were shown the list of amenities, and all of them were preselected. They were told that the amenities were currently included in the rent, but that they could choose to not install the amenities which they did not want, such that the landlord would deduct a small amount of money from the monthly rent (again, ranging from £2 to £8). Following Steffel and colleagues (2016), all options were either or not preselected in order to maximize the treatment effect. We measured the number of green amenities selected by the participants. This variable ranged from 0 to 14.

Measures.

Motivation to behave sustainably. We measured the motivation to behave sustainably with four items based on research on personal strivings (Emmons, 1986). We used items that measured commitment ("How committed are you to behaving sustainably?"), importance ("How important is behaving sustainably to you in your life?"), and value ("How much joy or happiness do you or will you feel when you are successful in behaving sustainably?" and "How much sorrow or unhappiness do you or will you feel if you fail to succeed in behaving sustainably?"). These four items were embedded in a lifestyle questionnaire that also measured the same strivings for two other behaviors: healthy eating and saving money. All questions were asked on a 7-point Likert scale, ranging from 1 (*not at all*) to 7 (*very much*). The filler items were not analyzed and were solely included to conceal the goal of the study. The measure had high internal reliability (Cronbach's $\alpha = .812$), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Subjective difficulty. We measured the subjective difficulty of having had to remember the pattern of dots, by asking participants four questions on a 7-point Likert scale, ranging from 1 (*not at all*) to 7 (*very much*). The four items are: "How difficult was it to remember the pattern of dots?", "How much effort did it cost to remember the pattern of dots?", "How much were you preoccupied with remembering the pattern of dots?", and "How easy was it for you to remember the pattern of dots?" (Reverse coded). The measure had high internal reliability (Cronbach's $\alpha = .887$), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Satisfaction with choice. Satisfaction with choice was measured with a single item ("How satisfied are you with the amenities that you chose?") on a 7-point Likert scale,

ranging from 1 (*not at all*) to 7 (*very much*). We included this item to explore whether there were differences between the conditions in satisfaction, but we did not have a priori hypotheses for this.

Demographics. We asked participants for their age in years and gender (female, male, other/rather not specify).

Goal of study. We inquired for suspicion of the goal of the study with one open-ended question ("What do you think was the goal of the study?").

Results

Data is available on the Open Science Framework (https://osf.io/uqgwv/).

Preprocessing steps. All data was screened for outliers, which we defined as 3 *SD*s above or below the mean for each variable. If we detected outliers, these values were set missing. We also checked whether participants detected the goal of the study, by having two independent coders code (1) whether participants mention any association between the cognitive load task and the amenity selection task, and (2) whether participants expected that the cognitive load task affected their choice. The two independent coders reached agreement in 88.93% of the cases for criterion 1 and 94.29% of the cases for criterion 2. A third independent coder evaluated the answer for the remaining cases. 45.12% of the sample ($n = 379$) detected any association between the cognitive load task and the amenity selection task (criterion 1), while 13.45% of the sample ($n = 113$) expected that the cognitive load task affected their choice (criterion 2).

The main dependent variable was the number of green amenities chosen. For completeness, we checked for normality by performing a Shapiro-Wilk test, which turned out significant ($W = .92$, $p < .001$). However, given our large sample size we suspected that the proposed ANOVA would be robust regardless of the normality of the data.

Preregistered analyses.

Randomization check. In order to check whether randomization of participants across the four conditions was successful, we ran separate ANOVAs with the four conditions as independent variable and age or motivation to behave sustainably as dependent variable. For gender, we ran a Chi-squared analysis. As expected, randomization was successful (all *p*s > .129).

Manipulation check. In order to check whether our manipulation was successful, we conducted an independent samples t-test with cognitive load (low load vs. high load) as independent variable and subjective difficulty as dependent variable. As expected, our manipulation was successful such that participants in the high load condition ($M = 4.09$, $SD = 1.33$) found it more difficult to remember the pattern of dots than participants in the low load condition ($M = 1.58$, $SD = 0.76$), $t(672.69) = -33.69$, $p < .001$, $d = 2.31$. For robustness, we also ran the manipulation check with the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement ($n = 646$). We also did the same with the subsample of participants in the high load condition who reported that they had not written down the pattern of dots ($n =$

830). Results remained similar across these two proposed subsamples. Table 3.1 presents the descriptives for the proportion of correctly remembered dots.

Table 3.1

Mean, SD, minimum and maximum proportion of correctly remembered dots in measurement 1 and measurement 2.

	Measurement 1	Measurement 2
Low load	1.00 (0.04) [0.25 – 1.00]	1.00 (0.05) [0.25 – 1.00]
High load	0.81 (0.23) [0.40 – 1.00]	0.80 (0.23) [0.40 – 1.00]

Main analyses. In order to evaluate our hypothesis, we ran a factorial ANOVA with choice format and cognitive load as independent variables and the number of green amenities as dependent variable. As expected, this revealed a significant main effect of the default nudge, $F(1, 836) = 462.22$, $p < .001$, $\eta_p^2 = .36$, such that participants in the opt-out condition ($M = 11.75$, $SD = 2.56$) chose more green amenities than participants in the opt-in condition ($M = 5.60$, $SD = 3.22$). There was no main effect of cognitive load, $F(1, 836) = 0.19$, $p = .663$. Crucially, as expected we also did not find a significant interaction effect, $F(1, 836) = 0.002$, $p = .964$. For robustness, we had proposed to run this main analysis with a variety of subsamples: 1) the subsample of participants who correctly remembered the complete pattern of dots at the initial measurement ($n = 615$; cf., Johnson et al., 2016), 2) the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement ($n = 646$), 3) the subsample of participants in the high load condition who did not write down the pattern of dots ($n = 830$), 4) the subsample of participants who had not detected the goal of the study according to criterion 1 ($n = 461$), and 5) the subsample of participants who had not detected the goal of the study according to criterion 2 ($n = 727$). Results remained similar across these five proposed subsamples, indicating a robust pattern (see Supplementary Materials). We also ran an additional Poisson model, which again revealed a strong default effect ($p < .001$), and no main effect of load or an interaction effect.

In order to further test the hypothesis that cognitive load does not affect default effectiveness, we ran a TOST independent samples t-test for the two opt-out groups (i.e., high and low cognitive load). We set the lower bound to $d = -0.3$ and the higher bound to $d = 0.3$, and used alpha level .05. As expected, the equivalence test was significant $t(418) = -2.65$, $p = .004$, while the null hypothesis test was not significant, $t(418) = 0.42$, $p = .673$. Taken together, this implies that the default effect is statistically equivalent, and we reject the existence of a meaningful effect.

Exploratory analyses (preregistered). We explored whether there are differences between the conditions in satisfaction with the choice. Therefore, we conducted a factorial ANOVA with choice format and cognitive load as independent variables and satisfaction as dependent variable. This analysis revealed a significant main effect of the

default nudge, $F(1, 805) = 11.70$, $p < .001$, $\eta_p^2 = .01$, such that participants in the opt-out condition ($M = 5.81$, $SD = 1.11$) were more satisfied with their decision than participants in the opt-in condition ($M = 5.45$, $SD = 1.10$).

Exploratory analyses (unregistered). In order to shed more light on the possible working mechanisms of this specific nudge, we conducted an exploratory factorial ANOVA with choice format and cognitive load as independent variables and the number of deviations from the status quo as dependent variable. This analysis revealed a significant main effect of the default nudge, $F(1, 836) = 147.07$, $p < .001$, $\eta_p^2 = .15$, such that participants in the opt-out condition ($M = 2.25$, $SD = 2.56$) changed away from the status quo to a lesser extent than participants in the opt-in condition ($M = 5.60$, $SD = 3.22$). We did not find a main effect of cognitive load ($p = .663$) nor an interaction effect ($p = .568$).

Study 2

In Study 2, we aimed to experimentally stimulate Type 2 processes. In Study 1, half of the participants received high cognitive load, thereby taxing working memory capacity, while the other half received low cognitive load. However, the mere availability of cognitive resources does not imply engagement with Type 2 processes per se. Therefore, in Study 2 we aimed to stimulate Type 2 processes by instructing participants to think thoroughly about their decision. We used the same scenario and default manipulation. Again, we expected to find a significant main effect of the default, and in Study 2 we expected to find a significant interaction effect, such that the default effect was attenuated when participants were instructed to deliberate.

Method

Participants and design. The smallest effect size for the main effect of the default on the number of green amenities chosen was $\eta_p^2 = .37$ (Steffel et al., 2016). Using this most conservative effect size for the main effect in G*Power (Faul et al., 2009), and assuming power of 80% and a significance level of .05, we would require a total sample size of 16 participants to replicate this main effect. As we aimed to demonstrate that deliberation instructions attenuated the default effect, we were interested in the possibility of finding an interaction effect. We defined our smallest effect size of interest as 20% attenuation, which would correspond with a decrease in the number of amenities chosen of about 1. Using the script for simulations in R by datacolada (Simonsohn, 2014), we ran 2000 simulations and found that we needed at least 336 participants to find a significant attenuation effect of 20% with 80% power and a significance level of .05. In order to guarantee enough power for our proposed analyses with subsamples, we oversampled by 10%, resulting in a final sample size of 372 (220 female, 152 male; $M_{age} = 35.75$, $SD_{age} = 13.29$).

Participants were recruited from Prolific Academic and we included adult participants with a UK nationality. In order to ensure quality of the data, we set approval rates to 95%. An additional exclusion criterion in Study 2 was that participants should not have participated in Study 1. Again, participants were rewarded with £0.60 for their participation. We used

a 2 (choice format: opt-in vs. opt-out) x 2 (instructions: no instructions vs. deliberation instructions) between-subjects design with the number of green amenities chosen as dependent variable.

Procedure. Study 2 was also run on Gorilla (Anwyl-Irvine et al., 2020), and we used the same lifestyle questionnaire as in Study 1, followed by the same scenario as in Study 1 (either in an opt-in or an opt-out format). Half of the participants received no additional instructions, while the other half of the participants were instructed to think carefully about their decision. This latter group of participants was also told that they had to provide reasons for their decisions. After they had chosen their amenities, we asked participants to report their reasons for choosing the amenities that they chose. The other half of the participants received no additional instructions and did not have to report the reasons behind their decisions. Next, satisfaction with their choice as well as demographics (age and gender) were measured. Finally, we queried for suspicion of the goal of the study, before debriefing the participants.

Materials.

Deliberation instructions. Participants in the deliberation instructions conditions were instructed to think carefully about their decision (Horstmann et al., 2010), and received the following instructions: "Please think carefully and thoroughly about your decision of which amenities to choose. After you have made your decision, we will ask you to provide at least three reasons for choosing the amenities that you chose. You can take as much time as you need to consider all options and reach a balanced decision". Participants in the no instructions conditions received no additional instructions and were not asked to report the reasons behind their decisions.

Scenario. We used the same scenario as in Study 1 and measured the number of green amenities selected by participants in the same way as in Study 1. This variable ranged from 0 to 14.

Measures.

Motivation to behave sustainably. We measured the motivation to behave sustainably with the same lifestyle questionnaire as in Study 1. The measure had high internal reliability (Cronbach's $\alpha = .817$), and thus, according to the proposed analysis plan, all four items were combined into one score by taking the mean over the four items.

Satisfaction with choice. Satisfaction with choice was measured in the same way as in Study 1. We explored whether there are differences between the conditions in satisfaction, but, again, did not have a priori hypotheses for this.

Demographics. We asked participants for their age in years and gender in the same way as in Study 1.

Goal of study. We inquired for suspicion of the goal of the study with the same question as in Study 1.

Results

Data is available on the Open Science Framework (https://osf.io/gyujs/).

Preprocessing steps. All data was screened for outliers, which we defined as 3 *SD*s above or below the mean for each variable. If we detected outliers, these values were set missing. We also checked whether participants detected the goal of the study, by having two independent coders code (1) whether participants mentioned any association between the instructions and the amenity selection task, and (2) whether participants expected that the instructions affected their choice. The two independent coders reached agreement in 99.73% of the cases for criterion 1 and 98.92% of the cases for criterion 2. A third independent coder evaluated the answer for the remaining cases. None of the participants detected the goal of the study according to criterion 1 or 2.

The main dependent variable was the number of green amenities chosen. For completeness, we checked for normality by performing a Shapiro-Wilk test, which turned out significant ($W = .93$, $p < .001$). However, given our large sample size we suspected that the proposed ANOVA would be robust regardless of the normality of the data.

Preregistered analyses.

Randomization check. In order to check whether randomization of participants across the four conditions was successful, we ran separate ANOVAs with the four conditions as independent variable and age or motivation to behave sustainably as dependent variable. For gender, we ran a Chi-squared analysis. As expected, randomization was successful (all *p*s > .497).

Main analyses. In order to evaluate our hypothesis, we ran a factorial ANOVA with choice format and deliberation instructions as independent variables and the number of green amenities as dependent variable. As expected, this revealed a significant main effect of the default nudge, $F(1, 368) = 205.17$, $p < .001$, $\eta_p^2 = .36$, such that participants in the opt-out condition ($M = 11.06$, $SD = 2.69$) chose more green amenities than participants in the opt-in condition ($M = 5.45$, $SD = 2.68$). Unexpectedly, we did not find a significant interaction effect, $F(1, 368) = 0.45$, $p = .501$, but instead we found a main effect of deliberation, $F(1, 368) = 8.59$, $p = .004$, $\eta_p^2 = .02$, such that participants who received deliberation instructions chose fewer green amenities ($M = 7.80$, $SD = 3.96$) than participants who did not receive such instructions ($M = 8.76$, $SD = 3.77$). Initially, we proposed to run the same analyses with the subsamples of participants who were not able to identify the goal of the study, but this had become redundant since none of the participants had successfully done so. We also ran an additional Poisson model, which again revealed a strong default effect ($p < .001$) and a significant effect of deliberation instructions ($p = .001$). This analysis further demonstrated a marginally significant interaction effect ($p = .067$).

Exploratory analyses (preregistered). Just as in Study 1, we explored whether there are differences between the conditions in satisfaction with the choice. Therefore, we conducted a factorial ANOVA with choice format and deliberation instructions as independent variables and satisfaction as dependent variable. This analysis again revealed a significant main effect of the default nudge, $F(1, 364) = 5.97$, $p = .015$, $\eta_p^2 = .02$, such that participants in the opt-out condition ($M = 6.05$, $SD = 0.81$) were more satisfied with their

decision than participants in the opt-in condition ($M = 5.85$, $SD = 0.91$).

Exploratory analyses (unregistered). We again conducted an exploratory factorial ANOVA with choice format and deliberation as independent variables and the number of deviations from the status quo as dependent variable. This analysis again revealed a significant main effect of the default nudge, $F(1, 368) = 81.94$, $p < .001$, $\eta_p^2 = .18$, such that participants in the opt-out condition ($M = 2.94$, $SD = 2.69$) changed away from the status quo to a lesser extent than participants in the opt-in condition ($M = 5.45$, $SD = 2.68$). We also found a small main effect of deliberation, $F(1, 368) = 8.59$, $p = .004$, $\eta_p^2 = .02$, such that participants who were instructed to deliberate upon their decision were less likely to change away from the status quo ($M = 4.08$, $SD = 2.78$) than participants who did not receive such instructions ($M = 4.26$, $SD = 3.12$). Finally, we found a small interaction effect of the default and deliberation instructions on the number of times participants changed away from the status quo, $F(1, 368) = 12.29$, $p = .001$, $\eta_p^2 = .03$. Post-hoc multiple comparison tests using Tukey HSD revealed that deliberation reduced the number of times participants changed away from the status quo in the opt-in conditions, $p = .019$, while this was not the case in the opt-out conditions, $p = .183$.

Discussion

Ever since the introduction of nudges, they have received an increasing amount of interest from both scholars and policy makers, as early nudging studies revealed promising results on behavior and cost-effectiveness (Benartzi et al., 2017). At the same time, there have been numerous discussions on the legitimacy of nudging interventions, mostly based on the core assumption of nudges as taking advantage of automatic processes (Bovens, 2009; Hansen & Jespersen, 2013). In two studies, we investigated this fundamental premise by using a default nudge which is often seen as the most prototypical of Type 1 nudges (Hansen & Jespersen, 2013; Jung & Mellers, 2016; Sunstein, 2016). We investigated the effectiveness of this default nudge under circumstances in which deliberation was either inhibited (Study 1) or stimulated (Study 2). Across two preregistered and high-powered studies, we found a strong and robust effect of the default nudge on the number of green amenities that were chosen. Thereby, we replicated the default effects as observed in Steffel and colleagues (2016) with effect sizes that were larger than typically observed in experiments with defaults (Jachimowicz et al., 2019). The default effect was similar in size across the two studies and also was robust to inclusion or exclusion of certain participants based on predetermined subsamples.

In Study 1, this main effect of the default nudge was, as expected, not moderated by cognitive load. In fact, the default effect was statistically equivalent in the low and high load conditions, even though participants found it considerably more difficult to remember the pattern of dots in the high load condition than in the low load condition. Together, this indicates that the default effect is not strengthened or weakened when people are bound to resort to Type 1 processes. Rather, the default nudge is similarly effective when Type 2 processing is successfully inhibited. This is in line with the default-

interventionist perspective on dual processing (Evans & Stanovich, 2013), which posits that Type 1 processing is already the default mode of operation, unless Type 2 processing deliberately intervenes. In other words, the mere availability of cognitive resources does not directly imply engagement of these processes. Thus, inhibiting Type 2 processing via a demanding cognitive load manipulation does not alter nudge effectiveness, as it does not alter the processes that are being used to reach this decision.

This does not imply that this default nudge cannot be overruled by extensive deliberation, as deliberation was not actively involved in Study 1. This is what we addressed in Study 2, by instructing participants to carefully deliberate upon their decision. Study 2 again revealed a significant main effect of the default, and also a main effect of deliberation on the number of green amenities that were chosen. Contrary to our expectations, we did not find a significant interaction effect that would reveal attenuation of the default effect under deliberation. Instead, we found a main effect of deliberation, indicating that fewer green amenities were chosen if people deliberated upon their choice. This diminishing effect of deliberation was present in both the opt-in and opt-out conditions. Together, this implies that defaults effects are strong and robust, but not necessarily impregnable as deliberation may operate in parallel and impact choice simultaneously.

Implications

Our results first and foremost shed light on the hypothesized working mechanisms of nudges as making use of automatic processes. Our results most stringently imply that this default nudge is effective as it is not *dependent* on elaborate processing. It is equally effective whether people have the capacity to engage in careful thought or not. Yet, if people are able and willing to deliberate upon their decision, this may in parallel lead to different choice outcomes. In other words, nudges are indeed effective as they do not depend on executive processing, which gives people the opportunity to stick with the default without investing cognitive resources, or to change away from the status quo by deliberating upon the decision. Our results do also indirectly add to the growing amount of literature on the specific working mechanisms of defaults. We explored the extent to which participants would change away from the default, and in both studies we found that the strength of the status quo was larger for participants in the opt-out condition than for participants in the opt-in condition. This points towards a qualitative difference between the opt-in and opt-out condition, which aligns well with the notion of endowment as a possible working mechanism of defaults (Dinner et al., 2011; Park et al., 2000; Tversky & Kahneman, 1981). In our studies, participants were less likely to give up to something already endowed as compared to actively choosing for specific amenities. Based on our results, it is less likely that effort is a driving force behind this default nudge, as participants took more effort to change away from the status quo in the opt-in condition than in the opt-out condition, while objectively the amount of effort required to change away from this status quo was equal. Similarly, implied endorsement is less likely to drive our effects, unless the implied endorsement is asymmetrical in the sense that the endorsement is

more genuinely felt in the opt-out condition than in the opt-in condition. This latter possibility could indirectly be reflected in our exploratory findings that participants in the opt-out conditions were more satisfied with their decisions than participants in the opt-in conditions.

Furthermore, our results fit in with recent developments in literature on dual processing. While nudges were originally based on the idea of dual systems, current evidence points towards two types of reasoning where the distinction lies in the involvement of working memory resources (Melnikoff & Bargh, 2018). In our studies, we inhibited working memory capacity in Study 1 and stimulated working memory involvement in Study 2. We thus focused on the core mechanisms of these processing types, and not on the typical correlates such as speed of the decision or level of consciousness. Extrapolating from our results and findings in the control conditions without high load or deliberation instructions, we indeed see a pattern of results that suggests that Type 1 processing is the default mode of operation. Inhibiting Type 2 processing did not alter our behavioral outcomes, while stimulating Type 2 engagement led to a parallel effect, thus suggesting that Type 1 reasoning is the standard mode of operation.

Finally, our results reveal whether people effectively use the cognitive resources that are available to them, which also bears implications for research on transparency information in nudging desirable behavior. Current evidence suggests that nudges remain effective if accompanied by transparency information (e.g., Bang, Shu, & Weber, 2020; Bruns, Kantorowicz-Reznichenko, Klement, Jonsson, & Rahali, 2018; Kroese, Marchiori, & De Ridder, 2015; Loewenstein, Bryce, Hagmann, & Rajpal, 2015; Steffel et al., 2016). In fact, there is also some evidence that default nudges become more effective with transparency information (Paunov, Wänke, Vogel, 2019a; 2019b). Yet, the mere provision of transparency information does not guarantee that decision makers take this information (sufficiently) into account (Kroese et al., 2015), and that they deliberate upon their decision (Loewenstein, Sunstein, & Golman, 2014). Our direct manipulation of deliberation underscores this explanation of why transparency information does not necessarily affect behavioral outcomes, as this would only be the case if this information is sufficiently incorporated in the decision by the decision maker. If not, the decision maker may resort to Type 1 processing without deliberating upon the information that is given.

Further research

Further research should attempt to increase knowledge on this fundamental premise of nudges as making use of automatic processes. In doing so, we like to point out that all too often it is assumed that control conditions in which participants do not receive cognitive load reflect Type 2 processing. However, the mere availability of cognitive resources does not guarantee engagement of those processes, and it is important to further study nudge effectiveness by considering both sides of the same coin.

Further research should also investigate different (kinds of) nudges. In this study we used defaults, which are arguably the most prototypical Type 1 nudges. After the initial

introduction of nudges as simple interventions in the choice architecture, conceptual nuances have been made distinguishing between Type 1 vs. Type 2 nudges (Hansen & Jespersen, 2013; Lin, Osman, & Ashcroft, 2017), or educative vs. non-educative nudges (Sunstein, 2016). It remains to be determined if our findings apply to other kinds of nudges, such as social proof nudges and reminders, which could appeal more to deliberate processes than default nudges.

In conclusion, across two high-powered studies we showed that defaults can strongly stimulate sustainable choices. Most importantly, we shed light on the hypothesized working mechanisms of nudges as making use of automatic processing and show that defaults are effective as they are not dependent on elaborate Type 2 processing. For behavior change interventions this is a promising result, since other interventions like educational campaigns typically rely more on Type 2 processing. For the debate on the ethics and legitimacy of nudging, these results provide empirical data that show that people with less cognitive capacity will not be more vulnerable to fall victim to the implementation of defaults.

Chapter 4

Motivated by default – How nudges facilitate people to act in line with their motivation

4

This chapter is based on:
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LG, MA, and DR conceptualized the research idea. LG developed the research design in consultation with MA. LG collected and analyzed the data, and interpreted the data in consultation with MA and DR. LG wrote the manuscript. MA and DR provided critical feedback on the manuscript.

Abstract

Nudges are defined as small adjustments in the choice architecture that help people perform desirable behavior. How nudges interact with individuals' motivation has not been studied empirically. We conducted three studies with different types of defaults in three different behavioral domains and investigated how defaults and different types of motivation affect choice outcomes. In Study 1, we investigated the effectiveness of a default to stimulate healthy eating choices implemented in a hypothetical online supermarket setting. In Study 2, we used a scenario in which participants could choose from a list of green amenities (either preselected or not). In Study 3, we asked participants if they wanted to participate in a basic or longer version of our questionnaire, with the longer version option set as the default in the nudge condition. Across three studies we show that defaults are effective in promoting desirable behavior, and that goal strivings and autonomous motivation have additional positive main effects. We did not find evidence that controlled motivation did affect behavioral outcomes. Exploratory analyses revealed that amotivation negatively affected behavior, but the measure had poor reliability. No significant interaction effects were observed. Together, these studies imply that both defaults and motivation have main effects on behavior, such that the default sets the anchor from which people can adjust according to the type and strength of their motivation. Implications for the practice and ethics of nudging are discussed.

People strive for all sorts of outcomes such as living a healthy lifestyle or making sustainable choices. Whatever it is that people strive for, research on self-control as a limited resource (Baumeister, Bratslavsky, Muraven, & Tice, 1998; Wilkowski, Ferguson, Williamson, & Lappi, 2018), autonomous and controlled motivation (Deci & Ryan, 2000), and the intention-behavior gap (Sheeran & Webb, 2016) – to name just a few examples – suggests that people often experience difficulty in achieving the behavioral end states that they strive for. In many cases, difficulties arise from the non-supportive context of desired behavior. For example, the obesogenic environment makes it complicated to act upon one's intention for healthy eating (Swinburn, Egger, & Raza, 1999), while easy access to one's own car makes it more convenient than public transport. In order to make the context more supportive of desired behavior, nudges – subtle changes in the environment that stimulate desirable behavior (Thaler & Sunstein, 2008) – have been introduced as a novel policy tool. However, the critical question whether effects of nudges depend on motivation has of yet received little empirical scrutiny and requires further research. In the current set of studies, we therefore intend to extend the knowledge base on nudging by studying the role of motivation as a potential moderator of nudges' effectiveness across different behavioral domains. In doing so, we focus on motivation strength (based on research on personal strivings; Emmons, 1986) as well as on different reasons underlying motivation (based on Self-Determination Theory; Ryan & Deci, 2000). Moreover, we examine the incremental value of nudges on top of existing motivation.

Given its relevance for so many behavioral outcomes, it is remarkable that little is known about how motivation relates to nudging. Thus far, only rather distally related constructs like attitudes and preferences have received attention in studies on nudges' effectiveness, while more proximal constructs like needs have only been studied as outcome variables (e.g., Arvantitis, Kalliris, & Kaminiotis, 2020; Wachner, Adriaanse, & De Ridder, 2020). Yet, it is important to know both for practical implementation and for the ethical debate about the legitimacy of nudges when and for whom nudges are effective (De Ridder, Kroese, & Van Gestel, in press). For example, one outstanding question is whether nudges and human motivation independently or interactively predict behavior. It has been suggested that there should at least be some kind of motivation for a nudge to be effective and legitimate (Bovens, 2009; Thaler & Sunstein, 2008), but whether this is indeed the case and how nudges and motivation together impact behavior is currently not clear. Moreover, it is also not known whether relatively small interventions like nudges can in fact add an increased propensity to perform desirable behavior when accounting for people's motivation. Both questions have, to the best of our knowledge, not been directly addressed in empirical research. In the current set of studies, we aim to advance the field on nudging desirable behavior by looking at motivational strength as well as at different types of motivation as potential moderators of the effect of default nudges in different behavioral domains.

Nudging

Nudges are defined as subtle changes in the immediate choice environment that

alter behavior in a predictable way, without interfering with financial incentives or forbidding any of the available options in the choice set (Thaler & Sunstein, 2008). The terms 'nudge' or 'nudging' are frequently used as an umbrella term for interventions in the choice environment that steer behavior towards a desirable outcome without relying on the availability of cognitive resources (Hunter, Hollands, Couturier, & Marteau, 2018; Van Gestel, Adriaanse, & De Ridder, 2020a). This is in contrast to other interventions such as educational campaigns or persuasion techniques which often do rely on the availability of cognitive resources (Beauchamp, Backholer, Magliano, & Peeters, 2014; Lorenc, Petticrew, Welch, & Tugwell, 2013; McGill et al., 2015). Nudges are therefore an interesting and novel policy instrument, exactly because the change in the environment does not require educating or training individuals, because it has the potential to reach many individuals, and because of high cost-effectiveness (Benartzi et al., 2017).

The interventions that fall under this umbrella term 'nudging' come in all sorts and shapes, ranging from interventions that rely on social proof (e.g., Venema, Kroese, Benjamins, & De Ridder, 2020) or environmental restructuring (e.g., Van Gestel, Kroese, & De Ridder, 2018; Van Gestel, Adriaanse, & De Ridder, 2020b). One of the most prototypical and robust examples of nudges are defaults (e.g., Johnson & Goldstein, 2003; Van Gestel et al., 2020a), which determine the choice outcome in case no active decision is made. That is, if people do not actively opt-out of the default setting, they will end up choosing the preselected option. Just like other nudges, defaults have been applied to many behavioral domains such as organ donation (Johnson & Goldstein, 2003), financial behavior (Madrian & Shea, 2001), and sustainable behavior (e.g., Pichert & Katsikopoulos, 2008). Default effects are typically robust with an average effect size of $d = 0.68$ (Jachimowicz, Duncan, Weber, & Johnson, 2019), although it should be noted that there can be quite some variation (95%CI = 0.53–0.83), as for example defaults are generally found to be more effective in consumer domains than in environmental domains. The notion that even such strong nudges like defaults do not always work for everyone suggests that other factors such as motivation may impact behavior apart from the default nudge.

The role of motivation

Nudges are often suggested as a helpful tool to promote behavior that people are assumed to be motivated for, in the sense that it is either beneficial for themselves (pro-self nudges; e.g., healthy eating) or for society at large (pro-social nudges; e.g., sustainable choices). In fact, there should at least be some kind of motivation for a nudge to be legitimate (Bovens, 2009). Thaler and Sunstein (2008) also highlight this in their 'nudge for good' principle: policy makers ought to have beneficent intentions and a nudge should only be effective if individuals at least have a minimum motivation to perform the behavior the nudge is aimed at. As a consequence, one could argue that the intention of the nudge should be reflected in an individual's motivation to perform that behavior. But whether this principle upholds in practice, or whether it is no more than a noble endeavor, is yet to be studied empirically. To be more precise, whether motivation moderates nudge

effectiveness and thus forms a boundary condition is still an open question. Moreover, whether this effect differs for motivation which reflects personal interests and values (i.e., autonomous motivation) as opposed to caused by external or internal pressures (i.e., controlled motivation) is still to be determined. Finally, an important question that remains to be studied is whether nudges do in fact facilitate desired behavior when accounting for individual's motivation.

To date, research that explicitly addresses the role of motivation in the context of nudges' effectiveness is lacking. We are also not aware of any studies that looked at proximal predictors of motivation such as needs. However, some research has been done to examine the impact of attitudes on nudge effectiveness, mostly in the realm of sustainable behavior (e.g., green electricity uptake or organic food choices). These studies have consistently revealed that default nudges and attitudes independently predict behavioral outcomes (Kaiser, Bernauer, Sunstein, & Reisch, 2020; Kuhn, Ihmels, & Kutzner, 2021; Taube & Vetter, 2019; Vetter & Kutzner, 2016). Thus, current evidence suggests that defaults and attitudes have distinct and independent effects on behavior.

Another construct that is related to motivation – preferences – has been given attention both in rather generic claims and empirical research. It has, for example, been stated that "if preferences […] are strong, we would expect defaults to have little or no effect" (Johnson & Goldstein, 2003, p. 1339). Similarly, it has been suggested that "well-formed preferences […] trump default rules" (Sunstein & Thaler, 2003, p. 1198). Findings from experimental studies in the realm of nudging healthy behavior suggest that specific a priori preferences such as liking for soft drinks or preferences for meat can affect behavioral outcomes apart from nudges' effectiveness (Venema, Kroese, De Vet, & De Ridder, 2019; Venema et al., 2020). Still, the remaining question is what happens to someone who does not necessarily have such specific preferences (for or against sugar sweetened beverages or meat), but does have an overarching motivation to live healthily or sustainably.

Together, these studies on attitudes and a priori preferences imply that nudges may add a propensity to choose a certain option that builds on existing attitudes and preferences. However, a direct test of motivational tendencies, rather than related constructs, is still required. To address this, we focus on a generic measure of motivation strength (goal strivings) to measure what people are trying to achieve, as well as on different types of motivation such as autonomous motivation, controlled motivation, and amotivation (Deci & Ryan, 2000) to measure why people are trying to achieve this. Autonomous motivation is a type of goal striving that is at least to some extent internalized such that autonomously motivated behavior is self-endorsed and performed out of interest or personal value. Controlled motivation is a type of goal striving that is pressured by forces of control. Behavior that results from controlled motivation is at least to some extent driven by internally or externally imposed forces such as reward, punishment or social approval. Still, both types of motivation represent intentionally caused actions, which is in contrast with amotivation. Amotivation describes a lack of intention or motivation to

act, and often results in inaction. How these different types of motivation relate to nudges' effectiveness bears implications for the meaning of 'nudge for good': whether it is for the greater good, related to external pressures, or originating from intrinsic needs.

The current studies

To study the role of motivation in nudging effectiveness, we conducted three experiments across different behavioral domains (pro-self and pro-social) with different types of default nudges. In all three studies, the default nudge stimulated rather undisputed behaviors, inspired by the 'nudge for good' principle (Thaler & Sunstein, 2008). In Study 1, we embedded a default nudge in a hypothetical online supermarket task to stimulate healthier food choices. This default was continuously presented over a series of repeated choices with three alternative options in the choice task. In Study 2, we used a default nudge in a scenario in order to stimulate sustainable choices. This default was applied to a list of options in which all of the options were preselected or not at the same time. In Study 3, we used a default nudge in a real choice situation in which participation in a longer study on sustainability was stimulated. This default was applied to the desirable option in a set of two options that were presented at the same time. Across these three studies, we measured different motivational constructs in order to increase generalizability of our results: a rather generic measure of motivation strength (goal strivings), as well as autonomous motivation, controlled motivation and amotivation. Similarly, we focused on different kinds of behavior to increase external validity.

All three studies were preregistered on AsPredicted.org. In Study 1, we preregistered the hypothesis that the default nudge would be effective in stimulating healthier food choices, and indicated to explore the role of motivation (without preregistering specific hypotheses for that; https://aspredicted.org/jh62s.pdf). In Study 2, we again preregistered the hypothesis that the default nudge would be effective, but based on results from Study 1, we now also preregistered the hypothesis that autonomous motivation and goal strivings would have a main effect as well. We did not expect a main effect for controlled motivation, nor an interaction effect between the nudge and any of the motivation constructs (https://aspredicted.org/45q7n.pdf). Finally, in Study 3 we preregistered the same hypotheses for autonomous motivation and goal strivings as in Study 2, but based on results from Study 1 and 2, this time we left two possibilities open for controlled motivation (either or not a main effect). We again did not expect to find interaction effects (https://aspredicted.org/8hv2x.pdf).

Study 1

Method

Participants and design. We conducted a power analysis for detecting a statistically meaningful effect of the default nudge with an independent samples t-test (one-tailed). We did not have any previous studies to base an effect size on and wanted to be conservative for this first study. Therefore, we set the minimum effect size of interest at $d = 0.2$, and used $\alpha = .05$ and $\beta = .80$, which resulted in a minimum required sample size of

620 participants.

We recruited 635 participants (454 female, 178 male, 1 Other/Rather not specify, 2 missing; $M_{age} = 34.93$, $SD_{age} = 12.60$[1]) from Prolific Academic. We included adult participants with a UK nationality and a minimum approval rate of 95%. Participants were rewarded with £0.50 for their participation. We used a one factor (Condition: control vs. default) between-subjects design with the proportion of nudged healthy products chosen as dependent variable. The study was approved by the Ethics Committee of the Faculty of Social and Behavioral Sciences of Utrecht University under number 18-046.

Procedure. Participants were invited to participate in an online supermarket study. After they provided active informed consent, we administered two questionnaires (Treatment Self-Regulation Questionnaire (TSRQ) and a bogus lifestyle questionnaire with goal striving items embedded). Next, participants entered our online supermarket. Participants were randomly allocated to the control or default condition and went through 14 different trials in which they were instructed to choose 1 out of a set of 4 products. After completing the supermarket task, we asked participants for their demographics and a few other measures. Finally, participants were debriefed, thanked and paid for their participation.

Materials.

Online supermarket task. In the online supermarket task, participants were instructed to select a food product to add to their online grocery basket. The task consisted of 14 trials, of which 4 were filler trials. In each trial, participants saw four food products. All stimuli were pilot tested ($N = 60$) for perceived healthiness and tastiness, and were combined in such a way that each relevant trial consisted of four products within the same food category (e.g., crisps or jam) of which two were considered to be healthier but less tasty than the other two products. The four filler trials included only healthier (e.g., vegetables only) or unhealthier products (e.g., sugar-sweetened beverages only), and did thus not include a trade-off between the options. All images were surrounded by a light grey box, which would turn black upon selection of the product. Participants could select their preferred product by clicking on the image and could continue to the next trial by clicking 'Add to basket', thereby confirming their decision. In the control condition, the four products were presented in a 2 by 2 matrix, with all pictures presented in equal size. In the default condition, one of the healthier products was preselected such that it already had a black box around the image and was made more salient by presenting the image in a larger size above the other three products (see Figure 4.1). Thus, sticking with the default would require clicking 'Add to basket' only, while changing away from the default would require a similar amount of effort as choosing a product in the control condition (one click to select a product plus one click to add it to the basket). Which of the two

1. Due to a technical error, 2 participants did not fill in the demographics questionnaire at the end of the study. All demographics that are reported are based on $n = 633$.

Figure 4.1. Example trial of online supermarket task in the control condition (left) and default condition (right). We obtained permission to reproduce the pictures from Tesco PLC.

healthier products was preselected was counterbalanced across participants. We counted the number of times that participants chose the product of interest (the nudged healthy product) over the ten relevant trials, and combined these counts in a proportion score, which served as the main dependent variable for this study. Higher scores represent a higher likelihood of choosing the nudged healthy product.

Measures.

Treatment Self-Regulation Questionnaire. In order to measure autonomous motivation, controlled motivation, and amotivation, we administered the TSRQ (Levesque et al., 2007). The questionnaire consisted of fifteen statements that provided possible reasons for eating a healthy diet (e.g., 'Because I feel that I want to take responsibility for my own health' (Autonomous motivation; 6 items), 'Because I feel pressure from others to do so' (Controlled motivation; 6 items), and 'I really don't think about it' (Amotivation; 3 items)). All statements were measured on a 7-point Likert scale, ranging from 1 (*not at all true*) to 7 (*very true*). Composite scores were created by averaging the items that belonged to each subscale. The subscales for autonomous motivation (Cronbach's $\alpha = .86$) and controlled motivation (Cronbach's $\alpha = .82$) showed good reliability, while the subscale for amotivation (Cronbach's $\alpha = .56$) showed to have poor reliability.

Goal strivings. As an additional measure of motivation, we administered a bogus lifestyle questionnaire which included four items based on research on personal strivings (Emmons, 1986). These goal strivings represent what a person is characteristically trying to do or accomplish with their behavior and we used this as a measure of motivation strength. We administered the four items for the behavior of interest (healthy eating) as well as for two unrelated behaviors (sustainable behavior and saving money). The four items measured commitment ("How committed are you to eating healthily?"), importance ("How important is eating healthily to you in your life?"), and value ("How much joy or happiness do you or will you feel when you are successful in eating healthily?" and "How much sorrow or unhappiness do you or will you feel if you fail to succeed in eating healthily?"). All questions were asked on a 7-point Likert scale, ranging from 1 (*not at all*)

to 7 (*very much*). The measure had acceptable internal reliability (Cronbach's α = .77), and thus all items were combined into one score by averaging the four items.

Demographics. We asked participants for their age (in years) and gender (female, male, other/rather not specify). We also asked how hungry and thirsty participants were at that moment, using two questions with a 7-point Likert scale, ranging from 1 (*not hungry/ thirsty at all*) to 7 (*very hungry/thirsty*). Finally, we asked for participants' weight, which they could enter in their preferred measurement unit (stones and pounds or kilograms), and height (feet and inches or meters and centimeters). From these measurements, we calculated participants' BMI.

Results

Data are available on the Open Science Framework (https://osf.io/javhx/).

Preprocessing steps. As preregistered, outliers (defined as 3 *SD*s away from the mean) were set missing for the most important variables: proportion of nudged healthy choices (5 participants), autonomous motivation (5 participants), amotivation (1 participant) and goal strivings (9 participants). All analyses were run with inclusion and exclusion of outliers, but this did not change any of the results (i.e., direction or significance of effects). Therefore, we report on the entire sample with inclusion of outliers.

Descriptives. On average, participants scored moderately high on the measure of goal strivings for eating healthily ($M = 4.95$, $SD = 1.14$). Especially autonomous motivation was high ($M = 5.53$, $SD = 1.02$), while controlled motivation was slightly below the midpoint ($M = 3.31$, $SD = 1.23$). Amotivation was relatively low ($M = 2.47$, $SD = 1.11$). See Table 4.1 for a full overview of the descriptives and correlation coefficients.

Table 4.1

Descriptives and correlation coefficients for autonomous motivation, controlled motivation, and goal strivings for eating healthily (Study 1).

	Mean (SD)	Range	1	2	3	4
1. Goal strivings	4.95 (1.14)	1 – 7	(.77)			
2. Autonomous motivation	5.53 (1.02)	1 – 6.50	.73***	(.86)		
3. Controlled motivation	3.31 (1.23)	1 – 7	.30***	.21***	(.82)	
4. Amotivation	2.47 (1.11)	1 – 6	-.46***	-.50***	.02	(.56)

Note. Cronbach's alphas are shown in the diagonal. $N = 635$. *** $p < .001$

Randomization check. In order to check whether randomization of participants across the two conditions was successful, we ran separate independent samples t-tests with condition (control vs. default) as independent variable and age, hunger, thirst, BMI, goal strivings, autonomous motivation, controlled motivation, and amotivation as continuous dependent variables. For gender, we ran a Chi-squared analysis. Results showed that randomization was successful (all *p*s > .182).

Main analyses. We ran a pre-registered one-tailed independent samples t-test to

analyze the effect of the default nudge on the proportion of nudged healthy choices. The default nudge had a small to medium effect, $t(633) = -4.33$, $p < .001$, $d = 0.34$, *95%CI* [-inf; -0.04], such that participants chose more healthy nudged products in the default condition ($M = 0.33$, $SD = 0.21$) than in the control condition ($M = 0.27$, $SD = 0.17$).

In order to analyze whether motivation moderates nudge effectiveness, we ran four separate hierarchical multiple regressions. In step 1, we created our base model by including the main effect of the default nudge (with the control condition coded as 0 and the default condition coded as 1). In step 2, we added the main effect of motivation (goal strivings, autonomous motivation, controlled motivation, and amotivation respectively). In step 3, we added the interaction term between the default nudge and motivation (see Table 4.2 for the complete regression results). Not surprisingly, the default nudge turned out significant in our regression models as well ($\beta = .17$, $p < .001$, $R_{adj}^2 = .03$). Adding goal strivings to the base model improved model fit ($R_{adj}^2 = .09$, $p < .001$), and goal strivings for eating healthily significantly predicted the proportion of nudged healthy choices ($\beta = .25$, $p < .001$). Similarly, adding autonomous motivation to the base model improved model fit ($R_{adj}^2 = .08$, $p < .001$), and autonomous motivation for eating healthy diets significantly predicted the proportion of nudged healthy choices ($\beta = .24$, $p < .001$). Adding controlled motivation to the base model did not improve model fit ($p = .193$). Adding amotivation

Table 4.2

Regression model with the proportion of nudged healthy food choices as dependent variable (Study 1).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1	.03					< .001
Constant		0.26 (0.01)		[0.24, 0.29]	24.41	< .001
Default		0.07 (0.02)	.17	[0.04, 0.10]	4.33	< .001
Step 2 – Goal strivings	.06					< .001
Constant		0.05 (0.03)		[-0.02, 0.12]	1.50	.133
Default		0.07 (0.01)	.17	[0.04, 0.10]	4.49	< .001
Goal strivings		0.04 (0.01)	.25	[0.03, 0.06]	6.63	< .001
Step 2 – Autonomous motivation	.05					< .001
Constant		0.02 (0.04)		[-0.06, 0.10]	0.47	.641
Default		0.06 (0.01)	.16	[0.03, 0.09]	4.12	< .001
Autonomous motivation		0.04 (0.01)	.24	[0.03, 0.06]	6.17	< .001
Step 2 – Controlled motivation	.00					.193
Step 2 – Amotivation	.07					< .001
Constant		0.42 (0.02)		[0.38, 0.45]	23.30	< .001
Default		0.04 (0.01)	.18	[0.02, 0.05]	4.78	< .001
Amotivation		-0.05 (0.01)	-.28	[-0.06, -0.04]	-7.32	< .001

Note. *β*'s are fully standardized. $N = 635$.

to the base model improved model fit (R_{adj}^2 = .10, p < .001), and amotivation for eating healthy diets significantly predicted the proportion of nudged healthy choices (β = -.28, p < .001). Adding interaction terms did not improve model fit for any of the motivational constructs (all *ps* > .545).

In order to analyze whether the default adds an increased propensity of choosing the desired option, we also ran stepwise regressions starting with motivation in step 1 and adding the condition variable in step 2. These analyses consistently revealed that the default predicted healthy food choices when accounting for motivation with small but significant improvements in model fit ranging from .02 to .03 (see Supplementary Materials for full details).

Discussion Study 1

In Study 1, we found that the default nudge had a significant effect on the number of nudged healthy choices in an online supermarket setting. This illustrates that nudges can successfully be implemented in online choice settings, which is in line with current evidence (Antonides & Welvaarts, 2020; Coffino, Udo, & Hormes, 2020; Kuhn et al., 2021). Moreover, we found that goal strivings and autonomous motivation also had a positive main effect on the number of nudged healthy choices. We did not find an effect of controlled motivation, nor did we find any significant interaction patterns. Finally, we found a significant negative effect of amotivation on the number of nudged healthy choices, even though it should be noted that amotivation had poor reliability. Generally, motivation to eat healthily was rather high, especially autonomous motivation, which corresponds with the idea that this was a pro-self nudge that was in line with people's own motivation.

The goal of Study 2 was to replicate these findings with another kind of default nudge and within another behavioral domain. In Study 1 the default was repeatedly implemented over different trials and nudged one out of four products. In Study 2 the default was applied to a list of options in which all of the options were preselected or not at the same time. We also wanted to test the robustness of our effects within the domain of sustainable choices, which arguably is more of a pro-social domain and a domain where we expected slightly more variance in motivation. We hypothesized that the default nudge would be effective in stimulating sustainable choices, and that autonomous motivation and goal strivings would also have a positive main effect. In line with Study 1, we did not expect to find a significant effect of controlled motivation and we did not expect to find any interaction effect.

Study 2

Method

Participants and design. In Study 2, we were particularly interested in the role of motivation, and based on the correlations between the motivation measures and the dependent variable in Study 1, we defined the smallest effect size of interest as small. Therefore, we conducted a power analysis in G*Power for a linear multiple regression with

2 predictors and a minimum effect size of interest of $f^2 = 0.02$ (small), and used $\alpha = .05$ and $\beta = .80$. This resulted in a minimum required sample size of 485. In order to be able to exclude participants who failed the attention checks, we oversampled with 10%, resulting in a desired sample size of 534.

We collected data on Prolific Academic from 535 participants. One participant failed two attention checks and was, according to our preregistration, excluded from further analyses. This resulted in a final sample size of 534 participants (333 female, 197 male, 4 Other/Rather not specify; $M_{age} = 35.05$, $SD_{age} = 13.02$). We used the same inclusion criteria as in Study 1 and participants were rewarded with £0.70 for their participation. We used a one factor (Condition: control vs. default) between-subjects design with the number of green amenities chosen as dependent variable. The study was approved by the Ethics Committee of the Faculty of Social and Behavioral Sciences of Utrecht University under number 20-378.

Procedure. Participants were invited to participate in a study on lifestyle and decision making. After they provided active informed consent, we administered two questionnaires (TSRQ and a bogus lifestyle questionnaire with goal striving items embedded). Next, participants read a scenario and were asked to choose the green amenities that they would like to have. In this amenity selection task, participants were randomly allocated to the control or default condition. After that, we asked participants for satisfaction with their choice. For exploratory purposes (i.e., to explore whether the nudged behavior would alter motivation), we administered the same two questionnaires (TSRQ and lifestyle) again. Subsequently, we asked participants for their demographics, queried for suspicion of the goal of the study and ended with an open question in which participants could write anything they deemed relevant. Finally, participants were debriefed, thanked and paid for their participation.

Materials.

Amenity selection task. To measure the effectiveness of the default nudge to stimulate sustainable choices, we used a scenario in which participants had to imagine that they had just decided to rent a newly constructed apartment (Steffel, Williams, & Pogacar, 2016). They read that, before signing the rental contract, the landlord offered them a list of 14 optional green amenities. In the control condition, none of the amenities were preselected and were thus not included in the rent. They could actively choose to select certain amenities from the list for an additional monthly price, ranging from £2 to £8 for each amenity chosen. In the default condition, all amenities were preselected and were included in the rent. Participants could unselect certain amenities from the list, such that the landlord would deduct a small amount of money from the monthly rent (again, ranging from £2 to £8). There was thus no pricing difference between the two conditions. We used the same list of amenities as in Van Gestel and colleagues (2020a), and measured the number of green amenities selected by the participants. This variable ranged from 0 to 14.[2]

Measures.

Treatment Self-Regulation Questionnaire. We again used the TSRQ to measure autonomous motivation, controlled motivation, and amotivation. Participants rated the same 15 items, although some items needed to be slightly rephrased in order to match the behavior of making sustainable choices. All statements were again measured on a 7-point Likert scale, ranging from 1 (*not at all true*) to 7 (*very true*). Similar to Study 1, composite scores were created by averaging the items that belonged to each subscale. The subscales for autonomous motivation (Cronbach's α = .93) and controlled motivation (Cronbach's α = .81) showed good reliability, while the subscale for amotivation (Cronbach's α = .67) showed to have questionable reliability. We had initially preregistered to not analyze the amotivation construct.[3]

Goal strivings. We included the same lifestyle questionnaire as in Study 1. This time the four items of interest were focused on the behavior of making sustainable choices. All questions were asked on a 7-point Likert scale, ranging from 1 (*not at all*) to 7 (*very much*). The filler items about eating healthily and saving money were not analyzed and were solely included to conceal the goal of the study. The measure had good internal reliability (Cronbach's α = .87), and thus all items were combined into one score by averaging the four items.

Demographics. We asked participants for their age (in years) and gender (female, male, other/rather not specify). Further, we queried for suspicion of the goal of the study with one open-ended question ("What do you think was the goal of this study?"), and with a final open-ended question in which participants could answer any final thoughts.

Results

Data are available on the Open Science Framework (https://osf.io/bj5kc/).

Preprocessing steps. One participant failed both attention checks and was excluded from all analyses. Outliers were defined as 3 *SD*s away from the mean and were set missing for the most important variables. This only applied to controlled motivation (1 participant) and amotivation (1 participant). All analyses were run with inclusion and exclusion of these outlier, but this did not change any of the results (i.e., direction or significance of effects). Therefore, we report on the entire sample with inclusion of outliers.

Descriptives. On average, participants scored moderately high on the measure of goal strivings for making sustainable choices (M = 4.65, SD = 1.22). Autonomous motivation was highest (M = 4.87, SD = 1.34), while controlled motivation was slightly below the

2. After the amenity selection task, we asked participants how satisfied they were with their choice with 1 item ("How satisfied are you with the amenities that you chose?") measured on a 7-point Likert scale ranging from 1 (*not at all*) to 7 (*very much*; see Supplementary Materials).

3. For exploratory purposes, we administered the same questionnaire again after participants had chosen the green amenities. These exploratory analyses did not yield any noteworthy results and thus these data are not reported.

midpoint (*M* = 3.33, *SD* = 1.14). Amotivation was relatively low (*M* = 2.70, *SD* = 1.26). See Table 4.3 for a full overview of the descriptives and correlation coefficients.

Table 4.3

Descriptives and correlation coefficients for autonomous motivation, controlled motivation, and goal strivings for making sustainable choices (Study 2).

	Mean (SD)	Range	1	2	3	4
1. Goal strivings	4.65 (1.22)	1 – 7	(.87)			
2. Autonomous motivation	4.87 (1.34)	1 – 7	.78***	(.93)		
3. Controlled motivation	3.33 (1.14)	1 – 7	.33***	.36***	(.81)	
4. Amotivation	2.70 (1.26)	1 – 7	-.47***	-.55***	-.17***	(.67)

Note. Cronbach's alphas are shown in the diagonal. *N* = 534. *** *p* < .001

Randomization check. We performed the same analyses as in Study 1 to check for randomization of participants across the two conditions, this time with age, gender, goal strivings, autonomous motivation, controlled motivation, and amotivation as dependent variables. Results showed that randomization was successful (all *ps* > .142).

Main analyses. We ran a two-tailed independent samples t-test to analyze the effect of the default nudge on the number of green amenities. The default nudge had a large effect, $t(532) = -24.59$, $p < .001$, $d = 2.13$, *95%CI* [-5.93; -5.06], such that participants chose more green amenities in the default condition (*M* = 11.65, *SD* = 2.56) than in the control condition (*M* = 6.15, *SD* = 2.60).

In order to analyze whether motivation moderates nudge effectiveness, we conducted the same separate hierarchical multiple regressions as in Study 1, starting with the default nudge in step 1 (with the control condition coded as 0 and the default condition coded as 1), the motivation measures in step 2, and the interaction effect in step 3 (see Table 4.4 for the complete regression results). Not surprisingly, the default nudge turned out significant in our regression models as well ($\beta = .73$, $p < .001$, $R_{adj}^2 = .53$). Adding goal strivings to the base model improved model fit ($R_{adj}^2 = .56$, $p < .001$), and goal strivings for making sustainable choices significantly predicted the number of green amenities ($\beta = .16$, $p < .001$). Similarly, adding autonomous motivation to the base model improved model fit ($R_{adj}^2 = .58$, $p < .001$), and autonomous motivation for making sustainable choices significantly predicted the number of green amenities ($\beta = .22$, $p < .001$). Unexpectedly, adding controlled motivation to the base model also improved model fit ($R_{adj}^2 = .54$, $p = .014$), but the improvement in model fit was negligible ($\Delta R_{adj}^2 = .00$). Controlled motivation for making sustainable choices significantly predicted the number of green amenities ($\beta = .07$, $p = .014$). Adding interaction terms did not improve model fit for any of the motivational constructs (all *ps* > .214).

Table 4.4

Regression model with the number of green amenities as dependent variable (Study 2).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1	.53					< .001
Constant		6.15 (0.16)	.73	[5.84, 6.46]	38.85	< .001
Default		5.50 (0.22)		[5.06, 5.93]	24.59	< .001
Step 2 – Goal strivings	.03					< .001
Constant		3.81 (0.45)		[2.94, 4.69]	8.54	< .001
Default		5.54 (0.22)	.74	[5.11, 5.97]	25.46	< .001
Goal strivings		0.50 (0.09)	.16	[0.32, 0.67]	5.57	< .001
Step 2 – Autonomous motivation	.05					< .001
Constant		3.08 (0.41)		[2.27, 3.89]	7.47	< .001
Default		5.48 (0.21)	.73	[5.06, 5.89]	25.91	< .001
Autonomous motivation		0.63 (0.08)	.22	[0.48, 0.79]	7.98	< .001
Step 2 – Controlled motivation	.00					.014
Constant		5.35 (0.36)		[4.64, 6.06]	14.84	< .001
Default		5.50 (0.22)	.73	[5.06, 5.93]	24.71	< .001
Controlled motivation		0.24 (0.10)	.07	[0.05, 0.43]	2.46	.014
Step 2 – Amotivation	.02					< .001
Constant		7.26 (0.29)		[6.70, 7.82]	25.37	< .001
Default		5.45 (0.22)	.72	[5.02, 5.88]	24.84	< .001
Amotivation		-0.40 (0.09)	-.13	[-0.57, -0.23]	-4.62	< .001

Note. *β*'s are fully standardized. *N* = 534.

Exploratory analyses. In addition to our preregistration, we decided to explore results with amotivation in the regression models as well. Adding amotivation to the base model improved model fit ($R_{adj}^2 = .55$, $p < .001$), and amotivation for making sustainable choices significantly predicted the number of green amenities ($\beta = -.13$, $p < .001$).

In order to analyze whether the default adds an increased propensity of choosing the desired option, we also ran stepwise regressions starting with motivation in step 1 and adding the condition variable in step 2. These analyses consistently revealed that the default predicted the number of green amenities when accounting for motivation with large significant improvements in model fit ranging from .52 to .54 (see Supplementary Materials for full details).

Discussion Study 2

In Study 2, we found that the default nudge had a significant effect on the number of green amenities chosen, and that goal strivings and autonomous motivation also had a positive main effect. Unexpectedly, we also found a main effect for controlled motivation, although with a smaller effect size than the other motivational constructs.

Finally, amotivation again had a significant negative effect on the number of green amenities chosen. No significant interaction effects were found. Compared to Study 1, scores for autonomous motivation and goal strivings were slightly lower, but still only few participants scored below the midpoint of the scales (21% and 23%, respectively). In Study 2, we thus conceptually replicated the results from Study 1, but this time in a prosocial domain, and with a stronger impact of the default nudge on choice outcomes when accounting for motivation.

In Study 3, we wanted to extend our findings to a context where the choice would have actual direct implications for the person making the choice. Therefore, we used a default manipulation which preselected the option to voluntarily participate in a longer version of a study on sustainability. In contrast to Study 1 and 2, this choice would therefore have immediate behavioral consequences. Besides, the operationalization of the default nudge was different than in previous studies, as this time only one decision would be required in which the desirable and alternative option were presented simultaneously. Also, the desirable behavior could be seen as an act of prosocial behavior, as participation in the longer version would be voluntarily and helpful for the researchers. To address this, we measured goal strivings, autonomous motivation, controlled motivation, and amotivation to help other people. In line with findings from Study 1 and 2, we hypothesized that goal strivings and autonomous motivation would have a main effect on participation in the longer version. For controlled motivation, we did not have a specific a priori hypothesis as results thus far were mixed. In line with Study 1 and 2, we also did not hypothesize any interaction effects. In addition to measuring motivational constructs related to helping, we also assessed the same motivational constructs related to the alleged context of the study, which was sustainable behavior. We did not have any a priori hypotheses for these measures.

Study 3

Method

Participants and design. In Study 3, we were again interested in the role of motivation, and based on the correlations between the motivation measures and the dependent variables in Study 1 and 2, we defined the smallest effect size of interest as small. Therefore, we conducted a power analysis in G*power for a logistic regression with a minimum effect size of interest of $OR = 1.3$ (small), and used $\alpha = .05$, $\beta = .80$, $Pr(Y=1|X=1)$ $H0 = .5$, and R^2 *other X* = .13 (medium). This resulted in a minimum required sample size of 544. Since we only had to exclude one participant who failed our attention checks in Study 2, we decided to not oversample.

In this study, none of the participants failed both attention checks and thus no participants were excluded from further analyses. This resulted in a final sample size of 544 participants (332 female, 209 male, 3 Other/Rather not specify; $M_{age} = 34.83$, $SD_{age} = 12.94$). We again recruited from Prolific Academic and used the same inclusion criteria as in Study 1 and 2. Participants were rewarded with £1.20 for their participation, ostensibly

with £0.50 for the first study and £0.70 for the second study (flat fee). We used a one factor (Condition: control vs. default) between-subjects design with participation in the longer version of our study as dependent variable. The study was approved by the Ethics Committee of the Faculty of Social and Behavioral Sciences of Utrecht University under number 20-395.

Procedure. Participants were invited to participate in two ostensibly unrelated studies. One study would concern lifestyle and the other would concern sustainability. After they provided active informed consent, participants started the first study on Gorilla (Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2020) with the TSRQ, which we administered for the primary behavior of interest (helping other people), the secondary behavior of interest (making sustainable choices) as well as for one filler behavior (healthy eating). After that participants filled in a bogus lifestyle questionnaire with items on goal strivings for helping other people and making sustainable choices, as well as with filler items on healthy eating and saving money. After that, we used a fake debriefing to ostensibly end the first study and redirected participants to another platform (Qualtrics) for the second study. This was done in order to enhance the cover story of two separate studies. The second study started with informed consent and by asking participants whether they would be willing to participate in a longer version of the questionnaire of this second study. After that, we asked participants for satisfaction with their choice, about acceptability of the default nudge, and for their demographics. Finally, participants were debriefed, thanked and paid for their participation.

Materials.

Study choice task. Based on Wachner and colleagues (2020), we asked participants if they wanted to participate in a basic or a longer version of our second study, which would ostensibly be about sustainability. The basic version was estimated to last about 7 minutes, and the longer version would take an additional 5 minutes. Participants were told that they would not be compensated for their extra efforts, but that it would help researchers in improving their future questionnaires. This was done in order to communicate the prosocial nature of this decision.[4]

4. Immediately after participants had decided to participate in the basic or longer questionnaire, we measured satisfaction with their choice with 1 item ("How satisfied are you with your decision?"), measured on a 7-point Likert scale ranging from 1 (*not at all*) to 7 (*very much*). After that, we asked about acceptability and intrusiveness of the question to participate in a longer version and the way the options were presented. In the default condition, we explicitly mentioned that one option was preselected. We used four items on a 7-point Likert scale, ranging from 1 (*not at all*) to 7 (*very much*): "How acceptable do you find this question?", "How intrusive do you find this question?", "How acceptable do you find this way of presenting the options?", and "How intrusive do you find this way of presenting the options?" (see Supplementary Materials).

Measures.

Treatment Self-Regulation Questionnaire. We again used the TSRQ to measure autonomous motivation, controlled motivation, and amotivation. Participants rated the same 15 items, although some items needed to be slightly rephrased in order to match the behavior of helping other people. All statements were again measured on a 7-point Likert scale, ranging from 1 (*not at all true*) to 7 (*very true*). Composite scores were created by averaging the items that belonged to each subscale. The subscales for autonomous motivation (Cronbach's α = .84) and controlled motivation (Cronbach's α = .80) showed good reliability, while the subscale for amotivation (Cronbach's α = .58) showed to have poor reliability. Again, we had initially preregistered to not analyze the amotivation construct.

In order to enhance the strength of our cover story that this first study was about lifestyle, we also administered the TSRQ on healthy eating and making sustainable choices. Administration of the TRSQ for making sustainable choices simultaneously allowed us to explore those motivational constructs, as this would be related to the topic of the second study (sustainability). The subscales for autonomous motivation (Cronbach's α = .93) and controlled motivation (Cronbach's α = .79) showed good reliability, while the subscale for amotivation (Cronbach's α = .68) showed to have questionable reliability.

Goal strivings. We included the same lifestyle questionnaire as in Study 1 and 2. This time the four items of interest were focused on the behavior of helping other people. All questions were asked on a 7-point Likert scale, ranging from 1 (*not at all*) to 7 (*very much*). We also administered goal strivings for making sustainable choices, as this was related to the topic of the second study. The filler items about eating healthily and saving money were not analyzed and were solely included to conceal the goal of the study. The measure for goal strivings for helping other people had good internal reliability (Cronbach's α = .81), and thus all items were combined into one score by averaging the four items. This was also done for the items for making sustainable choices, as this also revealed to have good internal reliability (Cronbach's α = .88).

Demographics. We asked participants for their age (in years) and gender (female, male, other/rather not specify).

Results

Data are available on the Open Science Framework (https://osf.io/7w4vf/).

Preprocessing steps. No participants failed both attention checks and thus no participants were excluded from the analyses. Outliers were defined as 3 SDs away from the mean and were set missing for the most important variables. This applied to our measures of autonomous motivation (5 participants), amotivation (4 participants) and goal strivings (4 participants). All analyses were run with inclusion and exclusion of these outliers, but this did not change any of the results (i.e., direction or significance of effects). Therefore, we report on the entire sample with inclusion of outliers.

Descriptives. On average, participants scored high on the measure of goal strivings

for helping other people (*M* = 5.37, *SD* = 1.07). Autonomous motivation was highest (*M* = 5.26, *SD* = 1.08), while controlled motivation was slightly below the midpoint (*M* = 3.78, *SD* = 1.18). Amotivation was relatively low (*M* = 2.33, *SD* = 1.09). See Table 4.5 for a full overview of the descriptives and correlation coefficients.

Table 4.5

Descriptives and correlation coefficients for autonomous motivation, controlled motivation, and goal strivings for helping other people (Study 3).

	Mean (SD)	Range	1	2	3	4
1. Goal strivings	5.37 (1.07)	1.25 – 7	(.81)			
2. Autonomous motivation	5.26 (1.08)	1.50 – 7	.67***	(.84)		
3. Controlled motivation	3.78 (1.18)	1 – 7	.29***	.30***	(.80)	
4. Amotivation	2.33 (1.09)	1 – 6.67	-.25***	-.28***	.22***	(.58)

Note. Cronbach's alphas are shown in the diagonal. *N* = 544. *** *p* < .001

Regarding the motivation to make sustainable choices, participants scored moderately high on the measure of goal strivings (*M* = 4.66, *SD* = 1.31). Especially autonomous motivation was high (*M* = 5.30, *SD* = 1.33), while controlled motivation was slightly below the midpoint (*M* = 3.61, *SD* = 1.20). Amotivation was relatively low (*M* = 2.36, *SD* = 1.29). See Table 4.6 for a full overview of the descriptives and correlation coefficients.

Table 4.6

Descriptives and correlation coefficients for autonomous motivation, controlled motivation, and goal strivings for making sustainable choices (Study 3).

	Mean (SD)	Range	1	2	3	4
1. Goal strivings	4.66 (1.31)	1 – 7	(.79)			
2. Autonomous motivation	5.30 (1.33)	1 – 7	.79***	(.93)		
3. Controlled motivation	3.61 (1.20)	1 – 6.67	.37***	.44***	(.79)	
4. Amotivation	2.36 (1.29)	1 – 6	-.48***	-.56***	.10*	(.68)

Note. Cronbach's alphas are shown in the diagonal. *N* = 544. *** *p* < .001, * *p* < .05

Randomization check. We performed the same analyses as in Study 1 and 2 to check for randomization of participants across the two conditions, and included age, gender, goal strivings, autonomous motivation, controlled motivation, and amotivation as dependent variables. Results showed that randomization was successful (all *ps* > .079).

Main analyses. We ran a Chi-squared analysis to analyze the effect of the default nudge on the likelihood of participating in the longer version. The default nudge had a small but significant effect, $\chi^2(1) = 5.04$, $p = .025$, $OR = 1.48$, such that more participants chose the longer version in the default condition (46.9%) than in the control condition (37.4%).

In order to analyze whether motivation moderates nudge effectiveness, we conducted separate hierarchical logistic regressions on the likelihood of participating in the longer version, starting with the default nudge in step 1 (with the control condition coded as 0 and the default condition coded as 1), the motivation measures in step 2, and the interaction effect in step 3. Not surprisingly, the default nudge turned out significant in our regression models as well ($\beta = .20$, $p = .025$, $R_N^2 = .01$). Model fit did not improve by adding main effects for goal strivings ($p = .412$), autonomous motivation ($p = .161$), controlled motivation ($p = .502$), or amotivation ($p = .602$), nor did it improve by adding interaction effects (all *p*s > .280), and no main effects of or interaction effects with motivation for helping other people were found.

Motivation sustainability. Since the second part of Study 3 was framed as a study on sustainability, we were also interested in the role of motivation to make sustainable choices in relation to the effectiveness of the nudge. We analyzed this by conducting the same hierarchical logistic regressions as for motivation for helping other people, but then with goal strivings, autonomous motivation, controlled motivation, and amotivation for making sustainable choices (see Table 4.7 for the complete regression results). Adding goal strivings to the base model improved model fit ($R_N^2 = .03$, $p = .003$), and goal strivings for making sustainable choices significantly predicted the likelihood of participating in the longer version ($\beta = .27$ $p = .003$). Similarly, adding autonomous motivation to the base model improved model fit ($R_N^2 = .03$, $p = .017$), and autonomous motivation for making sustainable choices significantly predicted the likelihood of participating in the longer version ($\beta = .21$, $p = .018$). Adding controlled motivation to the base model did not improve model fit ($p = .491$). Adding amotivation for making sustainable choices to the base model marginally significantly improved model fit ($R_N^2 = .02$, $p = .060$), and amotivation for making sustainable choices marginally significantly predicted the likelihood of participating in the longer version ($\beta = -.17$, $p = .062$). Adding interaction terms did not improve model fit for any of the motivational constructs (all *p*s > .185).

In order to analyze whether the default adds an increased propensity of choosing the desired option, we also ran stepwise regressions starting with motivation for making sustainable choices in step 1 and adding the condition variable in step 2. These analyses consistently revealed that the default predicted the likelihood of participating in the longer version when accounting for motivation with small but significant improvements in model fit of $R_N^2 = 0.01$ (see Supplementary Materials A for full details).

Discussion Study 3

In Study 3 we again found a significant default effect and showed that the default manipulation increased participation in the longer questionnaire. We intended to replicate findings from Study 1 and 2 with another default nudge that we anticipated to qualify as a different kind of behavior than the behaviors studied in Study 1 and 2. We expected to find effects of goal strivings and autonomous motivation to help other people on our behavior of interest (participating in a longer version). Yet, it seems that this behavior was

Table 4.7

Logistic regression model for motivation to make sustainable choices, and with the likelihood of participating in the longer version as dependent variable (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1	.01						.025
Constant		-0.52 (0.13)		0.60	[0.47, 0.76]	-4.13	< .001
Default		0.39 (0.17)	.20	1.48	[1.05, 2.08]	2.24	.025
Step 2 – Goal strivings	.02						.003
Constant		-1.47 (0.35)		0.23	[0.11, 0.45]	-4.20	< .001
Default		0.39 (0.18)	.19	1.48	[1.05, 2.10]	2.23	.025
Goal strivings		0.20 (0.07)	.27	1.23	[1.07, 1.41]	2.96	.003
Step 2 – Autonomous motivation	.01						.017
Constant		-1.37 (0.39)		0.25	[0.12, 0.54]	-3.55	< .001
Default		0.39 (0.18)	.20	1.48	[1.05, 2.09]	2.24	.025
Autonomous motivation		0.16 (0.07)	.21	1.17	[1.03, 1.34]	2.36	.018
Step 2 – Controlled motivation	.00						.491
Step 2 – Amotivation	.01						.060
Constant		-0.22 (0.20)		0.81	[0.54, 1.20]	-1.06	.288
Default		0.39 (0.18)	.20	1.48	[1.05, 2.09]	2.24	.025
Amotivation		-0.13 (0.07)	-.17	0.88	[0.77, 1.01]	-1.86	.062

Note. *β*'s are X-standardized. *N* = 544.

not seen as an act of helping other people, as our motivational measures of helping other people did not have any predictive value. This was also affirmed by anecdotal evidence from the open-ended question at the end of the study, in which participants could enter any final thoughts after the debriefing. In this open-ended question, some participants indicated that they would only regard participating in a longer questionnaire as helping other people if they knew whom they would be helping, which was not the case in our study. This complicated the goal of answering our main research question on the role of motivation.

However, we informed participants that the second study would be about sustainability and had included measures of motivation for making sustainable choices in the first study. Exploratory analyses revealed that the motivation to make sustainable choices did affect the decision to participate in a longer version of our questionnaire on sustainability, and thus it is likely that participants attributed this decision as an act of sustainability. When looking at the role of motivation to make sustainable choices in nudging participation in a longer version on sustainability, a similar pattern of results emerged as in studies 1 and 2. Autonomous motivation and goal strivings had positive main effects on the

decision, amotivation had a (marginally significant) negative main effect on the decision, but controlled motivation did not affect the decision. We also did not find any significant interaction effects.

General Discussion

In three high-powered studies we investigated the role of motivation for the effectiveness of nudging interventions. How nudges and motivation together predict behavior was thus far not clear, nor whether relatively small interventions like nudges can in fact add an increased propensity to perform desirable behavior when accounting for people's motivation. To address this, we conducted three studies with different types of defaults across different behavioral domains, and we focused on a generic measure of motivation strength (goal strivings) as well as on autonomous motivation, controlled motivation, and amotivation.

Across three studies we consistently found significant default effects. The effect sizes varied greatly and were thus heterogenous across the three studies, ranging from small (Study 3) to very large (Study 2). This is consistent with a recent meta-analysis which revealed an average medium to large effect size of defaults, but with considerable heterogeneity across studies (Jachimowicz et al., 2019). In our studies, the size of the default effect was likely to be contingent on the operationalization of the default as well as the impact of the choice on behavior. For example, in Study 2 – the study with the largest effect size – we implemented an all-or-nothing default such that all 14 amenities were either preselected or not. In contrast, in Study 3 – the study with the smallest effect size – only one option was preselected and the nudged choice had immediate behavioral implications. Nevertheless, in line with previous studies we found robust evidence in the current set of studies that defaults can effectively steer behavior across a range of different behaviors with practically meaningful effect sizes.

We also found consistent evidence over different behavioral domains that goal strivings and autonomous motivation significantly predict behavior in the presence of a default. That is, across the behavioral domains of healthy eating and sustainability, and thus across pro-self and pro-social nudges, we found that goal strivings and autonomous motivation had a significant main effect on the behavior of interest. This corroborates findings from studies on the role attitudes in nudges' effectiveness (Kaiser et al., 2020; Kuhn et al., 2021; Taube & Vetter, 2019; Vetter & Kutzner, 2016), which have also revealed main effects only on behavioral outcomes. Controlled motivation did not affect our behavioral outcomes in Study 1 and 3, while it significantly predicted sustainable behavior in Study 2, but with negligible predictive power and little practical significance. We thus showed that autonomous motivation is a consistent driver of healthy and sustainable behavior, while controlled motivation is not. This is largely in line with previous studies based on Self-Determination Theory which have shown larger and more consistent effects of more self-determined motivations across a range of healthy and sustainable behaviors (e.g., Hagger et al., 2014; Pelletier, 2002). Finally, amotivation negatively affected our behavioral

outcomes, but we only analyzed amotivation exploratory and with low internal reliability. We do note that in Study 3 we did not find any effects of motivation to help other people, but in retrospect we believe that participants did not consider the behavior of interest as an act of helping other people but rather as an act of sustainability since the cover story instructed them that the study would be about this topic.

Across the three studies, we did not observe any significant interaction patterns between the default nudge and motivation. We did not test for equivalence or weigh the strength of evidence for this null-effect, but we can conclude that we have not found evidence that the default effect was moderated by a certain level or type of motivation in our sample. The absence of these interaction effects could imply that motivation does not constitute a boundary condition for nudge effectiveness. Yet, we believe that this conclusion is premature, given that people were already moderately to strongly motivated to perform the behavior that was stimulated. As most nudges are intended to stimulate rather undisputed and simplified behaviors, we are not the first to encounter this practical limitation (e.g., Venema et al., 2019). Nevertheless, given these high scores on motivation, it was particularly relevant to observe that the default nudges still stimulated behavior when accounting for this strong motivation, suggesting that defaults do indeed have added value for motivated behavior. If motivation were to form a boundary condition such that people who are not motivated will not be nudged, one would require a more controversial behavior or a more diverse sample with more variation in the types and strength of motivation. In a way, this could eventually become more of a theoretical exercise than a practically informative discussion, as nudges are by and large intended to stimulate behaviors that are either beneficial for individuals themselves or society at large, but it nevertheless remains an important void in nudging research to fill.

Implications

In line with previous studies, we show that defaults can have a considerable impact on choice outcomes, but that people are still capable of deviating from the status quo (e.g., Van Gestel et al., 2020a). In a way one could compare this to the anchoring and adjustment heuristic (Tversky & Kahneman, 1974). The default sets an anchor, from which people can adjust according to the strength and type of motivation. Those who are strongly autonomously motivated are still capable of making more healthy or sustainable choices, while those who are less motivated, or amotivated, are capable of making less healthy or sustainable choices. Relative to each other, results showed that motivation was a stronger predictor in Study 1 and 3, but that the default had a higher impact on sustainable decisions in Study 2.

These findings have practical implications for policy makers who wish to steer behavior according to the libertarian paternalism principle (Thaler & Sunstein, 2003). Our results show that defaults are indeed paternalistic in the sense that policy makers have to make a normative decision about what option to make the default, but also that they are libertarian in the sense that individuals can adjust from this anchor according to their own

motivation. Yet, policy makers do have to be aware of the absolute strength of the default manipulation, which can differ across types of defaults, as well as its relative strength in relation to motivation, as our studies point out that in some cases the environment may have a larger effect on behavioral outcomes than intra-individual tendencies.

Similarly, this finding has implications for the debate on the ethics of nudges, which is extensively held based on theoretical assumptions. It is often assumed that nudges may violate autonomy through manipulation (Hansen & Jespersen, 2013; Wilkinson, 2013), and need for autonomy may be decreased if defaults are installed for choices that would be fairly simple to make without the nudge (Arvantitis et al., 2020). Yet, for more complex choices this hampering effect on autonomy does not seem to exist, and recent insights have also inspired the perspective that nudges may support people live their lives without the necessity of continuous deliberation to act upon their intentions (De Ridder et al., 2020; Vugts, Van den Hoven, De Vet, & Verweij, 2020). The current studies offer empirical data to inform this debate and demonstrate that default nudges are effective in stimulating behavior that people are generally motivated for. According to Bovens (2009), nudges that bring people's actions in line with their own preferences, or nudges that are in people's own interest, are more legitimate. In our studies we could argue that this was the case. Across all three studies, the default effects did not cancel out any motivational effects, but instead gave people an additional push in performing the behavior that they were motivated for. In fact, in our studies, in which people were overall highly motivated to perform the desirable behavior, one could argue that the default leverages individual's motivation, a phenomenon referred to as facilitation (Saghai, 2013). Yet, the more crucial question for the legitimacy of nudging is whether actions would remain in line with people's own intentions if those are opposite of the intention of the nudge. The implications of the current studies for the ethics of nudging are thus limited to the higher spectrum of motivational strength, while concerns are more pronounced for the lower spectrum.

Limitations and future research

There are several limitations to our studies that should be taken into account when interpreting these findings. First and foremost, our studies are limited to behaviors that people, at least on a group level, are moderately to highly motivated for. On average, motivation for healthy eating and making sustainable choices was relatively high in our samples, and these behaviors are the two most frequently nudged behaviors in empirical research (Szaszi, Palinkas, Palfi, Szollosi, & Aczel, 2018). One could argue that these kinds of behaviors that people are motivated for are ideally suitable for nudging and that this makes it more legitimate, but the legitimate use of nudges in empirical research could eventually hinder gaining further insights in possible boundary conditions. The implications of these studies are mostly applicable to the idea of nudging for the common good, while it may not shed as much light on those individuals who are clearly opposed to this given behavior. The linear trends that we found in our studies should thus be interpreted with

caution, and future research should investigate whether these trends uphold for behaviors with more variation in motivational strength across people. Especially, further research is needed to study the role of motivation in nudging people with low motivation (i.e., counteractive motives) and (stronger) amotivation (i.e., indifference), as in our studies we were only able to include amotivation exploratorily with poor to questionable internal reliability. We expect that the reduction of meat consumption could be an interesting behavior of interest for future research on boundary conditions, as the motivation to perform this behavior is less widespread across the population with a substantial group of fully committed meat eaters (Malek, Umberger, & Goddard, 2019). Furthermore, another approach where defaults are installed that promote unhealthy or unsustainable options could aid in painting a completer picture of the relationship between motivation and nudge effectiveness.

Another limitation of our studies is that we only used default nudges, which are oftentimes referred to as the most prototypical type 1 nudge (i.e., requiring the least cognitive effort to be effective; Hansen & Jespersen, 2013; Jung & Mellers, 2016; Sunstein, 2016). A remaining question is whether our results, which are robust across different types of defaults, are also robust across different types of nudges, such as social proof (Venema et al., 2020) or environmental restructuring (Van Gestel et al., 2018; Van Gestel et al., 2020b). Finally, it should be noted that we only measured motivation, and that manipulations of motivation could further strengthen the evidence base.

Conclusion

In conclusion, we showed that a default nudge and motivation both have a main effect on behaviors that people are at least moderately motivated for. The default nudge sets the anchor, from which people can adjust according to the type and strength of their motivation. This implies that autonomous decision making is still possible, even if the environmental manipulation may have a considerable effect on behavior. If nudges are indeed implemented as they are intended (i.e., "Nudge for good"), they will likely support people to act in line with their own motivation. However, our results are limited to those cases in which the goal of the nudge is in line with the motivational orientation of the individual and future research is still required in order to study whether nudges leave room for an emergency exit in case the direction of the nudge is against people's own wishes.

Chapter 5

Who accepts nudges? Nudge acceptability from a self-regulation perspective

5

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LG, MA, and DR conceptualized the research idea and design of the study. LG collected and analyzed the data and interpreted the data in consultation with MA and DR. LG wrote a first draft of the manuscript. MA and DR provided critical feedback on the manuscript.

Abstract

Background: Public acceptability of nudging is receiving increasingly more attention, but studies remain limited to evaluations of aspects of the nudge itself or (inferred intentions) of the nudger. Yet, it is important to investigate which individuals are likely to accept nudges, as those who are supposed to benefit from the implementation should not oppose it. The main objective of this study was to integrate research on self-regulation and nudging, and to examine acceptability of nudges as a function of self-regulation capacity and motivation.

Method: Participants (N = 301) filled in questionnaires about several components of self-regulation capacity (self-control, proactive coping competence, self-efficacy, perceived control, and perceived difficulty) and motivation (autonomous motivation and controlled motivation). To evaluate nudge acceptability, we used three vignettes describing three types of nudges (default, portion size, and rearrangement) that stimulated either a pro-self behavior (healthy eating) or pro-social behavior (sustainable eating) and asked participants to rate the nudges on (aspects of) acceptability.

Results: Results revealed that there were substantial differences in acceptability between the three types of nudges, such that the default nudge was seen as less acceptable and the rearrangement nudge as most acceptable. The behavior that was stimulated did not affect acceptability, even though the nudges that targeted healthy eating were seen as more pro-self than the nudges targeting sustainable eating. From all self-regulation components, autonomous motivation was the only consistent predictor of nudge acceptability across the three nudges. For self-regulatory capacity, only some elements were occasionally related to acceptability for some nudges.

Conclusion: The current study thus shows that people are more inclined to accept nudges that target behaviors that they are autonomously motivated for, while people do not meaningfully base their judgements of acceptability on self-regulatory capacity.

For years, behavioral science has focused on improving self-regulation as a central route for success in life (Baumeister & Vohs, 2004). More recently, the focus of behavior change experts gradually shifted from the individual and its capacities to the environment in which the individual navigates. With this shift came the interest in nudging as a novel and supplemental behavior change technique (Thaler & Sunstein, 2008). Yet, currently there is little knowledge about the interplay between self-regulatory processes and strategic changes in the environment to stimulate desirable behavior, and there is a need for integration of these two routes towards behavior change in order to obtain a better understanding of the promise and pitfalls of nudging. Research on a more detailed understanding of when and for whom nudges are effective is emerging (e.g., De Ridder, Kroese, & Van Gestel, in press; Van Gestel, Adriaanse, & De Ridder, 2020a; Van Gestel, Adriaanse, & De Ridder, in press), but similarly, one could expect that people who are more motivated and/or more capable to self-regulate could differ in the extent to which they are open to being nudged. For example, one could speculate that those who are successful at self-regulating could be the ones who welcome nudges as they do not feel threatened themselves, but one could similarly expect that those who are less successful in self-regulating might welcome nudges as they recognize its potential in helping them achieve their desired end states. In the current study we investigate the link between self-regulation and openness to being nudged through the lens of nudge acceptability.

Currently, there is a lack of knowledge about which individuals are more prone to accept nudges than others. It is important to get a better understanding of this, given that the very people who are supposed to benefit from a nudge intervention should welcome, or at the very least not oppose, the implementation of the nudge. We thus propose to focus more on individual self-regulation capacity and motivation in studying nudges' acceptability and aim to shed light on the question whether those who need it and/or those who want it have more favorable views about nudges. In doing so, we conducted a study on the relation between several important self-regulation concepts and nudges' acceptability across a pro-self (healthy eating) and a pro-social (sustainable eating) behavior. This distinction between pro-self and pro-social nudges has – apart from differing implications for self-regulation capacity and motivation – previously led to differences in judgements of acceptability, such that pro-social nudges that contributed to the greater good were judged as less acceptable (Hagman, Andersson, Västfjäll, & Tinghög, 2015).

Nudge acceptability

Population wide statistics regarding the acceptance of nudging are generally high and generally demonstrate majority support for the most well-known nudges (e.g., Hagman et al., 2015; Reisch & Sunstein, 2016; Sunstein, Reisch, & Rauber, 2018). So far, research on acceptability of nudges has mostly focused on factors inherent to the nudge, such as actual, perceived, and communicated effectiveness of the nudge (Bang, Shu, & Weber, 2020; Cadario & Chandon, 2019; Diepeveen, Ling, Suhrcke, Roland, & Marteau, 2013; Pechey,

Burge, Mentzakis, Suhrcke, & Marteau, 2014; Petrescu, Hollands, Couturier, Ng, & Marteau, 2016), the target group of the nudge (Bang et al., 2020), and perceived intrusiveness of the nudge (Evers, Marchiori, Junghans, Cremers, & De Ridder, 2018). Recent studies have also focused on nudges' acceptability in relation to dispositions and intentions of the policy maker implementing the nudge, such as the source of the nudge (Bang et al., 2020), his or her political orientation (Tannenbaum, Fox, & Rogers, 2017), and trustworthiness of the nudger (Evers et al., 2018). Taken together, there is a growing number of studies that focus on the acceptability of nudges, but, as illustrated above, the focus remains largely limited to aspects of the nudge or nudger.

Individual factors that reveal which people are more likely to accept nudges, however, have rarely been studied, and studies thus far focused on largely stable factors such as traits and demographics (e.g., Loibl, Sunstein, Rauber, & Reisch, 2018; Petrescu et al., 2016). These studies have mostly revealed no or inconsistent associations between those factors and nudge acceptability. Yet, nudges' acceptability may possibly also depend on self-regulatory capacity – that determines whether people are able to adjust their behavior in line with their goals – and motivation. From an implementation science perspective one ought to know whether nudges are accepted by those who are being targeted by the intervention, while from an integrative behavioral science perspective more knowledge is required about the interplay between individual motivation and capacity and interventions in the immediate choice environment. In other words, a thorough investigation of individual factors that move beyond demographics is crucially missing, and public policy makers should be better informed about whether their target group that should benefit from the nudge is also likely to accept it.

Self-regulation and nudge acceptability

When investigating nudge acceptability from a self-regulation perspective, one could identify a plethora of potentially relevant factors. Inspired by two related frameworks for understanding human self-regulatory behavior – the COM-B system which includes capacity, opportunity and motivation (Michie, Van Stralen, & West, 2011) and the Fogg Behavior Model which includes ability, prompts, and motivation (Fogg, 2009) – we distinguished between two main clusters: capacity and motivation. The nudges themselves can be seen as a prompt or opportunity in these behavioral models.

Capacity. Self-control is the ability to transcend short-term gratifications in order to achieve long-term goals (Tangney, Baumeister, & Boone, 2004). High trait self-control has been associated with advantageous outcomes in several domains such as school and work performance, social relationships and health (De Ridder, Lensvelt-Mulders, Finkenauer, Stok, & Baumeister, 2012). Traditionally, self-control has been viewed as the effortful inhibition of unwanted impulses (Baumeister, Bratslavsky, Muraven, & Tice, 1998). However, more recently, the notion of effortless self-control has been introduced (Gillebaart & De Ridder, 2015), which denotes that successful self-control relies on rather effortless strategies such as proactively avoiding response conflicts. Related concepts

such as situational self-control (Duckworth, Gendler, & Gross, 2016; Duckworth, Milkman, & Laibson, 2018) and self-nudging (Reijula & Hertwig, 2020) also highlight the potential of changing one's environment in order to achieve self-control successes. As a consequence, nudges may be appreciated as helpful in acting upon self-control, as nudges may take away potential barriers to achieve long-term success. On the contrary, nudges may also be seen as intrusive or irrelevant, and may invoke feelings of reactance (Brehm & Brehm, 1981), especially among those who consider themselves high in self-control. Nevertheless, recent developments in self-control research highlight its potential importance in predicting nudge acceptability, and give reason to suspect that successful self-control and nudging acceptability may be related.

Apart from having low or high self-control, individuals may differ in the extent to which they possess a rich toolbox of self-regulatory skills. We specifically focus on a set of skills that are referred to as proactive coping skills: "efforts undertaken in advance of a potentially stressful event to prevent it or to modify its form before it occurs" (Aspinwall & Taylor, 1997, p. 417). This set of skills consists of actions that one can take *prior to* exposure to a potential stressor, and it includes skills such as planning and monitoring. Those who are high in proactive coping competence may appreciate a nudge as it may take away an anticipated barrier to achieving one's goals. Finally, we also focused on self-efficacy, which refers to the belief that one can successfully pursue a course of action in light of potential setbacks (Bandura, 1982), as well as perceived control and perceived difficulty of performing the desired behavior.

Motivation. Surprisingly little attention has been paid to the role of motivation in nudging acceptability. A common finding in the literature on public attitudes towards policy measures is that people are motivated by self-interest (Blekesaune & Quadagno, 2003; Diepeveen et al., 2013). This implies that those who are likely to benefit from a certain policy measure are most likely to support the policy, while those who will not benefit or will be disadvantaged by a measure are most likely to dislike the policy. This finding has, for example, been found among smokers who are more likely to oppose measures aimed at reducing smoking (Diepeveen et al., 2013). However, this typical self-interest finding has thus far received little attention in nudging research. In line with previous research on other types of policies, it is likely that those who are motivated to perform a certain behavior are more likely to support nudges that stimulate that behavior. In our own research, we previously found that acceptability of a default correlated weakly but positively with autonomous motivation and negatively with amotivation, while no association was found with controlled motivation (Van Gestel et al., supplemental material, in press). Yet, apart from our own work, we are currently not aware of any other studies that focused on motivation and nudge acceptability. We intend to build upon our earlier findings and will also focus on autonomous motivation, controlled motivation, and amotivation (Ryan & Deci, 2000) in the present study.

The current study

To gain a better understanding of the relation between self-regulation and nudge acceptability, we conducted a study with three vignettes describing three types of nudges. We administered several questionnaires about self-regulation capacity and motivation, related to either healthy food intake or sustainable food choices. Next, depending on the experimental condition, participants read three vignettes with three different nudges that promoted either healthy food choices or sustainable food choices at work and rated the three nudges on our primary variable of interest (acceptability) and three related measures (intrusiveness, perceived effectiveness, and goal alignment). We used the same three vignettes in the healthy eating condition as in the sustainable eating condition, but manipulated the rationale for implementing the nudge such that it would either be seen as a pro-self behavior (healthy eating) or a pro-social behavior (sustainable eating). We did this in order to be able to generalize our results, as previous studies have revealed differences in acceptability dependent on the pro-self or pro-social nature of the nudge (Hagman et al., 2015). Moreover, these different dimensions could have implications for the understanding of self-regulation. While healthy eating has a rather distant benefit for individual health, sustainable eating possibly has an even more distant benefit that transcends individual gratification. Finally, healthy and sustainable behavior are the two most often used behaviors in nudging research (Szaszi et al., 2018). We did not formulate a priori hypotheses, but did anticipate to find associations between nudge acceptability and factors of self-regulation capacity and motivation. We preregistered the study at AsPredicted where we included a basic analysis plan (https://aspredicted.org/blind.php?x=hx2y8a).

Method

Participants and design

For half of the participants the study focused on the behavioral domain of healthy eating while for the other half the study focused on sustainable eating. The present study thus used a mixed design with the behavioral domain (healthy eating vs. sustainable eating) as between-subjects factor and type of nudge (default vs. portion size vs. rearrangement) as within-subjects factor. We decided a priori to collect data from 300 participants (150 per behavioral domain), which we deemed adequate to detect a medium-sized effect with 80% power, and substantial enough to explore the data with enough flexibility. To illustrate, with 150 participants one can detect correlations of at least $r = .20$ with $\alpha = .05$ and $\beta = .80$.

We collected data on Prolific Academic from adult participants with a UK nationality and a minimum approval rate of 95%. We included 301 participants (157 female, 142 male, 2 Other/Rather not specify; $M_{age} = 38.37$, $SD_{age} = 14.58$). None of the participants failed the two attention checks and thus no participants were excluded from further analyses. Participants were rewarded with £1.00 for their participation.

Procedure

Participants were invited to participate in a questionnaire study on self-regulation, motivation, and the environment in which people make food-related decisions. After they provided informed consent, participants were randomly allocated to either the healthy eating or sustainable eating condition. Next, we administered several questionnaires. The first set of questionnaires pertained to self-regulatory concepts related to healthy eating or sustainable eating. We then asked participants to rate the trustworthiness of their employer. Subsequently, in order to check our assumption that healthy eating would be seen as more pro-self than sustainable eating, we asked participants to rate the behavior of interest on a dimension of pro-self vs. pro-social. After these questionnaires, we showed participants three vignettes describing three different types of nudges in random order.

The vignettes described a situation after the COVID-19 pandemic in which the participant's employer had decided to promote good health or sustainability via a nudging intervention. For each vignette, participants were asked to rate acceptability of the nudge and related concepts, which served as our dependent measures. After the questions about the nudges, we asked participants for their demographics (age and gender), for their frequency of going to work, for their frequency of buying food at work, and ended with an open question in which participants could write any final thoughts. Finally, participants were debriefed, thanked, and paid for their participation.

Measures and materials

Self-regulatory capacity. Self-regulatory capacity was assessed in three parts: (1) Self-control, (2) proactive coping competence, and (3) self-efficacy, perceived control, and perceived difficulty.

Self-control. Self-control was measured with the Brief Self-Control Scale (BSCS; Tangney et al., 2004). The scale contains 13 items (e.g., "I am good at resisting temptations") measured on a 5-point Likert scale ranging from 1 (*not at all*) to 5 (*very much*). The scale has been validated and applied in previous research (e.g., Tangney et al., 2004). Nine items were reversed before creating the composite score and the scale had good reliability (Cronbach's $\alpha = .87$).

Proactive coping competence. Proactive coping was measured using the Utrecht Proactive Coping Competence scale (UPCC; Bode, Thoolen, & De Ridder, 2008). The scale consists of 21 items measured on a 4-point Likert scale ranging from 1 (*not competent*) to 4 (*very competent*). Participants were asked to rate their competency of several skills such as "Recognizing signals that something might go wrong", "Translating my desires into plans", and "Evaluating whether I accomplished the goal I wanted to reach". The scale has been validated and applied in previous research (e.g., Bode et al., 2008). The scale had good reliability (Cronbach's $\alpha = .89$).

Self-efficacy, perceived control, and perceived difficulty. Self-efficacy ("I am confident in my ability to eat a healthy/sustainable diet"), perceived control ("Eating a healthy/sustainable diet is in my own hands") and perceived difficulty ("I find it difficult to eat a

healthy/sustainable diet") were measured with one item each on a 7-point Likert scale ranging from 1 (*not at all*) to 7 (*very much*).

Motivation. In line with Van Gestel and colleagues (in press), we measured three different types of motivation: Autonomous motivation, controlled motivation, and amotivation. These different types of motivation for eating a healthy or sustainable diet were measured using the Treatment Self-Regulation Questionnaire (TSRQ; Levesque et al., 2007). Participants were asked to rate reasons for eating a healthy or sustainable diet, dependent on the condition they were in. The scale consists of 15 items: 6 for autonomous motivation (e.g., "Because I feel that I want to take responsibility for my own health"), 6 for controlled motivation (e.g., "Because I feel pressure from others to do so"), and 3 for amotivation (e.g., "I really don't think about it"). All statements were measured on a 7-point Likert scale, ranging from 1 (*not at all true*) to 7 (*very true*). The scale has been validated and used in previous research for both behaviors of interest (Van Gestel et al., in press; Levesque et al., 2007). The subscales for autonomous motivation (Cronbach's α = .90) and controlled motivation (Cronbach's α = .82) showed good reliability. The subscale for amotivation (Cronbach's α = .58) had poor reliability and was excluded from further analyses.

Trustworthiness. Trustworthiness of the employer was measured with 1 item ("To what degree do you regard your employer as trustworthy?") on a 7-point Likert scale ranging from 1 (*not at all trustworthy*) to 7 (*very trustworthy*).

Pro-self vs. pro-social dimension. To assess our assumption that eating a healthy diet is more of a pro-self behavior than eating a sustainable diet, we asked participants to rate one statement ("Eating a healthy/sustainable diet is something I do for…") on a continuous slider ranging from 0 (*myself*) to 100 (*society*).

Vignettes. We used three vignettes for each behavioral domain that described three different nudges: default, portion size, and rearrangement. We chose these three behaviorally-oriented nudges as these have been shown to be among the most effective in the domain of eating behavior (Cadario & Chandon, 2020). The vignettes described a situation in which the employer would encourage healthy or sustainable eating via one of these nudging interventions. The formulation of the vignettes largely followed the same structure: "In order to promote [good health/sustainability] by [behavioral outcome], your employer has decided that [nudge]." See Table 5.1 for a full description of all six nudge vignettes.

Ratings of nudges. For each vignette, we asked participants to rate several aspects of the nudge. The main dependent variable – the extent to which people would accept the nudges – was measured with three items ("How much would you accept the implementation of this measure?", "How much would you appreciate the implementation of this measure?", and "How much would you support the implementation of this measure?"), all measured on a continuous slider ranging from 0% to 100%. These three items were averaged into a composite score for acceptability of the nudge (Cronbach's α

Table 5.1

The nudge vignettes used in the healthy eating or sustainable eating conditions describing the default nudge, portion size nudge, or rearrangement nudge.

	Healthy eating	Sustainable eating
Default	In order to promote good health by eating less meat, your employer has decided that all lunch orders are now automatically vegetarian, unless otherwise specified.	In order to promote sustainability by eating less meat, your employer has decided that all lunch orders are now automatically vegetarian, unless otherwise specified.
Portion size	In order to promote good health by reducing portion sizes, your employer has decided to use smaller plates to reduce consumption in the self-service cafeteria.	In order to promote sustainability by reducing portion sizes, your employer has decided to use smaller plates to reduce food waste in the self-service cafeteria.
Rearrangement	In order to promote good health by eating differently, your employer has decided to rearrange the buffet such that healthier foods are presented first.	In order to promote sustainability by eating differently, your employer has decided to rearrange the buffet such that more sustainable foods are presented first.

= .94). Other measures pertained to ratings of intrusiveness ("How intrusive do you find this measure?"), perceived effectiveness ("How effective do you think this measure would be?"), and goal alignment ("To what extent is this measure in line with your own goal?"). These were measured with one item each, also on a continuous slider ranging from 0% to 100%.

Demographics. We asked participants for their age (in years) and gender (female, male, other/rather not specify). We asked participants for the frequency of going to work ("In the past two weeks, how often did you go to work?") with answer options ranging from 0 to 14 times in total, plus an option to indicate unemployment. We also asked for the frequency of buying food at work ("How often do you buy something to eat at work?") with the answer options never, rarely, sometimes, often, always, and not employed. Finally, we included one open question in which participants could write anything they deemed relevant.

Results

Data and code are available on the Open Science Framework (https://osf.io/cnsdm/).

Preprocessing steps. As preregistered, outliers were defined as 3 *SD*s away from the mean and were set missing. This only applied to the measure of perceived control (5 participants). All analyses were run with inclusion and exclusion of these outliers, but this did not change any of the results. Therefore, we report on the entire sample with inclusion of outliers.

Descriptives. On average, participants regarded their employer as quite trustworthy (*M* = 4.90, *SD* = 1.54). At the time of data collection (December 2020), about a third of the participants worked from home completely (*n* = 109). 62 participants were unemployed. The remaining 130 participants on average had gone to their work location 6.27 times (*SD* = 3.74) in the previous two weeks.[1]

Most participants were at least somewhat familiar with buying food items at work. Of those who had indicated to be employed, 31% often or always bought food at work, 23% sometimes did so, and finally 46% rarely or never did so.

Participants on average scored around the mid-point of the self-control scale (*M* = 3.00, *SD* = 0.70) and reported to be relatively competent in proactive coping (*M* = 2.91, *SD* = 0.41). Participants also reported to feel relatively efficacious about eating a healthy or sustainable diet (*M* = 4.67, *SD* = 1.51), felt in control (*M* = 5.76, *SD* = 1.19), and scored around the midpoint of the scale for perceived difficulty (*M* = 4.11, *SD* = 1.64). Motivation for autonomous reasons was relatively high (*M* = 4.86, *SD* = 1.29), while motivation for controlled reasons was considerably lower (*M* = 3.04, *SD* = 1.22). Full descriptives and correlation coefficients of the self-regulatory concepts are reported in Table 5.2.

Table 5.2

Descriptives and correlation coefficients for self-regulatory concepts.

	Mean (SD)	Range	1	2	3	4	5	6	7	8
1. Self-Control	3.00 (0.70)	1.23 – 5.00	(.87)							
2. Proactive Coping Competence	2.91 (0.43)	1.71 – 4.00	.49***	(.89)						
3. Self-efficacy	4.67 (1.51)	1.00 – 7.00	.15**	.14*						
4. Perceived Control	5.76 (1.19)	1.00 – 7.00	.00	.17**	.32***					
5. Perceived Difficulty	4.11 (1.64)	1.00 – 7.00	-.19***	-.06	-.59***	-.24***				
6. Autonomous Motivation	4.86 (1.29)	1.00 – 7.00	.03	.11	.50***	.15*	-.31***	(.90)		
7. Controlled Motivation	3.04 (1.22)	1.00 – 7.00	-.25***	-.15*	.24***	.00	-.05	.35***	(.82)	
8. Amotivation	2.88 (1.27)	1.00 – 6.00	-.11	-.14*	-.27***	.04	.29***	-.51***	.14*	(.58)

Note. Cronbach's alphas are shown in the diagonal. *** $p < .001$, ** $p < .01$, * $p < .05$

1. Given the relatively high number of unemployed participants, we also ran the main analyses with the subsample of employed participants (*n* = 239). The overall pattern of results was consistent with the results reported for the entire sample, although there were some minor differences in significance of certain predictor variables. In the results section we report on the entire sample, but we include the results for the subsample in the Supplementary Materials.

Ratings of nudges. The nudges that targeted healthy eating were evaluated as more pro-self (M = 15.93, SD = 17.82) than the nudges that targeted sustainable eating (M = 47.97, SD = 25.73), $t(273) = -12.61$, $p < .001$, $d = 1.44$, thereby confirming our underlying assumption that healthy eating would be seen as more of a pro-self behavior than sustainable eating. Descriptives and correlation coefficients for the ratings of all three nudges are reported in Table 5.3.

Table 5.3

Descriptives and correlation coefficients for the ratings of the three types of nudges.

	Mean (SD)	1	2	3	4
Default					
1. Acceptability	42.74 (34.08)				
2. Intrusiveness	65.24 (34.00)	-.69***			
3. Effectiveness	37.79 (28.30)	.62***	-.51***		
4. Alignment	41.20 (34.39)	.81***	-.55***	.53***	
Portion Size					
1. Acceptability	50.45 (34.08)				
2. Intrusiveness	46.98 (33.04)	-.52***			
3. Effectiveness	48.89 (27.78)	.65***	-.34***		
4. Alignment	47.14 (30.44)	.76***	-.31***	.59***	
Rearrangement					
1. Acceptability	70.81 (25.64)				
2. Intrusiveness	23.20 (28.26)	-.48***			
3. Effectiveness	53.92 (26.52)	.51***	-.11*		
4. Alignment	56.18 (27.41)	.65***	-.17**	.53***	

Note. *** $p < .001$, ** $p < .01$, * $p < .05$

Before investigating the relation between self-regulation and nudge acceptability, we first analyzed acceptability of the nudges as a function of the type of nudge and behavioral domain.

Acceptability. We found a large effect of the type of nudge on ratings of acceptability, $F(2, 598) = 105.42$, $p < .001$, $\eta_p^2 = .26$. Post-hoc comparisons using Bonferroni adjustment revealed that the rearrangement nudge (M = 70.81, SD = 25.64) was evaluated as significantly more acceptable than the portion size nudge (M = 50.45, SD = 30.69, $p_{adj} < .001$) which, in turn, was evaluated as significantly more acceptable than the default nudge (M = 42.74, SD = 34.08, $p_{adj} = .002$). Acceptability of the nudges did not differ by behavioral domain, $F(1, 299) = 1.74$, $p = .188$, but we did observe a significant interaction effect between the behavioral domain and type of nudge, $F(2, 598) = 3.45$, $p = .032$, η_p^2

= .01. This effect was driven by ratings of the portion size nudge, which was evaluated as more acceptable when targeting sustainable eating (M = 54.76, SD = 31.03) than when targeting healthy eating (M = 45.93, SD = 29.77, p_{adj} = .012). See Figure 5.1 for a graphical overview of the results for acceptability.

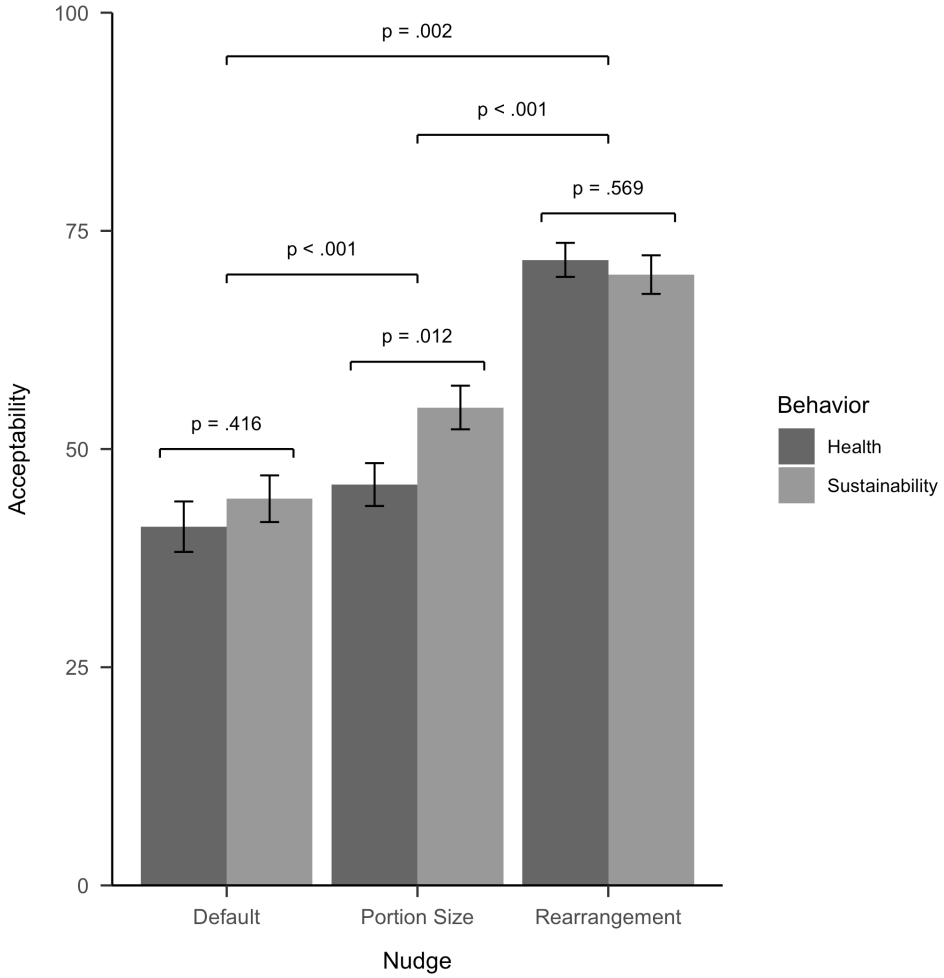

Figure 5.1. Mean scores of acceptability of the three nudges by behavioral domain.
Note. Error bars represent standard errors.

Intrusiveness, perceived effectiveness, and goal alignment. For the other three dependent variables that were related to acceptability, we found a similar pattern of results. Most importantly, we consistently found differences between the three types of nudges with medium to large effect sizes (all *p*s < .001). The direction of these effects was consistent with the overall pattern of nudge acceptability, such that the rearrangement

nudge was evaluated as less intrusive, more effective, and more in line with personal goals than the portion size nudge, which in turn was evaluated as less intrusive, more effective, and more in line with personal goals than the default nudge. For intrusiveness, we also found a small effect of the behavioral domain and a small interaction effect of the behavioral domain and type of nudge, and for goal alignment we also found a small interaction effect. See Table 5.4 for a complete overview of the results for all four dependent variables.

Table 5.4

Mixed ANOVAs predicting acceptability, intrusiveness, effectiveness and goal alignment.

	F	df_n, df_d	p	η_p^2
Acceptability				
Nudge	105.42	(2, 598)	< .001	.26
Behavior	1.74	(1, 299)	.188	.01
Nudge X Behavior	3.45	(2, 598)	.032	.01
Intrusiveness				
Nudge	184.47	(2, 598)	< .001	.38
Behavior	5.26	(1, 299)	.023	.02
Nudge X Behavior	4.91	(2, 598)	.008	.02
Effectiveness				
Nudge	37.69	(2, 598)	< .001	.11
Behavior	0.45	(1, 299)	.503	.00
Nudge X Behavior	0.20	(2, 598)	.816	.00
Alignment				
Nudge	27.84	(2, 598)	< .001	.09
Behavior	0.63	(1, 299)	.427	.00
Nudge X Behavior	4.89	(2, 598)	.008	.02

Main analyses. As the main purpose of this study, we performed the main analyses focusing on the relation between self-regulation and nudge acceptability. Taken together, the results above revealed that the rearrangement nudge was evaluated as more acceptable, less intrusive, more effective, and more in line with personal goals than the portion size nudge, which in turn was evaluated as more acceptable, less intrusive, more effective, and more in line with personal goals than the default nudge. Given this consistent pattern with medium to large effect sizes, we considered it warranted to conduct the main analyses separately for each type of nudge. The behavioral domain did not affect ratings

of acceptability, and thus we did not distinguish between healthy eating and sustainable eating in our main analyses. Thus, in order to explore the relation between self-regulation and nudge acceptability, for each nudge we regressed acceptability, intrusiveness, perceived effectiveness, and goal alignment on all measured self-regulation constructs.

Acceptability. For all three nudges, the linear regression models significantly fitted the data (all *p*s < .001), with model fit ranging from $R_{adj}^2 = .07$ to $R_{adj}^2 = .22$. Autonomous motivation significantly predicted acceptability of all three nudges: default ($\beta = .29$, $p < .001$), portion size ($\beta = .22$, $p = .001$), and rearrangement ($\beta = .52$, $p < .001$). Self-control ($\beta = .22$, $p = .001$) and self-efficacy ($\beta = -.19$, $p = .014$) also significantly predicted acceptability of the portion size nudge, while perceived control ($\beta = -.14$, $p = .017$) significantly predicted acceptability for the default nudge. Thus, regardless of the type of nudge and behavioral outcome, autonomous motivation was the only consistent predictor of nudge acceptability.

Intrusiveness, perceived effectiveness, and goal alignment. For intrusiveness, autonomous motivation (negatively) predicted ratings for the default nudge and rearrangement nudge.[2] For perceived effectiveness, autonomous motivation (positively) significantly predicted ratings for the default and rearrangement nudge, and marginally significantly predicted ratings of the portion size nudge ($p = .075$). For goal alignment, autonomous motivation again significantly (positively) predicted ratings for all three types of nudges.[3] Together, this shows that autonomous motivation was also the most important predictor of aspects of nudge acceptability.

Most other self-regulation constructs did not significantly predict aspects of nudge acceptability, and this did not reveal a consistent pattern across the ratings of aspects of nudge acceptability. For intrusiveness, perceived control also (positively) predicted ratings for the default nudge, while for perceived effectiveness, self-control also (positively) predicted ratings of the portion size nudge. Finally, for alignment, self-control (positively) and self-efficacy (negatively) also predicted ratings for the portion size nudge, while controlled motivation also (positively) predicted ratings of the rearrangement nudge. See Table 5.5 for a full overview of the regression results for all dependent variables.

2. The overall regression model for intrusiveness of the portion size nudge did not reach significance ($p = .379$) and none of the predictors were significant.

3. The overall regression model for effectiveness of the default nudge was marginally significant ($p = .055$).

Table 5.5

Linear regression models predicting acceptability, intrusiveness, perceived effectiveness and goal alignment for the three different types of nudges.

	Acceptability	Intrusiveness	Effectiveness	Alignment
	β (SE)	*β (SE)*	*β (SE)*	*β (SE)*
Default				
Self-control	.06 (.07)	.03 (.07)	.06 (.07)	.08 (.06)
Proactive Coping	-.04 (.06)	-.02 (.07)	-.02 (.07)	-.07 (.06)
Self-efficacy	.10 (.08)	-.02 (.08)	.03 (.08)	.04 (.08)
Perceived Control	-.14 (.06) *	.15 (.06) *	-.04 (.06)	-.09 (.06)
Perceived Difficulty	-.04 (.07)	.06 (.07)	-.06 (.07)	-.11 (.07)
Autonomous Motivation	.29 (.07) ***	-.20 (.07) **	.16 (.07) *	.34 (.07) ***
Controlled Motivation	.00 (.06)	-.02 (.06)	.01 (.06)	.02 (.06)
Portion Size				
Self-control	.22 (.07) **	-.12 (.07)	.19 (.07) **	.23 (.07) ***
Proactive Coping	.01 (.07)	-.06 (.07)	-.01 (.07)	-.03 (.06)
Self-efficacy	-.19 (.08) *	.05 (.08)	-.04 (.08)	-.21 (.08) **
Perceived Control	.03 (.06)	-.04 (.06)	.07 (.06)	.06 (.06)
Perceived Difficulty	.06 (.07)	-.03 (.07)	.07 (.07)	.07 (.07)
Autonomous Motivation	.22 (.07) **	-.03 (.07)	.13 (.07)	.29 (.07) ***
Controlled Motivation	.04 (.06)	-.06 (.07)	-.01 (.06)	.12 (.06)
Rearrangement				
Self-control	.00 (.06)	-.01 (.07)	-.01 (.07)	.01 (.06)
Proactive Coping	.07 (.06)	.05 (.07)	.07 (.07)	-.01 (.06)
Self-efficacy	-.02 (.07)	.15 (.08)	-.01 (.08)	.02 (.07)
Perceived Control	-.06 (.06)	.06 (.06)	.09 (.06)	-.03 (.05)
Perceived Difficulty	.06 (.06)	.09 (.07)	-.07 (.07)	.02 (.06)
Autonomous Motivation	.52 (.06) ***	-.35 (.07) ***	.19 (.07) **	.51 (.06) ***
Controlled Motivation	-.03 (.06)	.06 (.06)	.07 (.06)	.12 (.05) *

Note. Model fit for Acceptability: R_{adj}^2 = .11 *** (Default); R_{adj}^2 = .07 *** (Portion size); R_{adj}^2 = .22 *** (Rearrangement). Model fit for Intrusiveness: R_{adj}^2 = .04 * (Default); R_{adj}^2 = .00 (Portion size); R_{adj}^2 = .07 *** (Rearrangement). Model fit for Effectiveness: R_{adj}^2 = .02 (Default); R_{adj}^2 = .03 * (Portion size); R_{adj}^2 = .06 *** (Rearrangement). Model fit for Alignment: R_{adj}^2 = .15 *** (Default); R_{adj}^2 = .11 *** (Portion size); R_{adj}^2 = .30 *** (Rearrangement).

General Discussion

In this study we aimed to investigate the relation between two main components of self-regulation and nudge acceptability across different types of nudges targeting pro-self behavior (healthy eating) and pro-social behavior (sustainable eating). Previous studies generally demonstrated majority support for most nudges (e.g., Reisch & Sunstein, 2016; Sunstein et al., 2018) and have established evidence for important predictors of nudge acceptability that lie within aspects of the nudge itself (e.g., Bang et al., 2020; Cadario & Chandon, 2019) or are inferred from (the intentions of) the nudger (e.g., Tannenbaum et al., 2017). Yet, is it pivotal to identify whether the very people who are intended to benefit from the nudge are likely to accept or oppose it. Therefore, with this study we investigate predictors related to self-regulation capacity (self-control, proactive coping competence, self-efficacy, perceived control, and perceived difficulty) and motivation (autonomous and controlled) as possible determinants of nudge acceptability. In doing so, we also aim to bring together two routes to behavior change (through improving self-regulation or changing the environment) which to date have mainly been studied separately.

The current study showed that only autonomous motivation was a consistent predictor of (aspects of) acceptability across the three types of nudges. This finding is in line with a previous study that showed a correlation between autonomous motivation and acceptability of a default nudge (Van Gestel et al., in press) and provides first evidence for the notion that those who are motivated to perform a certain type of behavior are more likely to embrace a nudge that aims to stimulate that behavior as an acceptable policy instrument. As autonomous motivated behavior is performed out of interest or enjoyment (Deci & Ryan, 2000), the current findings indicate that motivation for personally endorsed reasons is positively related to nudge acceptability. This relation was consistent across pro-self and pro-social nudges, suggesting that nudge acceptability for autonomously motivated reasons transcends beyond purely individual benefits and also includes behavior that holds advantages for society at large. In a way, this study thus shows that acceptability of nudges is not purely limited to aspects of self-interest (Blekesaune & Quadagno, 2003; Diepeveen et al., 2013). Controlled motivation was not related to acceptability of any of the three nudges.

The capacity to self-regulate was only occasionally related to (aspects of) nudge acceptability. We suspected that self-regulatory capacity might predict nudge acceptability, as recent developments in the study of self-control have established the importance of embracing situational factors in achieving self-control success (Duckworth et al., 2018; Gillebaart & De Ridder, 2015; Reijula & Hertwig, 2020). In order to investigate this possible association, we included a wide variety of measures for self-regulation capacity. While proactive coping competence was not at all related to (aspects of) nudge acceptability, trait self-control only predicted acceptability of the portion size nudge. Similarly, perceived difficulty was not at all related to (aspects of) nudge acceptability, while self-efficacy and perceived control were only once associated with acceptability (of the portion size and

default nudge respectively). A potential reason for the rare occurrence of associations between these components of self-regulation capacity and nudge acceptability could be that not everyone possesses insight in their ability to self-regulate. People sometimes tend to overestimate their own self-control (Nordgren, Van Harreveld, & Van der Pligt, 2009) and therefore may become more reserved about receiving aid from policy makers (Schroeder, Waytz, & Epley, 2015). Differences in the extent to which people accurately assess their own ability to self-regulate, and in the extent to which people are open to being confronted with this insight, may thus have confounded the expected effects. Still, the current study shows that it is not likely that those who could need support in achieving their desired end states are those who are prone to accept nudges. Nor does the study provide evidence for the notion that those who are high in self-control embrace environmental interventions like nudges. Rather, the current study shows that especially those who want to perform the nudged behavior for autonomous reasons are more likely to accept those nudges.

We did not find meaningful differences in (aspects of) acceptability as a result of the behavioral domain, even though the target behavior of healthy eating was evaluated as more pro-self than the target behavior of sustainable eating. Healthy eating was seen as clearly pro-self, while sustainable eating was on average evaluated at the midpoint of the scale ranging from pro-self to pro-social. Thus, even though the nudges targeting sustainable eating were not decisively seen as pro-social, our results seem to contradict previous results that suggested that pro-social nudges are seen as less acceptable than pro-self nudges (Hagman et al., 2015). In previous studies, different types of nudges were classified as pro-self or pro-social, whereas in our studies we used the exact same nudge, while manipulating the pro-self or pro-social nature of the nudge. Thus, while we used the same nudges and found no differences by the prosocial nature of the nudge, previous findings may have been confounded by external factors like differences in the types of nudges.

Results also revealed that the three nudges in these studies did not receive unequivocal support. For example, the default nudge did not receive majority support and only the rearrangement nudge was seen as acceptable by a considerable margin. One possible explanation for this could be that we selected three behavior-oriented nudges, which have been found to be among the most effective (Cadario & Chandon, 2020). Actual effectiveness – as inferred from meta-analytic evidence – is negatively associated with public acceptability (Cadario & Chandon, 2019) and thus we may have selected nudges that are among the least supported. Moreover, so-called System 1 nudges like defaults and portion size nudges are generally less accepted than so-called System 2 nudges like disclosure of information (Jung & Mellers, 2016). The differences in acceptability between the three types of nudges in the present study were substantial, but our study is not the first to reveal lower ratings of acceptability of default nudges in comparison with other types of nudges (e.g., Reisch et al., 2016). Furthermore, the default nudge was evaluated

as the most intrusive and least effective, and these factors have been associated with lower support for nudges (Cadario & Chandon, 2019; Evers et al., 2018).

Limitations and future research

The current study was the first to relate self-regulation to nudge acceptability and was exploratory in nature. Confirmatory research with a pre-registered analysis plan and hypotheses would be required to further establish the robustness of these findings. In this light, both replicating the mostly non-significant effects of capacity as well as further analyzing the role of motivation in nudge acceptability could enhance our understanding of which people are most likely to accept nudges and for what reasons. Obviously, conceptual replications with different methods and operationalizations could be of significant value for answering these questions. We specifically note the limitation that over half of the participants in our sample were unemployed or worked from home, and that almost half of the participants hardly ever purchased food at work, thereby limiting the personal relevance of the vignettes to the participants. We also note that, on average, participants were generally motivated, at least for autonomous reasons, to make healthy and sustainable food decisions.

Another limitation of the current study was that we asked participants to rate fictitious nudges in a vignette study. Although this is currently the most frequently used method for gaining a better understanding of nudge acceptability (e.g., Reisch & Sunstein, 2016; Sunstein et al., 2018), we signal a need for more (field) studies with actual implementation of nudges in the real world (e.g., Kroese, Marchiori, & De Ridder, 2015; Van Gestel, Kroese, & De Ridder, 2018). This is all the more important given that there can be a discrepancy in subjective evaluations of nudges between hypothetical situations and actually implemented nudges (Wachner, Adriaanse, & De Ridder, under review). In line with this, individuals that had been exposed to a default for a meat-free lunch approved the nudge by a large majority (90%; Hansen, Schilling, & Malthesen, 2019), while comparable meat consumption limiting nudges were only approved by a small majority in fictitious scenarios (52% in the UK sample; Reisch & Sunstein, 2016). Field experiments should thus not only continue to focus on efficacy, but also measure and report on public acceptability of nudges implemented in real life. Finally, future work should continue to integrate the two routes to behavior change, also in terms of effectiveness, in order to further establish when and for whom nudges are effective and acceptable.

Conclusion

In this study we consistently found support for the relation between autonomous motivation and (aspects of) acceptability of three different nudges: default, portion size, and rearrangement. The pattern of results was not affected by the type of behavior that was targeted, even though healthy eating was seen as more pro-self. Altogether, the models including self-regulation capacity and motivation explained up to 22% in variability in acceptability of the nudges. Autonomous motivation was the only consistent predictor of acceptability across the three types of nudges. Despite having included a

wide variety of measures of self-regulatory capacity, we only found incidental effects of some aspects of self-regulatory capacity for some nudges. Together this suggests that people do not meaningfully base their judgments of acceptability on elements of self-regulation capacity, but rather on their own autonomous desire to perform the behavior of interest. The current study highlights the importance of addressing individual traits and states in predicting nudge acceptability in addition to earlier established aspects of the nudge itself or the nudger. Policy makers will benefit from further research into when and for whom nudges can be an acceptable means of stimulating desired behavior, and whether those who can and/or want to perform the behavior of interest are ultimately those who are also likely to accept nudges that promote such behavior.

Chapter 6

General discussion

Following the publication of the book 'Nudge: Improving decisions about health, wealth, and happiness' in 2008, nudging has received increasingly more attention from both scientific researchers and public policy makers (Sanders, Snijders, & Hallsworth, 2018; Szaszi, Palinkas, Palfi, Szollosi, & Aczel, 2018). Nudges have been introduced as novel and supplemental behavior change tools that go with the grain of human behavior instead of challenging all of its complexities. As such, nudges aim to stimulate desired behavior by strategically altering the immediate choice environment with insights from behavioral sciences. The promise of nudges lies in the assumption that they do not require elaborate processing in order to be effective, thus supplementing other behavior change tools such as persuasive communication or information provision which tend to rely on the availability of cognitive resources (Beauchamp, Backholer, Magliano, & Peeters, 2014; Lorenc, Petticrew, Welch, & Tugwell, 2013; McGill et al., 2015). Early nudging studies revealed promising results in terms of effectiveness (Johnson & Goldstein, 2003; Thaler & Benartzi, 2004), cost-effectiveness (Benartzi et al., 2017), and acceptability by the public (Hagman, Andersson, Västfjäll, & Tinghög, 2015; Reisch & Sunstein, 2016), which further fueled the interest in this type of intervention. Yet, as the interest from policy makers grew, the central role of psychological science in nudging research diminished (Marchiori, Adriaanse, & De Ridder, 2017). And while systematic reviews and meta-analyses point towards promising effects of nudges for stimulating desired behavior (e.g., Hummel & Maedche, 2019), they also point towards large heterogeneity between studies and types of nudges, and signal that less is known about the conditions under which nudges are effective and acceptable (Szaszi et al., 2018). In order to make informed policies and to increase the likelihood of successful implementation, it is important to understand which people are most likely to benefit from and accept nudges under what conditions. The current dissertation aimed to give meaning to previous calls to put the psychology back in nudging (Marchiori et al., 2017) and to contribute to a robust science of nudging that is theoretically sound and efficacious in its implementation. Therefore, this dissertation aimed to examine when and for whom nudges can be effective and an acceptable means of stimulating desired behavior. Throughout this dissertation several psychological aspects related to effort and motivation were addressed which had diverse implications for discussions about the theory, practice, and ethics of nudges.

In this final chapter I will summarize and discuss the main findings from the empirical chapters in light of the main aim of the dissertation (i.e., when and for whom nudges are effective and acceptable). Next, I will outline the most important theoretical and practical implications, reflect on the limitations of the empirical work, and provide recommendations for future research.

Summary of findings

Chapter 2 described two field experiments that aimed to shed light on the question *when nudges are effective*. In these studies, we investigated whether nudges can remain effective in more complex choice situations with multiple alternative options in the

immediate choice environment which better represent the environment in which people frequently make decisions. To that end, we investigated the effectiveness of a proximity nudge - that aimed to stimulate the choice for the most proximal option - in choice sets consisting of either three or nine different options. These different options (in this case pieces of chocolate) were objectively of similar utility and as a result most participants indicated that they did not have a prior preference for any of the options. Yet, the rearrangement of these options such that the target option that we were interested in (for the sake of answering the research question) was positioned more closely to the participant significantly affected choice outcomes regardless of the number of alternative options. In Study 1 we were able to demonstrate that the proximity nudge remained effective when more alternative options were available. In Study 2, we aimed to conceptually replicate the findings from Study 1 with a larger sample and an improved set-up. Study 2 replicated the main results of Study 1, and we again found strong evidence for the effectiveness of the proximity nudge regardless of the number of options in the choice set. In both studies, participants were more than twice as likely to select the target option when it was positioned closely to them, regardless of the number of alternative options available. The results extend previous studies on the proximity effect which were limited to choosing from either one or two options. Taken together, these studies reveal that a nudge may remain effective even when there are more alternative options in the immediate choice environment to choose from. We incorporated these findings in the other chapters and also included options that would impose a dilemma to the participant in the other chapters.

Chapter 3 took a more fundamental approach to addressing the question *when nudges are effective* and investigated to what extent nudges make use of automatic processes. This premise is at the core of many debates about the legitimacy of nudging, while it is also expressed as a unique feature that complements other behavior change interventions and thereby enriches the toolbox of the policy maker. Yet, to date there is little empirical research that directly addresses this crucial assumption. Therefore, in this Registered Report, we experimentally studied the effectiveness of a default nudge stimulating sustainable options under Type 1 and Type 2 processing. In two high-powered and pre-registered studies, the default nudge was highly effective such that participants chose more green amenities in the opt-out condition than in the opt-in condition. In Study 1, we successfully manipulated cognitive load with a dot memorization task and thereby inhibited Type 2 processing. As expected, the cognitive load manipulation did not affect the default effect that we observed. In fact, the default effect was statistically equivalent under low and high load. However, we reasoned that the mere availability of cognitive resources in the low load condition did not automatically imply that participants effectively would use these resources. Therefore, in Study 2, we manipulated deliberation with specific instructions and thereby stimulated Type 2 processing. Unexpectedly, these instructions did not attenuate the default effect and rather had an independent effect on

choice outcomes apart from the default nudge. Taken together, these two experiments demonstrated that defaults do not take advantage of Type 1 processing, but rather that the effects can occur without the need for deliberation.

In **Chapter 4** we proceeded with addressing the role of motivation in nudges' effectiveness in order to address the overall question *for whom nudges are effective*. Study 1 revealed that a default nudge can effectively stimulate healthy food choices in an online supermarket setting. Motivation strength and autonomous motivation for healthy eating also had a main effect on healthy food choices, but none of the motivation measures significantly interacted with the default nudge. This pattern of results was conceptually replicated in Study 2, in which we used the same default nudge as in Chapter 3 in order to stimulate sustainable choices. Again, the default nudge was effective in stimulating sustainable choices. Motivation strength and autonomous motivation for making sustainable choices also had a main effect on choice outcomes. In Study 3, we aimed to extend these findings to a behavior with actual and direct consequences for the decision maker. In this study, we used a default nudge that stimulated voluntary participation in a longer version of a questionnaire about sustainable behavior. We largely replicated the pattern of results from Study 1 and 2, even though the intended prosocial nature of the behavior of interest was most likely not experienced as such by the participants, as they indicated that they did not see it as an act of helping other people. Instead, motivation strength and autonomous motivation for making sustainable choices predicted voluntary participation in the longer questionnaire on sustainability. Across all three studies we found that the default nudges had incremental value on top of motivation. In sum, across three studies we showed that defaults and motivation independently predict behaviors that people are at least moderately motivated for, and that nudges have incremental value on top of existing motivation.

Chapter 5 presented a comprehensive self-regulatory perspective on nudges' acceptability in order to shed light on the question *for whom nudges are acceptable*. In this chapter we conducted a vignette study with three vignettes describing three different nudges (default, portion size, and rearrangement) that either stimulated healthy eating or sustainable food choices at work. We also administered a variety of questionnaires about self-regulation capacity (e.g., self-control, proactive coping competence, self-efficacy, perceived control, and perceived difficulty) and motivation (autonomous and controlled motivation). The study demonstrated that autonomous motivation was a significant and consistent predictor of nudge acceptability, while controlled motivation did not affect nudge acceptability. Moreover, elements of self-regulatory capacity had little impact on judgments of acceptability. Together this study shows that those who want to perform the behavior of interest for autonomous reasons are more likely to accept nudges that are in line with that goal, but that self-regulatory capacity does not meaningfully affect ratings of acceptability.

Collectively, these studies contributed to the overall aim of this dissertation to investigate

when and for whom nudges are effective and an acceptable means of stimulating desired behavior. We showed that nudges can remain effective in more complex choice situations with more alternative options (Chapter 2), that nudges are not dependent on deliberation in order to be effective (Chapter 3), that autonomous motivation independently predicts behavior in the presence of an effective nudge (Chapter 4), and that autonomous motivation is an important predictor of nudge acceptability (Chapter 5). The work presented in this dissertation has numerous theoretical and practical implications which will be elaborated upon below.

Theoretical implications

One of the main theoretical contributions of this dissertation pertains to the dual-process accounts of human processing and behavior on which much of the nudge literature is based (Thaler & Sunstein, 2008). Originally, nudging was largely inspired by dual *system* accounts of human processing, but recent advancements in research on dual-processing accounts have led to a refinement of such theories. It is now clear that dual *type* accounts – that include defining features and typical correlates – better align with scientific evidence (Evans & Stanovich, 2013), although much debate remains about the accuracy of this view as well (Bago & De Neys, 2017; Kruglanski & Gigerenzer, 2011; Melnikoff & Bargh, 2018; Osman, 2004). Regardless of which specific human cognitive architecture one assumes, core of many of the debates around nudging revolves around the assumption that they speak to Type 1 processes which place a minimal burden on working memory. Both proponents and opponents consider this assumed automatic nature of many nudges as one of the critical elements that distinguishes nudges from other types of interventions. But where proponents of nudging praise these insights as an enrichment to the policy maker's toolbox (Dolan, Hallsworth, Halpern, King, Metcalfe, & Vlaev, 2012), opponents raise concerns about manipulation and lack of autonomous decision making (Hansen & Jespersen, 2013; Wilkinson, 2013). Whatever stance one takes, this discussion is thus heavily based on an *assumed* theoretical framework (which in itself is heavily debated), while there is little empirical investigation of how nudges operate when cognitive resources are unavailable. Our studies demonstrate that concerns about manipulation can be alleviated to the extent that people do not fall victim to nudge effects if deliberation is (temporarily) inhibited. That is, even if people have the mental capacity to deliberate, this does not necessarily mean that they effectively do so, and thus people who do not have this capacity are no more vulnerable to being nudged than people who do have this capacity. Rather, our studies demonstrate that nudges do not require deliberation in order to be effective, suggesting that they indeed form a worthy supplement to existing measures in the toolbox of the policy maker that do require mental processing. Our results further show that if deliberation is stimulated, people can adjust the outcome of the decision by outweighing pros and cons in a similar way as in situations where nudges are not present. However, some nudges (like defaults) can be sticky and may, under some circumstances, provide a strong anchor from which people

can adjust accordingly. Moreover, we show that people cannot completely override the default effect when deliberating. The current dissertation thus makes a theoretical contribution to the nudge literature by putting the assumed theoretical basis of nudges to the empirical test. Besides, the current dissertation indirectly contributes to dual-processing theories themselves, as the pattern of results that we find in Chapter 3 aligns with the *default-interventionist* perspective on dual-processing (Evans & Stanovich, 2013; Kahneman, 2011), which posits that Type 1 processing is the default mode of operation, while Type 2 processing can intervene if need be.

After the initial introduction of nudging by Thaler and Sunstein in 2008, many discussions have been held about what constitutes a nudge and what not (e.g., Hansen & Jespersen, 2013; Hausman & Welch, 2010; Marchiori et al., 2017). Also, several attempts have been made to classify nudges according to various dimensions (Cadario & Chandon, 2020; Dolan, Hallsworth, Halpern, King, & Vlaev, 2010; Hollands et al., 2017; Münscher, Vetter, & Scheuerle, 2016). Some of these conceptual refinements have been based on the extent to which nudges are thought to make use of Type 1 or Type 2 processes. For example, scholars have suggested the use of 'Type 1 nudges' and 'Type 2 nudges' (Hansen & Jespersen, 2013; Lin, Osman, & Ashcroft, 2017) or 'Educative nudges' and 'Non-educative nudges' (Sunstein, 2016). Type 1 nudges are thought to influence behaviors that directly result from automatic processes, while Type 2 nudges are thought to stimulate reflective thought indirectly through automatic processes. To illustrate, nudges that exploit visual illusions are categorized as Type 1 nudges, while nudges that aim to capture attention (e.g., fly-in-the-urinal) in order to steer reflective behavior are categorized as Type 2 nudges. Similarly, non-educative nudges are meant to steer behavior without increasing knowledge (e.g., defaults), while educative nudges are supposed to enable people to easily make an informed decision (e.g., traffic light labels). Our studies show that defaults, which have frequently been described as the most prototypical Type 1 nudge (Hansen & Jespersen, 2013; Sunstein, 2016), do indeed not require working memory engagement in order to be effective. Based on our results one may thus place default nudges in the category of Type 1 nudges. However, given the overall difficulty of basing a group of interventions on a theoretical framework that by itself is so heavily discussed, the question becomes whether these refinements or reconceptualizations have additive value or further muddy the waters.

Apart from the focus on the human cognitive architecture, the current dissertation also focused on the role of motivation in nudges' effectiveness and acceptability. Nudges have often been suggested for stimulating behaviors that people are assumed to be motivated for, such that nudges target behavior that is either beneficial for the individual him or herself (pro-self nudges) or for society at large (pro-social nudges). This idea is indirectly reflected in the 'Nudge for good' mantra, that Richard Thaler famously uses to sign a copy of his book, and thus many studies have been conducted to stimulate desired behavior. Yet, the role of motivation in nudging has never directly been investigated.

Thus far, studies focused on related elements like attitudes (Kaiser, Bernauer, Sunstein, & Reisch, 2020; Kuhn, Ihmels, & Kutzner, 2021; Taube & Vetter, 2019; Vetter & Kutzner, 2016) and preferences (Venema, Kroese, Benjamins, & De Ridder, 2020; Venema, Kroese, De Vet, & De Ridder, 2019), but not on motivation itself. Based on Self-Determination Theory (Ryan & Deci, 2000), we focused on autonomous and controlled motivation. We showed that autonomous motivation is an important predictor of behavior, but that it does not intervene with the nudge per se. In other words, we find that the default nudge sets an anchor from which people can adjust according to their strength and type of motivation. This shows that even if people are being nudged, there is room for autonomous decision making. In a way, this aligns well with the libertarian paternalism perspective underlying normative debates about nudges' legitimacy (Thaler & Sunstein, 2003; 2008). According to this perspective, nudges steer behavior in a particular direction that a policy maker deems important (paternalism) but ultimately leave freedom of choice intact for the individual decision maker (libertarian). Yet, our studies are not conclusive as we only dealt with policy-relevant behaviors that most people are at least moderately motivated for, and we cannot conclude that the libertarian part upholds in practice under all circumstances as we simply did not cover the full range of motivation strengths and types. Thus, while adhering to the nudge for good principle, we can conclude that nudges indeed stimulate behavior that people are autonomously motivated for, but we cannot draw conclusions about whether people ultimately have the freedom to choose differently if the nudge goes against their own motivation. Nevertheless, it is also important to realize that even if motivation is relatively high, our studies suggest that nudges have incremental value on top of this motivation. This finding corroborates suggestions that nudges may be helpful for those individuals who want to pursue a given goal, but who ultimately fail to translate their intentions into actual behavior (Momsen & Stoerk, 2014; Sheeran & Webb, 2016).

Practical implications

Besides the theoretical implications of the research described in this dissertation, there are also important practical implications to consider. First of all, the empirical work in this dissertation shows that nudges can successfully be implemented across several situations. In a real-life choice context, we showed that the proximity nudge remained effective if there are more alternatives in the choice set than typically studied in experimental settings. Regardless of the number of alternatives, participants were more than twice as likely to choose the targeted option if it was placed proximally to the participant. This finding extends previous results on the proximity effect, which were mostly limited to either one or two options in the choice set (Hunter, Hollands, Couturier, & Marteau, 2018; Hunter, Hollands, Pilling, & Marteau, 2019; Maas, De Ridder, De Vet, & De Wit, 2012; Privitera & Zuraikat, 2014). Practically, this means that the proximity effect remains effective if there are less than ten options in the choice set, provided that these options are of similar utility. This is an important extension of earlier results, because many of the choices that we make on a daily basis are based on choice sets that include multiple options that differ from each

other in multiple ways. Also, in an online supermarket environment, we show that defaults can successfully be implemented to stimulate the choice for healthier alternatives. This corroborates results from previous studies in online shopping settings and shows that nudges can successfully be implemented in such online environments (e.g., Antonides & Welvaarts, 2020; Coffino, Udo, & Hormes, 2020; Hummel & Maedche, 2019; Kuhn et al., 2021). The frequency of online (food) shopping has tremendously increased over the last years and is expected to increase only further in the coming years (Pitts, Ng, Blitstein, Gustafson, & Niculescu, 2018). It is thus important to establish that nudges can effectively be implemented in such online environments as more and more food choices will be made in online settings. Moreover, as a rather general observation, we show that defaults can effectively stimulate healthy and sustainable behaviors across a variety of settings. This overall pattern aligns with previous findings on default effects (Jachimowicz, Duncan, Weber, & Johnson, 2019), as well as on nudges more generally in the domains of healthy (Cadario & Chandon, 2020) and sustainable behavior (Lehner, Mont, & Heiskanen, 2016).

Second, we show that nudges are generally effective and well-accepted for behaviors for which people are autonomously motivated. Throughout this dissertation we intended to include behaviors that meet the criteria for the nudge for good mantra, and thus we stimulated mostly health and sustainable behaviors. These two behaviors are not only the two most often studied behaviors in nudging research (Hummel & Maedche, 2019; Szaszi et al., 2018), they also differ in the extent to which they address behaviors that are either beneficial for the self or for society at large. Nevertheless, for both types of behavior we observed moderately strong to strong (autonomous) motivation in our samples, and we showed that nudges are effective in stimulating behaviors for which people are motivated. Given the rather high motivation in our samples, it is noteworthy to stress that these nudges had incremental value on top of existing motivation. Indeed, nudges could thus contribute to bridging the gap between intentions and actual behavior (Sheeran & Webb, 2016). However, the extent to which people can freely adjust behavior to be in line with their motivation remains a point of concern that policy makers need to take into account. Not only did we see that deliberation can lead to independent effects, we also showed that this was the case for autonomous motivation and motivation strength. Yet, across most of our studies (with the exception of Study 1 in Chapter 4), the effect of the default on behavioral outcomes was stronger than the parallel effects of deliberation or motivation. This bears important implications for the policy maker who wishes to steer citizen behavior while maintaining freedom of choice. When deciding to implement a particular nudge, policy makers should not only consider the absolute strength of that nudge but also the relative strength of it in relation to motivation and deliberation in order to make an informed decision about the usefulness and legitimacy of the nudge.

Finally, we shed new light on public acceptability of nudges from a self-regulation perspective. As we note in Chapter 5, the crucial focus on the nudgee him or herself has largely been missing in the study of nudge acceptability. Yet, policy makers need

to be informed about the evaluations of nudges by their target population in order to evaluate the promise of a given intervention in reaching those that could benefit from the implementation of the nudge. Our studies show that those who are autonomously motivated are more likely to accept the implementation of nudges that stimulate the specific behavior that they are motivated for. We also show that this effect of autonomous motivation transcends beyond individual benefits and also holds for behaviors with long-term effects for society at large. Acceptability of nudges is thus more generally affected by autonomous motivation than would be expected based on self-interest (Blekesaune & Quadagno, 2003; Diepeveen, Ling, Suhrcke, Roland, & Marteau, 2013). Combined with the finding from Chapter 4, this suggests that those who are autonomously motivated are generally more appreciative of nudges and could be reached effectively.

Limitations

Although the present dissertation contributes to the theory and practice of nudging, several limitations deserve to be mentioned.

First of all, even though we included a variety of different behaviors throughout this dissertation, it should be noted that most of these behaviors were hypothetical in nature with little to no actual consequences for the participants. Only in Chapter 2 and Chapter 4 (Study 3) we measured overt behavior. In Chapter 2, this behavior comprised choosing one alternative out of a set of options of similar utility and thus had little impact on the participant. In Study 3 of Chapter 4, this behavior comprised voluntarily choosing for a longer participation duration of five minutes, so although this decision had actual consequences, the impact was minimal. Chapters 2 and 5 were completely focused on hypothetical behavior, and so were Studies 1 and 2 of Chapter 4. Our primary goal throughout this dissertation was to investigate when and for whom nudges are effective and acceptable, rather than to establish general effectiveness and acceptability, as this has frequently been demonstrated in previous work (e.g., Hagman et al., 2015; Hummel & Maedche, 2019; Johnson & Goldstein, 2003; Reisch & Sunstein, 2016; Thaler & Benartzi, 2004). Therefore, we often favored more experimental control at the expense of ecological validity. To illustrate, the cognitive load manipulation that we used in Chapter 3 allowed us to critically reflect on the manipulation objectively (i.e., number of correctly memorized dots) and subjectively (i.e., self-reported difficulty of remembering the pattern of dots). It is not impossible to manipulate cognitive load in the field as for example the digit span task could also be used in more naturalistic settings (e.g., Brunner, 2013; Van der Wal & Van Dillen, 2013), but it often goes at the expense of experimental control. We deliberately chose nudge manipulations that had been proven effective in previous studies (e.g., Steffel, Williams, & Pogacar, 2016; Wachner, Adriaanse, & De Ridder, 2020) in order to be able to adequately address the main research question of this dissertation. Moreover, we often used continuous behavioral outcomes (e.g., proportion of healthy food choices or number of green amenities selected) in order to have more sensitivity in our dependent variables. On the positive side, the hypothetical scenarios gave us flexibility to fine-tune

the independent variables (i.e., nudge manipulation) and dependent variables (i.e., behavioral outcomes), such that we could measure the dependent variables with enough sensitivity. Future research on when and for whom nudges are effective and acceptable will inevitably have to deal with similar complexities in methodological choices, but increasing the personal relevance of the decision for the participant is one way to overcome some of the above-mentioned limitations.

Second, another limitation pertains to striking the right balance between running studies that adhere to the nudge for good mantra and running studies that address the full range of behaviors and accompanying motivations. Throughout almost all of our studies we closely adhered to the nudge for good principle (excluding Chapter 1 in which the options in the choice set did not differ). In doing so, we focused mostly on health and sustainable behaviors and intended to examine our research questions in experimental settings that would have relevance for public policy. The inevitable trade-off that this imposes – that this goes at the cost of painting a full picture of potential boundary conditions – is something that we particularly encountered in Chapter 4. In this chapter, throughout all of the three studies, most participants were generally quite to highly motivated to behave healthily or sustainably. Thus, while most concerns from ethicists revolve around the lower spectrum of motivation, we were not able to draw conclusions about that based on our data. The same issue that participants were generally motivated, although perhaps to a lesser extent, applied to Chapters 3 and 5. We were certainly not the first to encounter this problem (e.g., Venema et al., 2019), but it nevertheless limits the conclusions that we can draw and future research should thus address the role of motivation more comprehensively, ranging from weak to strong motivation, and from amotivation to fully internalized forms of motivation.

Third, even though we tried to include conceptual replications within our studies, we note that the conclusions are generally limited to the specific nudges that we used in our experiments. As described in Chapter 1 of this dissertation, we see nudging as an umbrella term under which multiple types of interventions exist that may likely differ from each other in several ways. We deliberately chose to include nudges that had been shown to be effective in previous research (e.g., Steffel et al., 2016; Wachner et al., 2020) as a starting point for addressing the main research questions. The findings that we report on for these specific nudges cannot be indiscriminately generalized to other types of nudges. As the research questions that we focused on throughout this dissertation have diverse and important implications, these should also be studied with different nudges in order to further understand when and for whom nudges are effective and acceptable.

Future research

The current dissertation comprises of a set of studies investigating when and for whom nudges are effective and acceptable. This general research aim was addressed from multiple angles related to effort and motivation. Both within and outside of this specific research angle important questions for future research remain. As a rather general

observation, future research should continue to address the void in nudging research and focus on potential moderators and mechanisms of nudges. The same research questions that we focused on in the empirical work of this dissertation could be focused on with different operationalizations of potentially moderating variables, but also other types of nudges should be considered. Underlying mechanisms remain equally crucial to identify under what conditions nudges can be effective and for what reasons. In line with this general direction for future research, carefully designed and well-executed nudges that unexpectedly yield non-significant effects on behavior deserve to be published as well. Little is currently known about the limits of nudging as relatively few studies report on unsuccessful nudges (Szaszi et al., 2018). Yet, in order to get a complete understanding of when and for whom nudges work, it is no less important to publish on failed experiments and field studies that identify possible limits of nudging interventions (e.g., De Wijk et al., 2016; Kristal & Whillans, 2020; Sunstein, 2017).

When are nudges effective? Building on our research aim in Chapter 2, we signal a need to further investigate simple behaviors in more complex environments. Many of the fundamental studies on nudging desired behavior continue to be conducted in overly simplistic situations, while field studies in more complex situations do not systematically account for the complexity of that situation, thereby limiting our understanding of which nudges work in what situations. It is important to identify whether aspects of the immediate environment pose boundaries and opportunities for nudging interventions. There may, for example, be elements in the choice environment that complicate optimal decision making and thus indicate fertile soil for successful implementation of nudges, such as an overload of options or attributes that go along with the options in the choice set (Schwartz, 2004). As alluded to in Chapter 2, there remains a possibility that nudges can be especially effective in situations of choice overload. One could argue that there is more to gain in such situations because it is exactly in those circumstances that people are prone to making suboptimal decisions (Schwartz, 2004). Moreover, there may also be something inherent to nudges that prevails in circumstances when people do not have a priori preferences (Venema, 2020), and choice overload mostly occurs if there is no dominant option in the choice set (Chernev, Böckenholt, & Goodman, 2015). Future research should thus continue to address elements of the immediate choice environment that could make the use of nudges valuable or not.

We also signal a need to investigate more complex behaviors. In many ways, the viability of nudging as a public policy tool has been established, but in establishing this viability the focus has long been on the low-hanging fruit (e.g., tax compliance; Hallsworth, List, Metcalfe, & Vlaev, 2017). While focusing on these relatively easier behaviors has certainly been fruitful, there are perhaps more pressing social issues like climate change, fake news, and poverty reduction – to name just a few – that possibly could benefit from nudges as well (Sanders et al., 2018; Van der Linden, 2018). That is, nudges should by no means been seen as substitutes for rules and regulations that contribute to solving those issues, but

perhaps they can contribute to advancing individual and planetary well-being more than that is currently being explored. In this light it is also important to recognize that even small effects on an individual level can have profound and policy-relevant consequences if successfully scaled up (Cialdini, Martin, & Goldstein, 2015). In order to explore this further, it is thus important to identify which behaviors are suitable for nudging interventions, and in what contexts. Attempts have been made to compare nudging interventions against other interventions like boosts (i.e., interventions that intend to train individual competencies) in order to identify when to consider nudges as a policy instrument (Hertwig, 2017; Hertwig & Grune-Yanoff, 2017), and future research should continue to systematically map elements of 'Nudgeability' to identify when people are most receptive to nudging interventions (De Ridder, Kroese, & Van Gestel, in press).

For whom are nudge effective? Future research should also continue to address the role of motivation for the effectiveness of nudges. Our studies demonstrate that autonomous motivation affects behavioral outcomes without moderating the effects of the nudges and that default nudges have incremental value on top of existing motivation. Yet, the behaviors under consideration were mostly behaviors that participants were generally motivated for. For ethical considerations about autonomous decision making and freedom of choice, future research should consider the entire spectrum of motivation. In order to do so, we foresee three possible routes for future research that each have their pros and cons. The first is to include additional experimental conditions in which the nudge targets undesired behavior (e.g., unhealthy or unsustainable options). These experimental conditions could easily be added to experimental paradigms like the online supermarket task that we included in Chapter 3, but the direct informative value for policy making would be little as policy makers would be unlikely to implement such 'nudges' targeting unhealthy food choices. Another option is to study different behaviors for which there is more variation in motivation strength and the types of motivation. As pointed out in Chapter 4, meat consumption could be a possible candidate as there is a large heterogeneity of motivations in society with both people that attempt to eat less meat and profound meat eaters (Malek, Umberger, & Goddard, 2019). Studying such a behavior would arguably justify the nudge for good principle, as reduced meat consumption would be good for individual and planetary health (Godfray et al., 2018), and it would most likely give more variation in motivation. Finally, representative samples could perhaps reveal more variation in motivation for the behaviors that we studied in our research, and also one could think of sampling from specific populations like climate change deniers which hold ideological positions about certain issues (Bain, Hornsey, Bongiorno, & Jeffries, 2012).

Improved insights into the effectiveness of nudges under Type 1 processes could filter into another important research area addressing socio-economic position. As it has been assumed that nudges do not require cognitive elaboration in order to be effective, some have speculated about possible egalitarian effects of nudges (Marteau, Hollands, & Fletcher, 2012; Marteau, Rutter, & Marmot, 2021). Indeed, for other types of interventions

research has shown that they do not necessarily bridge the divide in health outcomes for people with a lower income and/or education and possibly even create intervention-generated inequalities (Beauchamp et al., 2014; Lorenc et al., 2013; McGill et al., 2015). In principle, interventions that do not require deliberation in order to be effective and that can be implemented across a wide variety of settings in order to reach those who are difficult to reach with individualized programs (McGill et al., 2015) should be a good candidate to bridge gaps across socio-economic position. But whether nudges live up to the promise of decreasing inequalities is mostly still an empirical question (De Ridder et al., in press). Future research should thus continue to address effectiveness of various nudges under Type 1 and Type 2 processing to study whether they are indeed effective without the need for deliberation, and should consequently investigate whether those nudges have the potential to decrease inequalities.

When and for whom are nudges acceptable? As mentioned before, policy makers ought not only to know when and for whom nudges are effective, but also to know when and for whom nudges are acceptable in order to make an informed decision about which measure to install for what behavior in which context. The field of implementation science highlights the importance of elements like acceptability for bridging the gap between knowledge about what is efficacious in controlled settings and what is successfully delivered to a specific target population (Luszczynska, Lobczowska, & Horodyska, 2020; Proctor et al., 2011). Large-scale surveys with nationally representative samples thus remain important to quantify public support for a range of nudges, also in comparison to other, more coercive, measures (Reynolds et al., 2019). Yet, in order to obtain a more fine-grained picture, these surveys should also include (changeable) psychological factors related to self-regulation in order to better understand which individuals accept the implementation of such nudges. Our vignette study in Chapter 5 was only a starting point for addressing whether those who want to perform the behavior and/or those that struggle in achieving their goals welcome the implementation of a nudge that aligns with their goals.

Remaining questions. Next to the suggestions for future research that are presented above, a few remaining questions that fall outside of the scope of the current dissertation but nevertheless highlight the need for psychological science in nudging research deserve to be touched upon as well. First, an important question that remains is whether nudges only yield short-lived effects or whether effects can sustain over time. In the current dissertation, all studies were cross-sectional, so based on the current dissertation we cannot contribute to addressing this important question. Currently, little is known about long-term effects of nudges as only few studies have addressed effectiveness over time (Van Gestel, Kroese, & De Ridder, 2018; Venema, Kroese, & De Ridder, 2018). Second, future research should focus on the possibility of spillover effects of nudges towards other behaviors in other contexts (d'Adda, Capraro, & Tavoni, 2017; Fanghella, d'Adda, & Tavoni, 2019; Ghesla, Grieder, & Schmitz, 2019; Kuhn et al., 2021). Finally, future research should

continue to address downstream consequences on subjective states after having been nudged, such as feelings of autonomy and choice satisfaction (Wachner et al., 2020). For exploratory purposes, some of the studies in the current dissertation included measures of choice satisfaction, experienced diffulty of the choice, and experienced doubt, but throughout this dissertation we did not find a consistent pattern of results for such variables. Nevertheless, it is important to investigate how people actually experience being nudged in addition to asking people how they would think they would feel if they had been nudged (Wachner, Adriaanse, & De Ridder, under review). Altogether, such findings could also help policy makers in making informative decisions about which type of interventions to consider with the specific goals in mind.

Conclusion

The empirical work that was conducted as part of this dissertation addressed the main research question when and for whom nudges are effective and acceptable from the angle of effort and motivation. Several conclusions can be distilled from the empirical work. First, *the proximity nudge can remain effective if there are more alternative options in the choice set*. Previous studies on the proximity effect were limited to either one or two options in the choice set, but our research suggests that the proximity effect may persist with up to ten options of similar utility. Second, *default nudges are no more or less effective when cognitive resources are taxed.* This indeed suggests that nudges can supplement other behavioral interventions in the policy maker's toolbox, while slightly salving concerns about possible manipulation and lack of autonomous decision making. Third, *default nudges and motivation independently predict behavior for which people are generally motivated*. The pattern of results from this dissertation suggests that defaults set an anchor, from which people can adjust according to the strength and type of motivation. Fourth, *autonomous motivation is the sole main predictor from a range of self-regulatory variables that consistently predicts nudge acceptability*. The focus on effort and motivation enabled addressing core questions that surround many issues of the theory, practice, and ethics of nudging. As such, this dissertation aimed to contribute to building a robust science of nudging that is theoretically sound and more efficacious in its implementation, and tried to take a next step in nudging research by moving from black and white evaluations of absolute effectiveness or acceptability towards painting a full-color picture of when and for whom nudges are effective and acceptable.

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Supplementary materials

Chapter 3

List of amenities used in Study 1 and 2 (adapted from Steffel, Williams, & Pogacar, 2016):

- Energy-efficient oven and stove
- Tankless water heater
- Smart thermostat
- Double glazed windows
- Energy-efficient range hood and bathroom fan
- Radiator foil
- Energy-efficient dishwasher and refrigerator
- LED light bulbs
- Energy-efficient washer and dryer
- Dimmer switches for indoor lighting
- Low-flow toilets
- Solar-powered outdoor lighting
- Low-flow faucets and shower heads
- Motion sensors for outdoor lighting

Table S3.1.

Main results of Study 1 with the subsample of participants who correctly remembered the complete pattern of dots at the initial measurement (n = 615).

	$F(1, 611)$	p	η_p^2
Default	459.54	< .001	.43
Cognitive load	0.19	.666	< .01
Default x Cognitive load	< 0.01	.953	< .01

Table S3.2.

Main results of Study 1 with the subsample of participants in the high load condition who did not perfectly recall the pattern of dots during the second measurement (n = 646).

	$F(1, 642)$	p	η_p^2
Default	467.24	< .001	.42
Cognitive load	2.12	.146	< .01
Default x Cognitive load	0.23	.634	< .01

Table S3.3.

Main results of Study 1 with the subsample of participants in the high load condition who did not write down the pattern of dots (n = 830).

	F(1, 826)	*p*	η_p^2
Default	465.22	< .001	.36
Cognitive load	0.14	.705	< .01
Default x Cognitive load	< 0.01	.921	< .01

Table S3.4.

Main results of Study 1 with the subsample of participants who had not detected the goal of the study according to criterion 1 (n = 461).

	F(1, 457)	*p*	η_p^2
Default	250.50	< .001	.35
Cognitive load	2.15	.143	< .01
Default x Cognitive load	3.14	.077	< .01

Table 3.5.

Main results of Study 1 with the subsample of participants who had not detected the goal of the study according to criterion 2 (n = 727).

	F(1, 723)	*p*	η_p^2
Default	382.21	< .001	.35
Cognitive load	1.03	.310	< .01
Default x Cognitive load	0.95	.330	< .01

Chapter 4

Study 2

For exploratory purposes, we analyzed whether there was a difference between the two conditions in satisfaction with the chosen amenities. The independent samples t-test revealed that participants in the default condition ($M = 6.14$, $SD = .93$) were more satisfied than participants in the control condition ($M = 5.68$, $SD = .94$), $t\,(532) = -5.75$, $p < .001$, $d = .50$. Satisfaction correlated positively with autonomous motivation ($r = .19$, $p < .001$) and goal strivings ($r = .19$, $p < .001$), and correlated negatively with amotivation ($r = -.11$, $p = .009$), but not with controlled motivation ($p = .097$).

Study 3

For exploratory purposes, we again analyzed whether there was a difference between the two conditions in choice satisfaction. The independent samples t-test revealed no difference between the two conditions ($p = .455$). Satisfaction correlated negatively with controlled motivation ($r = -.13$, $p = .002$), but not with goal strivings ($p = .107$), autonomous motivation ($p = .116$) and amotivation ($p = .230$). We further explored for differences in acceptability and intrusiveness. The independent samples t-test revealed no difference between the two conditions in acceptability ($p = .132$) and revealed a marginally significant effect of the default on intrusiveness ($p = .053$), with higher scores in the default condition ($M = 3.38$, $SD = 2.14$) than in the control condition ($M = 3.01$, $SD = 2.25$). Acceptability correlated positively with goal strivings ($r = .09$, $p = .035$) and autonomous motivation ($r = .11$, $p = .010$), and negatively with amotivation ($r = -.09$, $p = .043$), but not with controlled motivation ($p = .188$). Intrusiveness did not correlate with goal strivings ($p = .373$), autonomous motivation ($p = .526$), controlled motivation ($p = .635$), or amotivation ($p = .085$).

Stepwise regressions starting with motivation (Study 1)

Table S4.1. Regression model with the proportion of nudged healthy food choices as dependent variable, and goal strivings (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 1).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Goal strivings	.06					< .001
Constant		0.08 (0.03)		[0.02, 0.15]	2.52	.012
Goal strivings		0.04 (0.01)	.25	[0.03, 0.06]	6.52	< .001
Step 2 – Goal strivings	.03					< .001
Constant		0.05 (0.03)		[-0.02, 0.12]	1.50	.133
Goal strivings		0.04 (0.01)	.25	[0.03, 0.06]	6.63	< .001
Default		0.07 (0.01)	.17	[0.04, 0.10]	4.49	< .001
Step 3 – Goal Strivings	.00					.545
Constant		0.07 (0.05)		[-0.02, 0.16]	1.50	.135
Goal strivings		0.04 (0.01)	.25	[0.02, 0.06]	4.16	< .001
Default		0.03 (0.07)	.17	[-0.10, 0.16]	0.41	.681
Goal strivings X Default		0.01 (0.01)	.02	[-0.02, 0.03]	0.60	.545

Note. *β*'s are fully standardized. *N* = 635.

Table S4.2. Regression model with the proportion of nudged healthy food choices as dependent variable, and autonomous motivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 1).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Autonomous motivation	.06					< .001
Constant		0.04 (0.04)		[-0.04, 0.12]	1.00	.319
Autonomous motivation		0.05 (0.01)	.24	[0.03, 0.06]	6.32	< .001
Step 2 – Autonomous motivation	.02					< .001
Constant		0.02 (0.04)		[-0.06, 0.10]	0.47	.641
Autonomous motivation		0.04 (0.01)	.24	[0.03, 0.06]	6.17	< .001
Default		0.06 (0.01)	.16	[0.03, 0.09]	4.13	< .001
Step 3 – Autonomous motivation	.00					.876
Constant		0.03 (0.06)		[-0.08, 0.13]	0.45	.654
Autonomous motivation		0.04 (0.01)	.24	[0.02, 0.06]	4.37	< .001
Default		0.05 (0.08)	.16	[-0.11, 0.21]	0.60	.552
Autonomous motivation X Default		0.00 (0.01)	.01	[-0.03, 0.03]	0.16	.876

Note. *β*'s are fully standardized. *N* = 635.

Table S4.3.

Regression model with the proportion of nudged healthy food choices as dependent variable, and controlled motivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 1).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Controlled motivation	.00					.291
Constant		0.28 (0.02)		[0.23, 0.32]	12.41	< .001
Controlled motivation		0.01 (0.01)	.04	[-0.01, 0.02]	1.06	.291
Step 2 – Controlled motivation	.03					< .001
Constant		0.23 (0.02)		[0.19, 0.28]	10.05	< .001
Controlled motivation		0.01 (0.01)	.05	[-0.00, 0.02]	1.30	.193
Default		0.07 (0.02)	.17	[0.04, 0.10]	4.40	< .001
Step 3 – Controlled motivation	.00					.697
Constant		0.25 (0.03)		[0.18, 0.31]	7.73	< .001
Controlled motivation		0.01 (0.01)	.05	[-0.01, 0.02]	0.64	.522
Default		0.05 (0.04)	.17	[-0.03, 0.14]	1.17	.244
Controlled motivation X Default		0.00 (0.01)	.02	[-0.02, 0.03]	0.39	.697

Note. *β*'s are fully standardized. *N* = 635.

Table S4.4.

Regression model with the proportion of nudged healthy food choices as dependent variable, and amotivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 1).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Amotivation	.07					< .001
Constant		0.41 (0.02)		[0.38, 0.45]	22.77	< .001
Amotivation		-0.05 (0.01)	-.27	[-0.06, -0.03]	-7.03	< .001
Step 2 – Amotivation	.03					< .001
Constant		0.38 (0.02)		[0.34, 0.42]	19.97	< .001
Amotivation		-0.05 (0.01)	-.28	[-0.06, -0.04]	-7.32	< .001
Default		0.07 (0.01)	.18	[0.04, 0.10]	4.78	< .001
Step 3 – Amotivation	.00					.811
Constant		0.38 (0.03)		[0.33, 0.43]	14.85	< .001
Amotivation		-0.05 (0.01)	-.28	[-0.07, -0.03]	-4.89	< .001
Default		0.08 (0.04)	.18	[0.01, 0.15]	2.17	.030
Amotivation X Default		-0.00 (0.01)	-.01	[-0.03, 0.02]	-0.24	.811

Note. *β*'s are fully standardized. *N* = 635.

Stepwise regressions starting with motivation (Study 2)

Table S4.5.

Regression model with the number of green amenities as dependent variable, and goal strivings (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 2).

	ΔR^2_{adj}	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Goal strivings	.02					.002
Constant		6.98 (0.64)		[5.72, 8.23]	10.92	< .001
Goal strivings		0.42 (0.13)	.13	[0.15, 0.68]	3.13	.002
Step 2 – Goal strivings	.54					< .001
Constant		3.81 (0.45)		[2.94, 4.69]	8.54	< .001
Goal strivings		0.50 (0.09)	.16	[0.32, 0.67]	5.57	< .001
Default		5.54 (0.22)	.74	[5.11, 5.97]	25.46	< .001
Step 3 – Goal Strivings	.00					.996
Constant		3.82 (0.60)		[2.65, 4.99]	6.41	< .001
Goal strivings		0.50 (0.12)	.16	[3.84, 7.23]	4.06	< .001
Default		5.53 (0.86)	.74	[0.26, 0.74]	6.43	< .001
Goal strivings X Default		0.00 (0.17)	.00	[-0.35, 0.35]	0.01	.996

Note. *β*'s are fully standardized. *N* = 534.

Table S4.6.

Regression model with the number of green amenities as dependent variable, and autonomous motivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 2).

	ΔR^2_{adj}	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Autonomous motivation	.05					< .001
Constant		5.72 (0.60)		[4.54, 6.90]	9.52	< .001
Autonomous motivation		0.65 (0.12)	.23	[0.42, 0.89]	5.50	< .001
Step 2 – Autonomous motivation	.53					< .001
Constant		3.08 (0.41)		[2.27, 3.89]	7.47	< .001
Autonomous motivation		0.63 (0.08)	.22	[0.48, 0.79]	7.98	< .001
Default		5.48 (0.21)	.73	[5.06, 5.89]	25.91	< .001
Step 3 – Autonomous motivation	.00					.874
Constant		3.02 (0.55)		[1.93, 4.11]	5.45	< .001
Autonomous motivation		0.64 (0.11)	.22	[0.43, 0.86]	5.85	< .001
Default		5.60 (0.80)	.73	[4.03, 7.17]	6.99	< .001
Autonomous motivation X Default		-0.03 (0.16)	-.00	[-0.34, 0.29]	-0.16	.874

Note. *β*'s are fully standardized. *N* = 534.

Table S4.7.

Regression model with the number of green amenities as dependent variable, and controlled motivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 2).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Controlled motivation	.00					.099
Constant		8.12 (0.50)		[7.14, 9.11]	16.18	< .001
Controlled motivation		0.24 (0.14)	.07	[-0.04, 0.52]	1.65	.099
Step 2 – Controlled motivation	.54					< .001
Constant		5.35 (0.36)		[4.64, 6.06]	14.84	< .001
Controlled motivation		0.24 (0.10)	.07	[0.05, 0.43]	2.46	.014
Default		5.50 (0.22)	.73	[5.06, 5.93]	24.71	< .001
Step 3 – Controlled motivation	.00					.389
Constant		5.66 (0.50)		[4.66, 6.65]	11.21	< .001
Controlled motivation		0.15 (0.14)	.07	[-0.13, 0.43]	1.03	.303
Default		4.93 (0.69)	.73	[3.58, 6.29]	7.17	< .001
Controlled motivation X Default		0.17 (0.20)	.03	[-0.22, 0.55]	0.86	.389

Note. *β*'s are fully standardized. *N* = 534.

Table S4.8.

Regression model with the number of green amenities as dependent variable, and amotivation (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 2).

	ΔR_{adj}^2	*b (SE)*	*β*	*95% CI b*	*t*	*p*
Step 1 – Amotivation	.03					< .001
Constant		10.24 (0.38)		[9.49, 10.99]	26.82	< .001
Amotivation		-0.49 (0.13)	-.16	[-0.74, -0.24]	-3.84	< .001
Step 2 – Amotivation	.52					< .001
Constant		7.26 (0.29)		[6.70, 7.82]	25.37	< .001
Amotivation		-0.40 (0.09)	-.13	[-0.57, -0.23]	-4.62	< .001
Default		5.45 (0.22)	.72	[5.02, 5.88]	24.84	< .001
Step 3 – Amotivation	.00					.214
Constant		7.55 (0.37)		[6.83, 8.28]	20.42	< .001
Amotivation		-0.51 (0.12)	-.13	[-0.75, -0.27]	-4.18	< .001
Default		4.87 (0.52)	.72	[3.85, 5.89]	9.36	< .001
Amotivation X Default		0.22 (0.17)	.04	[-0.13, 0.56]	1.25	.214

Note. *β*'s are fully standardized. *N* = 534.

Stepwise regressions starting with motivation for helping other people (Study 3)

Table S4.9.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and goal strivings for helping other people (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Goal strivings	.00						.365
Constant		-0.72 (0.45)	-.32	0.49	[0.20, 1.17]	-1.60	.111
Goal strivings		0.07 (0.08)	.08	1.08	[0.92, 1.27]	0.90	.367
Step 2 – Goal strivings	.01						.027
Constant		-0.88 (0.46)		0.42	[0.17, 1.01]	-1.91	.056
Goal strivings		0.07 (0.08)	.07	1.07	[0.91, 1.26]	0.82	.413
Default		0.39 (0.17)	.19	1.47	[1.05, 2.07]	2.21	.027
Step 3 – Goal Strivings	.00						.280
Constant		-1.32 (0.62)		0.27	[0.08, 0.88]	-2.12	.034
Goal strivings		0.15 (0.11)	.06	1.16	[0.93, 1.46]	1.32	.187
Default		1.35 (0.92)	.20	3.87	[0.65, 23.73]	1.48	.139
Goal strivings X Default		-0.18 (0.17)	-.10	0.84	[0.60, 1.16]	-1.08	.281

Note. *β*'s are X-standardized. *N* = 544.

Table S4.10.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and autonomous motivation for helping other people (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Autonomous motivation	.01						.136
Constant		-0.95 (0.44)	-.32	0.39	[0.16, 0.90]	-2.18	.030
Autonomous motivation		0.12 (0.08)	.13	1.13	[0.96, 1.32]	1.48	.138
Step 2 – Autonomous motivation	.01						.029
Constant		-1.11 (0.45)		0.33	[0.14, 0.78]	-2.49	.013
Autonomous motivation		0.11 (0.08)	.12	1.12	[0.96, 1.32]	1.39	.163
Default		0.38 (0.17)	.19	1.47	[1.04, 2.07]	2.18	.029
Step 3 – Autonomous motivation	.00						.558
Constant		-0.87 (0.61)		0.42	[0.12, 1.37]	-1.42	.155
Autonomous motivation		0.07 (0.11)	.12	1.07	[0.86, 1.34]	0.59	.557
Default		-0.12 (0.88)	.19	0.88	[0.16, 4.98]	-0.14	.889
Autonomous motivation X Default		0.10 (0.16)	.05	1.10	[0.80, 1.52]	0.59	.558

Note. *β*'s are X-standardized. *N* = 544.

Table S4.11.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and controlled motivation for helping other people (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Controlled motivation	.00						.620
Constant		-0.18 (0.29)		0.83	[0.47, 1.48]	-0.62	.535
Controlled motivation		-0.04 (0.07)		0.96	[0.83, 1.11]	-0.50	.620
Step 2 – Controlled motivation	.01						.022
Constant		-0.33 (0.30)		0.72	[0.40, 1.29]	-1.11	.268
Controlled motivation		-0.05 (0.07)	-.06	0.95	[0.82, 1.10]	-0.67	.503
Default		0.40 (0.18)	.20	1.49	[1.06, 2.11]	2.28	.022
Step 3 – Controlled motivation	.00						.646
Constant		-0.21 (0.40)		0.81	[0.37, 1.79]	-0.52	.606
Controlled motivation		-0.08 (0.11)	-.06	0.92	[0.75, 1.13]	-0.80	.425
Default		0.14 (0.59)	.20	1.15	[0.36, 3.66]	0.24	.808
Controlled motivation X Default		0.07 (0.15)	.04	1.07	[0.80, 1.44]	0.46	.646

Note. *β*'s are X-standardized. *N* = 544.

Table S4.12.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and amotivation for helping other people (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Amotivation	.00						.698
Constant		-0.25 (0.20)	-.32	0.78	[0.52, 1.17]	-1.21	.227
Amotivation		-0.03 (0.08)	-.03	0.97	[0.83, 1.13]	-0.39	.698
Step 2 – Amotivation	.01						.023
Constant		-0.42 (0.22)		0.66	[0.43, 1.01]	-1.92	.055
Amotivation		-0.04 (0.08)	-.05	0.96	[0.82, 1.12]	-0.52	.602
Default		0.40 (0.17)	.20	1.49	[1.06, 2.10]	2.27	.023
Step 3 – Amotivation	.00						.435
Constant		-0.58 (0.30)		0.56	[0.31, 1.01]	-1.94	.053
Amotivation		0.03 (0.12)	-.04	1.03	[0.81, 1.30]	0.24	.812
Default		0.69 (0.41)	.20	1.99	[0.89, 4.49]	1.67	.096
Amotivation X Default		-0.13 (0.16)	-.07	0.88	[0.64, 1.21]	-0.78	.435

Note. *β*'s are X-standardized. *N* = 544.

Stepwise regressions starting with motivation for making sustainable choices (Study 3)

Table S4.13.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and goal strivings for making sustainable choices (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Goal strivings	.02						.003
Constant		-1.27 (0.34)		0.28	[0.14, 0.54]	-3.79	< .001
Goal strivings		0.20 (0.07)	.27	1.23	[1.07, 1.40]	2.96	.003
Step 2 – Goal strivings	.01						.025
Constant		-1.47 (0.35)		0.23	[0.11, 0.45]	-4.20	< .001
Goal strivings		0.20 (0.07)	.27	1.23	[1.07, 1.41]	2.96	.003
Default		0.39 (0.18)	.20	1.48	[1.05, 2.10]	2.23	.025
Step 3 – Goal Strivings	.00						.627
Constant		-1.31 (0.48)		0.27	[0.10, 0.67]	-2.75	.006
Goal strivings		0.17 (0.10)	.27	1.19	[0.98, 1.44]	1.75	.081
Default		0.08 (0.67)	.19	1.08	[0.29, 4.07]	0.11	.909
Goal strivings X Default		0.07 (0.14)	.04	1.07	[0.82, 1.40]	0.49	.627

Note. *β*'s are X-standardized. *N* = 544.

Table S4.14.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and autonomous motivation for making sustainable choices (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Autonomous motivation	.01						.005
Constant		-1.17 (0.37)		0.31	[0.15, 0.64]	-3.14	.002
Autonomous motivation		0.16 (0.07)	.21	1.17	[1.03, 1.34]	2.36	.018
Step 2 – Autonomous motivation	.01						.025
Constant		-1.37 (0.39)		0.25	[0.12, 0.54]	-3.55	< .001
Autonomous motivation		0.16 (0.07)	.21	1.17	[1.03, 1.34]	2.36	.018
Default		0.39 (0.18)	.20	1.48	[1.05, 2.09]	2.24	.025
Step 3 – Autonomous motivation	.00						.956
Constant		-1.35 (0.54)		0.26	[0.09, 0.73]	-2.50	.012
Autonomous motivation		0.16 (0.10)	.21	1.17	[0.97, 1.42]	1.60	.110
Default		0.35 (0.75)	.20	1.42	[0.33, 6.21]	0.47	.637
Autonomous motivation X Default		0.01 (0.14)	.00	1.01	[0.77, 1.31]	0.06	.956

Note. *β*'s are X-standardized. *N* = 544.

Table S4.15.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and controlled motivation for making sustainable choices (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Controlled motivation	.00						.404
Constant		-0.54 (0.28)	-.32	0.58	[0.34, 1.00]	-1.94	.052
Controlled motivation		0.06 (0.07)	.07	1.06	[0.92, 1.23]	0.83	.404
Step 2 – Controlled motivation	.01						.028
Constant		-0.69 (0.29)		0.50	[0.28, 0.87]	-2.42	.016
Controlled motivation		0.05 (0.07)	.06	1.05	[0.91, 1.21]	0.69	.491
Default		0.38 (0.17)	.19	1.47	[1.04, 2.07]	2.19	.029
Step 3 – Controlled motivation	.00						.185
Constant		-0.34 (0.39)		0.71	[0.33, 1.54]	-0.86	.392
Controlled motivation		-0.05 (0.11)	.05	0.95	[0.77, 1.17]	-0.48	.628
Default		-0.32 (0.56)	.19	0.73	[0.24, 2.17]	-0.57	.569
Controlled motivation X Default		0.19 (0.15)	.12	1.21	[0.91, 1.62]	1.32	.186

Note. *β*'s are X-standardized. *N* = 544.

Table S4.16.

Logistic regression model with the likelihood of participating in the longer version as dependent variable, and amotivation for making sustainable choices (Step 1), default (Step 2), and interaction effect (Step 3) as independent variables (Study 3).

	ΔR_N^2	*b (SE)*	*β*	*OR*	*95% CI OR*	*z*	*p*
Step 1 – Amotivation	.01						.014
Constant		-0.02 (0.18)		0.98	[0.69, 1.40]	-0.10	.921
Amotivation		-0.13 (0.07)	-.17	0.88	[0.77, 1.01]	1.87	.062
Step 2 – Amotivation	.01						.025
Constant		-0.22 (0.20)		0.61	[0.54, 1.20]	-1.06	.288
Amotivation		-0.13 (0.07)	-.17	0.88	[0.77, 1.01]	-1.86	.062
Default		0.39 (0.18)	.20	1.48	[1.05, 2.09]	2.24	.025
Step 3 – Amotivation	.00						.362
Constant		-0.07 (0.26)		0.94	[0.56, 1.57]	-0.25	.802
Amotivation		-0.19 (0.10)	-.17	0.82	[0.67, 1.00]	1.93	.054
Default		0.10 (0.37)	.19	1.10	[0.54, 2.26]	0.27	.787
Amotivation X Default		0.13 (0.14)	.08	1.14	[0.86, 1.49]	0.91	.363

Note. *β*'s are X-standardized. *N* = 544.

Chapter 5

Table S5.1

Linear regression models predicting acceptability, intrusiveness, perceived effectiveness, and goal alignment for the three different types of nudges for the subsample of employed participants (n = 239).

	Acceptability	Intrusiveness	Effectiveness	Alignment
	β (SE)	β (SE)	β (SE)	β (SE)
Default				
Self-control	.06 (.07)	-.01 (.08)	.07 (.08)	.11 (.07)
Proactive Coping	-.01 (.07)	-.02 (.07)	.04 (.08)	-.06 (.07)
Self-efficacy	.11 (.08)	.01 (.09)	.06 (.09)	.06 (.08)
Perceived Control	-.20 (.07) *	.15 (.07) *	-.06 (.07)	-.11 (.06)
Perceived Difficulty	-.03 (.08)	.08 (.08)	-.04 (.08)	-.11 (.07)
Autonomous Motivation	.30 (.07) ***	-.22 (.08) **	.10 (.08)	.35 (.07) ***
Controlled Motivation	.01 (.07)	-.02 (.07)	.02 (.07)	.03 (.07)
Portion Size				
Self-control	.22 (.08) **	-.17 (.08) *	.19 (.08) *	.21 (.07) **
Proactive Coping	.02 (.07)	-.00 (.08)	-.01 (.08)	-.02 (.07)
Self-efficacy	-.20 (.09) *	.12 (.09)	-.07 (.09)	-.22 (.08) *
Perceived Control	.04 (.07)	-.01 (.07)	.04 (.07)	.03 (.07)
Perceived Difficulty	.05 (.08)	-.00 (.08)	.05 (.08)	.03 (.08)
Autonomous Motivation	.25 (.08) **	-.09 (.08)	.13 (.08)	.30 (.08) ***
Controlled Motivation	.08 (.07)	-.08 (.07)	.04 (.07)	.15 (.07) *
Rearrangement				
Self-control	-.01 (.07)	-.01 (.08)	-.04 (.08)	.04 (.07)
Proactive Coping	.13 (.07)	.08 (.07)	.13 (.07)	.01 (.06)
Self-efficacy	.05 (.08)	.14 (.09)	.02 (.09)	.06 (.08)
Perceived Control	-.07 (.06)	.02 (.07)	.08 (.07)	-.02 (.06)
Perceived Difficulty	.08 (.07)	.13 (.08)	-.04 (.08)	.10 (.07)
Autonomous Motivation	.50 (.07) ***	-.36 (.08) ***	.13 (.08)	.48 (.07) ***
Controlled Motivation	.00 (.06)	.09 (.07)	.10 (.07)	.14 (.06) *

Note. Model fit for Acceptability: R_{adj}^2 = .12 *** (Default); R_{adj}^2 = .08 *** (Portion size); R_{adj}^2 = .26 *** (Rearrangement). Model fit for Intrusiveness: R_{adj}^2 = .04 * (Default); R_{adj}^2 = .01 (Portion size); R_{adj}^2 = .08 *** (Rearrangement). Model fit for Effectiveness: R_{adj}^2 = .01 (Default); R_{adj}^2 = .02 (Portion size); R_{adj}^2 = .05 ** (Rearrangement). Model fit for Alignment: R_{adj}^2 = .12 *** (Default); R_{adj}^2 = .10 *** (Portion size); R_{adj}^2 = .29 *** (Rearrangement).

Nederlandse samenvatting
(Dutch summary)

De belangstelling van wetenschappers en beleidsmakers voor het onderwerp nudging is sinds de publicatie van het boek 'Nudge: Naar betere beslissingen over gezondheid, geluk en welvaart' in 2008 enorm toegenomen. In dit boek introduceerden gedragseconoom en Nobelprijswinnaar Richard Thaler en jurist Cass Sunstein nudges als een nieuw soort interventie om gedrag te veranderen ter aanvulling op bestaande instrumenten. Nudges zijn bedoeld om mee te deinen met de aard van de mens in plaats van hiertegen te vechten. Ze worden ingezet om gewenst gedrag te stimuleren door middel van strategische aanpassingen in de directe keuzearchitectuur (de omgeving waarin we keuzes maken) die gebaseerd zijn op inzichten uit de gedragswetenschappen. De belofte van nudging lijkt te liggen in de veronderstelling dat de effectiviteit van dit soort interventies niet afhankelijk is van uitgebreide informatieverwerking door het individu. In die zin vullen ze dus bestaande interventies zoals persuasieve communicatie of informatiecampagnes aan waarvan het succes doorgaans afhankelijk is van de beschikbaarheid van cognitief vermogen.

Een aantal toonaangevende studies in de begindagen van het onderzoek naar nudging lieten zien dat nudges zowel effectief als kosteneffectief kunnen zijn en dat ze over het algemeen aanvaard worden door de meeste mensen. Sindsdien is de interesse in het onderwerp alleen maar toegenomen. Maar terwijl de interesse van beleidsmakers toenam, nam de centrale rol van de psychologische wetenschap in onderzoek naar nudging af. En terwijl samenvattende artikelen (zogeheten systematische reviews en meta-analyses) veelbelovende resultaten lieten zien, toonden deze artikelen ook aan dat er momenteel relatief weinig bekend is over de condities waaronder nudges effectief en aanvaardbaar zijn. Om te zorgen voor goed onderbouwd beleid en om de kans op succesvolle implementatie te vergroten, is het belangrijk om te begrijpen welken mensen onder welke omstandigheden kunnen profiteren van een nudge. Dit proefschrift poogt invulling te geven aan oproepen om de psychologie weer een centrale rol te geven in onderzoek naar nudging en poogt zodoende bij te dragen aan een robuuste wetenschap van nudging die theoretisch solide is en doeltreffend in de praktijk. Het overkoepelende doel van dit proefschrift is om te onderzoeken wanneer en voor wie nudges effectief en aanvaardbaar zijn. Door dit proefschrift heen staan we stil bij verschillende psychologische aspecten gerelateerd aan inspanning en motivatie, omdat deze aspecten diverse implicaties hebben voor discussies over de theorie, praktijk en ethiek van nudging. Voordat ik de specifieke onderzoeksresultaten van dit proefschrift beschrijf, geef ik eerst een korte samenvatting van het huidige onderzoeksveld.

Nudging: definitie en onderliggende principes

Een nudge is een klein duwtje in de rug dat gewenst gedrag stimuleert. De oorspronkelijke definitie van een nudge zoals beschreven door Thaler en Sunstein luidt dat een nudge ieder aspect van de keuzearchitectuur is dat het gedrag van mensen op een voorspelbare manier beïnvloedt zonder daarbij keuzes te beperken of op een significante manier de economische prikkels te veranderen. Bekende voorbeelden van

nudges zijn de verandering van de standaardoptie voor orgaandonatie, het gebruik van sociale normen om energieverbruik te vergelijken met anderen, of het verplaatsen van gezonde voedselproducten naar de kassa. Andere interventies zoals het verstrekken van meer informatie, het proberen te overtuigen of het gebruik van wetgeving tellen niet als nudges, omdat ze het niet makkelijker maken om het gewenste gedrag uit te voeren. Eigenlijk is nudging een soort overkoepelende term waaronder verschillende interventies vallen die aan bovenstaande definitie voldoen. Hoewel nudges onderling veel van elkaar kunnen verschillen, delen deze interventies dat ze de menselijke interactie met de omgeving begeleiden door gebruik te maken van gedragsinzichten. Om te begrijpen hoe nudges deze aspecten van de omgeving gebruiken om gedrag te sturen moeten we eerst dual-process theorieën uiteenzetten.

Mensen hebben niet altijd de capaciteit of wilskracht om uitgebreid na te denken over hun keuzes. Nudges proberen daarop in te haken en gewenst gedrag te stimuleren door gebruik te maken van inzichten over automatische besluitvorming zoals door gewoontes, impulsen en heuristieken (zogenaamde vuistregels, zoals wanneer mensen geneigd zijn om de meerderheid te volgen of advies van autoriteiten over te nemen). Nudging is daarom sterk geworteld in dual-process theorieën die hun oorsprong vinden in de jaren 70 en 80 van de vorige eeuw. Er bestaan veel varianten van deze theorieën maar in grote lijnen delen ze dezelfde terminologie en onderliggende aannames. In dit proefschrift hanteer ik de meest recente inzichten en gebruik ik de termen Type 1 en Type 2 processen in plaats van Systeem 1 en Systeem 2 processen die vaak worden gebruikt in de (populair)wetenschappelijke literatuur. Het meest onderscheidende verschil tussen de twee procestypes is de betrokkenheid van het werkgeheugen. Type 1 processen belasten het werkgeheugen minimaal, terwijl Type 2 processen sterk het werkgeheugen belasten. Daarnaast zijn Type 1 processen vaak snel en automatisch en kosten ze weinig moeite, terwijl Type 2 processen meestal langzaam en gecontroleerd zijn en meer moeite kosten.

Er blijft tot op de dag van vandaag veel discussie over de meest accurate manier om de menselijke cognitieve architectuur weer te geven. Toch is het feit dat nudges erkennen dat mensen niet altijd even goed nadenken over hun keuzes een belangrijk inzicht dat afwijkt van de neoklassieke economische theorie die stelt dat mensen rationele wezens zijn die een optimale keuze maken gebaseerd op alle beschikbare informatie. Nudging is gebaseerd op het idee dat de menselijke rationaliteit begrensd is. We wegen lang niet altijd alle voor- en nadelen tegen elkaar af en maken meestal geen volledige kosten-baten analyse. In plaats daarvan kiezen we vaak voor de optie die goed genoeg is gezien de complexiteit van de keuze en de beschikbare cognitieve capaciteit. Het is belangrijk om te benadrukken dat dit niet betekent dat we continue verkeerde en bevooroordeelde keuzes maken, maar wel dat mensen niet altijd onder alle omstandigheden volledig rationeel zijn. Voortbouwend op deze inzichten erkennen nudges dus dat veel van ons dagelijks gedrag gebaseerd is op Type 1 processen. Hierdoor vormen nudges een additionele set gedragsveranderingsinterventies die in principe niet afhankelijk zouden moeten zijn

van welberedeneerde processen. Dus in plaats van alle complexiteiten van het menselijk gedrag te bevechten pogen nudges mee te deinen met de aard van het menselijk gedrag.

Effectiviteit en aanvaardbaarheid

De populariteit van nudging kwam mede tot stand door een aantal wetenschappelijke publicaties van zeer effectieve interventies, zoals het Save More Tomorrow programma wat ervoor zorgt dat Amerikanen meer sparen voor hun pensioen, en het opt-out systeem voor orgaandonatie wat ertoe leidt dat meer mensen zich als orgaandonor registreren. Inmiddels blijkt ook uit verschillende samenvattende artikelen zoals systematische reviews en meta-analyses dat nudges de potentie hebben om gewenst gedrag te stimuleren. Maar deze artikelen tonen ook aan dat de hoeveelheid en de kwaliteit van de studies nog beperkt is, dat de effectiviteit zoals aangetoond in de verschillende studies erg van elkaar verschilt, en dat op dit moment relatief weinig bekend is over de werkingsmechanismes van en randvoorwaarden voor effectieve nudges. Zo is het onder andere niet duidelijk of nudges effectief blijven in complexere keuzeomgevingen, of de veronderstelde manier van gedragsbeïnvloeding zonder afhankelijkheid van beredeneerde processen accuraat is, en wat de rol van motivatie is bij de effectiviteit van nudges.

Een vergelijkbaar patroon valt te ontdekken als het gaat om de publieke opinie over de aanvaardbaarheid van nudges, alhoewel hier op dit moment nog geen samenvattende artikelen over gepubliceerd zijn. Toch is er een reeks vragenlijst- en scenariostudies en ook studies met daadwerkelijk geïmplementeerde nudges die erop wijzen dat mensen het gebruik van nudging over het algemeen aanvaardbaar vinden. Tegelijk is het zo dat we niet goed weten wanneer en voor wie nudges aanvaardbaar zijn. Om de kans op succesvolle implementatie met betekenisvolle gevolgen te vergroten is het dus van belang om beter inzicht te krijgen in de omstandigheden waaronder nudges aanvaardbaar worden geacht door diegenen die mogelijk in aanraking komen met de nudge. Naast inschattingen van wat het volk van nudging vindt, zijn er ook veel discussies over de aanvaardbaarheid en legitimiteit van nudging onder ethici en filosofen. Een groot deel van deze discussies is gebaseerd op de theoretische aanname dat nudges gebruik maken van automatische processen, terwijl empirische inzichten hierin nog schaars zijn. Het is dus van belang om de kwaliteit van dit soort discussies te vergroten met empirische resultaten.

Wanneer en voor wie zijn nudges effectief en aanvaardbaar?

Om antwoord te geven op bovenstaande vragen en kwesties is het hoofddoel van dit proefschrift om te onderzoeken wanneer en voor wie nudges effectief en aanvaardbaar zijn. Niet alleen is er een gebrek aan onderzoek naar de omstandigheden waaronder nudges effectief en aanvaardbaar zijn, ook zijn er verschillende wetenschappers die het belang van onderzoek hiernaar benadrukken. Een groot deel van de openstaande vragen met diverse implicaties voor theorie, praktijk en ethiek zijn gerelateerd aan de psychologie van nudging en dan met name aan aspecten van inspanning en motivatie. Oorspronkelijk was de rol van de psychologische wetenschap bij het ontstaan van nudging groot (denk onder andere aan de dual-process theorieën), maar terwijl de populariteit

van het concept in publiek beleid groeide werd de rol van de psychologie in onderzoek naar nudging kleiner. Inmiddels zijn er vele onderzoekers geweest die het belang van de psychologische wetenschap voor nudging weer op de kaart proberen te zetten. In het huidige proefschrift probeer ik hier een bijdrage aan te leveren.

Samenvatting van empirische bevindingen

In hoofdstuk 2 van dit proefschrift rapporteren we twee veldonderzoeken die als doel hadden om te onderzoeken wanneer nudges effectief zijn. In deze studies onderzochten we of nudges effectief kunnen blijven in complexere keuzesituaties met meerdere alternatieve opties in de directe keuzeomgeving waardoor deze omgeving meer representatief was voor de omgeving waarin mensen doorgaans keuzes maken. We deden onderzoek naar de effectiviteit van een nabijheidsnudge (in het Engels: proximity nudge) die als doel had om de keuze voor de dichtstbijzijnde optie te stimuleren. Eerder onderzoek naar de nabijheidsnudge maakte vaak gebruik van één of hooguit twee opties in de keuzeset. In ons onderzoek gebruikten we keuzesets bestaande uit drie of negen verschillende opties. Deze verschillende opties (verschillende soorten chocolaatjes) waren objectief gezien van gelijke waarde en deelnemers gaven aan dat ze niet van tevoren al een voorkeur hadden voor een van de opties. In Studie 1 toonden we aan dat de nabijheidsnudge effectief bleef wanneer er meer opties beschikbaar waren. In Studie 2 probeerden we deze resultaten conceptueel te repliceren met meer deelnemers en een verbeterde onderzoeksopstelling. We vonden dezelfde resultaten als in Studie 1 en vonden sterk bewijs voor de effectiviteit van de nabijheidsnudge ongeacht het aantal opties in de keuzeset. In beide studies kozen deelnemers meer dan twee keer zo vaak voor de optie waarin we geïnteresseerd waren als deze optie nabij gepositioneerd was, ongeacht het aantal alternatieve opties in de directe keuzeomgeving. Tezamen laten deze studies zien dat deze nudge dus effectief kan blijven als er meer opties in de directe keuzeomgeving zijn. Deze bevindingen hebben we verwerkt in de andere hoofdstukken.

Hoofdstuk 3 had een meer fundamentele benadering voor het beantwoorden van de vraag wanneer nudges effectief zijn en onderzocht in hoeverre nudges gebruik maken van automatische processen. Dit uitgangspunt vormt vaak de kern van vele debatten over de legitimiteit van nudging en wordt ook vaak aangehaald als een uniek kenmerk waarmee nudges zich onderscheiden van andere gedragsveranderingsinterventies. Echter is er nog maar weinig empirisch onderzoek dat deze cruciale aanname direct onder de loep heeft genomen. Daarom hebben we de effectiviteit onderzocht van een standaardnudge (in het Engels: default nudge) die duurzame keuzes stimuleerde terwijl we gelijktijdig manipuleerden of mensen gebruik moesten maken van Type 1 of Type 2 processen. We hebben dit onderzoek gerapporteerd in een Registered Report: een onderzoeksartikel waarbij we eerst de introductie, methoden en voorgestelde analyses vaststelden en lieten beoordelen voordat we daadwerkelijk de data gingen verzamelen en analyseren. In twee studies met veel deelnemers vonden we dat de default nudge zeer effectief was: deelnemers kozen meer duurzame opties als deze opties al van tevoren

geselecteerd waren als standaard optie. In Studie 1 manipuleerden we daarnaast de mate van cognitieve belasting met een taak waarin deelnemers ofwel een makkelijk patroon ofwel een moeilijk patroon aan stippen moesten onthouden. Het moeten onthouden van een moeilijk patroon zorgde ervoor dat Type 2 processen onderdrukt werden en dat deelnemers gebruik moesten maken van Type 1 processen voor het nemen van een beslissing over de duurzame opties. Zoals verwacht was de mate van cognitieve belasting niet van invloed op het effect van de default nudge. Sterker nog, het default effect was gelijkwaardig in de condities met lage en hoge cognitieve belasting. Maar, het feit dat mensen cognitief vermogen tot hun beschikking hebben (zoals wanneer mensen een simpel patroon aan stippen moeten onthouden) betekent nog niet dat mensen dit vermogen ook daadwerkelijk gebruiken bij het maken van hun keuze. Daarom hebben we in Studie 2 deelnemers gestimuleerd om Type 2 processen te gebruiken. Dit deden we door gebruik te maken van specifieke instructies waarmee we deelnemers aanmoedigden om een afgewogen beslissing te maken. Tegen onze verwachting in zorgden deze instructies er niet voor dat het default effect verkleind werd maar leidde dit tot een effect op de keuze van alle deelnemers los van de aanwezigheid van de default nudge. Alles tezamen laten deze twee studies zien dat defaults geen gebruik maken van Type 1 processen, maar dat dit soort effecten tot stand kunnen komen zonder daarbij afhankelijk te zijn van de beschikbaarheid van cognitief vermogen.

In hoofdstuk 4 stonden we stil bij de rol van motivatie bij de effectiviteit van nudges om te onderzoeken voor wie nudges effectief zijn. Studie 1 toonde aan dat een default nudge gezonde voedselkeuzes kan stimuleren in een online supermarkt omgeving. De sterkte van motivatie en de autonome vorm van motivatie (motivatie die uit jezelf komt omdat je het zelf interessant of belangrijk vindt) om gezond te eten hadden daarnaast ook een positief effect op gezonde voedselkeuzes, maar geen van de gemeten vormen van motivatie waren van invloed op het effect van de default nudge. Dit patroon aan resultaten hebben we conceptueel gerepliceerd in Studie 2, waarin we dezelfde default nudge gebruikten als in hoofdstuk 3. Opnieuw was de default nudge effectief in het stimuleren van duurzame keuzes. Ook had de sterkte van motivatie en de autonome vorm van motivatie weer een positief effect op duurzame keuzes. In Studie 3 wilden we deze resultaten nogmaals repliceren, maar dit keer met een soort gedrag dat daadwerkelijke en directe consequenties zou hebben voor de deelnemers. In deze studie gebruikten we een default nudge die vrijwillige deelname aan een langere versie van het onderzoek naar duurzame keuzes stimuleerde. Ook in deze studie vonden we grotendeels hetzelfde patroon aan resultaten. De default nudge was effectief en de sterkte van motivatie en de autonome vorm van motivatie om duurzame keuzes te maken voorspelden de vrijwillige deelname aan de langere versie van de vragenlijst. Daarnaast vonden we in alle drie de studies dat de default nudge van toegevoegde waarde was boven op de bestaande motivatie van de deelnemers. Samenvattend tonen we aan dat default nudges en motivatie onafhankelijk van elkaar gedrag beïnvloeden, en dat nudges dus iets toevoegen

aan de uitkomsten van beslissingen boven op de bestaande motivatie van het individu.

In hoofdstuk 5 hebben we ons gefocust op de vraag voor wie nudges aanvaardbaar zijn. In dit hoofdstuk maakten we gebruik van korte scenario's die drie verschillende nudges beschreven (default, portiegrootte en herindeling) die gezondere of duurzamere voedselkeuzes stimuleerden. Voor elk van deze scenario's vroegen we hoe acceptabel deelnemers de nudge vonden. Daarnaast hebben we veel vragenlijsten afgenomen over zelfregulatie capaciteit (bv. zelfcontrole en zelfeffectiviteit) en motivatie (autonome en gecontroleerde motivatie). Het onderzoek toonde aan dat autonome motivatie een consistente voorspeller is van aanvaardbaarheid van nudges, terwijl gecontroleerde motivatie (motivatie die voortkomt uit sociale verwachtingen, beloningen en/of straffen) geen effect had op aanvaardbaarheid. Bovendien hadden aspecten van zelfregulatie capaciteit nauwelijks invloed op oordelen over aanvaardbaarheid. Dit toont aan dat nudges die een bepaald gedrag stimuleren als aanvaardbaarder worden beschouwd door mensen die dit gedrag willen uitvoeren vanwege autonome redenen, terwijl zelfregulatie capaciteit geen rol van betekenis speelt bij beoordelingen over aanvaardbaarheid.

Conclusie

Het empirische werk dat onderdeel uitmaakte van dit proefschrift was gefocust op de vraag wanneer en voor wie nudges effectief en aanvaardbaar kunnen zijn, met speciale aandacht voor psychologische aspecten zoals inspanning en motivatie. Verschillende conclusies kunnen gedestilleerd worden uit het empirische werk. Allereerst tonen we aan dat de nabijheidsnudge effectief kan blijven als er meerdere alternatieve opties in de keuzeset aanwezig zijn. Eerder werk naar de effectiviteit van de nabijheidsnudge was gelimiteerd tot keuzesets bestaande uit één of twee opties. Ons onderzoek toont aan de deze nudge effectief kan blijven met negen alternatieven in de directe keuzeomgeving. Ten tweede laten we in dit proefschrift zien dat default nudges niet meer maar ook niet minder effectief zijn als het cognitief vermogen belast wordt. Dit suggereert inderdaad dat nudges andere gedragsinterventies in de gereedschapskist van een beleidsmaker kunnen aanvullen, terwijl het tegelijkertijd zorgen over mogelijke manipulatie of gebrek aan autonome beslisvorming kan verzachten. Ten derde laten we in drie verschillende maar gerelateerde studies zien dat default nudges en motivatie onafhankelijk van elkaar gedrag beïnvloeden waar mensen op zijn minst redelijk gemotiveerd voor zijn. Het patroon wat uit deze studies blijkt is dat de default nudge als een soort anker werkt, waarna mensen de uiteindelijke keuze kunnen bijstellen naar gelang de sterkte en het type motivatie. Tot slot laten we zien dat autonome motivatie de enige factor is uit een reeks van zelfregulatie gerelateerde variabelen die consistent de aanvaardbaarheid van nudges voorspelt. Met deze bevindingen draagt dit proefschrift bij aan een robuuste wetenschap naar nudging die theoretisch solide is en doeltreffend in de praktijk. Alles bij elkaar genomen draagt het proefschrift bij aan het inkleuren van de tot op heden zwart-witte evaluaties van effectiviteit en aanvaardbaarheid door te onderzoeken wanneer en voor wie nudges effectief en aanvaardbaar zijn.

Curriculum Vitae

Laurens van Gestel was born on the 12th of May 1992 in Breda, the Netherlands. In 2010 he graduated from secondary school (VWO, Mencia de Mendoza Lyceum in Breda) and started studying at University College Roosevelt (UCR), the international honors college of Utrecht University in Middelburg. In 2013 he graduated cum laude from UCR with a major in psychology and economics and a minor in methods and statistics. In 2015 he graduated from the research master Social and Health Psychology at Utrecht University. After working for a year as a junior researcher at Utrecht University, he started his PhD project in 2016 at the department of Social, Health, and Organizational Psychology at Utrecht University, under supervision of prof. dr. Denise de Ridder and dr. Marieke Adriaanse. The project was part of the multidisciplinary research program HINTS (Health improvement through nudging techniques) financed by a top-grant from ZonMw. During his PhD trajectory, Laurens was active as a PhD representative to the Kurt Lewin Instituut (KLI) and as editor-in-chief of In-Mind Nederland. After defending his dissertation, Laurens will start as an assistant professor in behavioral interventions in public health management at the department of Health, Medical, and Neuropsychology at Leiden University.

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