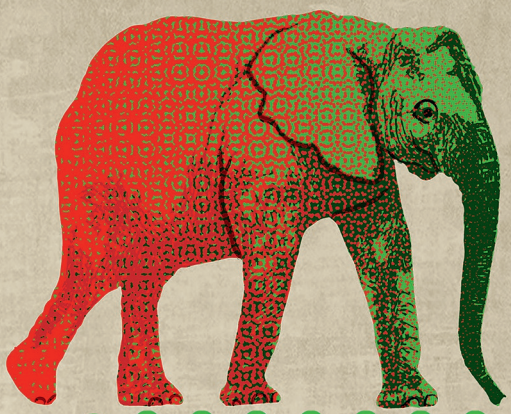
Nudging for impact

Beyond the immediate effectiveness of nudge interventions promoting healthy food intake



Merije van Rookhuijzen

Propositions

1. The presence and aim of a nudge should always be disclosed.
   (this thesis)

2. Nudge implementation is difficult.
   (this thesis)

3. The COVID-19 pandemic accelerated innovation in education and research methods.

4. All scientific journals should publish registered reports.

5. Remote education hampers social development in students.

6. Animals have a fundamental right to live.

Propositions belonging to the thesis, entitled

Nudging for impact: Beyond the immediate effectiveness of nudge interventions promoting healthy food intake

Merije van Rookhuijzen
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Nudging for impact:
Beyond the immediate effectiveness of nudge interventions promoting healthy food intake

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Nudging for impact: Beyond the immediate effectiveness of nudge interventions promoting healthy food intake

Merije van Rookhuijzen

Thesis

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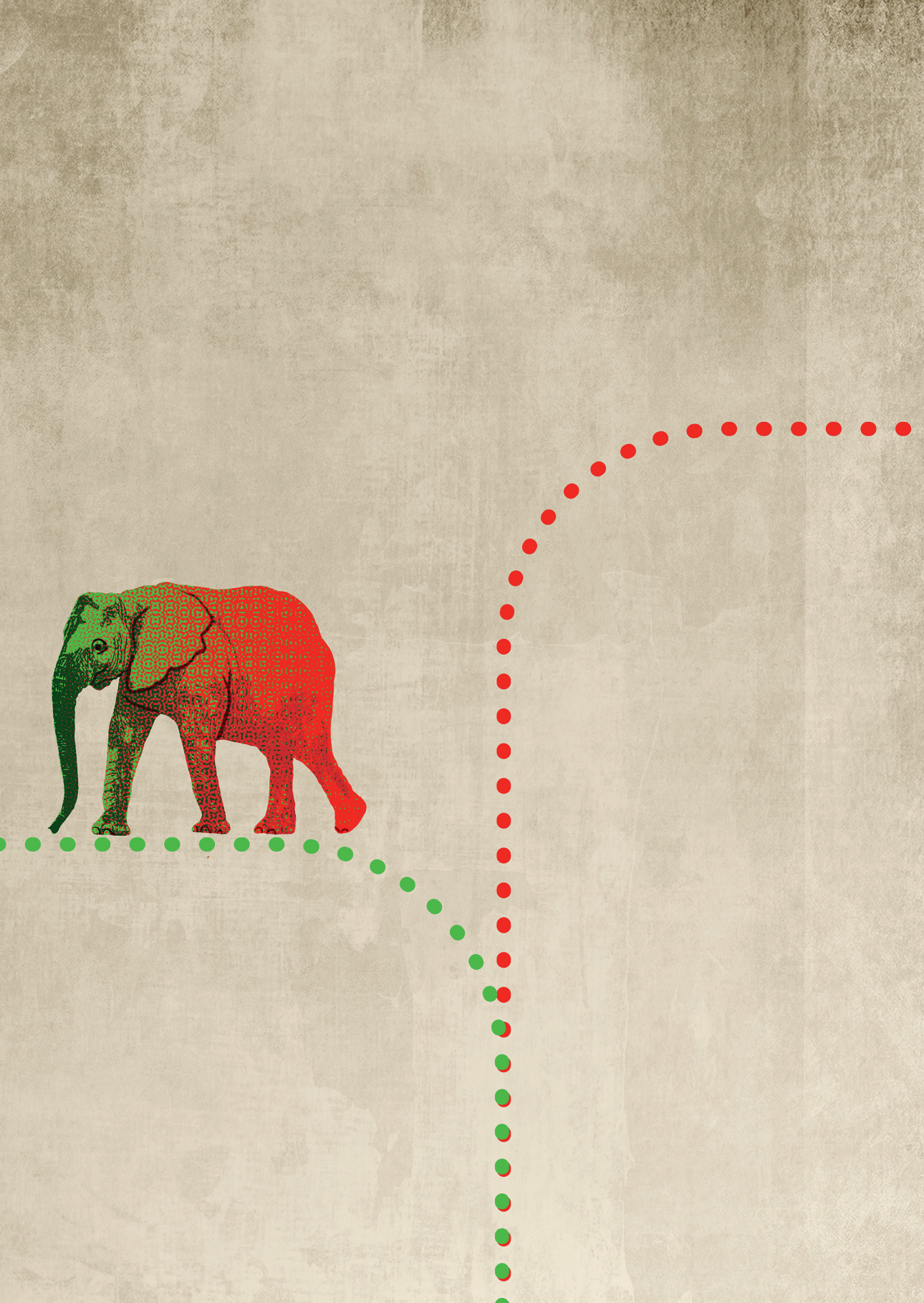
TABLE OF CONTENTS



CHAPTER 1

General introduction

Health problems, like obesity, cardiovascular diseases and type 2 diabetes resulting from unhealthy diets are an increasing worldwide problem (GBD 2017 Obesity Collaborators, 2019). For example, in 2016, 13% of the worldwide population of adults aged 18 and older, could be classified as obese (WHO, 2021). Although food intake is influenced by a complex interplay of factors at multiple levels (individual and social, physical, and macro-level environmental) (Leng et al., 2017; Schwartz et al., 2017), at least part of this problem can be attributed to the increasing accessibility and availability of, in particular, unhealthy foods high in saturated fats and sugar. To illustrate, Babey et al. (2008) demonstrated that the prevalence of obesity and diabetes is higher in areas with an abundance of fast-food restaurants and convenience stores compared to grocery stores and fresh produce vendors. Given the magnitude of problems caused by unhealthy diets, in the last couple of decades a lot of attention has been paid by scholars and policy makers to the question of how to limit the negative influence of the environment on food intake. Initially, it was thought that the solution could be found within the individual. It was assumed that by increasing knowledge on what a healthy diet entails or by strengthening self-control, people would be able to resist the temptations brought forward by the environment. However, gradually, more focus was placed on the environment and research aimed to investigate ways in which the environment could be altered to promote healthy eating. Serving as a catalyst for this shift was the 2008 book 'Nudge: Improving Decisions About Health, Wealth, and Happiness' by Richard Thaler and Cass Sunstein.

In this book, Thaler and Sunstein describe nudging as a behaviour change technique based on the notion that changes in the way options are presented influence our decisions. For example, in a study of Maas et al. (2012) participants were instructed to relax for 5 minutes while they could eat chocolate peanuts and read a magazine. The bowl of chocolate peanuts was either placed nearby (20 cm from the armrest), within reach (70 cm from the armrest), or distally (140 cm from the armrest). Results showed that when the bowl was placed nearby, participants ate more than when the bowl was placed further away. This example illustrates that something as seemingly trivial as distance to food can affect behaviour. In nudging, these insights are used to formulate strategies to alter the choice architecture in such a way to predictably change behaviour.

Up until now, much research has examined the effects of nudging on food choices, with meta-analyses on the topic (e.g. Cadario & Chandon, 2020) commonly concluding that there is strong evidence available that nudges can be used to affect behaviour. It is therefore no surprise that a Behavioural Insights Team (BIT) in the United Kingdom and related organisations across the globe have been established with the aim to inform and improve public health services by generating and applying behavioural insights (Behavioural Insights Team, n.d.).

However, it is still unclear whether and how the effects of nudges can persist (Congiu & Moscati, 2022). Such research is especially important in the food domain, because multiple food choices need

to be affected before any effects on eating patterns and ultimately overall health become visible. **Therefore, the aim of this dissertation is to investigate the extent to which nudges can have a lasting impact on eating behaviour.** More specifically, we will examine 1) the process and effects of implementing nudges over a longer period of time and 2) whether, and under which circumstances, nudges have the potential to extend their influence once they are removed (i.e. temporal spillover effect). After a short general introduction to nudging, this chapter will describe the theoretical background and discuss the existing empirical evidence related to the longevity of nudging. We will end with a description of the chapters in the current dissertation and how they contribute to answering the main research question.

nudge

/nʌdʒ/

verb

1. prod (someone) gently with one's elbow in order to attract attention.
   'people were nudging each other and pointing at me'

noun

1. a light touch or push.
   'he gave her shoulder a nudge'

NUDGING FOOD CHOICES

At the start of every year, people all over the world make New Year's resolutions related to weight loss or healthier eating practices. A survey conducted among 12410 women in six European countries revealed that about 50% had weight loss as a New Year's resolution during the last two years (Rössner et al., 2011). However, as we probably have experienced ourselves or seen in others, it is not easy to actually follow through on such resolutions. And indeed, perceived overweight is associated with weight loss attempts, but also with greater weight gain (Haynes et al., 2018).

This phenomenon, called the intention-behaviour gap (Sheeran & Webb, 2016), has interested scientists for many years. One of the reasons that we do not always put our intentions into action seems to stem from the automatic nature of many of our food decisions (Cohen & Farley, 2008; Moldovan & David, 2012). During the day, we make hundreds of (food) decisions. It would simply take too much time and energy to consciously think about all the possible options and implications before we decide what to do. Therefore, many of these decisions are made automatically. These automatic decisions are often based, and influenced by, seemingly trivial factors. For example, when the proportion of offered unhealthy foods becomes larger, the probability of choosing unhealthy foods also becomes larger (Van

Kleef et al., 2012a). Unfortunately, in the current environment in which unhealthy foods are available in abundance, such automaticity often leads to unhealthy food choices. Of course, with enough effort, this automaticity can be overcome and making food decisions becomes a conscious endeavour. This is what happens when we start to act on our New Year's resolutions. However, when other decisions require attention, it is easy to fall back into automatic ways of making food decisions. When the holiday period is over, and issues in our daily lives need attention, we may lapse from our resolutions.

The automatic nature of many of our food decisions shows that interventions solely based on providing information on what constitutes an (un)healthy diet will not always lead to healthier diets. An intervention may be able to increase food related knowledge, but this increased knowledge can only be used to guide conscious decisions. There is thus a need for interventions that target these automatic processes. A proposed solution that has gained a lot of attention is to circumvent having to use our cognitive system by 'tricking' our automatic system into choosing healthy foods. Simply put: let's make the automatic choice the healthy choice.

This brings us to the central topic of this dissertation: nudging. Nudging has been defined as:

> '*A nudge, as we will use the term, is any aspect of the choice architecture that alters people's behaviour in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid. Nudges are not mandates. Putting fruit at eye level counts as a nudge. Banning junk food does not*.' (Thaler & Sunstein, 2008, p. 6).

Nudging is thus a behavioural change technique that alters the environment in such a way to affect behaviour. In keeping with the dictionary definition of the word 'nudge', nudge interventions aim to gently prod a person in the desired behavioural direction. Importantly, nudges do not limit the choice set or forbid you to choose a particular option. The nudgee (the person being nudged) is free to choose the option that is not nudged. Moreover, nudged options do not provide economic benefits for the nudgee (Thaler & Sunstein, 2008).

The theory behind nudging is based on so-called dual process models (e.g. Kahneman, 2011; Strack & Deutsch, 2004; for an overview see Evans, 2008). The premise of these models is that information processing is facilitated by either one of two modes of thinking, often called system 1 and system 2. System 1 is fast and describes automatic thinking and behaviour (for example, processing the following sum: 2+2). System 2 is slow and effortful and used for deliberative thinking and behaviour (for example, processing the following sum: 258*45). Although pros and cons are carefully weighted before decisions are made by system 2, system 1 makes use of so called heuristics to make decisions. Heuristics can be seen as fast and frugal 'rules of thumb' for which effortful deliberation is not needed. An advantage

of using heuristics to make automatic decisions is that cognitive processes are still available to guide other behaviour. Nudging makes use of these heuristics by manipulating the environment in such a way, that the outcome of the heuristic is beneficial to the nudgee.[1]

Since many of our food choices are made via system 1, nudging would seem ideal for affecting food choices for the better. And indeed, since 2008, a plethora of research has been conducted on the use of nudges to influence eating behaviour. For example, Van Kleef et al. (2012b) conducted an experiment in which participants were served pasta in a large or small bowl. Although the amount of pasta was equal in both bowls, participants who were served pasta in the larger bowl consumed more than participants who were served pasta in the small bowl. In this case, external cues were used to estimate what a normal portion would be. This interest in nudging food choices has resulted in quite some meta-analyses and systematic reviews on the topic in recent years (Arno & Thomas, 2016; Allan et al., 2017; Bucher et al., 2016; Broers et al., 2017; Cadario & Chandon, 2020; Li et al., 2021; Mertens et al., 2022b; Vecchio & Cavallo, 2019). A common conclusion of these analyses is that nudges are indeed able to affect food choices, with effect sizes mostly ranging from small to medium. For example, Cadario and Chandon (2020) calculated an average effect size of Cohen's $d = 0.23$ for the included studies equivalating to -124 kcal/day. However, they also found that the magnitude of the effect sizes depended heavily on the type of nudge being used, with behaviourally oriented nudges (e.g. defaults) having higher effect sizes than cognitively (e.g. increasing visibility) or affectively oriented nudges (e.g. hedonic descriptions). Although other interventions may have a higher effect size, their cost-effect ratio is fairly high and their use recommended because of their low invasiveness and large potential reach (Benartzi et al., 2017).

LASTING EFFECTS OF NUDGING

Although the above discussion may give the impression that the use of nudges as a behavioural intervention tool to promote healthy eating is promising, this is not necessarily so. Most research on the effect of nudging on food choices concerns itself with effects on a single choice. For some behaviours, like organ donation, the desired end effect may be attained after only one single exposure to a nudge (Johnson & Goldstein, 2003). However, before eating patterns and ultimately health are affected, the effects of nudges on incidental behaviour is not sufficient. Being nudged once into taking an apple over a chocolate bar falls flat compared to the number of food choices made on a weekly basis. Instead,

[1] We do acknowledge that dual process models are not without criticism, which has led to the proposal of other, single system models (Lin et al., 2017). However, since the concept of nudging was based in large part on such dual process models, and to ease understanding and interpretation, we included the dual systems account in this General introduction.

nudges should have lasting effects on food choices before useful effects become visible. Since insights into the prolonged effects of nudges are so important, it may seem remarkable that relatively little research has been conducted to explore the effects of nudges after one single exposure. This is true for the lasting effects of nudges in general and for nudges aimed at affecting eating behaviour specifically. One possible explanation for this can be found in the methodological challenges for research with multiple measurements. Multiple measurements naturally require more effort, money and time relative to single measurements. Moreover, nudges are often placed in out-of-home settings, where many, though different, individuals are exposed to it. This makes it especially hard to measure the repeated effect of a nudge on a single individual.

When discussing the potential of nudges to have lasting effects, it is useful to differentiate two ways in which the effects of nudges can be prolonged. The first is the effect of nudges on behaviour multiple times in a row, here referred to as long-term effects. The second is the effect of nudges on behaviour after their removal, here referred to as spillover effects. These two forms of prolonged effects are discussed below.

Long-term effects of nudges

Whether an effect can be labelled as long-term is essentially a subjective question. In this dissertation, we define the long-term effect of a nudge as the effect of a nudge after multiple exposures. In this sense, long-term has a wide range, from several nudge exposures to hundreds of nudge exposures.

One concern regarding the long-term effects of nudging is that people become accustomed to, and gain experience with, the situation and the choice options (Croson & Treich, 2014; Lusk, 2014). The effects of nudges are probably most prominent when the choice set is novel to the nudgee (Löfgren et al., 2012). Getting acquainted to the choice setting and with the outcome of their decision may diminish the effect. For example, when presented with two novel dishes, a person may be nudged into trying one of these dishes. However, when the taste turns out disappointing, the person may, on following occasions, choose the other dish, even when the nudge is still in place. By contrast, nudges may be able to affect behaviour multiple times when other factors that influence the choice are limited, when the experienced outcome of the nudge was positive, or when the nudge acts as a reminder to choose the healthy, or otherwise preferred, option.

Although studies on the subject are limited, the empirical evidence does suggest that nudges used to promote healthy eating are able to extend, and in some cases even increase, their influence (Cadario & Chandon, 2020; Kurz, 2018; Thorndike et al., 2014; Vermote et al., 2020), For example, Kurz (2018) conducted a field experiment in which a vegetarian option in a restaurant was nudged by changing the menu order and by enhancing the visibility of this option. Results showed an increase of the effect of the nudge during the three-month period in which the nudge was implemented relative

to a control restaurant. However, an important sidenote to these studies is that the individuals who are exposed to the nudge are often not identical throughout the entire study period. This means that there are constantly participants who are exposed to the nudge for the first time. It is therefore difficult to draw conclusions on the ability of nudges to affect food choices of the same individual more than once and to study the underlying mechanisms of this process.

Lab versus field

In addition to the question whether nudges can affect food choices multiple times in a row in theory, it is also important to consider the ease by which nudges can be implemented in practice for a longer period. The impact an intervention has in real-life depends on many factors (Glasgow et al., 1999). Here, it is useful to distinguish between *efficacy* and *effectiveness*. *Efficacy* research assesses whether an intervention 'does more good than harm when delivered under optimum conditions'; *effectiveness* research assesses whether an intervention 'does more good than harm when delivered under real-world circumstances' (Flay, 1986). Results that are found with efficacy research are not necessarily the same when interventions are evaluated in real-life contexts. Implementation or adherence issues could for example result in low effectiveness. Moreover, other factors that influence eating behaviour, when not held constant, could exert their influence on food choices beyond that of the intervention. Importantly, the difference between efficacy and effectiveness research is not a strong dichotomy; it should more accurately be regarded as a continuum with on the one side research in which all factors of the intervention and the environment are kept constant, and on the other side research in which the intervention is implemented in real-life with no interference by the researcher whatsoever. Furthermore, both types of research are needed to assess whether an intervention is effective and is suited to implement in real life.

Although many studies have investigated nudge interventions, most of them are lab studies or controlled field studies where at least the presence of the nudge is held constant. These studies are important to assess the effects of nudges under ideal circumstances. However, because nudges usually consist of only small alterations to the environment, the active ingredient of nudges can easily be removed. This is a concern especially when the long-term effects of nudges are being assessed. For example, products in the supermarket which are placed on eye-level to increase their salience may be placed elsewhere, and menus with healthier meals placed on top may be altered with the hiring of a new chef. Research into the feasibility of nudging to impact eating behaviour should therefore not only examine the efficacy, but also the effectiveness of nudges (Ammerman et al., 2014, Glasgow et al., 2003).

Spillover effects of nudges

Although effectiveness research should investigate the effect and feasibility of implementing nudges over a longer period, nudges may not need to be implemented permanently to have prolonged effects. Years of research have established that past choices continue to influence our current choices (for an overview, see Dolan and Galizzi (2015). For example, in what has been named the *foot-in-the-door technique*, one is more likely to agree to a large request when one previously agreed to a small request (Freedman & Fraser, 1966). While long-term effects describe the effect of nudges after multiple exposures, spillover effects describe the effect of nudges after their removal. In this dissertation, a spillover effect is defined as the effect of a nudge on behaviour upon removal, after it has affected behaviour one or multiple times, while still being implemented. While long-term effects relate to the effect of nudges on the same behaviour after multiple exposures in the same contexts, spillover effects can be examined for the same behaviour in the same context, also known as *temporal spillover* (e.g. when nudging an apple in supermarket A leads to choosing apples in supermarket A after the nudge is removed), for the same behaviour in a different context, also known as *contextual spillover* (e.g. when nudging an apple in supermarket A leads to choosing apples in supermarket B after the nudge is removed) or for different, but related behaviour in the same context, also known as *behavioural spillover* (e.g. when nudging an apple in supermarket A leads to choosing fruit in supermarket A after the nudge is removed) (Nilsson et al., 2017). Determining whether nudges are able to affect choices after their removal is a crucial first step before assessing the extent to which this is dependent of behaviour and context (Schwartz et al., 2017; Weijers et al., 2021). Such research is highly relevant for assessing the lasting effects of nudges.

Spillover through altered attitudes

One process that could result in spillover effects of nudges is through altered attitudes. The central premise in both *self-perception theory* (Bem, 1972) and *self-herding* (Ariely & Norton, 2008) is that attitudes cannot only influence behaviour, but that behaviour can also influence attitudes. When a person is unaware what caused their behaviour, internal attribution of the behaviour takes place. Since nudges mostly exert their influence outside awareness (Dhingra et al., 2012; Van Gestel et al., 2018), a person will take their behaviour as an indication of having a positive attitude towards the behaviour that was just performed, especially when the person feels positive about the behavioural outcome. This positive attitude can, in turn, influence behaviour even after the nudge has been removed and can no longer influence behaviour directly.

Spillover through identity change

Performing behaviour without a known cause may not only alter attitude, but also identity. This notion, central in the work of Gneezy et al. (2012) and Bénabou and Tirole (2011), implies that when a nudge influences behaviour outside awareness, a person could consider that behaviour as part of their identity and subsequently perform the behaviour even when the nudge is no longer present. For example, Burger and Caldwell (2003) found that participants were more likely to volunteer sorting and boxing canned goods if they had been earlier asked to sign a homelessness petition. More importantly, they found that this effect was mediated by a change in identity (by asking participants to which extent they thought of themselves as persons who engage in various altruistic behaviours).

Although the discussed processes of attitudinal and identity change both could lead to sustained effects of nudges after their removal, they do differ in the generalizability of this effect. If choosing a healthy option leads to a positive attitude towards that option, is can be expected that that particular option is chosen more often, even in different contexts, but that this does not lead to choosing other healthy options (i.e. only temporal and contextual spillover effects). If, however, choosing a healthy option leads to the attribution of being a healthy person, then it could be expected that this behaviour would generalize to other healthy options. Nudging one behaviour could then alter different non-nudged behaviour in the same domain (i.e. temporal, behavioural and contextual spillover effects).

Empirical evidence for spillover effects

In recent years, only a few studies examined whether the effects of nudges can be extended after their removal and even fewer specifically examined these effects on eating behaviour. Therefore, the discussed empirical evidence on the spillover effects of nudges encompasses studies investigating the effect of nudges on a variety of behaviours, instead of focussing solely on eating behaviour. Caution should be exercised to generalise the findings of these studies to eating behaviour. Results of the studies examining the spillover effect are mixed, with some studies finding spillover effects (Burger & Shelton; 2011; Kurz, 2018; Venema et al., 2018), while others do not find such spillover effects (d'Adda et al., 2017; Donkers et al., 2020, experiments 1-3; Ghesla et al., 2019; Kuhn et al., 2021; Ozturk et al., 2020; Stroebele et al., 2009; Zimmermann & Renaud, 2021), or even compensating effects (Donkers et al., 2020, experiment 4). For example, Burger and Shelton (2011) nudged participants into taking the stairs by placing a sign mentioning that most people use the stairs instead of the elevator. This social norm nudge increased stair use and, more importantly, this effect continued during the week after the intervention. To understand and explain these mixed results, three aspects of these studies deserve consideration. First, when a nudge does not affect behaviour immediately following its implementation, then it is not likely that a nudge exerts any effect on choices after its removal. In a few of the abovementioned studies, however, the nudge failed to immediately affect behaviour. Second, in

many of these studies data is analysed on group-level. With such data it is unclear whether the same participants were exposed to the nudge when the nudge was implemented and removed. It is apparent that spillover effects cannot take place if an individual has not been exposed to the nudge in the first place. Third, the studies that did not find spillover effects mostly examined different types of choices (behavioural spillover), from which no conclusions about temporal spillover effects can be drawn.

THE ETHICS OF NUDGING

Although the concept and use of nudging was met with enthusiasm by both scholars and policy makers, it also attracted criticism (for a complete overview see Schmidt and Engelen (2020)). A question that is often raised is who is best to decide which behaviour should be nudged. One characteristic of nudges is that they should serve to influence a collectively or socially shared goal (Thaler & Sunstein, 2008). However, even if the choice that is promoted through the nudge is in line with this shared goal, this does not mean that the option would also have been chosen by those that are being nudged when they could have deliberated on the decision (Hausman & Welch, 2010; Qizilbash, 2011). Moreover, a single nudge can potentially reach a lot of people, but not all of them will share the goal put forward by the nudge. It could even be the case that the nudged behaviour is beneficial for some, but detrimental to others (e.g. nudging low-caloric products to underweight people who try to gain weight). Fortunately, there are now indications that nudges cannot affect behaviour when those that are being nudged have strong preferences for the behaviour that is not endorsed by the nudge (De Ridder et al., 2022).

A second, commonly voiced, concern is that nudging is manipulative and autonomy impairing (Baldwin, 2014). As mentioned, nudges often exert their influence without the nudgee being aware of it (Dhingra et al., 2012; Van Gestel et al., 2018). It thus seems contradictory that freedom of choice is an essential part of the definition of 'nudge' put forward by Thaler and Sunstein (2008). How can a person be free to decide whether or not to behave in line with the nudge, while they are not even aware that they are being nudged? This has led to an increased scholarly interest in nudge effectiveness when nudges are made transparent, which we define as informing the nudgee about the presence and purpose of the nudge.[2] Such nudge transparency would arguably be more ethical, mitigating concerns about manipulation by giving back the freedom to choose to the one being nudged (Baldwin, 2014).

Initially, it was thought that nudge transparency would be detrimental to their effectiveness (Hansen & Jespersen, 2013; Smith et al., 2013). Or, in the often cited words of Bovens (2009), nudges would work

[2] Although our definition of nudge transparency is similar to the dominant view of nudge transparency in the literature (Michaelsen et al., 2020), we do like to point out that, for some, nudge transparency refers to those instances in which the intention and means through which behaviour change is pursued can reasonably be known to the one being nudged (Hansen & Jespersen, 2013).

best, 'in the dark'. It was even thought that resistance or reactance would result in counter effects (Brehm, 1989). Fortunately, empirical evidence is now accumulating that shows that transparency does not, in fact, hinder nudge effectiveness (Bang et al., 2020; Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Steffel et al., 2016). Transparent nudges have even been found to be more effective than non-transparent nudges (e.g. Paunov et al., 2019a; Paunov et al., 2019b). An explanation for these findings is that the transparency message may be seen as an explicit recommendation (also see research into request justification, Langer et al. (1987)). Furthermore, Paunov et al. (2019b) suggest that the increased effectiveness may be the result of transparent nudges making people feel less deceived than the use of non-transparent nudges.

Although nudge transparency may thus not be detrimental to nudge effectiveness, this does not necessarily imply that a possible temporal spillover effect is not negatively affected by transparency. Internal attributions (e.g. attributing behaviour to one's positive attitude) may cause temporal spillover effects not to follow after transparent nudges, since the behaviour can then be attributed to a clear external cause. It thus remains unclear whether the effect of transparent nudges can spill over to later decisions.

SELF-NUDGING

Although nudge interventions can potentially affect a large number of people, their reach stops where policy makers do not have the authority to intervene: in people's homes. This is especially important when considering food choices, since most of food consumption takes place at home (e.g. 80% of food consumption in the Netherlands takes place at homes (Van Rossum et al., 2020)). For nudges to have lasting effects on food choices with the aim to change overall food patterns the question therefore arises whether nudging can cross this threshold and be introduced into people's homes. One of the proposed ways to accomplish this is by learning people to apply nudging themselves.

The concept of self-nudging can be described as structuring one's own environment in a way aligned with one's goals (Reijula & Hertwig, 2022). With self-nudging, the focus is not solely placed on equipping individuals with the capacity to resist temptations or on altering the environment to reduce temptations, but rather on learning individuals to take control over their environment and to shape it in line with their personal goals. Contrary to traditional nudging, self-nudging can help to improve food choices within homes. Moreover, since the nudgee becomes the nudger, he or she decides the ultimate goal. This decreases concerns about who should serve to decide what behaviour should be collectively nudged, since the nudgee him- or herself decides what behaviour he or she want to promote through self-nudging. Moreover, the nudgee is fully aware of the presence and aim of the nudges, which takes away ethical concerns about nudges being manipulative (Baldwin, 2014).

Habit formation

As earlier discussed, nudges' subtle way of working makes them susceptible to other changes in the environment that could affect their workings. The advantage of self-nudging is that it can take place in people's homes: an environment where the nudger is fully in control and not dependent on others to maintain the nudge. This makes self-nudging especially suited to assess the lasting effects of nudges. Moreover, it provides the opportunity to explore whether nudging can be used to install healthy eating habits.

A habit can be defined as 'a process by which a stimulus automatically generates an impulse towards action, based on learned stimulus-response associations' (Gardner, 2015). Habitual behaviour is behaviour that is automatically elicited by a contextual cue. Most of us are familiar with watching television with a full bowl of crisps, when suddenly you realise that the bowl is empty. The bowl automatically and subconsciously generates an impulse towards taking crisps. A characteristic of habits is that they are hard to break, since they occur automatically (Verplanken & Wood, 2006). This means that it is hard to quit unhealthy habits, but also, on a more positive note, that already installed healthy habits can have lasting effects.

Habits are formed through behavioural repetition in a specific context (Lally et al., 2010). This leads to an association of contextual cues with the behaviour (Wood & Neal, 2009). The contextual cues then acquire the potential to elicit the behaviour without conscious awareness (Bargh, 1994). When nudges repeatedly affect behaviour in a specific context, particular cues in the context may become associated with the behaviour, as a result of which the nudge intervention is no longer needed to elicit the behaviour, or, in other words, leads to a spillover effect. For example, nudging oneself to eat smaller dinner portions by using smaller plates may ultimately become associated with the concept of dinner itself or a specific time at which dinner is typically had. After a while then, a smaller plate may no longer be necessary to consume smaller dinner portions. However, it remains an empirical question whether nudges are able to install healthy eating habits (Hertwig & Grüne-Yanoff, 2017), since it is unclear whether other contextual elements will become associated with the behaviour that is automatically elicited by the nudge, or whether the nudge will have to stay in place to induce the behaviour.

AIM AND OUTLINE OF THIS DISSERTATION

The current dissertation explores the extent to which nudges can have a lasting impact on eating behaviour by examining 1) the process and effects of implementing nudges over a longer period of time and 2) whether, and under which circumstances, nudges have the potential to extend their influence once they are removed (i.e. temporal spillover effect).

In **Chapter 2**, we aimed to examine the effect and feasibility of long-term implementation of nudges in a real-life context. Chapter 2 therefore reports on an effect and process evaluation of a case

study in which healthier food choices were promoted using various nudges in two football canteens. In one of these canteens, we focussed on examining the potential of nudges to affect behaviour over a longer period of time. After a baseline period of 3 weeks, a nudge intervention consisting of salience, scarcity, availability and default nudges was implemented and its effect monitored for 26 weeks. Regular visits to the football canteen had to ensure that products and nudges were in place during the intervention period. In the other canteen, we focussed on examining the process of implementation and maintenance of the nudge intervention. After a baseline period of 15 weeks, nudges were implemented and their effect monitored for 16 weeks. However, in this canteen, regular visits were only planned to observe product and nudge maintenance, without interference. In both canteens, we gained access to sales records and measured various process evaluation indicators.

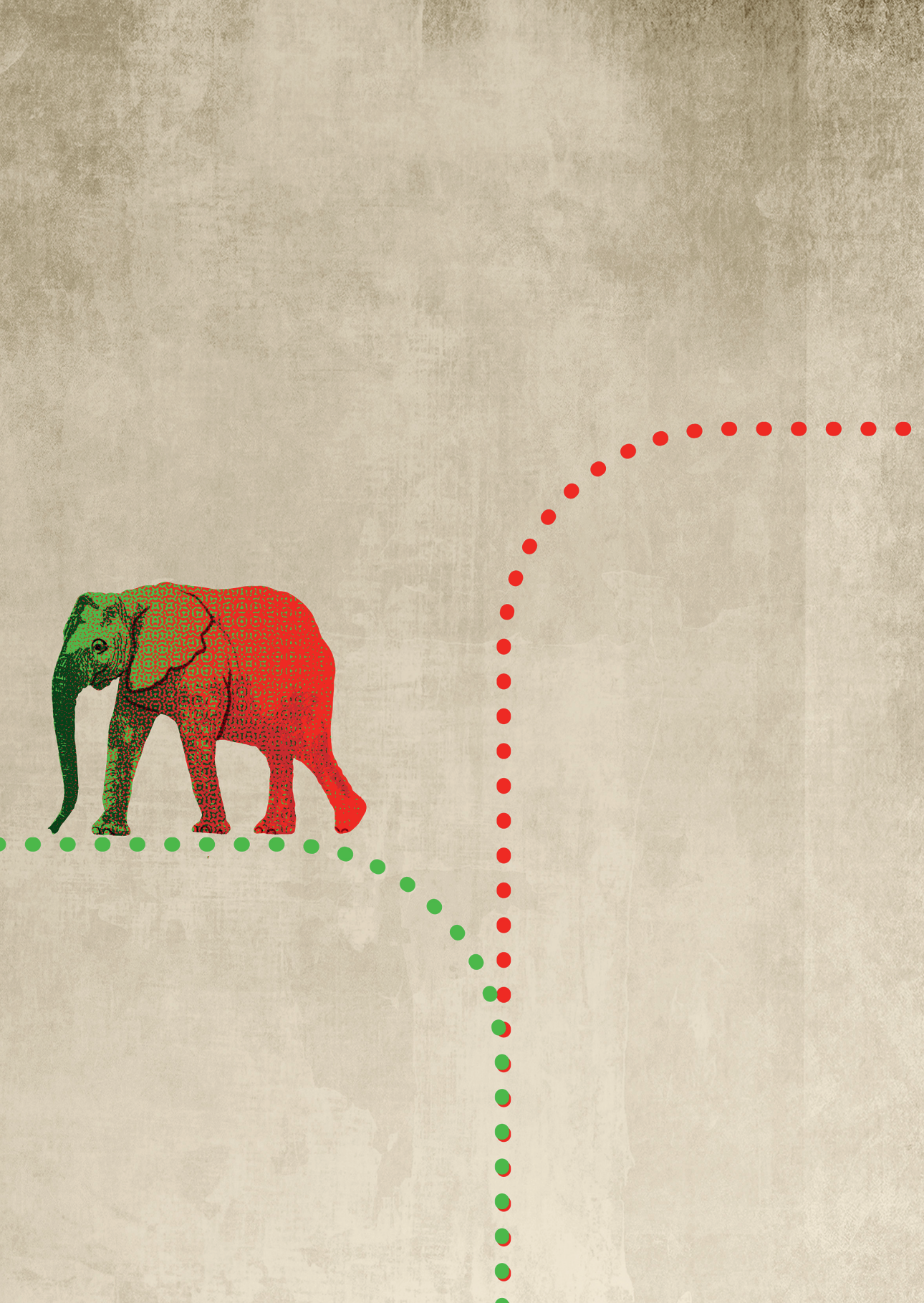
The aim of **Chapter 3** was to investigate the effect of a nudge on subsequent behaviour without the nudge, i.e. the temporal spillover effect. To do so, we analysed the initial effects of a default nudge and subsequent spillover effect on the next day when the nudge was removed in three studies. The first two of these studies acted as proof-of-principle studies, using a nudge that was earlier found to be able to influence a, for most people, new pro-social behaviour. Because these studies indicated that the influence of the nudge extended to the period after its removal, a similar set-up was used in the third study using hypothetical food choices as dependent variable. Moreover, we examined whether such temporal spillover effects are caused by a change in attitude towards the behaviour and/or identity.

Building on the observed temporal spillover effect and (partially) mediating role of attitude in Chapter 3, in **Chapter 4**, we aimed to examine the role of transparency in the temporal spillover effect. With spillover effects, it can reasonably be expected that internal attribution is disturbed by a transparency manipulation since it is explained that one's behaviour is being influenced by an external source. However, it could equally be the case that even with such transparency, people do not see themselves as being influenced by the nudge, which could still ensure internal attribution. In two studies, we therefore again investigated the effect of a successful nudge on prosocial behaviour on the day after its removal. However, an extra condition was now added in which the presence and aim of the nudge were explained to participants (i.e. the nudge was made transparent). We examined whether the number of participants who acted prosocially differed between the control, nudge and transparent nudge conditions.

In **Chapter 5**, we aimed to examine the prolonged effect of nudges promoting fruit intake and to investigate whether these effects can spill over to later choices without a nudge through habit formation. Unlike our previous studies in which participants were passively exposed to a nudge, in Chapter 5 we asked participants to nudge themselves. One of the advantages of self-nudging is that they are inherently transparent. Participants were randomly assigned to an experimental or control condition. In the experimental condition, participants had to choose a self-nudge promoting fruit

intake and implement it for eight weeks. They were subsequently asked to remove the self-nudge for one week. Fruit intake and habit formation were assessed and compared in the experimental and control conditions to assess whether any temporal spillover effects could be observed and whether these could be attributed to habit strength.

In **Chapter 6**, the General discussion, we discuss the results of the studies from Chapters 2-5 in light of the question whether nudges promoting healthier eating can impact public health. We provide a theoretical and methodological reflection, discuss their implications for research and practice, and end with concluding remarks.



CHAPTER 2

Nudging healthy eating in Dutch sports canteens: A multi-method case study

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ABSTRACT

Objective: To provide a micro-investigation into the long-term effects and process of implementation of a nudge intervention on food choice in sports canteens.

Design: Multi-method case study.

Setting: Eight products were added to the range of foods and drinks in two football canteens in the Netherlands for 3 and 15 weeks, serving as a baseline period. In the intervention period, these products were promoted with the use of salience, scarcity, availability and default nudges, for 26 and 16 weeks, respectively. Aside from the collection of sales and revenue data, reach, acceptability, adherence and applicability were measured using observations, questionnaires and interviews.

Participants: Questionnaires were filled in by 70 and 59 visitors of the canteens. Four interviews were held with board members and canteen personnel.

Results: Mixed results were obtained regarding the suitability of nudges to be used to promote healthy eating in sports clubs. Sales and revenue data did show positive trends, the intervention was seen as acceptable by all stakeholders and the intervention had a large reach. However, adherence to the intervention in both canteens and the effects of the nudges on the total consumption pattern were low. Factors were identified that promoted or hindered the intervention at an individual, interventional and organisational level.

Conclusion: Nudges seem to be a valuable addition to other efforts to combat unhealthy eating. However, the extent of their impact as a single intervention tool is limited in the current food-abundant environment.

INTRODUCTION

Following the increasing awareness that dietary habits are strongly linked to many health outcomes, much has been invested in the development of interventions aimed at improving our diets. Initially, interventions mainly focussed on encouraging people to reflect on their eating behaviour. And, although interventions promoting health behaviours have been found able to induce changes in intention, this has not been accompanied by equally large changes in behaviour. To illustrate, a meta-analysis of Webb and Sheeran (2006) found that a medium-to-large change in intention ($d = 0.66$) was followed by a small-to-medium change in actual behaviour ($d = 0.36$) according to categorisations by Cohen (1992).

One of the reasons put forward for the observation that people are unable to translate dietary knowledge into practice, even with good intentions, is that eating behaviour is largely under the control of automatic (rather than reflective) processes (Bargh, 2002; Cohen & Babey, 2012; Kahneman, 2011; Strack & Deutsch, 2004). This ensures fast and rather effortless decision-making for the large number of daily food choices we make. However, due to its reliance upon heuristics (rules of thumb), the system is prone to error (Kahneman & Tversky, 1973), which may lead to decisions opposite to those of our intentions.

'If you cannot beat them join them'

Considering the above, the answer to the question how healthy diets may best be promoted may seem obvious: by using the automatic nature of our dietary decisions to our advantage. Nudging is a strategy that exploits the manner in which automatic processes guide our behaviour to predictably affect it by changing the architecture in which we make decisions, without limiting the set of options (Thaler & Sunstein, 2008; Tversky & Kahneman, 1974). For example, placing unhealthy items out of direct reach reduces the number of participants consuming those items with 53% (Maas et al., 2012). Many scholars, therefore, consider nudges a potentially valuable addition or even alternative to current interventions (Marteau et al., 2012), which is reflected in the plethora of meta-analyses and systematic reviews on the effect of nudging on eating behaviour (Arno & Thomas, 2016; Roy et al., 2015; Szaszi et al., 2018; Veccio & Cavallo, 2019).

Most of these studies conclude that nudges are able to influence eating behaviour, although, according to the classification made by Cohen (1992), effect sizes tend to range from small to medium. For example, in their meta-analysis of nudges used in ninety-six field experiments, Cadario and Chandon (2020) report an average small effect size of $d = 0.23$ (translating into a 518.816 kJ change in energy intake). These findings tend to be stable across highly controlled laboratory studies and large field studies with multiple sites, which suggest a promising future for nudges to be included in interventions aimed at promoting healthy eating in various contexts.

From theory to practice

Although there seems to be enough evidence indicating the usefulness of nudging as a strategy to influence dietary behaviour, aspects that cause success in efficacy research are markedly different than those in effectiveness research (Glasgow et al., 2003). In other words, behaviour change under highly controlled circumstances does not equal substantial behaviour change under real-world circumstances. This means that although nudges may often be thought of as effective, and easy and cheap to implement and maintain, one could argue that their subtlety makes their effectiveness especially vulnerable to even the slightest of changes. For example, nudging products by changing their placement only needs a single-hand movement to be undone. A detailed look into the process of implementation and its effects on the ability of nudges to change behaviour will provide information about the barriers and challenges of the use of nudges in applied settings. Only then can the effects of nudge interventions be optimised.

Current study

In the current study, we aimed to assess the feasibility of using a nudge intervention in a real-life setting by studying outcome and process with a multi-method in-depth analysis over a longer period. More specifically, the process was studied by addressing the reach (the number of people potentially exposed to the intervention), acceptability (the extent to which the intervention is deemed acceptable by stakeholders), adherence (the extent to which the intervention was carried out as planned) and applicability (whether the population, intervention and context are suitable for the intervention) of the intervention.

We chose two football canteens to serve as cases. One reason for this choice was the often unhealthy nature of food served in these canteens, including only few healthy products (in 2011, 72% of the assortment in sport canteens was deemed unhealthy (Van Kalmthout, 2012)). Football (soccer) is one of the biggest sports of the Netherlands with approximately 3,000 clubs with 1.2 million members of all ages and backgrounds. The canteens that are part of most clubs provide an important source of income. Football clubs, therefore, provide an important setting for the promotion of healthy eating, potentially reaching many people. Moreover, sport facilities can be considered a more manageable and easier to regulate context than locations such as grocery stores for the implementation of nudges, since the choice architect himself/herself is often present on the floor.

METHODS

Canteens

Purposive sampling was used to recruit two canteens, denoted as canteens A and B, from moderately sized football clubs (with 800 and 1000 members, respectively) in the middle of the Netherlands. The clubs were required to play in different leagues to avoid that teams would visit each other's canteen. The opening hours of the canteens depended on the schedules of the trainings and matches, but, in general, they were open during weekends and some evenings during the week. During opening hours, all visitors were able to purchase products by asking for the desired product at the counter. Similar products were sold in both canteens, among which were deep fried and cold snacks, a variety of soft and alcoholic drinks, sandwiches and candy. Products were sold by both regular and episodic volunteers (mostly family members of football players).

Participants

Questionnaires were distributed to visitors in both canteens at two occasions during the study. The first questionnaire assessed reach and (parts of) applicability and was presented on a random day during the intervention phase. It was (partially) completed by seventy visitors from both canteens (70% male, $M_{age} = 36.91$ years, $SD = 20.14$). The second questionnaire assessed acceptability and (parts of) applicability and was presented during the last day of the study. It was (partially) completed by fifty-nine visitors from both canteens (74% male, $M_{age} = 22.64$ years, $SD = 18.77$). All visitors to both canteens, among which were players and spectators, could fill in the questionnaires. They had to actively take the questionnaires that were placed on tables and the counter by the first researcher. At the top of the questionnaires, participants were informed that by filling in the questionnaire, they consented to the use of the anonymous data for scientific purposes. Four semi-structured interviews were held by the first researcher at the end of the study with one board member and one member of the canteen personnel of each canteen (three males and one female) to inquire about their perceived adherence, acceptability and applicability. Both the board members and canteen personnel were chosen because they had the biggest influence on the assortment and placing of food products in the canteen. Written informed consent of the interviewees was obtained for each audio-taped interview.

Observations

Observations regarding the reach of the intervention were made on a random day during the intervention phase. Both canteens were each visited eight times on random moments to measure adherence.

Design and procedure

The design of the study can be found in Figure 2.1. A multiple case study was conducted, in which a nudge intervention was implemented to promote healthy eating in two football canteens. The study consisted of two phases. In the baseline phase, the assortment of the canteens was expanded with eight healthier products that were unobtrusively placed in the canteens (e.g. at the lowest shelf of the refrigerator). Prices for these products were determined by canteen personnel to match the price levels of other products sold in the canteen. In the nudge intervention phase, selection of these novel products was stimulated through the use of various nudges.

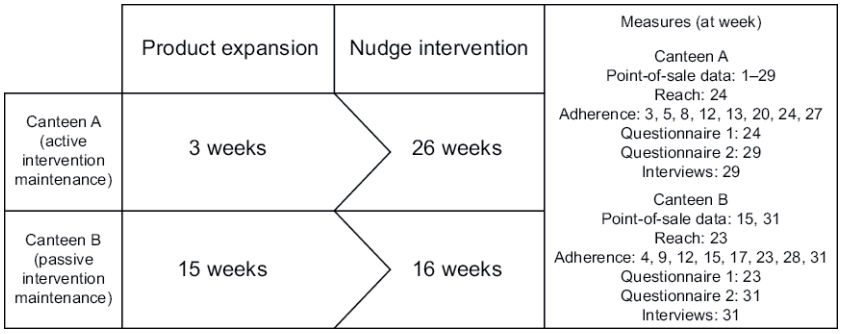

Figure 2.1: Design of the study and moments of measurement.

The study lasted an entire football season. The baseline phase lasted 3 weeks in canteen A and 15 weeks in canteen B. This phase was followed by a 26-week and 16-week nudge intervention in canteens A and B, respectively. The difference in length of the baseline and intervention period between both canteens was chosen because this enabled a long-term effect measurement in canteen A, while it enabled a longer baseline period in canteen B. During observations, intervention deviations from protocol were corrected by the first researcher in canteen A, but not in canteen B. This approach enabled a more controlled examination of the sales records of canteen A when the nudge intervention was implemented as intended and a more process-oriented examination of the intervention in canteen B under real-life circumstances.

Intervention

Food and drinks

The added products included bananas (as an alternative for highly processed snacks), flavoured water (as an alternative to sugared drinks), a zero-sugar sports drink (as an alternative to regular sugary sports drinks), a yogurt snack without added sugar and extra protein (as an alternative to sugary

yogurt snacks), a cereal bar low in energy content (as an alternative to chocolate bars), popcorn (as an alternative to crisps), a grilled high-fibre bread sandwich (as an alternative to regular grilled white-bread sandwiches) and a snack made of baked peas and maize (as an alternative to cocktail nuts). If sponsoring contracts did not allow for certain brands to be sold in the canteen, an equivalent of another brand was chosen. The products were selected by a sports dietician to be optimally suited for consumption before and after training or as healthier alternatives to existing snacks. The products contained less sugar, saturated fat and energy content and more protein and fibre relative to available alternative products in the same food category. All products were not yet available in both canteens before the study, except for the bananas, which were already sold in canteen B.

Nudges

Suitable nudges to promote the added products were selected based on (i) an examination of systematic reviews, meta-analyses and individual studies on the effect of heuristics and nudges on eating behaviour (Allan et al., 2017; Arno & Thomas, 2016; Bucher et al., 2016; Cruwys et al., 2015; Engbers et al., 2005; Escaron et al., 2013; Gittelsohn et al., 2012; Glanz & Yaroch, 2004; Harnack & French, 2008; Hollands et al., 2013; Libotte et al., 2014; Roy et al., 2015; Skov et al., 2013; Wilson et al., 2016) and (ii) contextual considerations about the feasibility of using certain types of nudges in the football canteens. This resulted in the use of salience, scarcity, availability and default nudges. Definitions of these nudges and their use during the intervention phase can be found in Table 2.1.

Measures

Sales figures and revenue data of every product in the canteen were based on point-of-sale data. In canteen A, sales figures were available per week. In canteen B, sales figures were aggregated to indicate sales data per phase because sales of some products needed to be recorded by personnel or volunteers themselves, which turned out not to be feasible on a weekly basis. For comparison reasons, revenue data of the same period as the intervention phase in the preceding year of both canteens were obtained.

Reach was measured by counting the number of people visiting the canteen on a single day, collecting sales figures and data on the number of transactions made that day and through questionnaires that were filled in on the same day. Visitors of the canteen were counted using a mechanical tally counter. Two people, among which the first researcher, would unobtrusively sit at the back of the canteen, counting the number of visitors entering the canteen during the entire day. If one of the observers noticed that a visitor had already entered before during that day, the individual was not counted a second time. In the questionnaires, aside from gender and age, visitors were asked how often they visited the canteen, how often they had bought products during the last month and whether and what they had bought that day.

Table 2.1: Nudges, definitions and their use during the intervention

Nudge	Definition	Use during intervention
Salience	Products that are prominently placed tend to draw our attention more than products that are less visible.	Many of the products used in the current study were placed at eye level (flavoured water, zero-sugar sports drink, yogurt snack in canteens A and B, popcorn in canteen A, baked peas and maize snack in canteen A*), placed more in sight or reach (bananas in canteen A, cereal bar, popcorn in canteen B) or the ratio of the product with similar products was increased (bananas in canteen B). Moreover, the zero-sugar sports drink was placed in the refrigerator in more than one row in both canteens, which was also the case for the yogurt snack in canteen A.
Scarcity	Products whose availability is limited are valued more than products whose availability is in abundance.	A picture of the grilled sandwich that was placed on the counter contained the message that it was available while supplies last.
Availability	The tendency to rely on immediate examples that come to mind when making a decision is called the availability heuristic.	A picture of the grilled sandwich was placed on the counter.
Default	The default option is the option that will be obtained when one does not actively intervene. Since intervening requires effort, we will often choose the option that is the default.	Personnel of canteen A was required to present visitors asking for the sports drink with the zero-sugar version. Visitors were only allowed to be presented with the regular version when specifically asked. Canteen B was not willing to implement the default nudge. Therefore, the default nudge was replaced with a prompted choice nudge: When visitors asked for a sports drink, personnel had to ask whether the person wanted the regular or zero-sugar version.

Note. No nudges were planned for the baked peas and maize snack in canteen B, since it was already taken out of product range before the nudge plan was drafted.

Acceptability was measured through questionnaires targeted at visitors and through semi-structured interviews with board members and canteen personnel. In the questionnaires, visitors were first asked age and gender and given a general explanation of nudges. Subsequently, they were asked to rate on five-point Likert scales ranging from 1 (totally unacceptable/undesirable/unagreeable/wrong) to 5 (totally acceptable/desirable/agreeable/right) how acceptable, desirable, agreeable and right they thought it was to (i) use nudges by the football canteen to let people make healthier choices (Cronbach's $\alpha = 0.93$), (ii) use nudges to influence *their* behaviour (Cronbach's $\alpha = 0.92$) and (iii) use nudges to influence *others'* behaviour (Cronbach's $\alpha = 0.97$). Moreover, they were asked whether they thought that nudges stimulating healthy eating were in place in the football canteen on a five-point

Likert scale ranging from 1 (totally disagree) to 5 (totally agree). Interviewees were asked: How acceptable is the use of nudges to promote healthy eating in the canteen to you? Why?

Adherence was measured by observations made by the researcher during visits of the canteens. Availability, placement and prices of the products were reported during every visit (eight times per canteen in total). Hawe et al. (2004) argue that adherence assessment should be based on the extent to which the function of different components of the intervention was delivered according to plan. Therefore, minor changes in placement that did not affect the function of the nudge were not regarded as deviations (e.g. drinks could be placed either to the right or the left of the refrigerator, but always at eye level). During the visits and interviews, respondents were asked about deviations from the protocol in terms of availability, placement and pricing: Why was the specific product not available, or why was it placed or priced differently?

An *applicability* analysis is essential in assessing whether the intervention can be implemented in similar settings (or whether and which changes have to made before the intervention can be implemented in similar settings). For this purpose, the wide range of questions asked through questionnaires and interviews was divided into determinants at the individual, interventional and organisational level.

Individual level: Healthy eating goals of visitors were assessed with the items 'I try my best to eat healthily' and 'Eating healthily is important to me' which had to be rated on a five-point Likert scale ranging from 1 (totally disagree) to 5 (totally agree) (Cronbach's $\alpha = 0.85$). They were also asked about unhealthy eating being part of the football culture with the items 'Unhealthy eating in the football canteen is just part of playing football' and 'It is normal to eat unhealthy in the football canteen' which had to be rated on a five-point Likert scale ranging from 1 (totally disagree) to 5 (totally agree) (Cronbach's $\alpha = 0.61$). Moreover, visitors were asked about the degree of habitualness of buying things in the canteen by agreeing or disagreeing with the items 'I often buy the same things in the football canteen' and 'I tend to buy things in the football canteen without thinking' on a five-point Likert scale ranging from 1 (totally disagree) to 5 (totally agree) (Cronbach's $\alpha = -0.74$; items discussed separately). Interviewees were asked about the characteristics of visitors of the canteen that could help or hinder implementation and effectiveness of the intervention.

Interventional level: During semi-structured interviews, board members and canteen personnel were asked about the use of nudges as a tool for improving healthy eating in football canteens, about the perceived effects of the intervention (whether they thought the intervention was able to reach its goals), about characteristics of the intervention that could help or hinder implementation and effectiveness of the intervention, suggestions for improvement of the interventions and about elements of the intervention they intended to maintain.

Organisational level: In questionnaires, visitors were asked to indicate on a five-point Likert scale ranging from 1 (totally disagree) to 5 (totally agree) whether they agreed with the items: 'I find it important that the football canteen stimulates healthy eating' and 'The supply of healthy products

in the football canteen is sufficient'. Interviewees were asked: What is the role of football canteens in the promotion of healthy eating? and How important is revenue for decisions made for the promotion of healthy eating?

Data analyses

Quantitative data from observations and questionnaires were analysed by calculating descriptive statistics. Since the number of participants who filled in the questionnaires was low, data from both canteens were averaged, unless answers between the canteens differed significantly. Scores on items from constructs that were measured with more than one item with Cronbach's $\alpha > 0.6$ were also averaged. When frequencies are reported, a differentiation is made between respondents rating constructs or items below (disagree), on (neutral) or above (agree) the centre of the measuring scale. Interview recordings were transcribed, coded with themes and compared across interviewees. When content between interviewees differed, this is discussed separately. Themes were partly driven by the different constructs that were measured but mostly emerged during coding.

RESULTS

Sales and revenue

Sales of the nudged products showed a mean increase of 87.2% (5.52 products) in canteen A and 138.9% (16.39 products) in canteen B per week during the intervention period relative to the baseline period. When the percentage of the total sales that is made up of nudged products is examined, there was an increase of 135.1% in canteen A and an increase of 148.1% in canteen B during the intervention period relative to the baseline period. However, although the added products made up of a substantial percentage of the total product range in both canteens (7.6% in canteen A and 4.5% in canteen B), only a small percentage of the total sales consisted of these products in the nudge intervention phase (0.9% in canteen A and 1.3 in canteen B). Moreover, nearly all of the best-selling product categories consisted of unhealthy products, high in sugar, saturated fat and energy content. Descriptive statistics for canteens A and B for different product categories can be found in the supplementary material, Supplemental Files 1 and 2, respectively. Figure 2.2 shows the percentage of the total sales made up of nudged products per week at canteen A. It can be observed here that sales do seem to have increased right after nudge implementation, then decreased and again gradually increased during the remainder of the intervention. The total revenue of both canteens slightly increased during the nudge intervention phase when compared with the same period the year before (an increase of 1.18% in canteen A and 1.9% in canteen B).

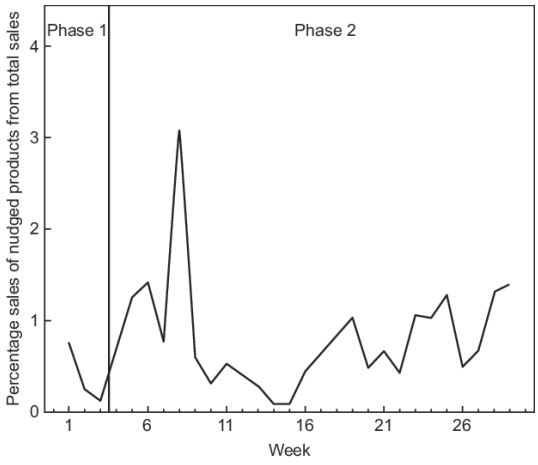

Figure 2.2: Sales of nudged products at canteen A as percentage of total food sales per week in the baseline and the intervention phase.

Reach

On the day of measurement, canteens A and B were visited by approximately 1,081 and 1,705 people, respectively. In canteen A, 1,020 products were sold during 468 transactions. In canteen B, 2,931 products were sold during 1,062 transactions. In the questionnaires, 64% of participants reported to visit the canteen at least once per week, and the average number of reported purchases during the last month was 5.61 ($SD = 5.04$).[3] Eighty percent of respondents had bought a product on the day the questionnaire was conducted. Consumed products could for the largest part be categorised into hot drinks (32%), sandwiches/bread/wraps (22%) and fried foods/foods from the grill (14%).

Acceptability

Descriptive statistics of the acceptability items can be found in Table 2.2. In both canteens, most visitors agreed with the statement that the football canteen uses nudging to influence eating behaviour while also finding it an acceptable means to use to influence eating behaviour in the canteen in general and to influence oneself and others. From the interviews it became apparent that the board members and canteen personnel also thought of nudges as acceptable to be used to promote healthy eating in both football canteens:

> *'Well, in the end, it's the consumer who is standing there and decides what he orders and consumes. Well yeah, and if we could trigger consumers a bit more by placing items at eye level or by placing them on the cash desk., well, that's fine.'* – Board member, male, canteen B

[3] One outlier was removed from analysis of the item assessing the frequency of bought products in the last month, since it was deemed an extreme estimate.

Table 2.2: Descriptive statistics of acceptability items

	M	95% CI	Disagree (%)	Neutral (%)	Agree (%)
Football canteen uses nudges	3.56	3.22, 3.90	20.00	24.00	56.00
Nudges acceptable means to use in canteen	4.14	3.88, 4.41	10.00	12.00	78.00
Nudges acceptable to influence oneself	3.84	3.57, 4.11	8.00	26.00	66.00
Nudges acceptable to influence others	3.89	3.60, 4.18	14.29	18.37	67.35

Adherence

(Non-)adherence to broadening the assortment. During the baseline phase, the added products were available in canteen A during 87.5% and in canteen B during 75.0% of the observations. Of these times, the products were placed according to plan (no nudges) during 71.4 and 87.5% of the observations in canteens A and B, respectively. Prices were as agreed during 100% of the observations in both canteens.

Two of the added products were removed from the assortment (the yogurt and savoury snack) by personnel of both canteens, because of their low shelf life, expensiveness and low sales. Moreover, the savoury snack had, according to the interviewees, a disappointing taste:

> '*People nearly broke their teeth on it and it doesn't taste like anything.*' – Personnel member, female, canteen B

Another reason for the unavailability of some products is that wholesalers sometimes could not deliver a product. Moreover, bananas were not always available, since they are often purchased only once per week, and therefore already sold out later that week or no longer acceptable to sell.

(Non-)adherence to the nudge intervention. During the nudge intervention phase, the added products were available during 66.7 and 71.9% of the observations in canteens A and B, respectively. Of these times, the products were placed according to plan (with nudges) during 68.8 and 52.2% of the observations in canteens A and B, respectively. Prices were as agreed during 100% of the observations in canteen A and during 95.7% of the observations in canteen B.[4,5,6] The high number of frequently changing volunteers working in the canteen was put forward as a reason for non-adherence:

[4] As was the case in canteen B, canteen A changed the default nudge of the sports drink with zero added sugar into a prompted choice, since canteen personnel noticed that visitors sometimes already opened the zero-sugar drink before realising that it was not their preferred choice.

[5] Also, at one instance of measuring adherence in canteen A, the canteen was closed due to bad weather. Therefore, adherence measurements are based on seven observations instead of eight.

[6] Moreover, the vending machine of canteen B broke down during the study. Therefore, other arrangements were made with the personnel for the placement of the products. However, the same type of nudges were used.

'*Well, actually, you have to instruct new volunteers every week. And if you once forget it, they will not know and take initiative and well, that's not what you want.*' – Board member, male, canteen A

Lowest adherence was observed for the default/prompted choice nudge. Personnel deemed it too time consuming at busy times. Moreover, after some time, visitors became familiar with the nudge, leading to personnel not willing to enforce the nudge. In addition, canteen B personnel failed to implement the salience nudge of the bananas:

'*You've only got 15 to 20 minutes during half-time. And when there are 100 people in front of you, you're not going ask such questions.*' – Personnel member, male, canteen A

Applicability

Determinants of the applicability of the intervention are discussed at the individual, interventional and organisational level. These determinants and their interplay can be found in Figure 2.3.

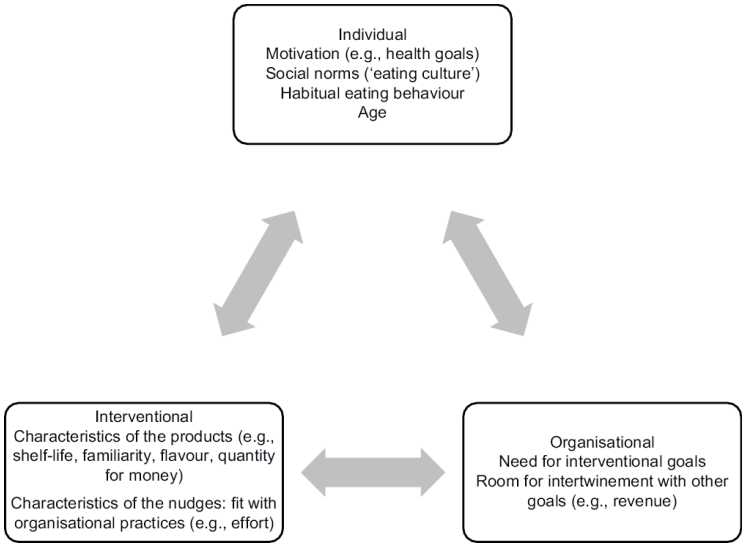

Figure 2.3: Interplay between factors determining applicability of the food-related nudge intervention in sports canteens.

User level

Descriptive statistics of the factors that could influence the effects of the intervention can be found in Table 2.3. Most visitors agreed with having the goal to eat healthily, but many still considered unhealthy eating a part of the football culture. A clear pattern regarding the habitual nature of purchasing products

Table 2.3: Descriptive statistics of factors at the user and organisational level

	M	95% CI	Disagree (%)	Neutral (%)	Agree (%)
Goal to eat healthy	3.70	3.41, 3.98	15.94	11.59	72.46
Unhealthy eating is part of the football culture	3.13	2.90, 3.36	31.03	22.41	46.55
Often buying the same products	2.95	2.60, 3.31	35.59	27.12	37.29
Often buying products without thinking	2.42	2.08, 2.77	59.32	18.64	22.03
Importance of stimulating healthy eating by canteen	4.21	3.90, 4.52	12.07	8.62	79.31
Current supply is healthy enough	3.75	3.48, 4.03	13.56	25.42	31.02

in the football canteen could not be observed.

The interviewees all mentioned unhealthy eating being part of the football culture, which could hinder the adoption of healthy eating practices:

> '*The football club has got a specific culture (. . .) which translates into a meatball during half-time*.' – Personnel member, male, canteen A

Most also mentioned the habitual nature of eating behaviour of adults, which also acts as a barrier to change. However, they did not specifically mention this habitual nature in relation to the context of football canteens, but in contrast to the eating behaviours of children which were considered less routinised and thus more receptive to change attempts. Therefore, some suggested that healthy eating promotion activities should especially focus on children, because the eating behaviour of adults was deemed resistant to change using small alterations such as nudges. Adults often have predefined what they want to consume:

> '*Well yeah, I do think that you have to start with children with that. Then it becomes a natural thing. . . then there's no need to switch. For us, it's more difficult*.' – Personnel member, female, canteen B

Personnel of canteen A noticed that especially children were consumers of the nudged products. Personnel of canteen B thought that especially healthy people consumed the nudged products.

Intervention level

As mentioned in the 'Adherence' section, many characteristics of the products used in the intervention could, according to the interviewees, be altered to increase adherence, which, in turn, could increase sales and vice versa: shelf-life (should be medium to long), familiarity of products (should be high), flavour (products should have a nice taste) and the quantity for money (more food for less money).

The effort (time and physical) in maintaining the nudge was mentioned as a characteristic of nudges that could increase the effect of the intervention. The fit between nudges and the organisational possibilities and constraints was deemed important.

Although interviewees were generally positive about the intervention, they all expressed the opinion that nudges alone are not enough to make a difference in the diets of visitors. Some of the interviewees were willing to implement more rigid measures, for example, by not selling fried foods to children and by radically reducing the number of unhealthy items sold. Others were more positive about expanding the product range with healthy items and by slowly reducing the product range of unhealthy items. For them, the way to a healthy canteen is a slow, multi-year process.

Interviewees of both canteens indicated to continue with the sale of most products after the study, except for the yogurt and the savoury snack in both canteens and the grilled sandwich in canteen B. They also indicated to continue using the nudges connected to these products, except for the prompted choice nudge. Although interviewees mentioned the benefit of increasing sales of these healthy products by continuing the nudges, they also mentioned that this choice was mostly based on the desire not to change the (by now well established) position of the products.

Organisational level

Many visitors agreed with the notion that it is important that the football canteen stimulates healthy eating. Most also agreed that the current supply of healthy products is high enough.

Interviewees of both canteens agreed that it is important for football canteens to promote healthy eating. However, they also placed importance on providing visitors with the choice between healthy and unhealthy products. They felt more responsible for children than for adults, although they also considered that the ultimate responsibility for a healthy diet does not rest upon the football canteen but with parents or guardians:

> '*I do think that, when they come here, we should offer an alternative for all the unhealthy products we sell. I do feel responsible for a healthy product range, and also that we show these products, but I don't feel responsible for obligating children or parents to buy such things*.' – Board member, male, canteen B

The importance that is placed on the promotion of healthy eating is reflected in board members of both canteens stating that the sale of healthy products can come with the drawback of lower revenues. In canteen B, healthy products are often sold against cost price. However, losses should not be too big, since the profits from the canteens constitute a significant part of the budget of the football clubs. Therefore, the interviewees of canteen B also mentioned that the best-sold products, although unhealthy, will not be removed from the product range.

DISCUSSION

The current study explored the potential of nudge interventions to promote healthy eating in sports canteens. Sales records showed an increase in the consumption of nudged products. Willingness and acceptability to use nudges were high for both receivers and choice architects, and many people were exposed to the intervention. However, adherence to the intervention turned out to be quite difficult. Moreover, in light of the overwhelming number of unhealthy products that were sold in the canteens in comparison with the small proportion of healthy alternatives, doubts were raised about the magnitude of the possible impact of the use of only nudges on eating behaviour. These results paint a moderately positive picture of the use of nudges in guiding consumers towards better food choices.

The current study demonstrates the difference between the effects of nudges achieved in earlier studies in controlled environments versus the real-life circumstances in the current study. The low adherence rates to the intervention are especially striking in this regard, since the intervention can be argued to be straightforward and its initial implementation appeared to be relatively easy. However, canteen personnel found it demanding to have all products in stock and to adequately follow the nudge protocol.

Only few studies explored the effects of repeated nudge exposure. It has been suggested that effects tend to fade over time, with 35-55% of changes persisting after the intervention (Brandon et al., 2017). However, other studies suggest that duration of nudge exposure does not moderate the effect of nudges on eating behaviour (Cadario & Chandon, 2020). The reported sales figures in our study do suggest a pattern of increase right after implementation, followed by a decrease and again by a slight increase. The inconclusive results regarding long-term effects of nudges suggest more long-term follow-up research on nudges is needed.

More importantly, the results also prompt questions about the ability of nudge interventions to significantly impact public health. Although positive, the effect of the nudges on sales was small. When the extra number of sold products is compared with the number of visitors, it can be deduced that the intervention is not likely to have induced a substantial health gain. Moreover, although a significant proportion of the products was nudged, sales records were still dominated by unhealthy products such as alcoholic beverages, sugary drinks and deep-fried foods. It thus seems that the nudges could not overcome the many factors that determine food choice such as the easy accessibility and high availability of unhealthy foods and existing food preferences and habits (Glanz et al., 1998). Because products used in the intervention were close alternatives to existing products, these results imply that more concurrent strategies are needed to significantly impact eating behaviour. For example, in a systematic review on point-of-sale strategies to increase healthier eating by Liberato et al. (2014), monetary incentives are put forward as an effective point-of-sale strategy, while evidence on point-of-sale nudges was found inconclusive.

The ethical implications of nudging are a topic of scholarly discussion (Goodwin, 2012; Wilkinson, 2013). Interestingly, both the public and the organisations of the football clubs were positive about the use of nudges to promote healthy eating. This finding is supported by other studies that focussed on the acceptability of nudges by those that are being nudged. For example, Reisch and Sunstein (2016) collected acceptability rates of five countries on a range of different nudges and found that on average, 63% approved of the nudges, although acceptability rates did depend on type of nudge and country. One could argue that behavioural influencing techniques accepted by those that are being influenced are inherently non-manipulative. Although the small minority that considered the use of nudges unacceptable in the current study should by no means be ignored, the current study does suggest that the acceptability of nudges does not pose a problem for the implementation of nudge interventions to promote healthy eating in sports canteens.

The habitualness of unhealthy eating behaviour and the unhealthy eating culture surfaced as having acted as a barrier for the influence of nudges on eating behaviour during interviews with board members and canteen personnel, but not from the questionnaires with visitors. This discrepancy may be caused by an inability to identify one's own behaviour as automatic (Bargh, 2002), since sales and interviews clearly show the canteens to be environments in which unhealthy eating dominates (despite the large number of visitors reporting a strong healthy eating goal). Because of these persistent unhealthy eating habits, board members and canteen personnel were sceptical about the ability of nudges to change behaviour, especially with adolescents and adults. However, eating behaviour of children was still seen as mouldable, a statement that needs further research (Brandon et al., 2017). This view coincides with the responsibility voiced by the interviewees to promote healthy eating behaviour among children.

Strengths and limitations

Although only two cases were examined in the current study, they enabled a multi-method in-depth analysis of many aspects of the outcome and process evaluation. Such analyses are uncommon in the outcome-oriented literature on nudges, which mostly consists of short-term experimental studies. This allowed the identification of elements that deserve consideration when designing and implementing a nudge intervention to promote healthy eating. Moreover, the timeline of the intervention enabled exploring the effect of nudges over a longer period, generating hypotheses for important unanswered questions.

The current study also has limitations. The effects of the nudges on sales are likely underestimated, since, during the interviews, it became clear that often incorrect buttons are pressed at the register when prices for products are similar (e.g. the regular instead of the zero-sugar version). Also, both canteens that participated in the study expressed a high motivation for adding and promoting

healthy products and underlined that this is uncommon among football clubs. This may have inflated acceptability rates. Although deviations from the protocol were corrected during visits at which adherence was measured in canteen A, between-visit adherence was still suboptimal (which follows the low adherence rate at most visits). Therefore, the added focus on the validity of results related to the outcome in canteen A was lost.

Future research

The current study highlights the importance of a further exploration of the potential and limits of nudges to impact public health. How sustainable are the effects of nudging on eating behaviour? What are the prerequisites for, and processes that underlie, long-term effects of nudges? Moreover, to substantially impact health, additional evaluation studies are needed to further assess the potential of nudges, with or without other measures that act in concordance with nudges.

CONCLUSION

It may have become clear that the reputation of nudges as an easy, cheap and effective means to use in interventions aimed at increasing healthy eating needs reconsideration. An adequate translation from theory to practice needs to take into account the many factors related to the target group and the context, all of which could affect the intervention's outcomes. Moreover, the impact of localised nudge interventions on health seems to be minimal considering the complex contexts in which food choices are made. Therefore, nudge research should make room for questions that answer which, where, how, for whom and how long nudges are beneficial in the battle against unhealthy eating.

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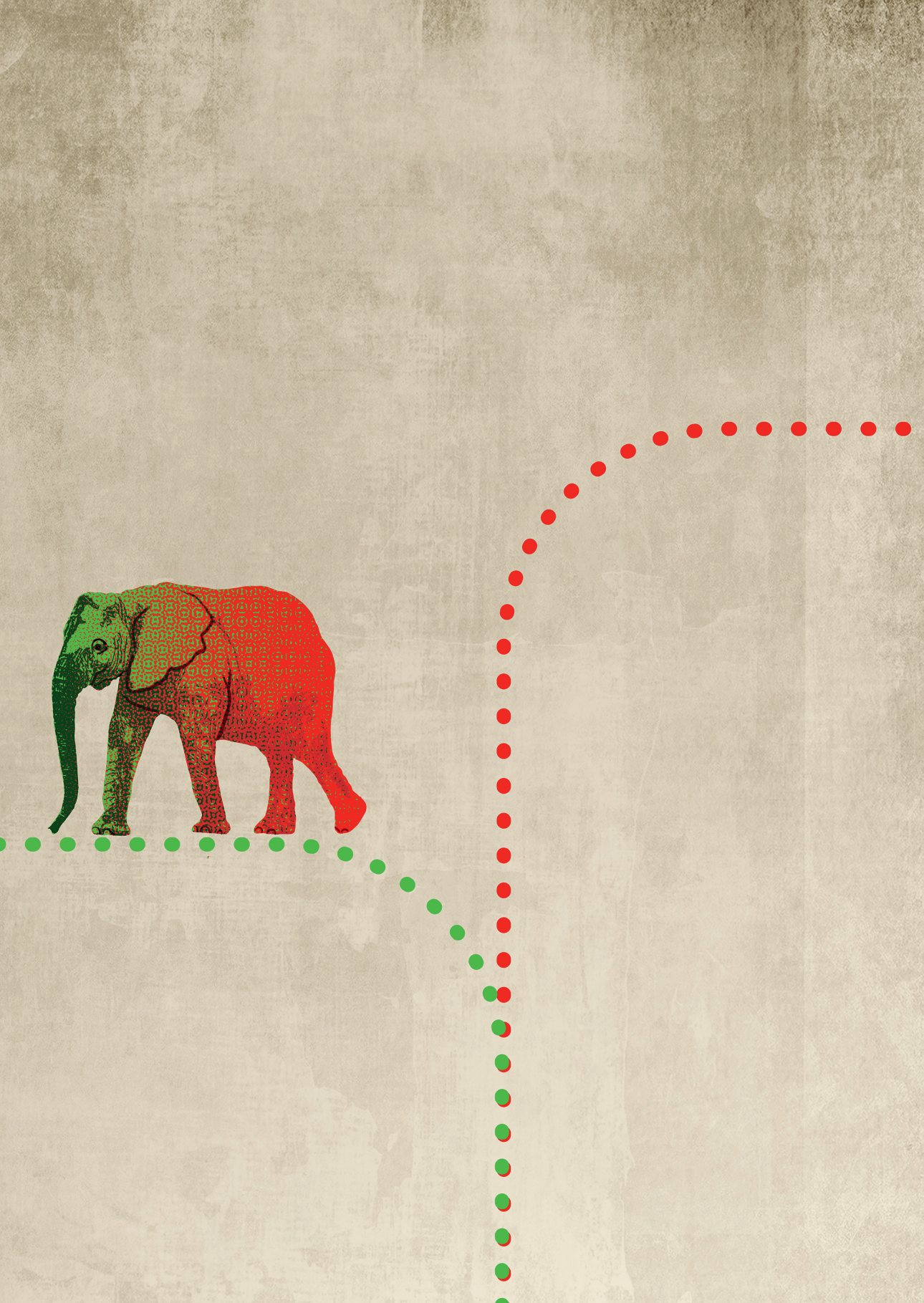
ETHICAL STATEMENT

The current study was conducted according to the guidelines laid down in the Declaration of Helsinki and all procedures involving research study participants were approved by the Social Sciences Ethics Committee of Wageningen University. Written informed consent was obtained from all interviewees.

For all questionnaires used in the study consent was implied, meaning that respondents were informed at the top of the questionnaire about the goal of the study and their rights. Moreover, people had to actively take the questionnaires without the researcher asking for participation.

PREREGISTRATION

The Netherlands Trial Register, no. 6718, date of registration: 17 August 2017, https://www.trialregister.nl/trial/6530.



CHAPTER 3

The effects of nudges: One shot only? Exploring the temporal spillover effects of a default nudge

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ABSTRACT

Nudges, such as defaults, are generally found to be effective in guiding immediate behavioural decisions. However, little is known about whether the effect of a nudge can be lasting, meaning that it spills over to subsequent similar choices without the presence of a nudge. In three experiments, we explored the temporal spillover effects of a default nudge. The results of Experiments 1 ($N = 1{,}077$) and 2 ($N = 1{,}036$) suggest that nudging participants into completing a longer questionnaire affected their decision for the same behaviour a day later without the presence of a nudge. However, nudging participants into a healthier food choice in Experiment 3 ($N = 969$) did not result in such a temporal spillover effect. The results indicated that participants' change in attitude towards the nudged behaviour may partly explain the temporal spillover effects. These findings suggest that for some, but not all behaviours, default nudges may have the potential to yield temporal spillover effects and warrant a further investigation of boundary conditions and facilitators of the spillover effects of nudges.

INTRODUCTION

Since Thaler and Sunstein's popular book 'Nudge' (2008), the term 'nudging' has become widely known and the concept has had quite some impact in the area of public policy. Perhaps most notably, it inspired the establishment of numerous Behavioural Insights Teams (BITs) all over the globe, whose main aim is to inform and improve public services by generating and applying behavioural insights (Behavioural Insights Team, n.d.). This seems to be with good reason: Nudges are able to affect a wide range of behaviours (from increasing organ donation to improving healthy eating) while preserving the freedom of choice of individuals (Vecchio & Cavallo, 2019; Madden et al., 2020). Yet, surprisingly, the question of whether or not the effects of nudges are sustained over time has hitherto not received considerable attention. For some behaviours, such as opting to be an organ donor or choosing an energy provider, this may not be a particularly pressing question, since these are generally decisions that one does not or cannot revise often, and that can have a major impact in and of themselves. However, many nudge interventions actually target behaviours that people perform multiple times a day, such as physical activity, recycling, and eating. In this instance, influencing a single decision (e.g. nudging train travellers to take an apple at a snack shop (Kroese et al., 2016)) would probably have a rather limited impact on behaviours, such as general physical activity levels or overall healthy eating patterns. A more sustained behavioural change across time and contexts is generally required to make an impact on physical activity levels, recycling behaviour, or healthy eating patterns. For such behaviours, it is, therefore, worthwhile to explore whether nudges can also influence decisions beyond the nudged choice.

There is a large body of literature investigating the effects of behaviour on subsequent behaviour, so-called spillover effects (e.g. Guadagno et al., 2001; Mead et al., 2009; Sachdeva et al., 2009). Here, spillover effects are often defined as the effects of one behaviour on a second, *different* behaviour that occurs after the first behaviour (Dolan & Galizzi, 2015). We will label these types of spillover effects as behavioural spillover effects. The results of these studies suggest that behaviour can impact subsequent different behaviour in both a promoting and an inhibiting manner. However, little is known about the extent to which behavioural spillover effects also occur when a certain behaviour is the result of nudging. If nudged choices could have a positive impact on related but unnudged choices, this could dramatically increase the impact of nudges on overall and sustained behaviour change. This would, for example, be the case when nudging people into buying fruit would lead to an increase in choosing other healthy products. In addition to behavioural spillover effects, spillover effects can also be temporal in nature. Such spillover effects occur when a decision in a specific context is repeated in the same context at a later point in time. This would be the case when, for example, one has to choose between a light and regular soda drink in the supermarket, when the light option was previously successfully nudged.

Although a few studies have empirically investigated temporal spillover effects in the context of nudging, there are several established theories in psychology that do point to this possibility, such as self-perception theory (Bem, 1972) and self-herding (Ariely & Norton, 2008). That is, in both self-perception theory and the literature on self-herding, the central premise is that new attitudes and preferences are not just determinants of our behaviour, but can sometimes also be the product of behaviour as they can be formed by observing our own behaviour. This is especially true when we are unable to attribute our behaviour to an external source. Without such a source to justify behaviour then, internal attribution takes place, reasoning that it must have been a positive attitude towards the behaviour that caused it. This change in attitude may in turn affect subsequent decision-making in line with the changed attitudes, as has also been observed in research on cognitive dissonance (Festinger, 1962; Harmon-Jones & Mills, 2019). Considering that there is ample evidence demonstrating that people are frequently unaware of or underestimate the impact of nudges on their decisions (e.g. Van Gestel et al., 2021), it is not unlikely that nudged choices are frequently misattributed to internal states. This study examines whether this indeed implies that nudges yield significant changes in attitudes and whether these changes are translated into subsequent temporal spillover effects.

We could even go a step further in our understanding of the possible positive spillover effects of nudges through internal attribution. That is, the nudged behaviour could even affect identity formation. The idea that behaviour can serve as the input to the formation of our identity stems from the work of Gneezy et al. (2012) and Bénabou and Tirole (2011). They argue that, without any external justification for our behaviour, we may attribute it to us being 'the person that does that kind of things.' This identity formation can then serve as the input for later behaviours. To illustrate, Burger and Caldwell (2003) found that a change in self-concept (by asking participants to which extent they thought of themselves as persons who engage in various altruistic behaviours) could explain why participants were more likely to spend their time on voluntarily sorting and boxing canned goods if they were earlier asked to sign a homelessness petition. In the case of nudges, this would mean that the behaviour originally driven by a nudge could become internalised so that, in turn, this change in identity triggers similar behaviour once the nudge is no longer present. In contrast to a behaviour-specific attitude change, a change in identity implies that a possible spillover effect could even generalise to *different* but related domain-specific behaviours, and thus yield behavioural as well as temporal spillover effects. Next to these theoretical arguments why spillover effects could occur because of the misattribution of behaviour to positive attitudes or identity, spillover effects could also occur because of the desire to act consistently (Dolan & Galizzi, 2015): One simply behaves as one behaved previously, directly repeating the previous choices that are not mediated by attitudes or other relevant cognitions.

Although it can be argued that nudges could induce temporal spillover effects based on the abovementioned theories, it should not be ignored that there is also the possibility of no spillover of the nudged behaviour to subsequent behaviour. More specifically, we assume that people are aware of the behaviour they perform following the nudge. It is this behaviour that could serve as the input for a changed attitude or identity. However, it is not necessarily the case that people are aware of their behaviour following the nudge. In that case, the default tendencies of people to behave in a particular way may be overridden by the nudge, but after removal, they will just resort to the decisions they would have made without the nudge. For example, by placing healthy snacks within the reach of a person would make him or her more likely to eat the snack without much thinking just because it is easy to grab. Intake of the health snack may seize when put back into the drawer that it came from, i.e. when the nudge is removed.

To date, research on nudging and spillover effects is relatively limited. Most studies examining nudge interventions solely consider their effects during the intervention itself. Once the nudge has been removed, data collection generally comes to a halt. However, in those few studies that did continue behavioural measurements after nudge removal, it is commonly found that the effect of the nudge indeed continues, albeit to a lesser extent than during the intervention. For example, Venema et al. (2018) used a default nudge to promote stand-up working for 2 weeks. The effects of the intervention were still noticeable even after 2 months, although they were not as strong as during the intervention. Although these results are promising, a major drawback of these studies is that data collection is almost solely based on group-level observations. Therefore, it cannot be concluded with certainty that the effects of nudges can persist on an individual level. Moreover, the study of responsible mechanisms using group level observations presents problems since the internal states of the individual cannot be coupled with their behaviour. To improve our understanding of whether, when, and how nudges may have the potential of spillover to subsequent decisions, it is thus important to systematically study the consequences of nudges on an individual level after their removal.

We know of only four studies (with a total of 11 experiments) specifically examining the spillover effects of nudges in the individual level. In these experiments, behaviour is measured following a nudge and is measured again after nudge removal. In four experiments, no default effects were found on the first measurement (Donkers et al., 2020, experiments 1–3; D'Adda et al., 2017), making it impossible to conclude whether spillover effects follow effective nudges. In the experiments in which the nudge does influence the initial behaviour, no spillover effects (Ghesla et al., 2019; Kuhn et al., 2021, experiments 1–3; Zimmermann & Renaud, 2021) or even compensating effects (Donkers et al., 2020, experiment 4) are found. However, in all these experiments, the initial choice set differed from the subsequent choice set in one or more ways. In other words, all these studies investigated *behavioural* spillover effects. For example, in the study of Ghesla et al. (2019), a dictator game was used to first nudge participants into

donating money to charity, using either a weak or strong default nudge. They subsequently played another dictator game. However, this time they were not invited to donate money to charity but to another participant. While the aforementioned studies suggest that behavioural spillover effects of nudging may not be very likely, it is yet to be determined whether nudges may in fact lead to temporal spillover effects. Seeing that attitudes are more predictive the more specific they are to the behaviour that is predicted (Ajzen & Fishbein, 1977), any change in attitudes regarding a previously performed behaviour should therefore be particularly likely to affect the same behaviour in the same situation at a later time point. This would suggest that if attitudes are indeed affected by nudging interventions, temporal spillover effects are more likely to be observed than behavioural spillover effects.

Building on the aforementioned rationale, in the present paper we aimed to systematically explore potential temporal spillover effects of a default nudge in three preregistered experiments. We explicitly chose a default nudge since it is generally considered a prototypical System 1 nudge (Thaler & Sunstein, 2008; Hansen & Jespersen, 2013). Such nudges are thought to influence behaviour through System 1 processing, which is fast, automatic, and intuitive (Kahneman, 2011). We consider it as a prerequisite that people are unaware of the influence of the nudge on their behaviour for the occurrence of temporal spillover since only then can behaviour be misattributed to internal states and serve as an input for later behaviour.

In all three experiments, we measured the behaviour of participants on two consecutive days. Participants were randomly assigned to either a control condition or an experimental condition. On the first day, a default nudge was used to influence participants' behaviour in the experimental condition. On the second day, the default nudge was removed and the behaviour was measured again. This setup allowed an examination of whether the effect of the nudge on the first day continued to the second day when it was no longer present. In Experiment 1, we tested the temporal spillover effect of a default nudge on prosocial behaviour, by asking participants whether they opted for completing a longer version of a questionnaire that would take five additional min without getting any extra reimbursement. In Experiment 2, we aimed to replicate and extend the findings from Experiment 1 and used a similar design to explore possible changes in the attitude of the participants and their identities as mechanisms responsible for the temporal spillover effect. In Experiment 3, we tested whether the results of Experiments 1 and 2 could be replicated with food choices, by asking participants to choose between unhealthy food products and healthier alternatives.

EXPERIMENT 1

In Experiment 1 (preregistered at the Open Science Framework: https://osf.io/s2f3j), we aimed to investigate whether the effect of a nudge continues once the nudge is removed. To this end, we asked participants on two consecutive days whether they opted for completing a longer version of a questionnaire that would take 5 min more without getting extra reimbursement (Wachner et al., 2020 based on Paunov et al., 2020). Participants were randomly assigned to an experimental condition or a control condition. On the first day, participants in the experimental condition were nudged into completing the longer questionnaire by preselecting the option (a default nudge). No nudge was used in the control condition. On the second day, the nudge in the experimental condition was removed. Building on the theories that predict that behaviour can also be seen as the input to affect internal states (e.g. attitudes and identities), which, in turn, affect subsequent behaviour (Bem, 1972; Ariely & Norton, 2008; Bénabou & Tirole, 2011; Gneezy et al., 2012), we expected to find (1) an effect of the nudge on questionnaire choice on the first day and (2) an effect of the initial nudge on questionnaire choice on the second day even when the nudge was no longer present.

Methods

Participants

Participants were recruited via the online crowdsourcing website *Prolific Academic*. Participants could only participate when they were aged 18 years or older, spoke English fluently, had two or more previous submissions on Prolific Academic, and had a 95% or more approval rate on Prolific Academic. These last two criteria were added to minimise the attrition rate. Participants were encouraged to participate on a desktop and rewarded with £2.00.

A sample size calculation with the software program *G*Power 3.1.9.2* (Faul et al., 2007) resulted in a recommended sample size of 263 (with 0.90 power and small to medium effect size of $\varphi = 0.2$). Because we expected some dropout, we recruited 50% extra participants on day one of the experiment, resulting in 395 participants on the first day (of which 358 also participated on day two).

Initial analyses of the effectiveness of the nudge manipulation on questionnaire choice on day one of this data showed a clear trend for the expected effect of the manipulation, with 57.9% of participants in the experimental condition choosing the longer version of the questionnaire vs. 51.4% in the control condition. This signalled that the effect of the default on day one may have been smaller than expected ($\varphi = 0.065$), which may have caused the effect of the manipulation to be non-significant. As an effect of the manipulation on day one needed to be detected before any possible temporal spillover effects on day two could be assessed, we decided to recruit more participants to be able to detect a small effect on day one. A sample size calculation (with 0.90 power and small effect size of $\varphi = 0.1$) resulted in a

recommendation of 1,051 participants, which meant an addition of 693 participants. Assuming a dropout rate similar to that of the first data wave (90.6%), 765 extra participants were needed on day one.[7]

Combining the data of the two waves, a total of 1,163 participants finished the questionnaire on day one, of which 1,077 also finished the questionnaire on day two (92.6% response rate). Of these 1,077 participants, 533 (49.5%) had been randomly allocated to the experimental condition. The average age was 32.56 years ($SD = 10.48$), with 50.0% men, 49.8% women, and 0.3% indicating 'other.' The highest completed level of education was a high-school diploma with 34.7% and a bachelor's degree with 40.0% of participants. Participants had 55 different nationalities with most participants coming from the UK (33.5%) and Poland (14.2%).

Design

The experiment was conducted with a 2 (between-subjects factor = Condition: experimental vs. control) × 2 (within-subject factor = Day: one vs. two) mixed design with the questionnaire choice (normal/longer) as the dependent variable.

Procedure

On day one, participants were invited to complete a questionnaire about lifestyle, as part of our cover story. They were told that, based on their answers, they might be invited for another study that would be conducted the next day. Participants were kindly requested only to participate in the present study if they felt that they could also complete the second study the next day. After giving their informed consent, participants were asked about some demographics. Half of the participants were then nudged into filling in a longer questionnaire as this would help improving future questionnaires without getting any extra reimbursement. After selecting a questionnaire, participants completed a normal or longer bogus questionnaire in line with our cover story to make participants actually perform the behaviour.

On the second day, only participants who completed the questionnaire on day one were invited for another study on lifestyle. Participants first had to give their informed consent and were asked about some demographic variables. Participants were then asked to choose a normal or longer version of a questionnaire about lifestyle. None of the participants were nudged on this day. As before, a bogus

[7] We are aware that adding extra participants in a second wave of data collection after initial analyses is not recommended, and could be seen as p-hacking. We do not take this threat to the reliability of our findings lightly, which is why we are transparent about this and urge readers to be careful in interpreting the findings of the present study. However, we also think that our decision is justified as we only looked at an effect of the nudge on the first day and did not run any analyses on the hypothesised temporal spillover effect. In order to test the potential of a temporal spillover effect, it is a prerequisite to be able to detect an initial effect of the nudge. Therefore, we argue that the extra recruitment is justified in this case to allow for a sufficiently large sample for this small effect (which is a major limitation, please see section General discussion) to become statistically significant. Please note that the study is preregistered and other than the sample size, we did not deviate from it.

questionnaire, which corresponded in length to the selected version (long or normal), was completed. However, the questions differed from the questions on the first day.[8] Participants were then thanked and debriefed about the real aim of the study.

Measures and materials

Demographics. Participants were asked for their age (in years), gender (male, female, or other), nationality (from a dropdown list of 193 nationalities), and the highest degree of completed educational level (less than a high school diploma, high school degree or equivalent, bachelor's degree, master's degree, doctorate, or other educational levels [please specify]).

Manipulation. Using a previously tested nudge manipulation (Wachner et al., 2020 based on Paunov et al., 2020), all participants were asked the following question on day one: 'Please indicate whether you will participate in the long version of this study (12 min) or normal version (7 min). If you choose to participate in the long version, you will not receive additional payment; however, you will help to improve future questionnaires.' Participants were randomly assigned to the experimental or control condition. In the experimental condition, the option of completing the longer version was preselected on day one (a default nudge). No option was preselected in the control group. On day two, the same question was asked, but this time, both conditions received the question without any nudge (no specific option was preselected). We chose this manipulation because the decision made by participants to complete the normal or longer questionnaire was not a hypothetical one, but was real and actually impacted the amount of time that participants spent on the questionnaire (the behaviour of interest in this study). Moreover, the manipulation is credible, since being asked to complete two studies on two consecutive days and to complete a longer version of these questionnaires without getting any extra reimbursement would not immediately raise suspicion.

Bogus questionnaire. On both days, (parts of) existing questionnaires or made-up items in the domains of personality, lifestyle, and eating behaviour were used. Fewer items were used in the normal version than in the longer version.

Data preparation. Being preregistered, participants were only included when they finished both questionnaires. If answers to demographic questions differed between the two days (e.g. two different nationalities), participants were contacted for the right information. Cells with less than five observations were set missing (as was the case with gender) or merged (as was the case with nationality) to make the variable viable for inclusion in the analyses. When participants indicated 'other' as their education, their specification was transformed into one of the listed options by the researcher. When

[8] For exploratory purposes only (and not further analysed in the current paper), prosocialness was measured and participants were asked which version of the questionnaire they selected and why they did so, to see whether they were aware of the nudge.

outliers (three *SD*s more than or less than the mean) were detected within variables for a particular analysis, these values were set missing.

Results

Confirmatory analyses

Randomisation check. We first examined whether the demographic variables were equally distributed across the control and experimental condition using an individual *t*-test and a chi-square tests. No differences were found regarding age ($t(1{,}065) = 0.266$, $p = 0.791$), gender ($\chi^2(1) = 0.301$, $p = 0.583$), nationality ($\chi^2(13) = 19.957$, $p = 0.096$), and the level of education ($\chi^2(4) = 0.543$, $p = 0.969$), indicating successful randomisation.

Manipulation. To test whether the nudge had the intended effect on day one, a chi-squared test was conducted with condition (experimental/control) as the independent variable and questionnaire choice day one (QC1) (normal/longer) as the dependent variable. As expected, significantly more participants choose the longer version of the questionnaire in the experimental condition (57.2%, 95% CI [53.0, 61.4]) than in the control condition (50.9%, 95% CI [46.7, 55.1]) ($\chi^2(1) = 4.308$, $p = 0.038$). This means that the manipulation was successful although the effect was small ($\varphi = 0.063$).

Temporal spillover effect. To test for a temporal spillover effect, a chi-squared test was conducted with condition (experimental/control) as the independent variable and questionnaire choice on day two (QC2) (normal/longer) as the dependent variable. Figure 3.1 shows the percentages of participants choosing the normal or longer version on day two relative to their choice on the first day in the control and experimental condition. A trend was observed in which more participants in the experimental condition (51.4%, 95% CI [47.2, 55.7]) chose the longer version than in the control condition (45.6%, 95% CI [41.4, 49.8]) ($\chi^2(1) = 3.650$, $p = 0.056$). However, this effect was small ($\varphi = 0.058$).

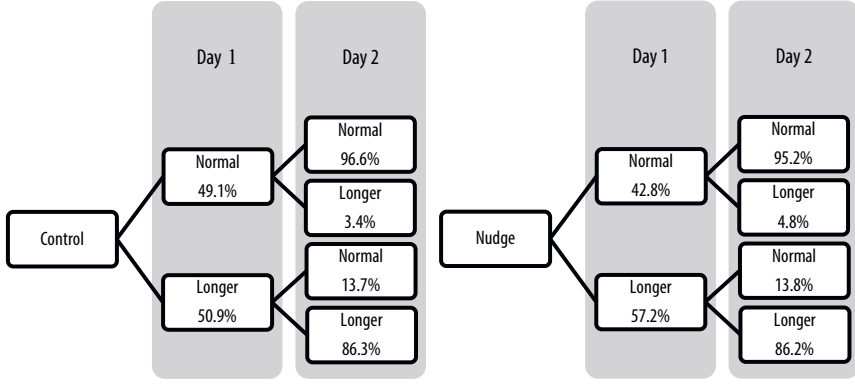


Figure 3.1: Percentages of participants choosing the normal and longer version of the questionnaire on days 1 and 2 in Experiment 1.

Exploratory analyses

Mediation of condition on QC2 through QC1. For exploratory purposes, we also assessed whether the nudge had an indirect effect on QC2 through its initial effect on QC1. Therefore, we conducted a mediation analysis according to the steps described in Iacobucci (2012) to test whether QC1 (normal/longer) mediated the effect of condition (experimental/control) on QC2 (normal/longer). The mediation analysis resulted in a significant *Z*-mediation value of 2.051 ($p = 0.040$). This means that the nudge influenced QC1, which, in turn, influenced QC2.

Discussion

In Experiment 1, a trend in which the effect of the default nudge on prosocial behaviour spilled over to subsequent similar behaviour was observed. However, this effect was only small, which may partly be attributed to the small effect of the nudge on day one. A significant mediation of QC1 on the effect of condition on QC2 was found, suggesting that the effect of the nudge on the first day may be seen as a prerequisite for its continued effect on the second day once the nudge is removed.

As discussed in the introduction, several theories predict that an attitude (Bem, 1972; Ariely & Norton, 2008) or even identity (Bénabou & Tirole, 2011; Gneezy et al., 2012) change could explain the temporal spillover effect. Therefore, in Experiment 2, we examined the options of a mediated pathway of changed attitudes and changed identity between two similar behaviours of which the first was initially elicited by the nudge.

EXPERIMENT 2

To substantiate the trend observed in Experiment 1 for a temporal spillover effect, we replicated the manipulation in Experiment 2 (preregistered at AsPredicted: #37220). Moreover, we examined a change in attitude towards the initial behaviour and a change in prosocial identity as possible mediators explaining the relation between the nudge and the behaviour on the second day. We used a similar setup as in Experiment 1 while adding participants' attitude towards taking longer questionnaires and their prosocial identity as possible mediators. As in Experiment 1, we expected to find (1) an effect of the nudge on QC1, (2) an effect of the nudge on QC2, (3) a mediation effect of condition on QC2 through QC1, and (4) a mediating effect QC1 on QC2 through participants' attitude towards taking longer questionnaires and/or prosocial identity (Figure 3.2 is a summary of the hypotheses).[9]

[9] Only Hypotheses 1 and 2 were preregistered as confirmatory hypotheses.

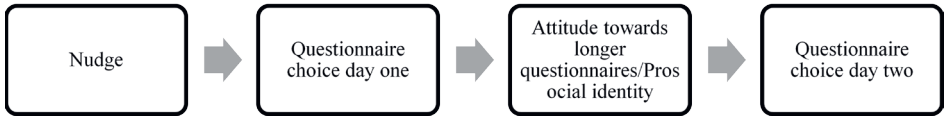


Figure 3.2: Hypothesised pathways for Experiment 2.

Methods

Participants

Participant recruitment and inclusion criteria were similar to Experiment 1. A sample size calculation using the software program *G*Power 3.1.9.2*. resulted in a recommended sample size of 1,051 (with 0.90 power and a small effect size of $\varphi = 0.1$) for the effect of the nudge on days 1 and 2. Taking into account the dropout rate of Experiment 1 (6.90%), 1,150 participants were recruited on day one.

On day one, 1,150 participants finished the questionnaire, of which 1,044 also finished the questionnaire on day two (90.8% response rate). Of these, eight participants were excluded since they correctly guessed the aim of the study. Therefore, analyses were conducted on 1,036 participants. Of these participants, 512 (49.4%) had been randomly allocated to the experimental condition. The average age was 29.01 years ($SD = 8.55$), with 53.9% men, 45.9% women, and 0.2% indicating 'other.' The highest level of education completed by the participants was a high school diploma in 36.6% and a bachelor's degree in 36.6% of participants. Participants had 64 different nationalities with most participants coming from the UK (22.7%), Poland (13.5%), and Portugal (12.1%).

Design

The design of Experiment 2 was similar to Experiment 1 with the addition of two possible mediators: participants' attitude towards taking longer questionnaires and their prosocial identity.

Procedure

The procedure of Experiment 2 was similar to that of Experiment 1. However, after the manipulation on day one, participants' attitude towards taking longer questionnaires and their prosocial identity were additionally measured. The order of these two variables was counterbalanced. On day two, these variables were measured again.[10] At the end of the second day, participants were asked about the goal of the study.

[10] Preference for consistency was also measured. However, this variable was only measured for exploratory purposes and not further analysed.

Measures and materials

Manipulation. The manipulation in Experiment 2 was similar to that of Experiment 1. However, instructions were slightly changed to highlight the prosocial element of the choice. The instructions now read: 'Please indicate whether you will participate in the longer version of this study (12 min) or normal version (7 min). If you choose to participate in the longer version, you will not receive additional payment; however, you will help researchers in improving their future questionnaires.' This wording made it more apparent that taking the longer version would help others and allowed for matching our attitude measure to the nudged behaviour. Since the duration of both questionnaires was shorter than expected in Experiment 1, we added the extra questions to the questionnaire measuring attitude and identity instead of replacing questions.

Attitude towards taking longer questionnaires. The attitude of participants towards taking longer questionnaires was measured by taking the mean of six items on a seven-point semantic differential scale (as in Aertsens et al., 2011). Participants were presented with the sentence 'Filling in the longer questionnaire is. . .' followed by sliders with various anchors (good/bad, positive/negative, satisfying/unsatisfying, enjoyable/unenjoyable, pleasant/unpleasant, and preferable/unpreferable). Cronbach's alpha ($\alpha = 0.894$) was deemed high enough to average the items into one scale.

Prosocial identity. Participants' prosocial identity was measured by taking the mean of two items: 'To what extent do you see yourself as a helpful person' and 'To what extent do you see yourself as an unselfish person,' to which participants had to rate themselves on a seven-point Likert scale ranging from (1) not at all to (7) a great extent. The two items showed a moderately positive correlation ($r(1) = 0.458$, $p < 0.001$), which for our purposes was deemed high enough to continue with the items as one scale. To mask the goal of the study, several filler items were added between the items of interest.

Data preparation

Data preparation in Experiment 2 was similar to that in Experiment 1.

Results

Confirmatory analyses

Randomisation check. The randomisation check using an individual t-test and a chi-squared tests showed no difference between the control and experimental condition regarding gender ($\chi^2(1) = 0.353$, $p = 0.552$), nationality ($\chi^2(14) = 9.135$, $p = 0.822$), and level of education ($\chi^2(4) = 1.675$, $p = 0.795$). However, participants in the experimental condition ($M = 29.82$, $SD = 9.19$) were significantly ($t(1{,}022) = 2.983$, $p = 0.003$, $d = 0.186$) older than participants in the control condition ($M = 28.23$, $SD = 7.80$). Therefore, all analyses were also conducted with age added as a covariate. Since this did not change the results, the final analyses are reported without the addition of age as a covariate.

Manipulation. To test whether the nudge had the intended effect on day one, a chi-squared test was conducted with condition (experimental/control) as the independent variable and QC1 (normal/longer) as the dependent variable. As expected, significantly more participants chose the longer version of the questionnaire in the experimental (64.3%, 95% CI [60.1, 68.4]) than in the control (55.5%, 95% CI [51.3, 59.8]) condition ($\chi^2(1) = 8.216$, $p = 0.004$). This means that the manipulation was successful although the effect was small ($\varphi = 0.089$).

Temporal spillover effect. To test for a temporal spillover effect of the nudge on the behaviour after nudge removal, a chi-squared test was conducted with condition (experimental/control) as the independent variable and QC2 (normal/longer) as the dependent variable. Figure 3.3 shows the percentages of participants choosing the normal or longer version on day two relative to their choice on the first day in the control and experimental condition. Although the effect was small ($\varphi = 0.094$), significantly more participants chose the long version of the questionnaire in the experimental condition (62.5%, 95% CI [58.3, 66.7]) than in the control condition (53.2%, 95% CI [49.0, 57.5]) ($\chi^2 = 9.096$, $p = 0.003$). This means that there was a temporal spillover of the nudge on QC2.

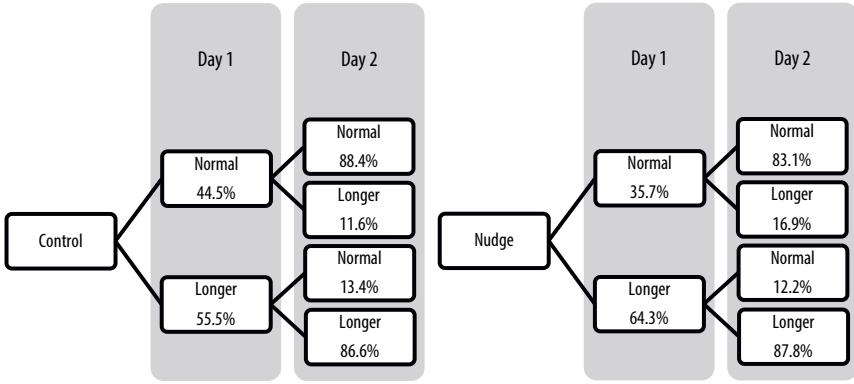


Figure 3.3: Percentages of participants choosing the normal and longer version of the questionnaire on days 1 and 2 in Experiment 2.

Exploratory analyses

Mediation of condition on QC2 through QC1. To examine a possible mediation effect of QC1, a mediation analysis according to the steps described in Iacobucci (2012) was conducted to test whether QC1 (normal/longer) mediated the effect of condition (experimental/control) on QC2 (normal/longer). The mediation analysis resulted in a significant *Z*-mediation value of 2.834 ($p = 0.005$). This means that the nudge influenced QC1, which, in turn, influenced QC2.

Mediation of QC1 on QC2 through attitude towards taking longer questionnaires and prosocial identity. Participants choosing the longer questionnaire on day one had a significantly more favourable

attitude towards taking longer questionnaires ($M = 5.06$, $SD = 1.15$) than participants choosing the normal questionnaire on day one ($M = 3.86$, $SD = 1.03$) ($t(1,034) = -17.213$, $p < 0.001$, $d = 1.091$). Moreover, participants choosing the longer questionnaire on day one also had a significantly higher prosocial identity ($M = 5.11$, $SD = 0.91$) than participants choosing the normal version on day one ($M = 4.94$, $SD = 1.00$) ($t(1,025) = -2.881$, $p = 0.004$, $d = 0.183$). However, the experimental and control condition did not differ significantly regarding their attitude towards taking longer questionnaires ($t(1,034) = -0.065$, $p = 0.948$) and their prosocial identity ($t(1,025) = 1.149$, $p = 0.251$).

A mediation analysis was conducted with the PROCESS macro for SPSS (model 4) using a 95 percentile bootstrap approach with 5,000 samples to test whether the attitude of the participants towards taking longer questionnaires and their prosocial identity mediated the effect of QC1 on QC2 (Hayes, 2017). The mediation analysis indicated a significant indirect effect of participants' attitude towards taking longer questionnaires ($B = 0.4930$, SE = 0.1067, 95% CI [0.2968, 0.7144]), but not for the prosocial identity of the participants ($B = -0.0059$, SE = 0.0180, 95% CI [0.0423, 0.0323]). This means that only participants' attitude towards taking longer questionnaires, and not their prosocial identity, mediated the effect of QC1 on QC2.[11]

Discussion

In Experiment 2, the results suggested a small temporal spillover effect of the nudge on QC2 and also a mediation effect of the nudge on QC2 through QC1. Moreover, participants' attitude towards taking longer questionnaires mediated the effect of QC1 on QC2. These findings suggest that the nudge affected the behaviour on day one, which affected participants' attitude towards the behaviour, which, in turn, affected the behaviour on day two. No mediating role of the prosocial identity of the participants was found. In Experiment 3, we aimed to see whether a temporal spillover effect could also be present in another domain in which choices are made on a daily basis: eating behaviour.

EXPERIMENT 3

In Experiment 3 (preregistered at AsPredicted: #41062), we aimed to test whether a temporal spillover effect could be observed by using food choice as our behaviour of interest, using the manipulation of Van Gestel et al. (2021). To this end, participants had to make choices in an online supermarket. Participants were randomly assigned to an experimental or control condition. In the experimental condition,

[11] Although we found a mediating effect of attitude towards taking longer questionnaires on the effect of QC1 on QC2, we did not find a direct mediating effect of attitude towards taking longer questionnaires ($B = 0.0084$, $SE = 0.0698$, 95% CI [-0.1284, 0.1516]) and prosocial identity ($B = 0.0042$, $SE = 0.0081$, 95% CI [-0.0089, 0.0240]) on the effect of condition on QC2.

participants were nudged into choosing the healthier option on the first day using a default nudge. Similar to the previous experiments, we expected to find (1) an effect of the nudge on the proportion of nudged food choices on day one (NFC1), (2) an effect of the nudge on the proportion of nudged food choices on day two (NFC2), (3) a mediation effect of the nudge on the proportion of nudged food choices on NFC2 through NFC1, and (4) a mediation effect of NFC1 on NFC2 through participants' attitude towards choosing healthier food products.

Methods

Participants and design

Participant recruitment and inclusion criteria were identical to those used in Experiment 1, with the exception that only people from the UK could participate, because some food stimuli are only available in the UK. A sample size calculation with the software program *G*Power 3.1.9.2*. resulted in a recommended sample size of 1,052 (with 0.90 power and a small effect size of $d = 0.2$). Averaging the dropout rate (92.13%) of Experiments 1 and 2 led to a necessary recruitment of 1,142 participants on day one. Participants were rewarded with £1.00.

On day one, 1,139 participants finished the study, of which 982 also finished the study on day two (86.2% response rate). Of these, 13 participants were excluded since they correctly guessed the aim of the study. Therefore, analyses were conducted on 969 participants. Of these participants, 485 (50.1%) had been randomly allocated to the experimental condition. The average age was 38.76 years ($SD = 13.54$), with 36.8% men, 63.0% women, and 0.2% indicating 'other.' The highest level of education completed by the participants was a high school diploma in 36.6% and a bachelor's degree in 39.3%.

The experiment was conducted with a 2 (between-subjects factor = Condition: experimental vs. control) × 2 (within-subject factor = Day: one vs. two) mixed design with the proportion of healthier food choices that were nudged on the first day (varying from 1 to 10) as the dependent variable. The attitude of the participants towards choosing healthier food products was added as a possible mediator.

Procedure

The procedure of Experiment 3 was similar to that of Experiment 2, with some minor alterations. Instead of asking participants whether they were willing to fill in a longer questionnaire, participants had to make ten hypothetical food choices. Every choice set consisted of four options with two unhealthy products and two healthier alternatives. In the experimental condition, one of the healthier products was nudged by preselecting it and making it more salient. Moreover, the identity measure was removed from the questionnaire since it was found not to mediate the effect in Experiment 2. Finally, at the end of the second day, participants were asked what they thought about the goal of this study.

Measures and materials

Demographics. In addition to measuring age, gender, and level of education as in Experiments 1 and 2, participants were now also asked how many hours and minutes ago they last ate and drank something. The answers were transformed into total numbers of minutes.

Manipulation. We used a task originally developed by Van Gestel et al. (2021). Participants were presented with 14 food choice sets in an online supermarket environment. All 14 food choice sets consisted of four options that could be chosen by the participant. Ten of these choice sets consisted of two unhealthy products and two healthier alternatives. Four of these choice sets consisted of solely unhealthy or healthier food products. These were only incorporated as filler choices. Participants were given the instructions to choose the product of their liking and to put it in their online grocery basket. Participants were randomly assigned to the experimental or control condition. In the experimental condition, one healthier product was preselected by putting a border around it and made salient by enlargement (an example trial of the task can be found in Van Gestel et al., 2021).

Since every (non-filler) trial consisted of two unhealthy and two healthier options and because only one healthier product was nudged, the chosen product to be nudged was randomised between the participants. The corresponding healthier food item in the control condition was always placed at the top left of the screen.

Attitude towards choosing healthier food products. Participants' attitude towards choosing healthy food products was measured with a semantic differential scale (as in Aertsens et al., 2011). The scale started with the sentence 'Choosing healthier food products is. . .' followed by seven-point Likert scales with various anchors (good/bad, positive/negative, satisfying/unsatisfying, enjoyable/unenjoyable, pleasant/unpleasant, and preferable/unpreferable). Participants had to provide answers on visual analogue scales ranging from 0 to 100. Cronbach's alpha ($\alpha = 0.856$) was deemed high enough to average the items into one scale.

Data preparation

Data preparation in this experiment was similar to that in Experiment 2.

Results

Confirmatory analyses

Randomisation check. A randomisation check showed no differences between the control and experimental condition regarding age ($t(966) = -1.340$, $p = 0.180$), gender ($\chi^2(1) = 0.390$, $p = 0.532$), level of education ($\chi^2(4) = 3.029$, $p = 0.553$), minutes after eating on day one ($t(967) = 0.221$, $p = 0.825$), and minutes after eating on day two ($t(942) = 0.325$, $p = 0.745$), indicating a successful randomisation.

Manipulation. To test whether the nudge had the intended effect on day one, a t-test was conducted with condition (experimental/control) as the independent variable and NFC1 as the dependent variable. As expected, NFC1 was significantly [$t(963) = -5.709$, $p < 0.001$] higher in the experimental (29.4%, 95% CI [27.6, 31.2]) than in the control (22.7%, 95% CI [21.2, 24.1]) group. This means that the manipulation was successful, although the effect was small ($d = 0.366$).

Temporal spillover effect. To test for a temporal spillover effect of the nudge on the behaviour after the removal of the nudge, a *t*-test was conducted with group (experimental/control) as the independent variable and NFC2 as dependent variable. NFC2 was larger in the experimental condition (25.5%, 95% CI [23.8, 27.1]) than in the control condition (23.5%, 95% CI [22.0, 25.1]). However, this difference was not significant ($t(961) = -1.680$, $p = 0.093$). This means that there was no temporal spillover effect of the nudge on NFC2.[12]

Mediation of condition on NFC2 through NFC1 and attitude towards choosing healthier food products. Participants with a higher NFC1 than expected based on chance (> 0.25) had a significantly more favourable attitude towards choosing healthier food products ($M = 76.40$, $SD = 15.81$) than participants with a lower NFC1 than expected based on chance (< 0.25) ($M = 63.97$, $SD = 17.05$) ($t(961) = 11.674$, $p < 0.001$, $d = 0.756$). However, the experimental and control condition did not differ significantly regarding their attitude towards choosing healthier food products ($t(965) = 0.017$, $p = 0.987$).

To examine a possible mediation effect of condition on NFC2 through NFC1 and the attitude of the participants towards choosing healthier food products sequentially, a mediation analysis was conducted with the PROCESS macro for SPSS (model 6) using a 95 percentile bootstrap approach with 5,000 samples (Hayes, 2017). The mediation analysis indicated a significant serial mediation effect of NFC1 and the attitude of the participants towards choosing healthier food products for the effect of condition on NFC2 ($B = 0.0017$, $SE = 0.0007$, 95% CI [0.0006, 0.0032]). This means that the nudge influenced NFC1, which, in turn, influenced the attitude of the participants towards choosing healthier food products, which, in turn, influenced NFC2.[13]

Discussion

In contrast to Experiments 1 and 2, the results of Experiment 3 did not point towards a temporal spillover effect of the nudge on food choice after its removal. However, as in Experiment 2, we found a chain of mediation in which the nudge affected food choice on the first day, which in turn affected

[12] Data would initially be analysed by using a Repeated Measures ANOVA. However, after careful consideration, we decided that such an analysis would not be best given the main question regarding the difference in food choice on the second day. Therefore, we chose similar approaches to analysing the data as in Experiments 1 and 2.

[13] Although we found a mediating effect of attitude towards choosing healthier food products on the effect of NFC1 on NFC2, we did not find a direct mediating effect of attitude towards choosing healthier food products ($B = -0.0017$, $SE = 0.0009$, 95% CI [-0.0038, -0.0002]) on the effect of the condition on NFC2.

the attitude of the participants towards choosing healthier food products, which in turn affected the food choice after removal on the second day. This means that the nudge was not able to impact food choice after its removal directly, but it does provide preliminary evidence for behaviour to influence attitudes, as predicted by self-perception theory (Bem, 1972).

GENERAL DISCUSSION

In the current paper, three experiments were conducted to systematically, and on an individual level, explore the effect of nudges on subsequent similar choices once they have been removed, i.e. their potential temporal spillover effect. In Experiments 1 and 2, the results seemed to point towards a small temporal spillover of the default nudge promoting prosocial behaviour. However, in Experiment 3, no temporal spillover effect was found for a default nudge promoting hypothetical healthier food choices. In all three experiments, the initial choice predicted the subsequent non-nudged choice. Moreover, in Experiments 2 and 3, participants' attitude towards the nudged behaviour was measured and found to mediate the effect of the initial behaviour on the behaviour after the removal of the nudge.

These results seem to suggest that a single encounter with a nudge can affect subsequent similar behaviour after the removal of the nudge. More specifically, the mediating effect of the initial behaviour indicated that nudges are able to *prolong* their effects. This implies that it is a prerequisite of the nudge to affect the initial behaviour for it to continue after removal. However, these small temporal spillover effects do not occur for all behaviours as they were only found for prosocial behaviour and not for food choice. Future research is needed to see whether these mixed findings are indeed related to the behavioural domain or whether there are other reasons for this inconsistency.

That is, the experiments in which the small temporal spillover effect was found (Experiments 1 and 2) differed from the experiment in which no temporal spillover effect was found (Experiment 3) beyond the difference in the behavioural domain (eating or prosocial). For example, in Experiment 3, participants had to make multiple choices consisting of four options every day. All these choices were nudged on the first day in the experimental condition. In Experiments 1 and 2, participants only had to make one choice consisting of two options per day. Additionally, in Experiments 1 and 2, the choice participants made had actual immediate behavioural implications, which was not the case in Experiment 3. Each of these, and other differences, including the difference in behavioural domain, may have affected the potential of the nudge spilling over to a subsequent decision. Future research is therefore required not only to replicate the findings of Experiments 2 and 3 but also to systematically explore these and other factors facilitating or hindering the potential of temporal spillover effects.

Regarding the hypothesis that a change in attitude may be one of the processes responsible for the temporal spillover effect, we found that the effect of the actual choice (which was influenced by

the nudge) on day one on the behaviour on day two was mediated by attitude towards the initial behaviour. In fact, although no temporal spillover effect was found, Experiment 3 showed some evidence for a sequential mediation model in which the nudge influenced the behaviour on day one, which influenced attitude towards the behaviour, which, in turn, affected the behaviour on day two. These results are in line with predictions from the self-perception theory of Bem (1972), which states that behaviour may follow attitudes but that behaviour can also be an input and a source for the formation of attitudes. In the current studies, participants were arguably not aware that an external stimulus, i.e. the nudge, affected their behaviour. To explain their own behaviour, they assigned it to a favourable attitude towards the behaviour, which, in turn, led them to continue the behaviour once the initial trigger (the nudge) was removed. Note that it is thus not the manipulation itself, but acting in line with this manipulation which is setting this attitude change and corresponding second choice in motion.

No evidence was found for the mediating role of participants' identity on the temporal spillover effect in Experiment 2. This may also explain why no spillover effects were found in previous studies (Ghesla et al., 2019; Kuhn et al., 2021), in which the behaviour was initially targeted by the nudge differed from the spillover behaviour (i.e. targeting behavioural instead of temporal spillovers) as such behavioural spillover effects would require attribution to more global cognitions affecting a broader range of behaviours. It should be noted that although an identity change did not mediate the temporal spillover effect of nudged prosocial behaviour in Experiment 2, it could have mediated the temporal spillover effect in Experiment 3. However, we did not measure healthy eating identity in Experiment 3. Therefore, we cannot yet draw a definitive conclusion about the role of identity in the temporal spillover effect.

Limitations and suggestions for future research

Some limitations of the current study should be noted. First, we only found temporal spillover effects in the behavioural domain of prosocial behaviour and not on food choice (although the sequence of events was similar to that of prosocial behaviour). Future research is needed to test whether the temporal spillover effect may be more pronounced in certain behavioural domains than others or whether the discrepancy between the experiments lies in the methodical differences like the multiple and hypothetical choices participants had to make in Experiment 3.

Second, the effect sizes of both the nudge manipulation and the temporal spillover effect were only small. In Experiment 1, the unexpected small effect size of the nudge made it necessary to include another wave of participants for the power to be high enough to detect such small effect sizes. In addition, the observed temporal spillover effect was only marginally significant, so the results of this study should be interpreted with care. Although the results of Experiment 1 were replicated and found to be significant in Experiment 2, still both the initial effect of the nudge as well as the temporal

spillover effect were small. We should be aware of the fact that the nudge manipulation needs to work for temporal spillover effects to occur and that small nudge effects are likely to yield small temporal spillover effects. However, the non-invasive nature of nudges does imply that they can be implemented on a large scale, which means that even small effects can impact behaviour on a population level.

Third, as attitudes were not measured at baseline, it cannot be ruled out that participants already had formed attitudes towards taking longer questionnaires before participation and therefore consistently chose the normal or longer version on both days. While this is theoretically possible, this alternative explanation seems unlikely in view of the novelty of the target behaviour in the two studies in which we observed a temporal spillover effect (Experiments 1 and 2). That is, in these studies we explicitly chose to target a relatively unfamiliar choice—between a normal and a longer version of a questionnaire—for most participants, for which they likely did not have *a priori* attitudes. In addition, participants were randomly assigned to a nudge or control condition, which affected their attitudes and subsequent behaviour, making it unlikely that baseline differences in attitudes are responsible for the observed effects.

Fourth, while the present study focused on attitude change as the mechanism driving the temporal spillover effect, other potential mechanisms explaining the temporal spillover effect should not be ruled out. As mentioned in the introduction, one such mechanism may be the desire to act consistently (Dolan and Galizzi, 2015). Andrade and Ariely (2009) argue that such direct behavioural consistency is especially likely when contexts are highly similar, such as in the present studies where we investigated temporal spillover effects. Future research should be conducted to investigate the possibility and relative share of these processes in temporal spillover effects.

Fifth, when interpreting the findings of the present studies, it is also important to realise that we used only one type of nudge: a default nudge. Whether or not the potential of nudges to yield temporal spillover effects is restricted to defaults as a specific type of nudge remains unclear and should be explored in future research.

Finally, we only showed the temporal spillover effect for similar choices in similar contexts after one day. Future research should investigate whether spillover effects generalise to other behaviours and/or contexts and whether conclusions can also be generalised to behaviours with increasing time gaps.

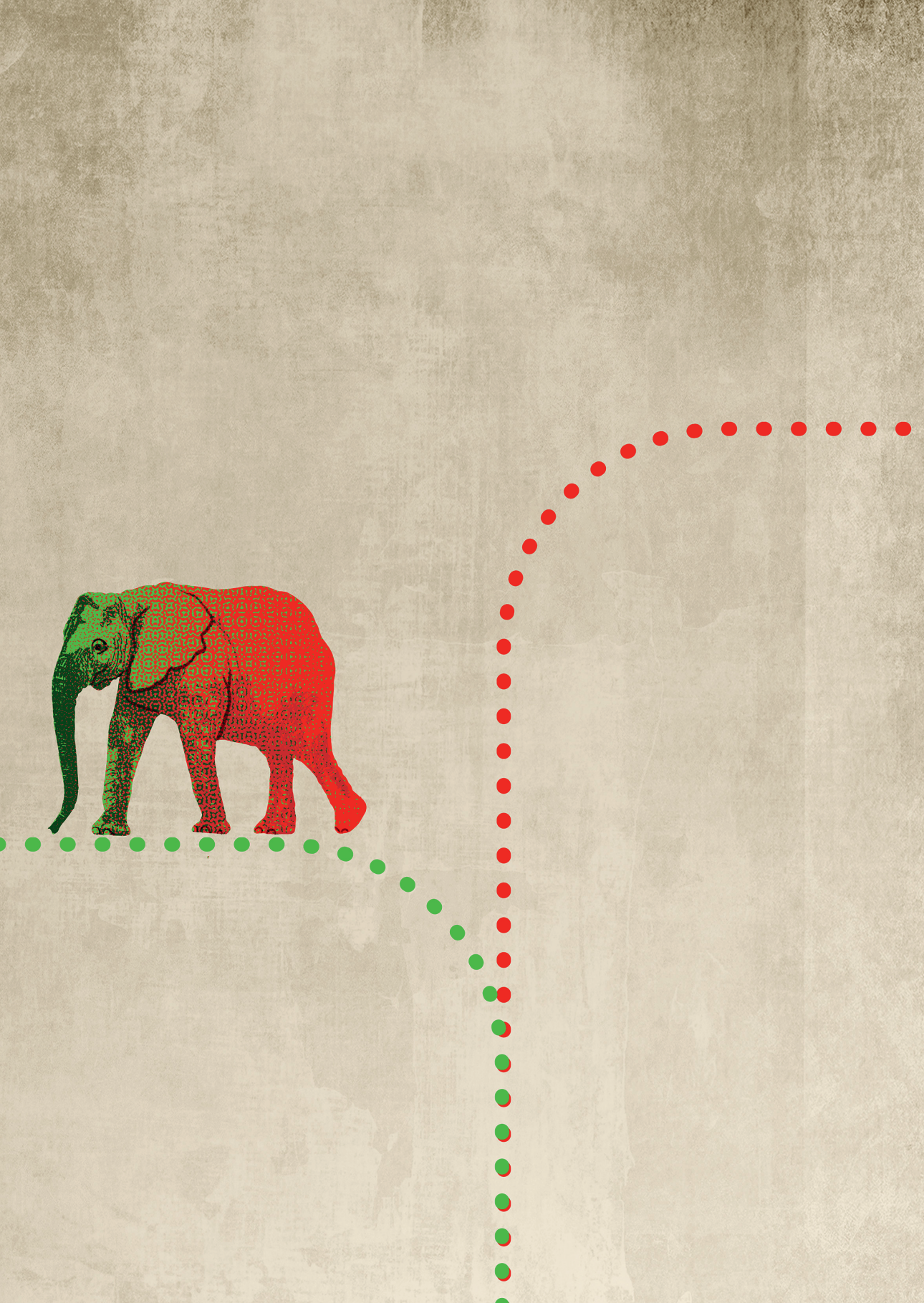
CONCLUSION

Notwithstanding these limitations, the present findings are among the first to show that default nudges can influence later choice decisions without nudges even after a single exposure, with the initial behaviour serving as an input for the later behaviour through an altered attitude. This result is a promising first step in the assessment of the extended influence of nudges although replication of

these findings is warranted. Future research is also needed to examine this effect in the case of other behaviours and in other (real-life) situations.

ETHICAL STATEMENT

The studies involving human participants were reviewed and approved by the Social Sciences Ethics Committee Wageningen University. The patients/participants provided their written informed consent to participate in this study.



CHAPTER 4

The effect of transparency on the temporal spillover effect of default nudges

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ABSTRACT

Previous studies show that the effects of (non-transparent) nudges can spill over to later similar decisions without nudges. In the current study, we aimed to determine whether such nudge temporal spillover effects are affected by making nudges transparent. The latter is recommended to (partly) mitigate ethical concerns surrounding the use of nudges. In two experiments, we nudged participants to complete a longer version of a survey. Participants were randomly assigned to either a control, a non-disclosed nudge (using a default to promote taking the longer survey) condition or a disclosed nudge (in which the use of the default nudge was explained) condition. In both Study 1 ($N = 1,270$) and Study 2 ($N = 1,258$) we found that the temporal spillover effect was not affected by the transparency message. This suggests that the small temporal spillover effect of the default nudge that was observed is robust to transparency manipulations.

INTRODUCTION

Initial evidence suggests that the effect of a nudge on a single decision can spill over to a subsequent similar decision under the same circumstances when the nudge is no longer present (Van Rookhuijzen et al., 2021). This reaffirms the notion that the use of nudge interventions is promising, especially when taken into account that nudges affect behaviour without forbidding any options or changing economic incentives (Thaler & Sunstein, 2008, p. 6). However, the use of nudges is not free of ethical scrutiny, with concerns being risen about autonomy and manipulation (Schmidt & Engelen, 2020). A proposed solution to (partly) mitigate these concerns, is to explain the use of the nudge to the nudgee, i.e. making the nudge transparent. The current paper tries to investigate the role of transparency in the temporal spillover effect of nudges.

Spillover effects

After the publication of Thaler and Sunstein's book 'nudge' in 2008, a rapid increase in interventions aimed at changing behaviour through contextual alterations followed. This is not surprising, considering that nudges can affect a large variety of behaviours, ranging from tax compliance (Antinyan & Asatryan, 2020) to food intake (Arno & Thomas, 2016), without forbidding any options and thus maintaining the freedom to choose. Unfortunately, most nudging studies only look at the effect of a nudge on a single choice, which, for some behaviours, only is a proverbial drop in the ocean. For example, one single healthy food choice does not have much impact on overall healthy eating patterns or weight. In such cases, nudges need to be able to influence more than one choice to have a meaningful impact.

Although most studies examining the effect of nudges focus on single choices, there is growing interest in the question of whether the effects of nudges can spill over to subsequent behaviour after the nudge has been removed. General spillover effects, in which behaviour influences subsequent behaviour, are well known and documented in the broader psychological and decision-making literature (Dolan & Galizzi, 2015). However, empirical evidence examining the spillover effects of behaviour following nudges specifically is scarce. The limited evidence suggests that spillover effects following nudges are not likely (Donkers et al., 2020; Ghesla et al., 2019; Kuhn et al., 2021; Zimmerman & Renaud, 2021). However, all of these studies investigated the effect of initially nudged choices on similar, but different choices, which we will call *behavioural* spillover effects. For example, Kuhn et al. (2021) successfully made use of a default nudge to promote organically produced products in a hypothetical online shop scenario. However, they did not find any spillover effects, which may have been caused by presenting participants with choices between products that were different from initially nudged.

In Van Rookhuijzen et al. (2021), however, initial evidence was found for the ability of a nudge to influence subsequent similar behaviour under the same circumstances once the nudge is removed,

labelled the *temporal* spillover effect. In that study, participants had to choose between completing a normal version or a longer version of a bogus lifestyle survey. Participants were randomly assigned to a no-nudge or a default nudge condition in which the choice for the longer version was preselected. A day later, all participants were again asked to choose between taking the normal or the longer survey. However, this time the nudge was removed in the nudge group. Results showed that, on both days, participants who had been nudged on the first day were more likely to choose the longer version. This suggests that nudges can have effects on similar subsequent decisions, even after the nudge is removed.

Importantly, results of the study of Van Rookhuijzen et al. (2021) showed that the temporal spillover effect may partly be attributed to a changed attitude towards the nudged behaviour. This means that nudging participants into taking the longer survey changed their attitude towards taking longer surveys, which, in turn, affected their behaviour when the nudge was removed. The theoretical basis for this can be found in Bem's Self Perception Theory (1972) and is related to the concept of self-herding by Ariely and Norton (2008). Here, attitudes are theorized to be inferred from behaviour. That is, when people are confronted with their behaviour, and especially when one cannot find an external source to explain his or her own behaviour, they are assumed to infer attitudes by looking at behaviour just like an outside observer. This rationale also aligns with the observation that people often do not recognize that their behaviour was influenced by a nudge (e.g. Dhingra et al., 2012; Van Gestel et al., 2018), making it prone to (mis)attribution to attitudes.

Aside from a changed attitude, the desire to act consistently may also have been responsible for the observed temporal spillover effect in Van Rookhuijzen et al. (2021) (Dolan & Galizzi, 2015; Falk & Zimmermann, 2013). One might simply behave as one previously behaved without any mediating cognitions. According to Andrade and Ariely (2009), such direct (or as Andrade and Ariely call them, mechanical) consistency effects are especially likely when two contexts are highly similar. This may explain why temporal, but not behavioural, spillovers following nudges have been found. Taken together, there is some initial evidence suggesting that nudges may have the potential of a small temporal spillover effect. If it is indeed the case that nudges have temporal spillover effects after being removed, this implies that even for those behaviours that need to be affected multiple times before any impact becomes noticeable, such as healthy food choices, or contexts in which nudges cannot be sustained over time and may be removed (e.g. Van Rookhuijzen & De Vet, 2021), nudges can be promising intervention tools.

Nudge transparency

Unfortunately, however, despite promising results, the use of nudges is not uncontroversial. Concerns have been risen about nudges curtailing individuals' autonomy, being manipulative and being vulnerable to being abused in line with the goals of the nudger instead of the nudgee (for further discussion

see Schmidt & Engelen, 2020). A proposed solution to mitigate these concerns is by making nudges transparent, meaning that the intention of the nudge, and/or the way the choice architecture is designed to manipulate a choice are disclosed to the nudgee. The reasoning behind this is that such nudge transparency would prompt people to reflect on the choice they are making and to consider whether the nudge promoted a decision that is best for themselves (Hansen & Jespersen, 2013). For a long time, however, it was thought that nudging 'works best in the dark' and that transparency would even hinder nudge effectiveness through compensatory reactance (Bovens, 2009). However, empirical evidence suggests that this concern is not valid as transparency manipulations do not hamper, or even increase, the effectiveness of nudges (Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Paunov et al., 2019a; Paunov et al., 2019b; Steffel et al., 2016). As making nudges transparent does not negatively impact effectiveness, and mitigates some of the ethical concerns surrounding the use of nudging (but see Wachner et al., 2020 for a study showing that ethical concerns related to nudging may not be supported by the evidence), it would seem that there are no reasons to avoid their use.

However, in view of the recent finding on the potential temporal spillover effect of nudges, a novel concern about using transparency manipulations may be that they negatively influence the temporal spillover potential of nudges. That is, it could be argued that processes responsible for the temporal spillover effect could become interrupted when the nudge is made transparent. The previously discussed attitudinal change following the nudge that (partly) explained the temporal spillover effect (Van Rookhuijzen et al., 2021) may be dependent on the absence of external attributions in order to promote internal attributions to attitudes. This effect could be reduced when the nudge is made transparent. By making a nudge transparent, a clear external source for one's behaviour can be found in the nudge, which could potentially make internal attribution less likely. If this were the case, then the mechanism for the temporal spillover effect may become interrupted, resulting in an absent or less pronounced temporal spillover effect. However, if the temporal spillover effect is mainly driven by a need to act consistently, it should not be affected by transparency. Such a need is arguably not altered by whether the source of the initial behaviour is internally or externally attributed.

To our knowledge, there have only been two studies examining the role of transparency in the spillover effect. Loewenstein et al. (2015) report on a study in which both non-transparent and transparent defaults were used in a hypothetical scenario to nudge participants into making decisions on specific medical life-prolonging interventions. After participants in all conditions were made aware of the presence of the nudge, participants had to make the same decisions again without default. Results showed that both the non-transparent and transparent defaults affected both the first and second decisions, indicating that transparency did not affect the results. In contrast, Michaelsen et al. (2021b, study 2) did not find support for any spillover effects. Here, both non-transparent and

transparent defaults were used to nudge participants' decisions about whether or not to complete an additional survey without extra payment. Thereafter, participants were asked whether they would want to sign-up for a future survey for which they would also not get any extra payment (which was never administered). Although both the non-transparent and transparent default affected the first decision, no evidence was found for a spillover effect in both conditions.

Still, it is hard to draw any conclusions on the potential negative effects of transparency on the temporal spillover effect from these studies. Both the studies of Loewenstein et al. (2015) and Michaelsen et al. (2021b, study 2) did not include a control group without any nudge, limiting the extent to which conclusions can be drawn about the (spillover) effects of (transparent) nudges in contrast to having to make an active initial decision. Moreover, in Loewenstein et al. (2015), participants in all conditions were told about the use and purpose of the default nudge before they had to make the second decision, preventing this design to draw any conclusions about the effect of transparency on the spillover effect. Furthermore, Loewenstein et al. (2015) used hypothetical choice tasks. Although participants' first choice did have real consequences in the study of Michaelsen et al. (2021b, study 2), in the second choice they were asked to make a decision that would only affect them in the future, when they still could easily opt-out of their initial decision (which was never actually measured), limiting the extent to which the conclusions can be generalised to decisions bearing real-life consequences.

Current study

In the current study, we build upon previous research investigating the effect of nudge transparency on the temporal spillover effect by conducting two experiments with the inclusion of control groups and decisions with real-life consequences for participants. In both experiments we made use of a nudge manipulation that has been previously found effective (Paunov et al., 2020; Wachner et al., 2020). Participants were asked to complete a longer survey without receiving extra reimbursement on two consecutive days. On the first day, participants 1) had to either actively choose the normal or longer survey (control condition), 2) were nudged into taking the longer version by preselecting this option (non-disclosed nudge condition), or 3) were nudged into taking the longer version by preselecting this option + were given a transparency message explaining the goal of the preselection (disclosed nudge condition). On day two, all participants had to actively choose a version of the survey without a nudge present. We were interested in the percentage of participants in the different conditions choosing the longer survey on the second day.

STUDY 1

In Study 1[14] (preregistered at AsPredicted: #48249), we aimed to assess whether nudge transparency would influence the earlier found temporal spillover effect of a default nudge. Temporal spillover effects were assessed in both a non-disclosed and disclosed nudge condition relative to a control condition. We expected that on day one, participants in the non-disclosed and disclosed nudge condition would choose the longer version of the survey more often than participants in the control condition. Moreover, we expected that this effect would spill over to the survey choice on day two in the non-disclosed nudge condition. We had no a priori predictions about whether the effect of the nudge in the disclosed nudge condition would also spill over to the second day. Study 1 was approved by the Social Sciences Ethics Committee of Wageningen University.

Methods

Participants

The software *G*Power* 3.1.9.2. was used to compute the required sample size. To detect a small effect ($w = .1$, based on an earlier study we conducted using the same manipulation (Van Rookhuijzen et al., 2021)) of the type of nudge on the decision to choose the normal or longer survey on the first and second day with a power of .90 a sample size of 1266 was recommended. We recruited 10% ($n = 127$) extra participants on the first day, to account for possible drop-out.

The online platform Prolific Academic was used to recruit participants. Inclusion criteria were: 1) being aged 18 or older, 2) speaker of the English language, 3) two or more previous submissions on Prolific Academic, 4) a 95% or more approval rate on Prolific Academic, and 5) having indicated to be willing to participate in research in which one will initially be unaware of its purpose. Moreover, we encouraged participants to solely participate on a desktop computer to keep the circumstances of both choices as similar as possible. Participants were rewarded with £2.00 for their participation, which covers the minimum required amount of compensation set by Prolific for completing two 12-minute long surveys. In other words, all participants where thus compensated at a higher than usual payment.

The survey on day one was completed by 1,392 participants, of whom 1,270 participants also completed the survey on day two. Participants were randomly assigned to the control condition ($n =$

[14] For the sake of transparency and to avoid file-drawer problems in potential future meta-analyses, we find it important to mention that Study 1 was preceded by an earlier study, which was highly similar to Studies 1 and 2, but with some minor methodological differences (preregistered at AsPredicted: #44203). In this earlier study we did not find any spillover effects of the nudge in both the non-disclosed and the disclosed nudge conditions. As this prohibited us from investigating the potential moderating influence of transparency on the spillover effect, this study was omitted from this paper. Data is available upon request.

413, 32.5%), the non-disclosed nudge condition ($n = 425$, 33.5%) or the disclosed nudge condition ($n = 432$, 34.0%). The average age of the participants was 30.00 ($SD = 10.91$) years, with 54.2% males, 45.4% females and 0.5% indicating 'other'. Most participants (38.7%) indicated a high school diploma or a bachelor's degree (37.7%) as their highest-achieved education. Participants mostly indicated the UK (32.7%) or Poland (11.7%) as their nationality, although as many as 59 different nationalities were mentioned among participants.

Design

The experiment used a 3 (between-subjects factor = Condition: control vs non-transparent nudge vs transparent nudge) x 2 (within-subject factor = Day: one vs two) mixed design with survey choice (normal/longer) as dependent variable.

Procedure

Participants filled in a survey on two consecutive days. On day one participants started by giving informed consent for their participation, after which some demographics were asked. After that, participants were randomly assigned to one of the three conditions in which they were asked to indicate whether they were willing to fill in a longer version of the survey without receiving additional payment. Our manipulation consisted of different ways in which this question was presented in the different conditions (see *Manipulation*). Next, participants had to fill in a bogus survey.

On the second day, participants were again asked about some demographics and they were again presented with a choice between a normal and a long version of a survey and had to indicate which version of the survey they wanted to fill in. However, this question was now similarly presented in all conditions as in none of the conditions a nudge was present. After that, participants again had to fill in a bogus survey. The survey ended by thanking participants for their participation and by debriefing them about the real purpose of the study, after which they could leave any remaining comments.[15]

Measures and materials

Demographics. At the beginning of the surveys on both days, participants were asked to provide demographics, including age, gender, nationality and their highest level of education.

Manipulation. We used a similar paradigm as Wachner et al. (2020). On the first day, all participants were asked whether they wanted to complete a normal, 7-minute version of the survey, or whether they would be willing to complete a longer, 12-minute version of the survey. Participants were

[15] After completing the survey on day two, participants were asked about the extent to which they thought the preselection influenced their decision of choosing the normal or longer version of the survey. However, since this variable was only measured for exploratory purposes, results will not be presented here, but can be found in the Supplementary File 3.

informed that there was no additional reimbursement for the longer version of the survey, but that it would help researchers with improving future surveys. Participants in the control condition had to actively choose between the normal and longer survey. In the non-disclosed nudge condition, taking the longer survey was promoted by preselecting it and making it bold. The disclosed nudge condition was similar to the non-disclosed nudge condition, with the addition of the text: '*Please note the preselected default option. It is meant to encourage people to choose the longer version of this questionnaire. People are usually unaware of its influence*.' (identical to the *purpose + unawareness* condition in Wachner et al. (2020)). On the second day, all participants were again asked whether they wanted to complete the normal or longer survey without receiving extra reimbursement. However, in all three conditions, participants had to actively choose which version they wanted to complete without the use of a nudge and transparency message.

Bogus surveys. After participants indicated which version they wanted to complete, a bogus survey which was said to be about 'lifestyle, personality and eating behaviour' had to be completed to divert participants from the real aim of the study and to have them actually fill in a normal or longer version (the longer version was estimated to take 5 more minutes than the normal version) on both days. Items were made up or taken from existing questionnaires.

Data preparation. As preregistered, we only included participants in the analyses when they completed the surveys on both days. They were contacted when answers on demographic variables were not identical for both days (e.g. two different ages). 'Other' entries for education were transformed into one of the listed options. Cells with less than five observations were set to missing when Chi-square tests were conducted.

Results

Confirmatory analyses

Randomisation check. ANOVA and chi-square tests were conducted to check for unintended differences in demographics across the experimental conditions. Results were consistent with successful randomisation, with results indicating no differences between the conditions on age ($F(2) = .754$, $p = .471$), gender ($\chi^2(2) = .761$, $p = .683$), nationality ($\chi^2(22) = 20.340$, $p = .562$) and level of education ($\chi^2(8) = 2.871$, $p = .942$).

Manipulation. Figure 4.1 displays the percentages of participants opting for either version of the survey on day one and two in the control, non-disclosed and disclosed nudge conditions. On day one, the longer survey was chosen by 40.7% in the control condition, 44.7% in the non-disclosed nudge condition and 56.9% in the disclosed nudge condition. To test the effect of the nudge and disclosed nudge on day one, a chi-square test with condition (control/non-disclosed nudge/disclosed nudge) as

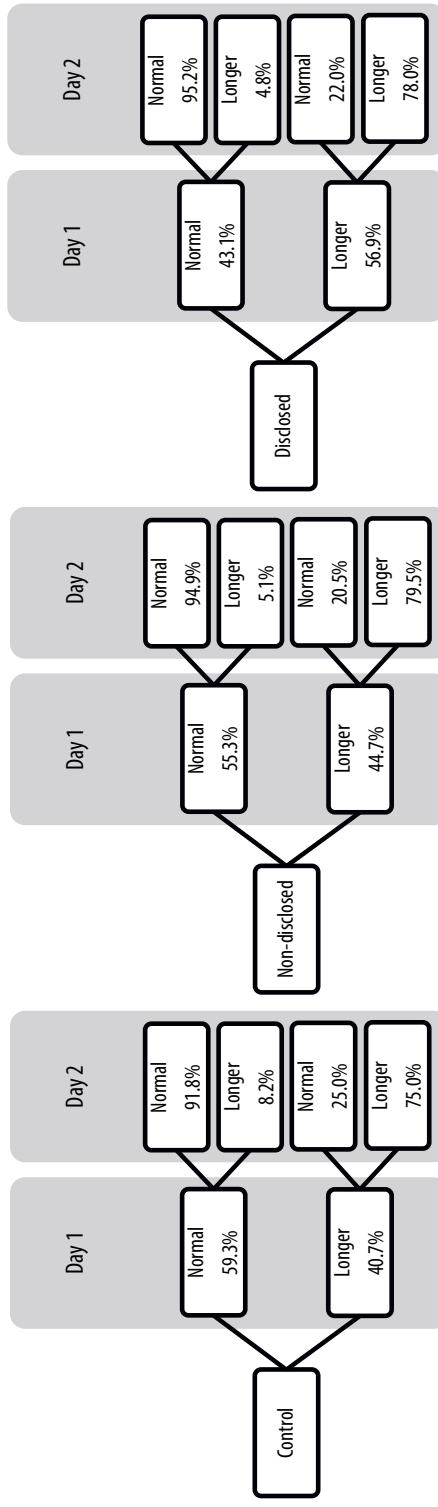


Figure 4.1: Percentages of participants opting for the normal and longer versions of the survey on days one and two in Study 1.

independent variable and survey choice day one (normal/longer) as dependent variable was conducted, which was significant ($\chi^2(2) = 24.486$, $p < .001$, $\varphi = .139$). Post-hoc chi-square tests between the different conditions were then conducted. These showed no significant difference between the control and non-disclosed nudge conditions on survey choice day one ($\chi^2(1) = 1.389$, $p = .239$). However, significant differences were found between the control and disclosed nudge conditions ($\chi^2(1) = 22.356$, $p < .001$, $\varphi = .163$) and the non-disclosed and disclosed nudge conditions ($\chi^2(1) = 12.839$, $p < .001$, $\varphi = .122$). This means that participants in the disclosed nudge condition were more likely to choose the longer version on day one than in the control and non-disclosed condition. However, no significant differences were found between the control and non-disclosed nudge conditions.

Temporal spillover effect. On day two, the longer survey was chosen by 35.4% in the control condition, 38.4% in the non-disclosed nudge condition and 46.5% in the disclosed nudge condition. To test whether the different conditions differed on survey choice day two, a chi-square test with condition (control/non-disclosed nudge/disclosed nudge) as independent variable and survey choice day two (normal/longer) as dependent variable was conducted, which was significant ($\chi^2(2) = 11.841$, $p = .003$, $\varphi = .097$). Post-hoc chi-square tests between the different conditions were then conducted. These showed no significant difference between the control and non-disclosed nudge conditions on survey choice day two ($\chi^2(1) = .811$, $p = .368$). However, significant differences were found between the control and disclosed nudge conditions ($\chi^2(1) = 10.898$, $p = .001$, $\varphi = .114$) and the non-disclosed and disclosed nudge conditions ($\chi^2(1) = 5.860$, $p = .015$, $\varphi = .083$). This means that participants in the disclosed nudge condition more often chose the longer version on day two than participants in the control and non-disclosed nudge conditions, indicating that the effect of the transparent nudge that was observed on day one spilled over to day two.

Mediation effect of survey choice day one. To test for an indirect effect of condition on survey choice day two through survey choice day one, three mediation analyses were conducted (see Iacobucci, 2012). The mediation analysis with the control and non-disclosed nudge conditions yielded a *Z*-mediation value of 1.17, which was not significant ($p = .242$) The mediation analysis with the control and disclosed nudge conditions resulted in a significant *Z*-mediation value of 4.51 ($p < .001$). Lastly, the mediation analysis with the non-disclosed and disclosed nudge conditions also resulted in a significant *Z*-mediation value of 3.48 ($p < .001$). This means that survey choice day one mediated the effect of condition on survey choice day two when comparing the control and non-disclosed conditions with the disclosed nudge condition, but not when comparing the control condition with the non-disclosed condition.

Discussion

Results from Study 1 show that the nudge manipulation was only able to affect the decision to choose the longer survey on the first and second day when its aim was disclosed to participants. Although in the studies of Van Rookhuijzen et al. (2021) the nudge manipulation was also successful when the aim of the nudge was not disclosed, the results of Study 1 do suggest that nudge transparency does not (negatively) affect the temporal spillover effect of nudges.

STUDY 2

The results of Study 1 suggest that transparency does not influence the temporal spillover effect of nudges. However, because the nudge manipulation (and the temporal spillover effect) in the non-disclosed nudge condition was not significant, no conclusions can be drawn about the spillover effect of the transparent nudge relative to the spillover effect of the non-transparent nudge. Therefore, the methods of Study 1 were replicated in Study 2 (preregistered at AsPredicted: #51288) to be able to draw more definitive conclusions. Study 2 was approved by the Social Sciences Ethics Committee of Wageningen University.

Materials and methods

Participants

Sample size calculation, inclusion criteria and participant recruitment were identical to Study 1. The survey on day one was completed by 1,389 participants, of whom 1,258 participants also completed the survey on day two. Participants were randomly assigned to the control condition ($n = 418$, 33.2%) the non-disclosed nudge condition ($n = 422$, 33.5%) or the disclosed nudge condition ($n = 418$, 33.2%). The average age of the participants was 27.30 ($SD = 9.58$) years, with 61.4% males, 37.8% females and 0.8% indicating 'other'. Most participants (46.7%) indicated a high school diploma or a bachelor's degree (30.8%) as their highest achieved education. Participants mostly indicated Poland (26.9%) or UK (21.1%) as their nationality, although as many as 61 different nationalities were mentioned among participants.

Design, procedure and measures and materials

The design, used procedure, measures and materials, and data preparation were identical to Study 1.

Results

Confirmatory analyses

Randomisation check. ANOVA and chi-square tests were conducted to check for unintended differences in demographics across the experimental conditions. Results were consistent with successful randomisation, with results indicating no differences between the conditions on age ($F(2) = 407.064$, $p = .109$), gender ($\chi^2(2) = 1.193$, $p = .551$), nationality ($\chi^2(18) = 13.523$, $p = .760$) and level of education ($\chi^2(8) = 6.136$, $p = .632$).

Manipulation. Figure 4.2 displays the percentages of participants opting for the normal and longer versions of the survey on day one and two in the control, non-disclosed and disclosed nudge conditions. On day one, the longer survey was chosen by 36.8% in the control condition, 49.1% in the non-disclosed condition and 55.0% in the disclosed nudge condition. To test whether both nudges had the intended effect on choice on day one, a chi-square test with condition (control/non-disclosed nudge/disclosed nudge) as independent variable and survey choice day one (normal/longer) as dependent variable was conducted, which was significant ($\chi^2(2) = 28.833$, $p < .001$, $\varphi = .151$). Post-hoc chi-square tests between the different conditions were then conducted. These showed significant differences between the control and non-disclosed nudge conditions ($\chi^2(1) = 12.775$, $p < .001$, $\varphi = .123$) and between the control and disclosed nudge conditions ($\chi^2(1) = 27.820$, $p < .001$, $\varphi = .182$), but not between the non-disclosed and disclosed nudge conditions ($\chi^2(1) = 3.000$, $p = .083$). This means that the nudge indeed had the intended effect, both with and without a transparency message.

Temporal spillover effect. On day two, the longer survey was chosen by 33.5% in the control condition, 40.5% in the disclosed nudge condition non-disclosed nudge condition and 47.8% in the disclosed nudge condition. To test whether the different conditions differed on survey choice day two, a chi-square test with condition (control/non-disclosed nudge/disclosed nudge) as independent variable and survey choice day two (normal/longer) as dependent variable was conducted, which was significant ($\chi^2(2) = 17.856$, $p < .001$, $\varphi = .119$). These showed significant differences between the control and non-disclosed nudge conditions ($\chi^2(1) = 4.449$, $p = .035$, $\varphi = .073$) and between the control and disclosed nudge conditions ($\chi^2(1) = 17.846$, $p < .001$, $\varphi = .146$). Moreover, a significant difference was found between the non-disclosed and disclosed nudge conditions ($\chi^2(1) = 4.570$, $p = .033$, $\varphi = .074$). This means that the effect of the nudge, both with and without transparency message spilled over to day two.

Mediation effect of survey choice day one. To test for an indirect effect of condition on survey choice day two through survey choice day one, three mediation analyses were conducted (see Iacobucci, 2012). The mediation analysis with the control and non-disclosed nudge conditions resulted in a significant *Z*-mediation value of 10.47 ($p < .001$), the mediation analyses with the control and disclosed nudge conditions resulted in a significant *Z*-mediation value of 5.03 ($p < .001$). However, the mediation

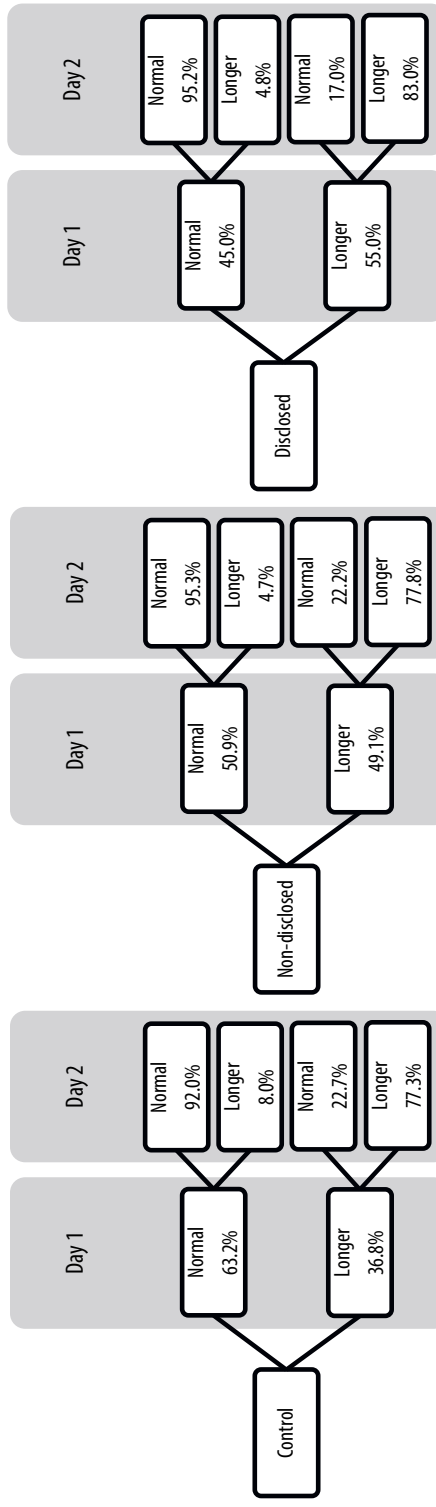


Figure 4.2: Percentages of participants opting for the normal and longer versions of the survey on days one and two in Study 2.

analysis with the non-disclosed and disclosed nudge conditions was not significant (Z-mediation = 1.73, $p = .084$). This means that the nudge (whether non-transparent or transparent) influenced survey choice on day, which, in turn, influenced survey choice day two. This mediation effect was not found when comparing the non-disclosed nudge condition with the disclosed nudge condition, which is most likely the result of the non-significant difference between these conditions on survey choice day one.

Discussion

Results from Study 2 demonstrate that the nudge manipulation was able to influence the decision to choose the longer survey in both the non-disclosed and disclosed nudge conditions. This effect spilled over to the next day in both conditions, with an even higher effect in the disclosed nudge condition than in the non-disclosed nudge condition. These results support our conclusion from Study 1: There is no indication that transparency would (negatively) affect the temporal spillover effect. In contrast, it may even aid the temporal spillover effect.

GENERAL DISCUSSION

In this study, we investigated the effect of making a default nudge transparent on its potential to spill over to a similar later choice. To this end, we conducted two experiments in which we assessed whether the effect of a transparent default nudge promoting prosocial behaviour also affected behaviour on the following day after the nudge had been removed. Taken together, we could not find any evidence indicating that making nudges transparent negatively affects the temporal spillover effect. In fact, the results of our studies even show that transparency may aid in increasing initial nudge effectiveness as well as temporal spillover effectiveness.

As discussed in the introduction, we theorized that the processes responsible for nudges' temporal spillover effects may be reduced by making them transparent. More specifically, we argued that the mis(attribution) of behaviour to positive internal states as a mechanism (partly) responsible for the temporal spillover effect, may be reduced or even absent when nudges are made transparent, revealing to participants that their behaviour may have been influenced by factors other than their own beliefs and cognitions. However, since we observed a temporal spillover with both non-disclosed and disclosed nudges, our results do not support this line of reasoning. It seems then, that consistency effects play an important role in explaining the spillover effect, whether or not people are made aware of the nudge. Regardless of the source of what was driving the behaviour, people have a tendency to be consistent and their behaviours. Alternatively, it may have been possible that despite our transparency manipulation, participants underestimated the effect the nudge had on their behaviour (see e.g., Van Gestel et al., 2018).

In any case, whatever the underlying mechanism(s) responsible for the temporal spillover of non-transparent and transparent nudges may be, results of our mediation analyses underline the importance of a focus on the initial behaviour of people, whether or not this was influenced by a nudge, since these first choices will act as an anchor for future behaviour. To change behaviour, it seems that it is of less importance how the behaviour is initially changed, because once it is changed, the behaviour serves as input for later decisions. However, it is also important to note that the spillover effect of the nudge was not as strong as its initial effect. Clearly, future research is required to assess whether and to what extent the spillover effect persists over time. Interestingly, the effects of the disclosed nudge were more pronounced than that of the non-disclosed nudge. Although it was clearly communicated to participants that they would not receive any extra payment, they may have felt an implicit demand to comply with the request to complete the longer survey. This demand may have been even more pronounced in the disclosed nudge condition, where participants were again told that it was encouraged to complete the longer survey. This could have led to increased compliance on the first day in the disclosed nudge condition and subsequently on the second day. However, it is unlikely that the presence of such a demand effect affects our conclusion that transparency does not negate the spillover effect of default nudges. More research is needed to investigate the role of experienced demand in the (spillover) effect of transparent nudges.

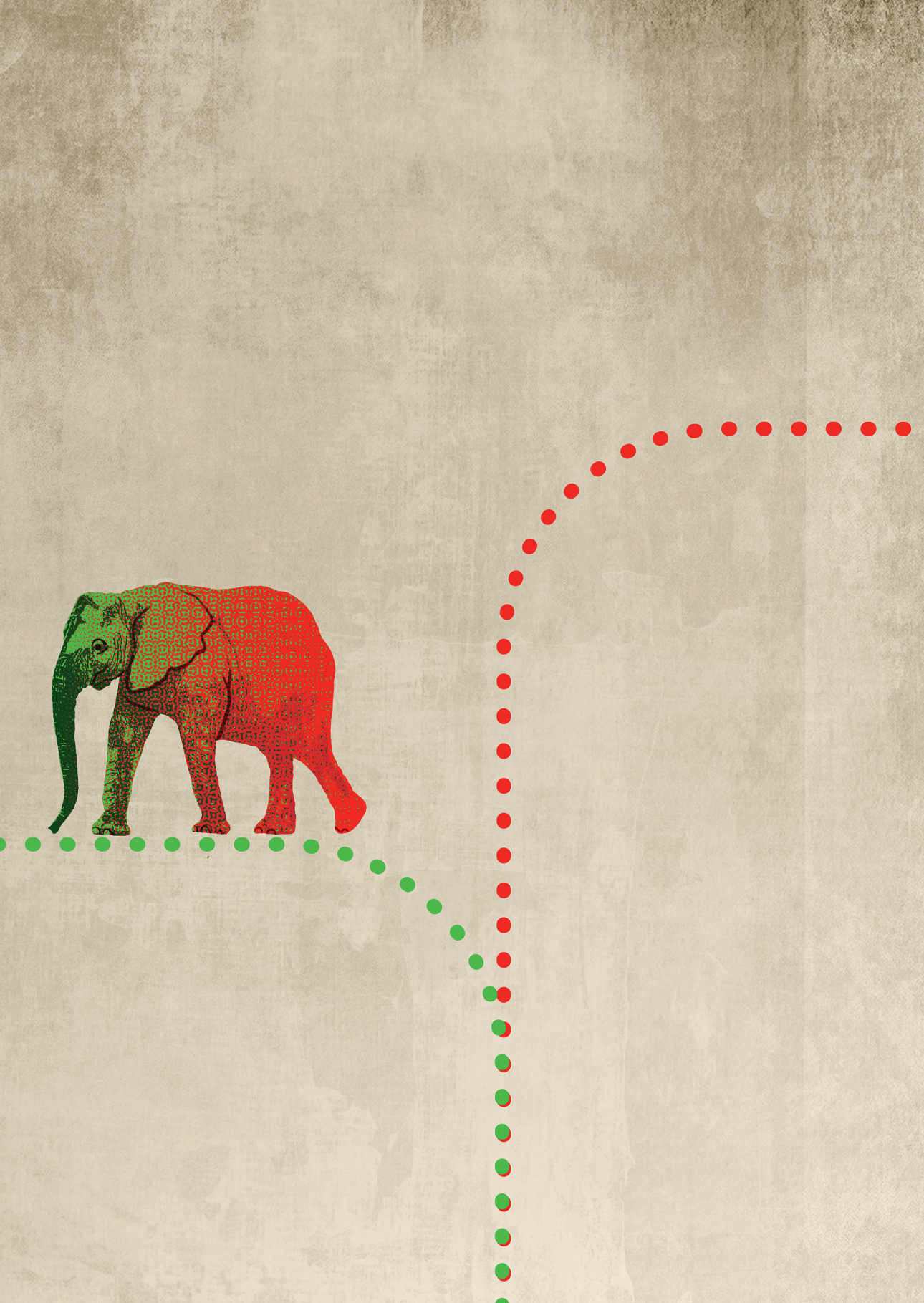
Limitations for future research

Although the current study supports the practice of disclosing the intention and the manner in which behaviour change is pursued when nudges are used to affect behaviour, there are still some limitations that warrant further investigation. First of all, effect sizes of the initial nudge effect as well as of the temporal spillover effect in both the transparent and non-transparent condition were small. This may be partly due to the specific context and the behaviour that was nudged. Future research should be conducted investigating whether more effective nudges also lead to bigger temporal spillover effects and whether this holds true for both non-transparent and transparent nudges.

Secondly, in both studies, we only used one type of nudge on one type of behaviour once, with one day in between measurements under highly controlled circumstances. These results cannot readily be generalized to other types of nudges and (real-life) situations. For example, in the control condition, participants had to actively choose a survey version, while the default may have led to more automatic decision making. Therefore, different processes may have been responsible for the initial and subsequent choice for the different conditions. Other types of nudges may still require more active decision-making, which may result in different (spillover) effects. Therefore, more research is needed to address the extent, and under which circumstances, to which transparent nudges can be used to influence behavioural patterns in real-life situations.

CONCLUSION

Nudge transparency is often opted as a solution for many of the ethical concerns surrounding the use of nudges to affect behaviour. The current study shows that transparent nudges might even be more effective than non-transparent nudges and that this effect continues once the nudge, and its transparency message, are removed, countering Bovens' (2009) assumption that nudges 'work best in the dark'. Although transparent nudges may not be completely void of all ethical scrutiny, this study shows that it does not harm immediate nudge effectiveness or temporal spillover effects. This suggests that more attention should be given to nudge transparency in policy-making. However, research on the effectiveness, processes and boundary conditions of transparent nudges is in its infancy. Future research should unravel whether this advice holds for all nudges in all circumstances.



CHAPTER 5

When nudgees become nudgers: Exploring the use of self-nudging to promote fruit intake

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Under revision

ABSTRACT

Background: The current study explored 1) the immediate and prolonged effects of self-nudges promoting fruit intake in the home environment, whether 2) the effect of self-nudges on fruit intake persists after self-nudges are no longer used (i.e. a temporal spillover effect) and 3) whether self-nudges can install healthy eating habits that, in turn, explain the temporal spillover effect.

Methods: Participants ($N = 331$) were randomly assigned to either a control or self-nudge condition in which they had to choose a self-nudge promoting fruit consumption for eight weeks. Thereafter, participants were asked to remove the self-nudge for one week to assess a possible temporal spillover effect.

Results: Results showed a positive effect of the self-nudges on fruit consumption right after implementation that continued during the eight weeks in which the self-nudge was implemented, which was accompanied by an increase in fruit intake habit strength. However, a mixed picture emerged regarding the temporal spillover effect and no support was found for a mediation effect of habit strength.

Conclusion: Although this study is only a first exploration of self-nudging to increase healthy food consumption, results indicate that self-nudging may be a promising extension of traditional nudging, that can influence behaviour beyond out-of-home settings.

INTRODUCTION

After years of research, there is now ample evidence showing that nudging can be used to effectively promote healthy food choices (e.g. Arno & Thomas, 2016; Cadario & Chandon, 2020). However, traditional nudges are mainly used to affect food choices in out-of-home settings such as supermarkets or restaurants. Still, most of our daily food choices and consumption happen in the private home environment, where nudges are not implemented by someone else (e.g. a policy maker), but we ourselves are the agent shaping our choice-architecture. With self-nudging, people are taught to use the principles underlying nudging and to alter their own surroundings in line with their behavioural goals. In this way, the nudgee becomes the nudger. In the current study, we investigated the immediate and prolonged effects of self-nudges promoting fruit intake. Furthermore, we explored whether these effects persist after the self-nudges are no longer present through the formation of habits.

Self-nudging

With the increasing prevalence of diseases related to unhealthy diets (GBD 2017 Diet Collaborators, 2019), effective interventions that sustainably improve healthy eating are desperately needed. It is therefore no surprise that numerous studies have been, and are being, conducted that investigate the best ways to promote (lasting) healthy eating behaviour. One popular approach that focuses on altering the environment to steer people's behaviour is called 'nudging'. Nudges are described as alterations of the environment (called choice architecture) that affect behaviour in a predictable way without forbidding any options and without changing their economic incentives (Thaler & Sunstein, 2008). To illustrate, in their study on the influence of container size on food intake Marchiori et al. (2012) found that participants ate more M&M's out of a large container than out of a small container, even though the containers contained a similar number of M&M's. Meta-analyses indicate that, overall, nudges are effective in increasing healthy food choices, with effect sizes ranging from small to medium (Arno & Thomas, 2016; Cadario & Chandon, 2020) (but see Maier et al. (2022) for a recent debate on the overall effectiveness of nudging).

Although past research indicated that nudges can be effectively used to affect single food choices, they are often implemented in out-of-home settings where someone else influences the choice architecture. These are generally environments where the food itself will not be consumed (e.g. supermarkets) or in environments where consumers will only sporadically consume food (e.g. football canteens or restaurants). Their impact on overall healthy eating patterns may therefore be limited. Most of our actual food consumption takes place in our homes. For example, in the Netherlands, 80% of our food consumption takes place at home (Van Rossum et al., 2020). It may therefore be worthwhile to investigate the use of nudging as an intervention tool used by individual themselves to stimulate

healthier food intake in people's homes. Although it may be difficult to alter the choice architecture in people's homes, a new way of using nudges has been proposed in which the person who is being nudged is also the choice architect: self-nudging.

Self-nudging can be described as structuring one's own environment in a way congruent with one's goals (Reijula & Hertwig, 2022). For example, somebody with the goal of changing their sedentary behaviour may adjust the height of a sit-stand desk to standing height at the end of the day to increase the chance of starting work standing the next day. In this sense, self-nudging shows similarities with other concepts that stress the importance of aligning one's surroundings with one's goals such as situational self-control (Duckworth et al., 2016) and stimulus control (one of the Processes of Change from the Transtheoretical Model, Velicer et al., 1998). Self-nudging may also be viewed as a specific form of boosting. The goal of boosting is to equip people with tools to make the right choices, by altering the decision-making environment or through extending decision-making skills (Hertwig & Grüne-Yanoff, 2017). Self-nudging does just this: it lets people in on the manner in which they can use their environment to achieve their goals.

Traditional nudging vs self-nudging

One of the key differences with traditional nudging is that the person who uses self-nudging is aware that nudges are used to steer their behaviour, because he or she is the choice architect themselves. Being unable to recognize the influence of the nudge used to be thought of as a key ingredient for nudges' effectiveness (Bovens, 2009). However, more recent studies examining the effect of explaining the aim of the nudge show that such nudge transparency does not compromise, and can even enhance, its effectiveness (Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Paunov et al., 2019a; Paunov et al., 2019b; Steffel et al., 2016). Awareness of the nudge when self-nudging may thus not compromise its effectiveness. In fact, so-called 'transparent nudges' have even been found to be more effective than non-transparent nudges. Better yet, such nudge transparency has been regarded as a solution to (partly) mitigate concerns surrounding the manipulative character of nudges and their potential negative effect on autonomy (Hansen & Jespersen, 2013; Wachner et al., 2020). Moreover, self-nudging per definition circumvents the critique that is often voiced against traditional nudging, namely that behavioural goals are defined by others and are exactly similar for all that encounter the nudge, even though individual needs may differ (Hausman & Welch, 2010; Qizilbash, 2011). With self-nudging, behavioural goals are set by the nudgee themselves. In other words: The nudgee becomes the nudger.

Self-nudging may thus seem as a promising addition to traditional nudging since it promotes food choices in a stable environment in which much of our actual food consumption takes place, and in a way that aligns with the individual's own goals in a transparent manner, making the nudge potentially

even more effective and alleviating ethical concerns related to autonomy and manipulation. In view of these potential benefits, the first aim of the present study is to assess whether self-nudging is effective and whether effects remain over time.

Temporal spillover effects

In addition to assessing the immediate and prolonged effects of self-nudges when they are in place, this study also aims to examine whether these effects continue once the self-nudge is no longer present. Because of their subtlety, features crucial to nudges' effectiveness may easily get changed or removed. For example, a study by Van Rookhuijzen & De Vet (2021) found that implemented nudges in two football canteens were often removed by others not involved in the study. Although this study examined nudges in out-of-home settings, home environments are not impervious to such effects, for example through family members that are not aware of the self-nudge. The question therefore arises how lasting the effects of self-nudges are once they are no longer in place; effects also known as temporal spillover effects.

Although an earlier study (Van Rookhuijzen et al., 2021) did find temporal spillover effects of a nudge promoting prosocial behaviour, no temporal spillover effects were found for a nudge promoting healthier food choices. However, the choices in that study involved only hypothetical food choices that were only nudged once. In the current study, a nudge is implemented and real food consumption is reported for an extended period of time (eight weeks). Such an extended period of time in which nudges repeatedly affect behaviour allows for forming habits that may be needed for any temporal spillover effects to occur.

While traditional nudging mostly encourages the sporadic consumption of healthier foods, self-nudging can be implemented in the home environment, which likely is a more stable environment in which many food choices take place. An advantage of such stability is that this increases the potential for habit formation. Habits are automatic behavioural tendencies in response to a cue (Wood & Neal, 2009). An advantage of habits is that they are hard to break and that they thus can have lasting effects on health in general. The third aim of this study is therefore to assess whether self-nudges can install healthy eating habits and whether these habits can induce behaviour when the self-nudge is no longer present, i.e. mediate the temporal spillover effect. In testing whether nudges are able to install habits we address one of the most central unanswered questions surrounding the use of nudges in the long run (Congiu & Moscati, 2022).

Current study

In the current study, we conducted an experiment to explore 1) the immediate and prolonged effects of self-nudges promoting fruit intake, and, if effective, whether 2) the effect of self-nudges on fruit

intake persists after self-nudges are no longer used (i.e. temporal spillover effect) and 3) whether self-nudges can install healthy eating habits that, in turn, mediate the temporal spillover effect. Hereto, a study was conducted in which participants were randomly assigned to either a self-nudge or control condition. Participants in the self-nudge condition were asked to choose a self-nudge and to implement this nudge for eight weeks. After these eight weeks, participants in the self-nudge condition were asked to remove the nudge for one week. Fruit intake and fruit intake habit strength were measured six times: at baseline, four times during the self-nudge intervention and one time at the end of the study when the self-nudge had been removed for one week to assess potential temporal spillover effects.

METHODS

Transparency and openness

In this article, we report how we determined our sample size, all data exclusions, all manipulations, and all measures that were included in the study. The study was approved by the Social Sciences Ethics Committee of Wageningen University. Data were analysed using R version 4.2.1 (R Core Team, 2020). This study was preregistered at https://aspredicted.org/G1J_Z4T (#63981).

Participants

Three hundred and forty-four participants[16] were recruited via the online crowdsourcing website *Prolific Academic*. To be eligible for participation participants had to 1) express an intention to increase their fruit consumption[17], 2) indicate to be working from home during the COVID-19 pandemic, 3) be aged

[16] As preregistered, sample size was calculated with the software *G*Power* 3.1.9.2. to detect a medium sized effect ($f = .25$) of the effect of group on fruit intake (one-way ANOVA) with a power of .90. This resulted in a recommended sample size of 172. To account for a drop-out rate of 50%, 344 participants were recruited at baseline. However, after data was collected, advancing insights resulted in the addition of a statistician to our team. Therefore, the use of linear mixed models was not planned. Therefore, this is not reported in the preregistration and is not used to base the sample sized used in this study on.

[17] To ensure that participants would have the intention to increase their fruit intake a pilot study ($N = 150$) was conducted to gauge the percentage of people with the intention to increase their fruit intake, operationalised as a mean of 4.00 or higher on the items 'I intend/plan/want to increase my fruit consumption' measured on a 7-point Likert scale ranging from totally disagree to totally agree. Since 76.6% of participants fulfilled this criterium, a pre-study ($N = 1,000$) was conducted measuring willingness to participate in the current 9-week study and intention to increase fruit consumption. Two hundred and ninety-eight participants were excluded because they indicated not to work from home. Of the remaining participants, 702 participants indicated to be willing to participate and had a mean of 5.00 or higher on their intention to increase their fruit consumption (Cronbach's $\alpha = .911$). These 702 participants were then invited to participate in the main study with 344 participant slots.

18 or older, 4) be of UK nationality and speaker of the English language, and 5) have 2 or more previous submissions and a 95% approval rate on Prolific Academic.

The intention to increase one's fruit intake was deemed a necessary precondition for the fruit intake promoting self-nudges to work in this study, since a defining feature of self-nudging is structuring one's surrounding congruent with one's goals. The inclusion criterion that participants should work from home was chosen because the COVID-19 pandemic resulted in the mandate of the government to work from home if possible. This was seen as an opportunity to study the use of self-nudging in a relative stable environment. Such stability is necessary for habits to form.

Participants in the main study were rewarded for each finished questionnaire according to the minimum standard set by Prolific Academic: £0.59 for the 7-minute baseline questionnaire and £0.42 for the five 5-minute follow-up questionnaires. Moreover, they were given a bonus of £2.00 once they completed all six questionnaires.

Of the 344 participants who completed the baseline questionnaire, 331 (96.2%) participants also completed at least one follow-up questionnaire and were therefore included in the analyses. Of these 331 participants, 170 (51.4%) had been randomly assigned to the control condition and 161 (48.6%) had been randomly assigned to the self-nudge condition. Seventeen participants (5.1%) completed two questionnaires, 25 participants (7.6%) completed three questionnaires, 28 participants (8.5%) completed four questionnaires, 52 participants (15.7%) completed five questionnaires, and 209 participants (63.1%) completed all six questionnaires.

The mean age of the 331 participants who were included in the analyses was 38.81 years (*SD* = 11.63, age range 19-74), with 41.7% males and 58.3% females. A high school degree was the highest obtained diploma by 26.6% and a BSc diploma by 49.5% of the participants. Of all participants, 85.5% indicated to work from home at least 5 days a week. Regarding household size, 11.8% of participants indicated to live alone, 38.1% with two people, 23.6% with three people, 21.5% with four people and 5.1% with more than four people.

Design

The study used a 2 (between-subjects factor = Condition: control vs self-nudge) x 6 (within-subjects factor = Time (moment of measurement): Tbaseline vs T1 vs T2 vs T3 vs T4 vs Tspillover) mixed design with fruit intake and fruit intake habit strength as main dependent variables. A week before the end of the study, participants were asked to remove the self-nudge in the self-nudge condition. A schematic presentation of the study design can be found in Figure 5.1.

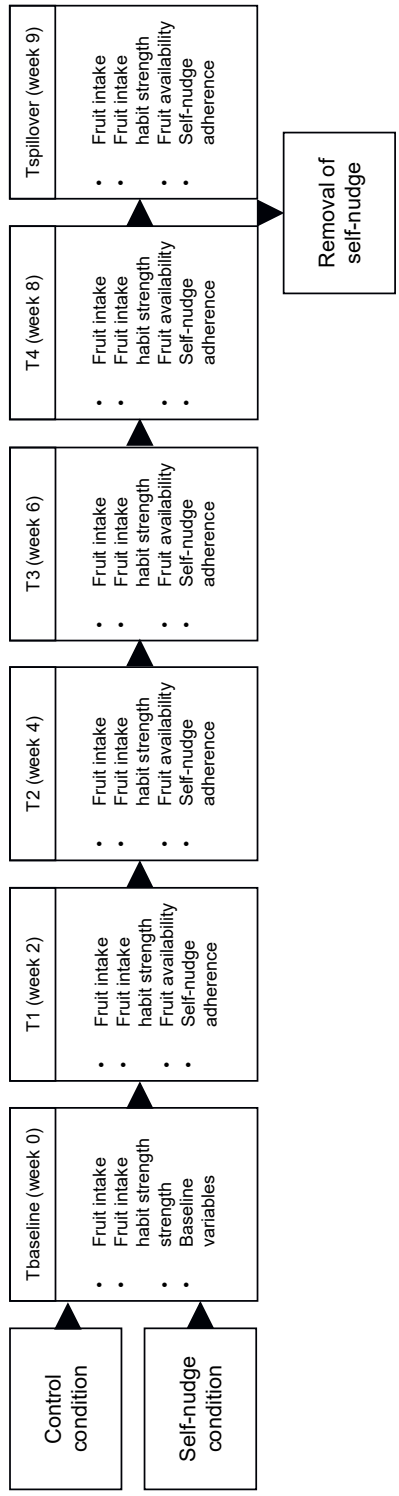


Figure 5.1: Experimental design.

Procedure

Data collection took place during February-April 2021. The entire study lasted nine weeks and consisted of two periods: an intervention period (eight weeks) and a spillover period (one week). This duration was chosen to allow for habit formation to occur (Lally et al., 2010; Keller et al., 2021). During these nine weeks, participants received six similar questionnaires: One baseline questionnaire, four questionnaires during the intervention period, and one questionnaire at the end of the spillover period.

At the start of the study (Tbaseline), participants were randomly assigned to either a control or self-nudge condition. After having given informed consent, participants completed a baseline question, measuring demographics, baseline fruit intake and fruit intake habit strength. Participants in the experimental condition were urged to choose one out of six self-nudges that they had to implement during the following eight weeks: the intervention period. During this period, participants received four questionnaires, again measuring their fruit intake and fruit intake habit strength. Moreover, in the self-nudge condition, adherence to the chosen self-nudge was measured. These four questionnaires were administered with two weeks in between every questionnaire (T1-T4). To account for different intake levels of fruit during the week, the days at which these four questionnaires were distributed alternated between Wednesdays and Fridays. At the end of the intervention period (T4), participants in the self-nudge condition were urged to remove the self-nudge that they implemented during the intervention period for one week: the spillover period. In this way, we could assess possible temporal spillover effects of the intervention period. After this week, participants received their last questionnaire measuring fruit intake, fruit intake habit strength, fruit availability and self-nudge adherence (not using the self-nudge in this case).

Measures and materials[18]

Baseline questionnaire

At Tbaseline, participants were asked their age (in years), gender (male, female, other), highest degree of completed educational level (less than a high school diploma/high school degree or equivalent/bachelor's degree/master's degree/doctorate/other [please specify]) and household size. At Tbaseline and Tspillover, participants were also asked the number of days they worked and the number of days they worked from home. The intention to increase fruit intake was measured during all measurements (Tbaseline-Tspillover) by taking the mean of the following items: 'I intend to increase my fruit consumption', 'I plan to increase my fruit consumption', and 'I want to increase my fruit consumption' to which participants had to indicate the extent to which they agreed with each statement on a 7-point Likert scale ranging from 'strongly disagree' to 'strongly agree'. For Tbaseline-Tspillover, Cronbach's α's for the intention to increase fruit intake were $> .935$.

[18] Full instructions and materials can be found in Supplementary File 4.

Manipulation

At the end of the baseline questionnaire (Tbaseline), participants in the self-nudge condition were explained what nudges are and asked to choose one nudge that they had to implement for two months to help them put their intention to eat fruit into practice. They were asked to choose a nudge that they did not yet use and that was feasible to implement. Participants could choose one nudge from two accessibility, two salience and two reminder nudges. Chosen nudges were based on the typology of self-nudges by Reijula and Hertwig (2022) and were formulated during brainstorm sessions. After two months, participants were asked to remove the nudge. Participants could choose one of the following nudges:

1. Prepare and cut fruit I want to eat in advance (accessibility), which was chosen by 17 participants (10.6% of the self-nudge condition).
2. Place a portion of fruit within reach of where I spend much of my time (accessibility), which was chosen by 27 participants (16.8% of the self-nudge condition).
3. Put a fruit basket within sight of where I spend much of my time (salience), which was chosen by 43 participants (26.7% of the self-nudge condition).
4. Place fruit in the refrigerator in a clearly visible place (salience), which was chosen by 28 participants (17.4% of the self-nudge condition).
5. Set an alarm that reminds me to eat a piece of fruit (reminder), which was chosen by 28 participants (17.4% of the self-nudge condition).
6. Place a reminder that I have to eat fruit in a clearly visible place (e.g. a note on the refrigerator) (reminder), which was chosen by 18 participants (11.2% of the self-nudge condition).

Fruit intake

Fruit intake of participants in both the self-nudge and control condition was assessed at baseline (Tbaseline), during the intervention (T1-T4) and after participants were asked to remove the self-nudge (Tspillover). Participants were asked what types and the number of units of fruit they had consumed the day before they received the questionnaire and the day before that to calculate the portions of fruit consumed during the two days before measurement.[19] For both days participants were presented with a so-called fruit diary: A list of common fresh fruits, dried fruits, canned fruits and fruit juices of which they had to indicate *what* and *how much* they had consumed, which could be a whole number or fraction. For all categories, multiple 'other' options were also presented. Standard units were given

[19] Analyses were conducted both for fruit intake during the day before the participants received the questionnaires and for fruit intake during the two days before participants received the questionnaire, since recalling consumed fruits of two days ago may be subject to more bias than for one day ago. Since results of both the analyses showed a similar pattern, results are only reported for the fruit intake recall during the two days before participants received the questionnaire.

as default for each fruit (e.g. 'hands' for grapes), but participants could choose between 'pieces', 'hands', 'slices' and 'glasses'. To calculate the number of consumed portions, guidelines of the National Health Services in the United Kingdom to what consists of one portion were followed (National Health Service, n.d.). This means that for some fruits, one piece was considered one portion (e.g. apple), for other fruits more than one piece was considered a portion (e.g. tangerine) and for others, one piece was considered as more than one portion (e.g. mango). Moreover, on each day, fruit juice could count as a maximum of one portion per day. These portions were added together to calculate the number of consumed fruit portions.

Fruit intake habit strength

Fruit intake habit strength was measured by administering the self-reported habit index (SRHI, Verplanken & Orbell, 2003). Participants had to indicate on a 7-point Likert scale ranging from totally disagree to totally agree to which extent they agreed with the following statement: Eating fruit at home is something...followed by 12 items (e.g. ...is something I do automatically). For Tbaseline-Tspillover Cronbach's α's of the SRHI were > .963. The mean of these items was calculated and taken as a measure for fruit intake habit strength.

Fruit availability

Since the effect of the self-nudges to increase fruit consumption in the home environment is reliant on the availability of fruit, we measured fruit availability in both the control and self-nudge condition on the five timepoints after baseline (T1-Tspillover). Participants were asked whether they had any fruit in their home on both two days prior to receiving the questionnaire ('yes'/'no').

Self-nudge adherence

Adherence to the chosen self-nudge was reported at T1-T4 in the self-nudge condition. Participants were asked whether their chosen self-nudge was in place on both two days prior to receiving the questionnaire ('yes'/'no'). Moreover, participants were given room to comment. At Tspillover, a week after participants were asked to remove the self-nudge, participants in the self-nudge condition were asked whether they indeed not implement the nudge on these days. If they indicated that they did use the self-nudge on one of these days, they were asked why this was the case (I did it without thinking/I forgot the instructions/I did not want to change back to the old situation/I did not read the instructions/I did it automatically/other) and to explain their answer.[20]

[20] All described analyses were also conducted with only those participants who adhered to the self-nudge at all timepoints. Since results showed a similar pattern to those reported, results are only reported for analyses including all participants.

End questions

At Tspillover participants in both the control and self-nudge condition were asked to indicate how often they used each of the self-nudge strategies during the first eight weeks of the study to help them increase their fruit intake for every strategy with a 5-point Likert scale ranging from 'never' to 'always'. Participants in the self-nudge condition were also asked to indicate on a 5-point Likert scale from 'strongly disagree' to 'strongly agree' the extent to which they found it easy to consistently use the chosen self-nudge during these eight weeks, the extent to which they found the chosen self-nudge helpful in increasing their fruit intake and whether the chosen self-nudge helped others in their household to consume more fruit (with the addition of a 'not applicable' option).[21]

RESULTS[22]

Descriptive information

Participants on average had a relatively high baseline intention to eat more fruit ($M = 5.96$, $SD = .88$). Results showed very high numbers of fruit availability, with percentages ranging from 89.4% to 95.8% in the control condition and percentages ranging from 95.0% to 98.7% in the self-nudge condition.

Randomisation check

We first examined whether the control and self-nudge condition were comparable on a number of variables at Tbaseline using ANOVA and chi-square tests. No differences were found regarding age ($F(1, 329) = .144$, $p = .705$), gender ($\chi^2(1) = .063$, $p = .802$), education ($\chi^2(5) = 4.999$, $p = .416$), number of work days ($F(1, 329) = .076$, $p = .783$), number of days working from home ($F(1, 329) = .003$, $p = .956$) and household size ($F(1, 329) = 2.007$, $p = .158$). However, the intention to consume more fruit was significantly higher in the self-nudge ($M = 6.22$, $SD = .60$) than in the control condition ($M = 5.72$, $SD = .98$) ($F(1,329) = 30.44$, $p < .001$, $\eta_p^2 = .085$). Moreover, intention to consume more fruit at Tbaseline significantly correlated with fruit intake at T1 ($r = .114$, $p = .043$). Conform preregistration, intention to consume more fruit was therefore added to our statistical models.

[21] For exploratory purposes, we also measured unhealthy snack intake and unhealthy snack intake habit strength at Tbaseline-Tspillover. In the last questionnaire (Tspillover), participants were also again asked about their 'work from home' behaviour and how often they used each of the self-nudges that were included in the study during the eight experimental weeks. Discussing these variables is beyond the scope of this paper, but a discussion of unhealthy snack intake can be found in Supplementary File 5.

[22] As mentioned in the discussion on sample size calculation, advancing insights resulted in the addition of a statistician to our team. Therefore, the preregistered analyses differ slightly from the performed analyses (e.g. the use of linear mixed models to account for both fixed and random effects).

Manipulation check: self-nudge adherence

Throughout the study, self-nudge adherence was measured in the self-nudge condition. During the time participants were asked to implement the chosen self-nudge (T1-T4), the percentage of participants reporting that they made use of the self-nudge ranged from 81.6% to 84.8%. During the last week, participants were asked to remove the self-nudge. Most of the participants adhered to this request, with 91.1% of the participants reporting to have adhered to this request.

Main analyses[23]

Descriptive statistics and intercorrelations for fruit intake and fruit intake habit strength can be found in Table 5.1. To structure our analyses, we first examined the effect of the self-nudges on fruit intake during the intervention period (T1-T4) and spillover period (Tspillover). Thereafter, we examined whether any habit formation occurred and whether this mediated fruit intake at Tspillover. Finally, we discuss questions about the subjective evaluation of the self-nudging intervention.

Fruit intake during the intervention period (Tbaseline-T4) and spillover period

Linear mixed models comprising of both fixed and random effects were formulated to assess the effect of self-nudging fruit intake for Tbaseline-Tspillover. After visual inspection of the normality of residuals, a log-transformation was performed for fruit intake and 0.5 was added to account for the number of participants who did not consume any fruits.

The overall model including both the self-nudge and control condition and intention to increase fruit intake showed a significant condition*time interaction effect ($F(5, 1420) = 5.840$, $p < .001$, $\eta_p^2 = .020$). Estimated marginal means of fruit intake on the different timepoints for the control and self-nudge condition can be seen in Figure 5.2.

Simple effects within time: intervention period (T1-T4). Although participants were randomly assigned to either the control or self-nudge condition, surprisingly, baseline fruit intake was significantly higher in the self-nudge condition than in the control condition ($p = .025$, $\eta_p^2 = .250$). Post-hoc tests for the intervention period (T1-T4) were therefore conducted comparing fruit intake in the control and the self-nudge condition at every timepoint while controlling fruit intake at Tbaseline. When controlling for fruit intake at Tbaseline, significant differences between the groups were observed at T1 ($p < .001$, $\eta_p^2 = .429$), T2 ($p < .001$, $\eta_p^2 = .466$) and T4 ($p < .001$, $\eta_p^2 = .533$) and a marginally significant difference was found for T3 ($p = .059$, $\eta_p^2 = .288$), all indicating a higher fruit consumption in the self-nudge condition.

In addition, post-hoc tests were conducted comparing the control and the self-nudge condition at every timepoint while controlling for the previous timepoint. These tests were conducted to test

[23] Although not mentioned when discussing each specific test, when conducting post-hoc tests, p-values were adjusted for multiple testing using the multivariate approach.

Table 5.1: Descriptive statistics and intercorrelations for Fruit Intake and Fruit Intake Habit Strength

					Control Condition			Self-Nudge Condition													
		n	*M*	*SD*	*n*	*M*	*SD*	*n*	*M*	*SD*	1	2	3	4	5	6	7	8	9	10	11
Fruit Intake																					
1.	Tbaseline	331	4.0	3.0	170	4.2	2.9	161	3.8	3.2	-										
2.	T2	316	5.0	3.4	165	4.8	3.6	151	5.3	3.2	.49*	-									
3.	T3	294	5.3	4.3	151	4.7	3.7	143	5.9	4.8	.43*	.58*	-								
4.	T4	283	5.3	3.8	142	5.3	4.1	141	5.3	3.4	.43*	.53*	.62*	-							
5.	T5	257	5.1	4.0	132	4.6	4.1	125	5.6	3.9	.37*	.56*	.53*	.57*	-						
6.	Tspillover	254	5.6	4.3	133	5.6	4.6	121	5.5	4.0	.31*	.58*	.56*	.66*	.55*	-					
Fruit Intake Habit Strength																					
7.	Tbaseline	331	3.4	1.3	170	3.5	1.8	161	3.3	1.3	.49*	.28*	.21*	.26*	.25*	.21*	-				
8.	T2	316	3.7	1.4	165	3.8	1.4	151	3.7	1.3	.43*	.34*	.25*	.30*	.29*	.25*	.84*	-			
9.	T3	294	3.9	1.4	151	3.9	1.4	143	3.8	1.3	.43*	.34*	.31*	.32*	.24*	.27*	.83*	.89*	-		
10.	T4	283	4.1	1.4	142	4.1	1.5	141	4.1	1.4	.41*	.35*	.30*	.39*	.25*	.31*	.77*	.85*	.89*	-	
11.	T5	257	4.1	1.5	132	4.0	1.5	125	4.2	1.4	.43*	.36*	.34*	.39*	.34*	.39*	.75*	.82*	.87*	.89*	-
12.	Tspillover	254	4.2	1.4	133	4.2	1.4	121	4.2	1.4	.40*	.35*	.28*	.37*	.31*	.40*	.72*	.78*	.83*	.86*	.93*

Note. *$p < .001$

out expectation that fruit intake would increase after Tbaseline, but that thereafter, fruit intake would remain stable. As expected, differences between the self-nudge and control condition were observed at T1 (when controlling for baseline) ($p < .001$, $\eta_p^2 = .429$), while no differences between the two conditions were observed at T2 (when controlling for T1) ($p = .998$), at T3 (when controlling for T2) ($p = .476$), and at T4 (when controlling for T3) ($p = .201$). Overall, then, these results indicate that the difference between the self-nudge and control conditions remains similar throughout the study.

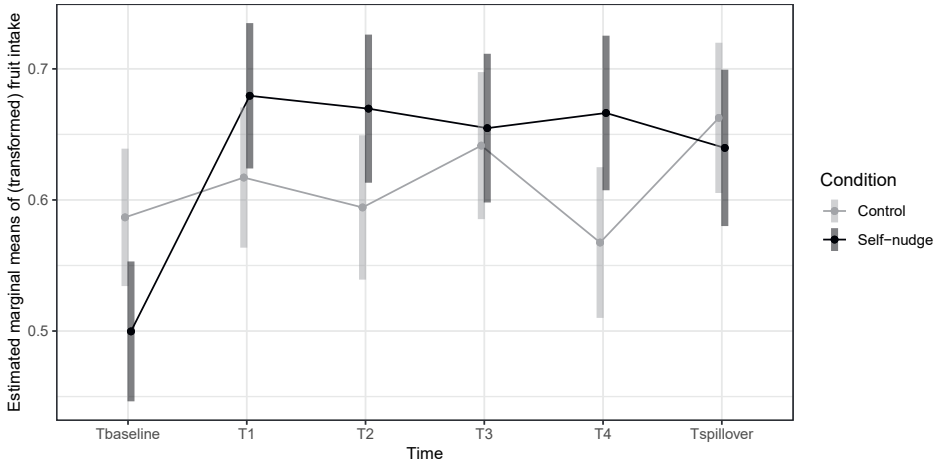


Figure 5.2: Estimated marginal means of Fruit Intake (portion) at Tbaseline-Tspillover for the control and self-nudge condition.
Note. Error bars represent 95% confidence intervals.

We also analysed the effect of time within the self-nudge condition only to investigate how fruit intake is affected throughout the intervention period for those using self-nudging. Comparing Tbaseline to all of the following timepoints separately revealed significant differences for all contrasts (all p's $<$ .001, all η_p^2's between .445 and .516) indicating that fruit intake was higher during all timepoints of the intervention period than during baseline. In addition, we compared fruit intake in the self-nudge condition at every timepoint during the intervention period with the previous timepoint, indicating that only the difference between Tbaseline and T1 was significant ($p < .001$, $\eta_p^2 = .516$, all other p's $> .985$).

Simple effects within time: spillover period (Tspillover). To assess whether the effects of the self-nudges continued once they were removed, a post-hoc test was conducted comparing the control and the self-nudge condition at Tspillover while controlling for fruit intake at Tbaseline. This comparison did not show a significant difference ($p = .423$), which means that we could not find support for a difference in fruit intake between the two conditions after the self-nudge was removed. In addition, a post-hoc test was conducted comparing the control and self-nudge condition at Tspillover while

controlling for the previous timepoint (T4). This test did show a significant difference between the two conditions ($p = .03$, $\eta_p^2 = -.349$), meaning that the difference between the two conditions at T4 did not continue to Tspillover, with fruit intake in control condition increasing and fruit intake in the self-nudge condition decreasing from T4 to Tspillover.

The lack of a significant difference between the two conditions at T5 does not seem to stem from a decreasing fruit intake in the self-nudge condition, but a surprising increase in fruit intake in the control condition. A post-hoc test comparing Tspillover with T4 in the control condition namely showed a significant effect ($p = .011$, $\eta_p^2 = .273$), with higher fruit intake at Tspillover than T4. In addition, post hoc tests in the self-nudge condition showed a significant difference between Tbaseline and Tspillover ($p < .001$, $\eta_p^2 = .402$), with higher fruit intake at Tspillover than at Tbaseline, while no difference was found when comparing T4 with Tspillover ($p = .893$).

Fruit intake habit strength

To assess whether the self-nudge promoted habit formation, linear mixed models comprising of both fixed and random effects were formulated to assess the effect of the self-nudge on fruit intake habit strength for Tbaseline-Tspillover. In the overall model including both the control and self-nudge condition and intention to increase fruit intake, a significant condition*time effect was observed ($F(5, 1401) = 3.740$, $p = .002$, $\eta_p^2 = .013$). Figure 5.3 shows the estimated marginal means of fruit intake habit strength on different timepoints for the control and self-nudge condition.

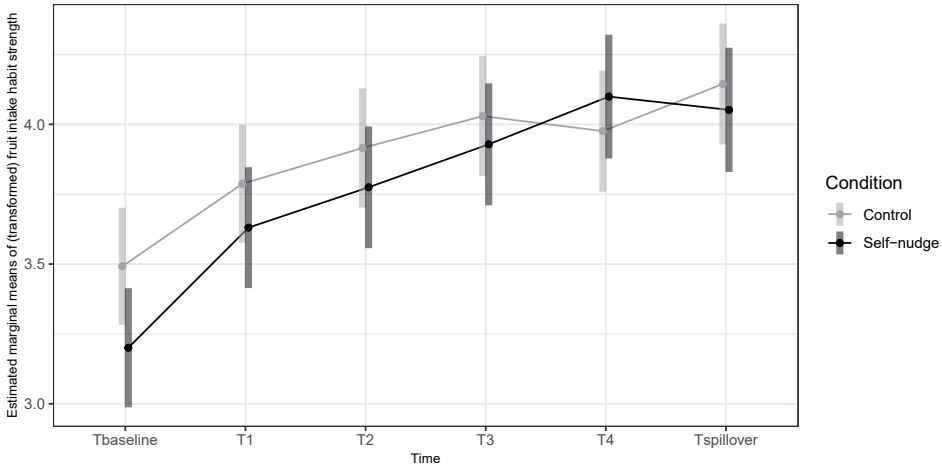


Figure 5.3: Estimated marginal means of Fruit Intake habit strength at Tbaseline-Tspillover for the control and self-nudge condition.
Note. Error bars represent 95% confidence intervals.

Since the difference between the control and self-nudge condition on fruit intake habit strength at Tbaseline was marginally significant ($p = .060$, $\eta_p^2 = .211$, post-hoc tests were conducted comparing the control and self-nudge condition at every timepoint while controlling for habit strength at Tbaseline. A significant difference was observed at T4 ($p < .001$, $\eta_p^2 = .300$, but not for T1 ($p = .448$) and T2 ($p = .359$), T3 ($p = .166$) and Tspillover ($p = .163$). This means that the increase in habit strength initially was similar in the control and self-nudge condition, but that fruit intake habit strength increased significantly stronger in the self-nudge condition compared to the control condition at T4.

To test whether fruit intake habit strength at Tspillover mediated the effect of condition on fruit intake at Tspillover, a mediation analysis was conducted with the PROCESS macro for SPSS (model 4) using a 95 percentile bootstrap approach with 5000 samples. The mediation analysis revealed no mediating effect of fruit intake habit strength (B = .0009, SE = .2278, 95% CI [-.4461, .4687]).

Subjective evaluation of self-nudging intervention

Most participants (85.1%) (strongly) agreed with the statement that it was easy to consistently use the self-nudge. Of those that did use the self-nudge, 75.2% (strongly) agreed with the statement that the self-nudge helped them consume more fruit. Of those participants who shared their household with other people, 50.9% believed that the self-nudge helped others in the household consume more fruit.

DISCUSSION

Where traditional forms of nudging can be used to promote healthy eating in out-of-home settings, self-nudging (Reijula & Hertwig, 2022) may be a viable tool to use in the promotion of healthy eating inside people's homes. In this study, we 1) investigated the immediate and prolonged effects of a self-chosen self-nudge promoting fruit intake, 2) whether these effects persist after the self-nudge is removed (i.e. temporal spillover effect), and 3) whether self-nudges can install healthy eating habits that, in turn, mediate the temporal spillover effect. Results showed clear evidence for an increase in fruit intake during the eight weeks that the self-nudge was implemented. Mixed results were found for the presence of a temporal spillover effect. That is: fruit intake in the self-nudge condition remained high relative to baseline fruit intake. However, it is difficult to interpret this effect as an indication for a temporal spillover effect or as result of other influences, since fruit intake in the control condition suddenly increased to a similar level as the self-nudge group during the spillover phase. The stronger increase in fruit intake in the self-nudge condition was accompanied by an increase in fruit intake habit strength relative to the control group. However, although we did find an increase in fruit intake habit

strength, no mediation effect of fruit intake habit strength was found for the temporal spillover effect. In sum, these results imply that self-nudging should be considered as a promising intervention tool in addition to traditional nudging, but no convincing evidence was found indicating a spillover effect.

One of the key differences between traditional nudging and self-nudging is that the nudger 'knows' about the use and purpose of the self-nudges. Although nudges' effectiveness was originally thought to depend upon the nudgee being oblivious to their use (Bovens, 2009), our findings show that nudges can still affect behaviour even when implemented by the nudgee themselves. These results are in line with recent findings that show that openness about the goal and mechanisms of nudging does not negate its effectiveness (Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Paunov et al., 2019a; Paunov et al., 2019b; Steffel et al., 2016).

After we explained the concept of self-nudging, we let participants choose one out of six predefined self-nudge to help them increase their fruit intake. The question remains whether such specific directions are needed for self-nudging to effectively alter behaviour, or whether people can effectively be taught the theory behind self-nudging and how to alter their environments in line with their goals, without specifying which specific alterations should be made. An advantage of such general skill training is that the self-nudging technique is not limited to one single behaviour in one context, but can be used to promote all kinds of behaviours in multiple contexts. It can be argued that this flexibility could enhance prolonged effects (Hertwig & Grüne-Yanoff, 2017). Future research is needed to uncover the effect and bandwidth of self-nudging as a form of general skills training.

Although the importance of studies into the prolonged effect on nudges is often underlined, it has not been topic of many studies. In this study, we found that fruit intake remained stable after an initial increase during the eight weeks that the self-nudge was implemented. Although future research is needed on this topic, we hypothesize that self-nudging may be particularly suited to affect behaviour for an extended period for two reasons. Firstly, unlike with traditional nudging, the behaviour that is targeted by a self-nudge always aligns with the goals of the nudgee. Observing a change in one's own behaviour through the use of self-nudges may act as positive reinforcement, making it more likely to continue the behaviour. Secondly, it may be easier to keep nudges in place in home settings than in out-of-home settings, where nudges are easily removed when people confronted with them do not know their purpose, since they often consist of very small changes in the choice architecture (Van Rookhuijzen & De Vet, 2021). This is illustrated by the large number of participants who was able to keep the nudge implemented throughout the study.

Despite the clear pattern that was found for the immediate and prolonged effects of the self-nudges to affect fruit intake, the data did not allow for any definitive conclusions regarding the prolonged effect of self-nudges after their removal (i.e. temporal spillover effect). It is unclear what may have

caused the sudden increase in fruit intake during the last week of the study in the control group and to assess whether this also affected the self-nudge condition, or whether the high fruit intake in the self-nudge condition can be attributed to the intervention period. Still, we did find a higher increase in fruit intake habit strength in the self-nudge condition than the control condition. Clearly, more research in this area is needed.

Limitations and suggestions for future research

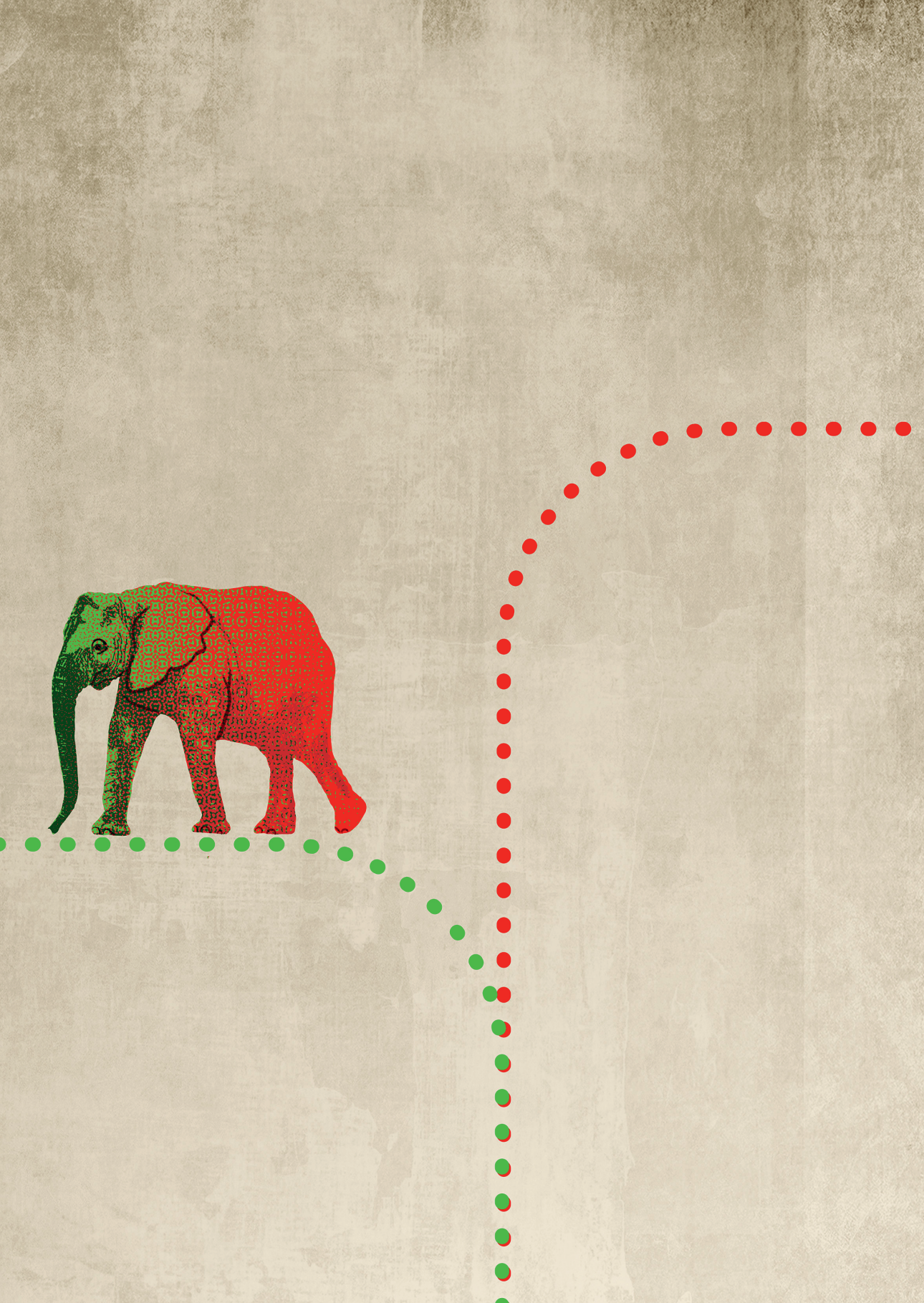
Some limitations that warrant future research should be mentioned. First, the data in this study provide strong support for the positive effect of self-nudging on fruit intake and habit formation during the eight weeks of implementation. However, even though participants were randomly assigned to either the control or self-nudge condition, there were baseline differences in fruit intake and fruit intake habit strength between the two conditions, with participants in the self-nudge condition having a lower fruit intake and fruit habit strength than participants in the control condition. It can be argued that the steeper increase in fruit intake in the self-nudge condition is the result of this baseline difference, since there was more room for improvement. Future research should replicate this study's findings to further uncover the effect of self-nudging on fruit intake.

Second, during the entire study, only self-report measures were used. For example, fruit intake was measured with fruit diaries and habit strength was measured using the self-reported habit index (Verplanken & Orbell, 2003). This may have resulted in bias for two reasons. First, memory recollections are not infallible. Bias in memories is a common occurrence and may result in bias in self-reports (Stone et al., 1999). Secondly, self-reports may partially act as interventions themselves. Knowing that you have to write down what you ate during the day may already affect your intake, which may have caused the increase in fruit intake in the control condition. Moreover, anticipating and filling in the questionnaires may also have acted as reminders for participants in the self-nudge condition to keep the self-nudge implemented. It is unclear whether instructing participants only once would cause the same effects. Moreover, although we asked participants whether they adhered to the self-nudge, we did not objectively check whether the nudge was implemented as described. Future research into self-nudging should include more objective measurements (e.g. observations) to confirm the results found in this study.

Third, the found effect sizes were only small. This is not surprising, since studies into nudge effectiveness generally find small to medium effect sizes (Arno & Thomas, 2016; Cadario & Chandon, 2020). Still, bringing about a change in behaviour through such a small self-nudge intervention as used in the current study can be regarded as promising because it is strategy that may potentially be implemented in large groups of people and thereby create impact. It is not unlikely that a combination of more self-nudges aimed at the same goal could enhance the small effect found in this study. We recommend future research to study the effects of using more than one self-nudge.

CONCLUSION

Compared to more traditional forms of nudging, self-nudging may be especially suited for the promotion of healthy eating patterns, since most of our food consumption takes place in the home environment. As it is an inherently transparent form of nudging which focusses on goals set by the nudgee themselves, many of the ethical concerns surrounding traditional nudging do not hold for self-nudging. This study is a first step in investigating the (prolonged) effects of self-nudging in the promotion of healthy eating. We conclude that self-nudging shows promise as an interventional tool in addition to traditional nudging to promote healthy eating.



CHAPTER 6

General discussion

RESEARCH AIMS AND DISCUSSION OUTLINE

One of the biggest current global health challenges is to improve people's unhealthy diets, leading to health problems such as obesity, cardiovascular diseases and type 2 diabetes (GBD 2017 Obesity Collaborators, 2019). The issue is prioritised by governments worldwide (Musuwo, 2019), but far-reaching measures, such as a sugar tax or product bans, remain controversial, which inhibits their uptake (Buckton et al., 2019). It may come as no surprise then, that the introduction of the libertarian paternalistic approach of nudging was met with great enthusiasm (Hansen et al., 2016; Jones et al., 2014; Whitehead et al., 2014). However, a deeper examination of the literature shows that research into the effectiveness of nudges promoting healthy eating has mainly focussed on incidental, one time only, behaviour change. For actual health impact, however, interventions need to be able to alter more than one single food choice. It is unclear whether, and how, nudges could induce such changes. Therefore, in this dissertation, we focussed on examining the question to which extent nudges can have a lasting impact on eating behaviour by answering the question whether nudges promoting healthy food choices can extend their effect beyond incidental one-time only behaviours. We set out to study the possibility of extended effects of nudging by examining 1) the process and effects of implementing nudges over a longer period of time and 2) whether, and under which circumstances, nudges have the potential to extend their influence once they are removed (i.e. temporal spillover effect).

SUMMARY OF RESEARCH FINDINGS

To examine the effect and feasibility of long-term implementation of nudges in a real-life context, **Chapter 2** presented a case study in which we observed a nudge intervention promoting healthier eating in two Dutch football canteens. In the first canteen, the healthier products were added to the product range for 3 weeks, after which nudges were implemented to promote the sale of these products for a period of 26 weeks. In this canteen, regular visits had to ensure that products and nudges were in place during the intervention period. In the second canteen, the healthier products were added to the product range for 15 weeks, after which they were nudged for 16 weeks. In this canteen, regular visits were only planned to observe product and nudge maintenance, without interference. The sale of these healthier products was promoted using salience, scarcity, availability and default nudges. Sales data were collected and observations, questionnaires and interviews were conducted to measure intervention reach, acceptance, adherence and applicability. Results showed a rapid increase in the percentage of sales of the nudged products from the total sales directly after implementation, which was, however, followed by rapid decrease. The nudge intervention did have a large reach and was accepted by both visitors and canteen personnel. Most importantly, however, we found that adhering

to the nudge intervention by the nudgers appeared very difficult in both canteens. The high number of volunteers who were not all well informed about the nudge intervention and time constraints let to slight alterations to, for example, the position of products. Although such changes may appear small, they often led to a removal of the nudges. Moreover, it became clear that the success of the nudge intervention was influenced by multiple characteristics, and their interplay, of the target group, the nudge intervention and organisational practices.

Since the study of Chapter 2 demonstrated the difficulty of correctly implementing a nudge intervention for a longer period of time in a real-life context, in **Chapter 3**, three studies were conducted to investigate whether repeated exposure is a necessary prerequisite for nudges to have prolonged effects. In other words, in these studies, the effect of a nudge after its removal was investigated (i.e. temporal spillover effect). In studies one and two, participants were asked to voluntarily complete a longer questionnaire on two consecutive days. With half of the participants, a default nudge promoted taking the longer questionnaire by preselecting this option. On the second day, the same question was asked to participants, but this time without any nudge. Moreover, in study two, we investigated whether a change in attitude towards the nudged behaviour or identity could explain the temporal spillover effect. In both studies, results suggested that the effect of the default nudge on the decision to complete a longer questionnaire spilled over to the second day, with the second study showing that this effect could partly be attributed to a changed attitude towards the nudged behaviour. These first two studies were set up as proof-of-principles studies using a straightforward, and for most participants, probably new, behaviour. The third study used a similar set-up, but this time participants had to indicate their preference for a food product in a hypothetical food choice task. With half of the participants, a combined salience and default nudge was used to promote one healthier product on the first day, which was removed again on the second day. Although the nudge did have an effect on food choice on the first day, contrary to the first two studies, this effect did not spill over to the second day. This suggests that the temporal spillover effect may not be present or as pronounced for all behaviours.

With rising concerns about nudges' negative impact on autonomy and nudges being manipulative (Schmidt & Engelen, 2020), a call is being made for nudge interventions to be transparent, meaning that the presence and/or the purpose of the nudge are explained to the one being nudged. In **Chapter 4**, we investigated whether the temporal spillover effect that was found in Chapter 3 was robust to a transparency manipulation. We hypothesized that, by making the nudge transparent, it would be less likely that participants would attribute their behaviour to internal states such as attitude, as an external explanation was readily available. As a result, we expected that this would make a temporal spillover effect less likely to occur. To test this hypothesis, in two studies, we used the same set-up as the first two studies of Chapter 3, with the addition of a condition in which the default nudge was made transparent by explaining to participants that the longer version of the questionnaire was preselected

to encourage people to choose this version. In the first study, and in contrast to our findings in Chapter 3, we did not find an effect of the non-transparent default nudge on the decision to choose the longer version on either the first or second day in the non-transparent nudge condition relative to the control condition. However, participants in the transparent nudge condition did opt for the longer version more often on the first and second day relative to the control and non-transparent nudge condition. Because the nudge did not affect behaviour in the non-transparent nudge condition, we replicated the study. This time, we did find an effect of the nudge in both the non-transparent and transparent nudge conditions on the decision to choose the longer version on the first day. Moreover, in both conditions this effect spilled over to the second day. We therefore concluded that transparency does not seem to negate the temporal spillover effect of nudges.

In **Chapter 5**, we investigated the prolonged effect and temporal spillover effect of self-nudges promoting fruit intake at home. The home setting was chosen as response to our observation in Chapter 2 that it is difficult to consistently implement nudges in out-of-home settings. Moreover, since we did not find a temporal spillover effect on food choice in Chapter 3, in Chapter 5 we examined whether such a temporal spillover would occur after more than a single exposure to the nudge. We only included participants who already had the intention to increase their fruit intake and instructed them to implement a self-nudge promoting fruit intake for eight weeks. Participants in the self-nudge condition had to choose one out of six self-nudges which consisted of two accessibility, two salience, and two reminder nudges. Thereafter, participants were instructed to remove the self-nudge for one week to examine any temporal spillover effects. We argued that for temporal spillover effects in eating behaviour to occur, a longer nudge period might be needed to be able to form healthy eating habits. Results showed a steeper increase in fruit intake, that was accompanied by an increase in fruit intake habit strength, in the self-nudge condition relative to the control condition. However, results for the temporal spillover effect were mixed and no support was found for a mediation effect of habit strength on the temporal spillover effect of self-nudging on fruit intake.

THEORETICAL REFLECTION

In this theoretical reflection, we will first discuss our results in relation to the long-term effects of nudging, which we defined as the effects of a nudge after multiple exposures. Here, the importance of the context in which the nudge is placed is given special attention. We will then discuss our findings in relation to the temporal spillover effect. We will specifically focus on how such temporal spillover effects can come about and what may explain the mixed findings we found. We will end with a reflection on our contribution to some ethical concerns regarding nudging; more specifically, concerns related to manipulation and the question who should decide what and how to nudge.

Long-term effects

Both Chapter 2 and Chapter 5 report on studies examining the effects of nudges on food choices for an extended period of time. In Chapter 2, healthier food products were nudged for respectively 26 and 16 weeks in two football canteens. In both canteens, a small increase in the sales of the nudged products was observed. When examining the percentage of sales of the nudged products from the total sales, a steep increase was observed, followed by a rather sizable decrease. In Chapter 5, self-nudges promoting fruit intake were implemented for a period of eight weeks. Here, the initial increase in fruit intake remained stable during the entire intervention period.

Although these studies suggest that nudges are able to influence behaviour multiple times in row for an extended period of time, they do differ in the stability of these prolonged effects. One possible explanation for this difference may be found in the strength of preferences towards the nudged choices. There is now evidence that suggests that nudges' impact is strongest for people with less developed preferences for a specific choice. People with highly developed preferences are less likely to be affected by a nudge, since they already strongly prefer one option over the other (De Ridder et al., 2022; Sunstein, 2017, Venema et al., 2019). In Chapter 2, the initial large increase in the sales of the nudged products may be attributed to the fact that many of the products were new to participants. As argued by Löfgren et al. (2012), nudges' effects may be most prominent when the choice set is novel to the nudgee, arguably because strong preferences have not been formed yet. However, when the outcome of the nudge was experienced as dissatisfactory, strong preferences for alternative products may have formed. This, in turn, could explain the rapid decrease that was observed in the percentage of sales of the nudged products from the total sales. Unlike the study discussed in Chapter 2, where no inclusion criteria were formed for participants, in Chapter 5, participants were only allowed to partake in the study when they had a high intention to increase their fruit intake. Here, participants clearly wanted to act in line with their self-selected nudge. Having experienced a positive outcome, in turn, could explain the stability of the effect. Future research is needed to test the hypothesis that nudges' prolonged effects are more prominently present when the nudged behaviour is in line with the nudgee's intention.

The importance of context

An important implication of our studies is that they show that the success of a nudge intervention is partly reliant upon the context in which the nudge intervention is implemented. In Chapter 2, we implemented a variety of nudges that have been found successful in affecting food choices in the literature, such as default, availability, scarcity and salience nudges (e.g. Cadario & Chandon, 2020). However, we did not anticipate that these nudges did not completely align with the organizational complexity and reality of football canteens. For example, it was hard to communicate how nudges were used to

promote healthier choices to the large number of volunteers working in the football canteens, who, well-meant, often placed products in different places. This resulted in products 'losing' their nudge. Although, in advance, it seemed fairly easy to make the small alterations needed to nudge the healthier products in football canteens, in reality this turned out to be more difficult. In contrast, in Chapter 5, participants had to choose and implement a nudge themselves in their home environment. Here, the nudgee was in control over implementation and maintaining the nudge throughout the study. This may have caused the high adherence rate to the intervention. To promote healthy eating then, the home environment seemed better suited than the football canteens.

Moreover, the results of Chapters 2 and 5 also suggest that intentions and habitual behaviour in a particular context play a role in the success of nudge interventions. In Chapter 2, customers did not seem to come to the football canteen with the intention to choose a healthy product. Interviewees mentioned that consuming an unhealthy snack was part of a football match, suggesting an intention or habitual behaviour that did not align with the behaviour put forward by the nudge. In contrast, in Chapter 5, participants could only partake in the study when they had a high intention to increase their fruit intake. This may have ensured a better alignment between the goal of the study and the intention of participants in that particular context. In addition, the high number of unhealthy products offered and bought in the football canteens meant that the effect of the nudges dwarfed compared to the overall ratio of sales numbers between healthy and unhealthy products. Even with the addition of nudges for the healthier products, some of the unhealthy products were inevitably also presented in ways that attract attention.

Spillover effects

In addition to examining the process and effects of implementing nudges over a longer period of time, Chapters 3, 4 and 5 all report on studies in which we investigated whether nudges can extend their effects beyond incidental one-time only behaviours through affecting behaviour after the nudge is removed (i.e. temporal spillover effect). In Chapters 3 and 4, we saw that a default nudge promoting prosocial behaviour also affected behaviour the following day when the nudge was no longer in place. However, mixed results were obtained for possible temporal spillover effects for nudges promoting healthy eating behaviour. In Chapter 3, no temporal spillover effect was found for a combined salience and default nudge on hypothetical food choices, and inconclusive results were found for a temporal spillover effect after one week following a self-nudge intervention that lasted eight weeks in Chapter 5.

The temporal spillover explained

Based on the work of Bem (1972), Ariely and Norton (2008), Gneezy et al. (2012), and Bénabou and Tirole (2011), we hypothesized that the temporal spillover effect could be explained by a change in attitude towards the behaviour, or even in one's identity. We argued that, since people are often unaware of the influence of nudges on their own behaviour (Dhingra et al., 2012; Van Gestel et al., 2018), they would attribute their behaviour to their attitude or identity, which, in turn, would cause the person to repeat their action under similar circumstances. In Chapter 3, we indeed found evidence for our hypothesis that attitude mediates the temporal spillover effect. Even in study 3 on food choice, where we did not find a direct temporal spillover effect, we did find a mediation effect of attitude. However, no support was found for a change in identity to cause the temporal spillover effect. Surprisingly, in Chapter 4, results showed that the temporal spillover effect did not diminish after exposure to transparent nudges. We hypothesized that, by making a nudge transparent, it would be less likely that internal attribution (such as attributing behaviour to one's attitude) would occur, since the nudge would provide a clear external reason for following the behaviour promoted by the nudge.

An explanation for these findings may be that participants still underestimated the influence of the nudge on their own behaviour (Dhingra et al., 2012; Van Gestel et al., 2018), meaning that a change in attitude could still occur. Such underestimation of external influences on behaviour has been found in earlier studies (e.g. Bang et al., 2020). Other explanations may be that direct consistency (Dolan & Galizzi, 2015; Falk & Zimmermann, 2013) or mental laziness (Loewenstein et al., 2015) are more important drivers of the temporal spillover effect than a change in attitude, which are arguably not affected by attribution of behaviour to an external source. Of course, the temporal spillover effect of the transparent default nudge may also have been driven by other processes than the temporal spillover effect of the non-transparent nudge. For example, the transparency message may be seen as an explicit recommendation which enhances its impact, in line with request justification (Langer et al., 1987). Another example can be found in Loewenstein et al. (2015), where it is argued that spillover effects of transparent nudges may be attributed to the transparency message evoking participants to think differently about the given choice, coming up with more and better arguments for choosing in line with the default. Still, the temporal spillover effect might also be explained by altered perceptions of the choice architect (Steffel et al., 2016). More specifically, the choice may be seen as fairer through the use of a transparency message. These explanations could all account for an effect of a transparent nudge after its removal. Future research is needed to unravel the shared and non-shared processes underlying the temporal spillover effects of non-transparent and transparent nudges.

Because we did not find a direct temporal spillover effect for food choice in Chapter 3 after one successful nudge exposure, in Chapter 5, we investigated whether nudging food choices for a longer period would lead to temporal spillover effects. More specifically, we hypothesized that such a temporal

spillover could be induced through the formation of habits. Habits can be described as automatic behavioural tendencies in response to a cue (Wood & Neal, 2009). One of the advantages of habits is that they are hard to break and can protect against temptations (Lin et al., 2016). When nudges are able to install healthy habits then, nudges may no longer be needed to induce the initially nudged behaviour, thus leading to temporal spillover effects. Although the self-nudges used in Chapter 5 were able to increase fruit intake for eight weeks, accompanied by an increase in fruit intake habit strength, we found mixed evidence for a temporal spillover effect and no evidence that this temporal spillover effect was caused by the increase in habit strength. An explanation for these findings may be that more time is needed for strong enough habits to form that can induce behaviour without the nudge. Another explanation may be that habits were formed with the nudge as cue to enact the behavioural response. Without the nudge then, the behaviour did not occur. For habits to be able to induce the behaviour without the nudge, other aspects of the environment need to act as cues for the behaviour. For example, instead of seeing fruit lying on eye-level in the refrigerator (a nudge) leading to one grabbing a piece of fruit, merely walking towards the refrigerator would lead to one grabbing a piece of fruit. Whether or not other aspects of the environment can ultimately act as drivers of the habitual response, instead of the nudge itself, remains to be uncovered.

Targeting eating behaviour

The difference between the clear temporal spillover effect that we observed in Chapters 3 and 4, where participants were asked to voluntarily complete a longer questionnaire, and the mixed results for eating behaviour in Chapters 3 and 5 are striking. One of the reasons for this discrepancy might lie in the complexity of eating behaviour. Decades of research have shown us that food intake is influenced by a complex interplay of factors at multiple levels (individual and social, physical and macro-environmental) (Leng et al., 2017; Schwartz et al., 2017). In some cases, nudges may be able to override the influence of these factors by targeting the automatic system with which many food choices are made (Cohen & Farley, 2008; Moldovan & David, 2012). However, once the nudge is removed, all these other influences on eating behaviour come into play again. In such situations, the processes that cause spillover effects may simply be too small to exert any effects on behaviour. For example, the preference to act consistently may not be as pronounced with eating behaviour, since many food choices are made and a variety of foods are eaten every day. In contrast, it is relatively easy to remember one's decision to complete a normal or longer version of the questionnaire, since it only involves a single decision, that may have been new to many participants. Consistency efforts may play a much larger role in such situations.

As Dolan and Galizzi (2015) put it: 'No behaviour sits in a vacuum, and one behaviour can greatly affect what happens next'. This does not only apply to instances in which the same behavioural choice has to be made at a later point in time, but also to other contexts and behaviours. In the General intro-

duction, we labelled such spillover effects *contextual* (the same behaviour in a different context) and *behavioural* (different, but related behaviours) spillover effects. One question that was not addressed in this dissertation, but that is of great importance to understand the potential health impact of nudges, is whether nudges promoting healthy eating can induce such behavioural spillover effects. Although we found mixed results for temporal spillover effects of nudges promoting healthy eating, contextual or behavioural spillover effects might still occur in the food consumption domain. It is important to note that such spillovers can be both promoting (having a healthy lunch after being nudged into consuming a healthy breakfast) or permitting (having an unhealthy dessert after being nudged into consuming a healthy diner) (Dolan & Galizzi, 2015; Mullen & Monin, 2016).

Together, these results show the need for further research on the temporal spillover effect of nudging. Specifically, more research is needed on the potential for temporal, contextual and/or behavioural spillover effects of nudges promoting healthy eating. Moreover, more knowledge on the possible mediators that explain such effects of both transparent and non-transparent nudges is needed. Furthermore, future studies should also assess how long, and under what circumstances, spillover effects can persist and whether, and how, this is influenced by the duration of the nudge period.

Nudge transparency

Critics of the nudge approach have argued that nudges can be seen as manipulative and autonomy impairing, since they often exert their influence without the nudgee being aware of it (Baldwin, 2014). To mitigate these concerns, recent interest has emerged into so-called transparent nudges (Sunstein, 2015), which we defined as nudges that inform those that are being nudged about the presence and the purpose of the nudge. Although it was thought that transparent nudges would be less effective (Bovens, 2009; Hansen & Jespersen, 2013; Smith et al., 2013), recent studies show that nudge effectiveness is not affected, and sometimes even increased, by disclosing the presence and/or purpose of the nudge (Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Paunov et al., 2019a; Paunov et al., 2019b; Steffel et al., 2016). For example, Kroese et al. (2015) promoted the sales of healthy snacks at a Dutch train station by repositioning healthy and unhealthy snacks in snacks shops. More importantly, in one of the snack shops, a sign was placed near the cash register stating 'we help you make a healthy choice'. Results showed that the sales of the nudged products were not affected by disclosing information about the purpose of the nudge. The results of Chapter 4 are in line with these studies, showing that the transparent default nudge was just as (or even more) effective as the non-transparent nudge. Moreover, although we did not compare self-nudging with traditional nudging, we did show that self-nudges (which are transparent per definition) were able to affect behaviour in Chapter 5. In addition, in Chapter 4, we showed that the temporal spillover effect of a transparent default nudge was just

as likely as that of a non-transparent nudge and in Chapter 5, we showed that self-nudges affected behaviour for at least eight weeks. This suggests that nudge transparency is not detrimental for the prolonged effect of nudging.

These results support the practice of disclosing the presence and aim of nudges in interventions. Still, we would like to briefly mention that nudge transparency does not diminish the relevance of further ethical scrutiny of nudging practices. For example, one might argue that nudging should only be endorsed if those that are being nudged think of it as acceptable to do so. Surprisingly, there have not been many studies investigating nudges' acceptability from a consumer perspective. A study of Cadario and Chandon (2019) did investigate the public's acceptability of nudges promoting healthy eating. They found that, on average, 56% of consumers accepted different types of nudges. However, acceptability depended on the type of nudge and was found to be inversely related to its effectiveness. In addition, public acceptance has generally been found higher for nudges that target or benefit from deliberative processing (Felsen et al., 2013; Sunstein, 2016). In a first exploration on how transparency influences acceptability, Michaelsen et al. (2020) found that participants rated a transparency default nudge as fairer than a non-transparent default nudge, but only when the choice that had to make was hypothetical. In contrast, when the choice had real-life consequences for participants, the transparent nudge was rated as more unfair than the non-transparent default nudge. Further research is needed to investigate how, and under which circumstances, transparency influences the public's acceptability of nudges.

Self-nudging

Another point of discussion about the use of nudges relates to the question who should decide what behaviour is preferable to nudge (Schmidt & Engelen, 2020). Nudges can potentially reach a lot of people. It can be assumed that the needs of a large heterogenous group are not all the same. How then, can we best decide on which options should be nudged? With self-nudging, people structure their own environment to align with their goals (Reijula & Hertwig, 2022). In this way, people can decide for themselves which behavioural options they would like to promote. Another advantage of self-nudging related to eating behaviour is that is can be used in private settings; settings where most of our food consumption takes place (Van Rossum et al., 2020) and that are hard to reach with traditional nudging. In Chapter 5 then, we explored the use of self-nudging to promote fruit intake. Participants could choose themselves which nudge would best fit in their environment and practices. Results showed an increase in fruit intake for the eight weeks in which participants used the self-nudge. These promising results suggest that nudging should not only be seen as an instrument to which people are passively exposed, but as tools that people can actively use to reach their behavioural goals. An important, unanswered question about self-nudging pertains to the extent to which people can be taught the principles of

self-nudging in such a way that they can use this as a general skill in various contexts. In Chapter 5, participants were presented with six different self-nudges from which they had to implement one. Since we explained how the self-nudge could aid in increasing their fruit consumption, the same, or other proposed self-nudges may be deployed to help with other behavioural goals. More research is needed to investigate the possibility of such effects and on the manner in which self-nudges can best be communicated to achieve these generalised effects (see Reijula and Hertwig (2022) for a proposed way of communication in the form of a 'fact box').

METHODOLOGICAL REFLECTION

Within every research project many choices need to be made that all influence the certainty with which conclusions can be drawn (internal validity) and the extent to which these conclusions can be generalised to other behaviours, times, situations and contexts (external validity). Although they sometimes can go hand in hand, often, choices that increase internal validity come at the cost of a decreased external validity and the other way around. In this dissertation, we have tried to study the question to which extent nudges can have a lasting impact on eating behaviour in a balanced manner, with studies high in internal validity as well as studies high in external validity. What now will follow is a reflection on these choices in light of our research.

Design

One of this dissertations' strengths is the variety in studies that we used to answer our question. For example, in Chapters 3 and 4, we conducted very controlled proof-of-principle studies, studying less complex behaviour than eating behaviour, that were more focused on assessing the question whether nudges can have temporal spillover effects in theory (efficacy). This in contrast to Chapters 2 and 5, where we studied eating behaviour and more focus was placed on whether nudges can have lasting and temporal spillover effects in practice (effectiveness; for a discussion on the difference between efficacy and effectiveness studies in health promotion see Flay (1986)). Moreover, we also examined some hypothesized processes underlying nudges' lasting effects, such as attitudes and habits (Chapters 3 and 5). Furthermore, in addition to the use of randomized controlled trials in Chapters 3, 4 and 5, we conducted a case study in Chapter 2 in order to thoroughly study the context and its interplay with the nudge intervention.

Participants

To determine the effects of repeated nudge exposure, it is important to include the same participants during measurements throughout the study and preferably to investigate effects within participants. Although we did try to include the same participants throughout individual studies discussed in this dissertation, we cannot know with certainty whether this is the case, and studying within participants changes was not always possible due to using aggregate measures. For example, in Chapter 2, we studied the effect and process of implementing a nudge intervention in football canteens. To do so, we used, among other measures, sales data. However, although we assumed so, we do not know whether the same visitors came back every week in the canteens. We do therefore not know with certainty whether the sales data reflect what happens when visitors are repeatedly exposed to the same nudges, or whether the behaviour of new visitors that are exposed to the nudges for the first time are also part of the weekly sales data. Moreover, even when behaviour of the same individuals is assessed over time, as in Chapter 5, there is still the possibility of high drop-out rates that could distort the research findings. Participants who drop out of long-term studies may have certain characteristics that cause the intervention to affect, or not affect, those who dropped out. Fortunately, in Chapter 5, the drop-out rate was very small.

In addition to the importance of including the same participants throughout studies measuring prolonged effectiveness, it is also important to reflect upon the representativeness of participants for the population that one would want to draw conclusions for. In Chapter 2, a case study is discussed in which participants had to partake on the site itself. However, the studies in Chapters 3, 4 and 5 were all conducted through the online crowdsourcing website Prolific Academic. The use of such online platform research is not without its concerns, which are summarized by Newman et al. (2021). Concerns related to the sampling of participants via such online platforms relate to sampling bias (a lower or higher chance of including some members of the group to which one wants to generalise their findings), data non-independence and in-group bias (e.g. participants with certain characteristics choosing to partake in certain studies more often than participants with other characteristics), and non-naivety (learning about research tools and acting in line with researchers' expectations). In addition, concerns are also voiced about the quality and ethics of the use of online platforms. However, research comparing data from online platforms and offline sources shows that the quality of the data is generally good (Walter et al., 2019). Moreover, we tried to minimize sampling and ethical concerns through strict inclusion criteria, using informed consent and by debriefing participants whenever we could not disclose our main research goal at the start of the study. Still, we do encourage future research to not only rely on online platforms and to replicate our findings in more traditional research settings.

Interventions

Although we used a variety of types of nudges in this dissertation, such as default, salience, scarcity, availability, accessibility and reminder nudges, we did not study whether the long-term or temporal spillover effects were more prominent for certain types of nudges. In their meta-analysis, Hummel and Maedche (2019) differentiated between 10 types of nudges and found, for example, that the effects of default nudges were, on average, larger than that of precommitment nudges. It is therefore likely that different types of nudges also have different long-term effects or that the temporal spillover effect may be less pronounced or absent for certain types of nudges. For example, as Sunstein (2017) argues, some nudges, such as salience nudges, may initially have strong effects, but may become like background noise after multiple exposures. In contrast, defaults may be more likely to induce persistent effects, since people have to actively do something to choose the alternative. In addition, it has been suggested that nudges that target or benefit from deliberative processing may be more likely to induce sustained change (Lin et al., 2017). We therefore argue that nudging should not be treated as one intervention type that can or cannot have certain effects, but that we should more systematically examine which nudge has the most potential for lasting behavioural impact. The need for a better understanding of the effects of different types of nudges is exemplified by a recent study of Maier et al. (2022), in which no evidence for the behavioural effects of nudges was found after controlling for publication bias. Although the effect size of nudging as a whole may be inflated by the large number of studies on nudging that did not find effects, still numerous studies, including those discussed in this dissertation, do find behavioural effects. Research should therefore aim for a better understanding on the heterogeneity of these effect sizes (Mertens et al., 2022a; Szasi et al., 2022).

Measures

Although our main research question focussed on eating behaviour, in none of the studies discussed in this dissertation did we observe actual food choice and consumption. Instead, we base our conclusions on sales data (Chapter 2), hypothetical food choices (Chapter 3) and self-reported consumption (Chapter 5). Future research should aim to replicate our findings with other measures, such as observation. Still, we do like to point out that, except for the hypothetical food choice made in study 3 in Chapter 3, all measured proxies relate to actual behaviour. In Chapter 2, sales data reflected the actual sales of the (nudged) products sold in the football canteens, in Chapters 3 and 4, participants really had to complete a longer questionnaire if they chose to do so, and in Chapter 5, participants had to report their actual fruit intake.

As already discussed, consumption patterns are very complex and food decisions made early in the day may affect other food choices later that day (Dolan & Galizzi, 2015). In Chapter 2, we therefore looked at the sales of the nudged products in relation to other products. Moreover, in Chapter 5, in

addition to measuring fruit intake that was promoted through the use of self-nudges, we also measured unhealthy snack intake. Here we did not find any indications that the increase in fruit consumption backfired into an increase in unhealthy snack consumption. Still, we again like to stress that future research should aim not to only measure direct effects of nudge interventions on the main measure of interest, but also investigate whether, and how, these effects influence other choices.

IMPLICATIONS FOR RESEARCH AND PRACTICE

In addition to the discussed studies contributing to our theoretical understanding of nudging, their results also hold implications for research, policy and practice. Surely, we acknowledge that our studies only provide an exploration into the extent to which nudges can have a lasting impact on eating behaviour and that many more studies on the subject of the potential of nudges to impact behaviour beyond single choices is needed. Still, together with existing literature, they do advance our understanding of the direction that the research agenda on nudging should take and on how nudges can best be used in practice.

From passive nudgee to active involvement

One of the key recommendations resulting from our research and from the emerging literature on the effect of transparency on nudge effectiveness (Bruns et al., 2018; Cheung et al., 2019; Kroese et al., 2016; Loewenstein et al., 2015; Michaelsen et al., 2020; Michaelsen et al., 2021a; Paunov et al., 2019a; Paunov et al., 2019b; Steffel et al., 2016) for both research and practice is to actively inform and involve those that are being nudged in the process of nudging. This suggests that nudging should no longer be regarded as a behaviour change intervention technique where the behaviour that needs nudging and the method of nudging are determined from higherup to influence the unknowing masses. Instead, as the results of Chapters 3, 4 and 5 show, the public can be informed about the use and purpose of nudges without it negatively affecting behavioural impact. In addition, as we showed in Chapter 5, people can be successfully taught to implement and use nudges themselves to affect their behaviour over a longer period of time. We therefore advise those that use or contemplate using nudge interventions, such as policy makers or dieticians, to more often consider whether and how people can be informed and/or actively involved in nudge interventions.

Seeing nudges as empowering tools, that can actively be used by those that would like to alter their behaviour by aligning their environment with their goals suggests that nudging, as is often the case, should not be considered as conceptually and practically distinct from other behaviour change techniques. For example, boosting, a technique where the individual and its capacities are central,

is often contrasted to nudging, where the environment is the central focus of attention (Hertwig & Grüne-Yanoff, 2017). However, our research shows that this difference is not as clear-cut as may initially seem, and that nudging can be deployed as a self-regulation strategy helping to empower people to help them make decisions. We should therefore aim for an integrated knowledge base where we can learn from insights from these different streams of thought instead of treating them as separate.

Still, our research is just a first exploration of taking a more active approach to nudging. One of the key assumptions underlying our recommendation to inform people about the use and purpose of nudging is that they would be more accepting of these nudges and be able to better decide for themselves whether to act in accordance with the nudge. More research is needed to explicitly address these assumptions, although preliminary evidence suggests that experienced autonomy may not be affected by nudge transparency (Wachner et al., 2022). Moreover, more research is needed to uncover how we best can involve the public in the design and implementation of nudge interventions. Here, one of the most important questions pertains to the generalizability of self-nudging. Would teaching people about the principles of nudging result in them translating and using it in multiple aspects of their lives?

What, whom, where and how?

Chapters 2 and 5 also show the need to move from studies on the general effectiveness of nudging to a research agenda in which more focus is placed on the fit and interplay between type of nudge, context, target group and targeted behaviour to induce lasting behaviour change. Food consumption is integral in the lives of most people and plays a role in a variety of contexts, such as at home, in the workplace, in restaurants, on the go, or during online shopping. In these different contexts, different drivers of food choice and consumption may be present. For example, taste may be a more important driver in restaurants, while the ease of preparation or healthiness may be more important for food choices in the workplace. In these situations, it is important to implement nudges that enact upon these drivers. To continue with our example, it may be hard to nudge consumers into taking a banana for dessert instead of a chocolate cake in a restaurant, since the chocolate cake is regarded as tastier, which, arguably, may be an important driver of food choices in restaurants. In contrast, consumers may be more easily nudged into taking a banana instead of a chocolate bar as a snack in the workplace, since both fulfill the need for easiness of preparation. In addition, nudges' effects may pale in contexts with an abundance of food choices and unhealthy temptations. In Chapter 2, although we saw a slight increase in the sales of the nudges healthier products, the percentage of the total sales that were made up by these products was very small. The football canteens still offered a large variety of unhealthy options that were frequently sold. The home environment, in which we studied a nudge intervention in Chapter 5, may be seen as better suited for a nudge intervention promoting healthy eating, since there might be less temptations.

Moreover, different target groups may be more susceptible to different types of nudges. For example, teenagers may be more susceptible to social norms nudges, while adults may be more susceptible to nudges that are time preserving, such as defaults. Instead of assuming that results on the effectiveness of nudging can be applied in all situations, these examples show the importance of designing nudge interventions that connect to the target group and context. In addition, it is also important to consider what type of behaviour change is needed and how nudging can best contribute to such change. Ideally, we would like to replace unhealthy eating patterns by healthy eating patterns. Although seemingly two sides of the same coin, there is a difference in promoting healthful choices and inhibiting or replacing unhealthy choices (Hansen et al., 2016). It might be easier to nudge new healthy behaviour than it is to replace existing unhealthy behaviour, especially eating behaviour, that is often automatic and habitual in nature (Lally & Gardner, 2013; Van 't Riet et al., 2011). It might have been the case that in Chapter 2, existing preferences and habits, decreased the effect of the nudge, while in Chapter 5, fruit intake was added as a new behaviour to the diets of participants. Still, although it may be easier to nudge new healthy behaviour than to replace unhealthy habits, it may be necessary to target unhealthy food choices before new healthy habits can be formed.

Together, these findings imply that we need a more structured and more focused research agenda to obtain a more nuanced understanding of what type of nudge works best in which context, with which target group and targeting what type of behaviour (see Sunstein (2017) for more examples on instances in which nudge interventions were unsuccessful because of their misfit with the situation). We need to move away from the question whether nudging works to come to an understanding of what, for whom, where and how nudges can work to induce lasting behavioural change. To do so, it is important to first map what behaviour needs to change, who needs to change and how the context provides facilitators and barriers, before an appropriate nudge intervention can be designed (Ensaff, 2021; Meder et al., 2018).

Nudging in a broader policy context

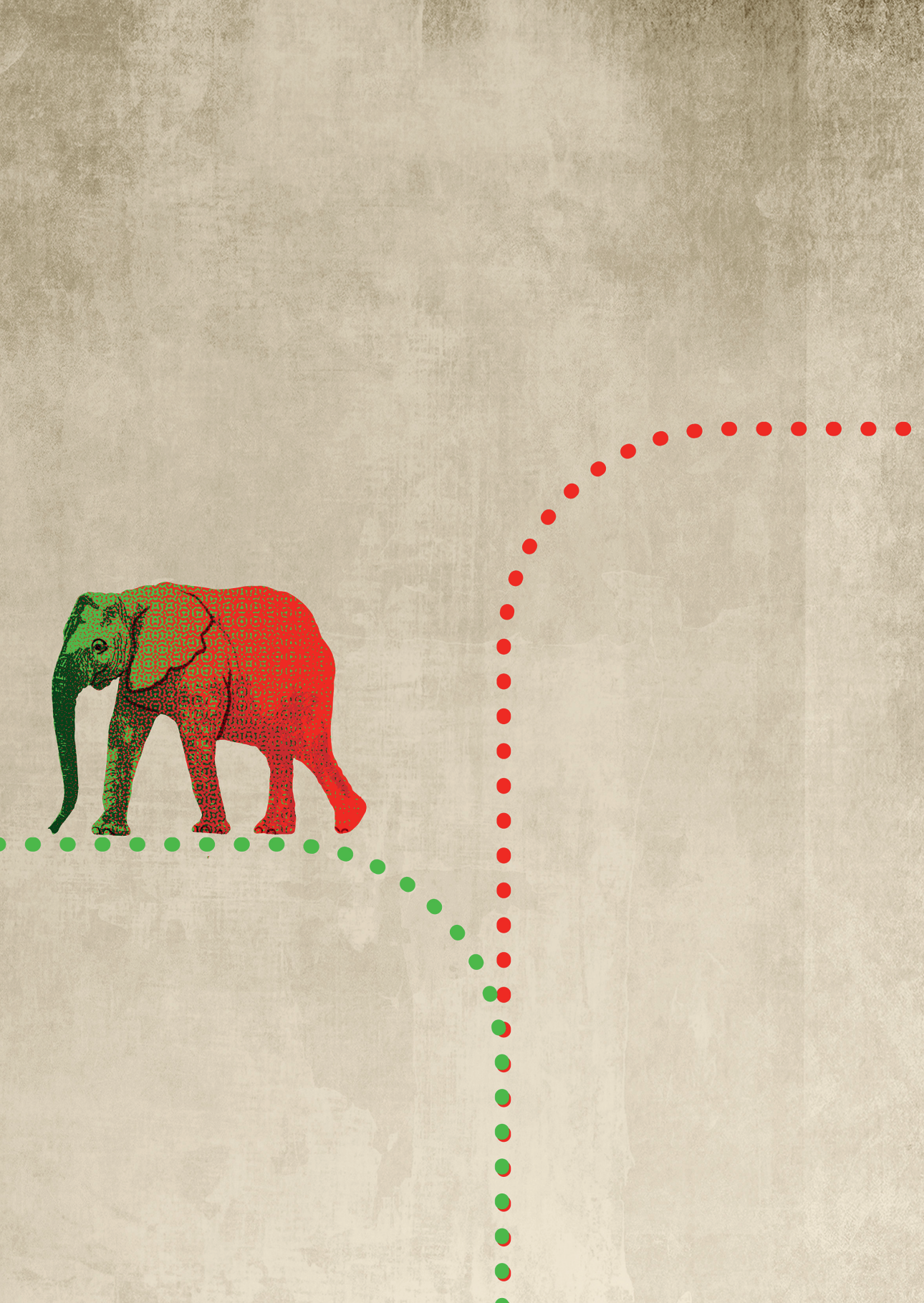
An important question is what role nudges should play in the promotion of healthy eating patterns as part of broader policy measures. Here, our studies contribute to this question by showing that nudges do have the potential to affect behaviour beyond single decisions, although the parameters under which this potential is expressed still need to be systematically studied. In the broader literature, however, more arguments can be found that support the use of nudging to promote healthy eating in policy. Nudging may be especially suited for improving food related behaviours, because nudging makes use of the automaticity with which food choices are made (Cohen & Farley, 2008; Ensaff, 2021; Moldovan & David, 2012). In addition, Benartzi et al. (2017) compared effects and costs of interventions targeting a variety of behaviours, such as college enrollment and influenza vaccinations, using nudges, tax incen-

tives, rewards or education programs. They found that nudges' impact to cost ratio compared more favorable relative to the other, more traditional, interventions. Still, the results of Chapter 2 show that that easiness of implementing nudge interventions through small changes in the environment also makes them vulnerable to being removed. Moreover, the effect sizes that we found in our studies and that are reported in the literature on nudging are generally small to medium (e.g. Cadario & Chandon, 2020). Since nudges can reach a large number of people, such small effects do hold the potential to impact public health, although their effects on an individual health level may remain limited.

What role then, can nudging play in the policy maker's toolbox to promote healthy eating? The most prevailing thought is that nudging should be seen as an addition to other behavioural interventions and to other measures available for policy making, such as regulatory measures, economic incentives and educational resources (Ensaff, 2021; Loewenstein & Chater, 2017; Marteau et al., 2021). Eating behaviour is such a multifaceted concept, that nudging alone does not hold the key to population health, especially in our obesogenic environment where temptations can be found on every corner (Swinburn et al., 1999). To truly alter unhealthy eating patterns and associated health problems, an integrated holistic approach of behavioural interventions and more traditional policy-making tools is needed where structural-environmental, psychological and behavioural factors are targeted (Meder & Fleischnut, 2018) and behavioural maintenance is given central importance.

CONCLUSION

The aim of this dissertation was to study the possibility of nudges to have effects beyond single decisions by examining 1) the process and effects of implementing nudges over a longer period of time and 2) whether, and under which circumstances, nudges have the potential to extend their influence once they are removed (i.e. temporal spillover effect). Taken together, we did find some initial evidence that suggests that nudges can repeatedly influence behaviour and that nudges do have the potential for temporal spillover effects that are robust to transparency manipulations. However, the found effects were only small. Moreover, our results also exemplify the importance of considering the context in which nudge interventions are implemented. Eating behaviour is a multifaceted concept and more research is needed to uncover which food choices in which context, and for whom, are especially suited for which types of nudge interventions to promote lasting effects. In this way, we can slowly uncover the role of nudging in addition to other measures, to alter unhealthy eating patterns and associated health problems.



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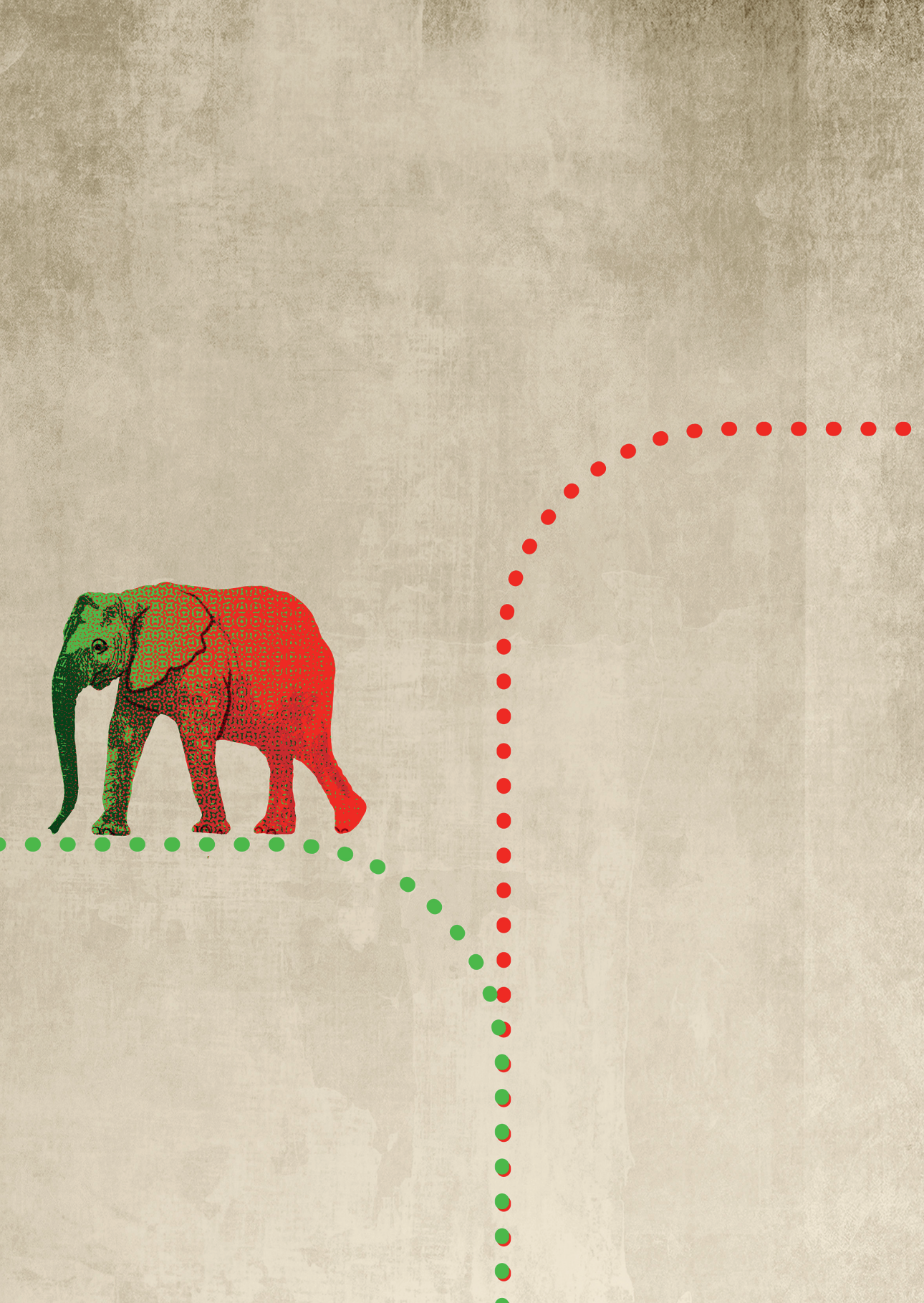


SUPPLEMENTARY FILES

Table S1.1: Mean frequencies and percentages of sales and revenue per category per week per phase of canteen A

Category (number of products)	Phase 0 (26 weeks) Revenue: *M* (*SD*) (%)	Baseline phase (3 weeks) Sales: *M* (*SD*) (%)	Intervention phase (26 weeks) Sales: *M* (*SD*) (%)	Revenue: *M* (*SD*) (%)
Nudged products (combined) (9)	-	6.33 (5.13) (0.37%)	11.85 (10.51) (0.87%)	18.78 (16.14) (0.75%)
Hot drinks (10)	557.76 (252.91) (22.57%)	504.00 (71.08) (29.35%)	423.96 (212.14) (31.13%)	608.17 (304.10) (24.33%)
Alcoholic beverages (9)	699.61 (296.46) (28.31%)	354.67 (77.02) (20.65%)	237.58 (96.72) (17.44%)	586.59 (240.14) (23.46%)
Sugary drinks (18)	463.80 (214.49) (18.77%)	300.67 (63.32) (17.51%)	232.58 (120.46) (17.08%)	505.52 (258.67) (20.22%)
Deep fried foods/snacks from the grill (25)	304.72 (179.60) (12.33%)	143.33 (31.90) (8.35%)	113.88 (63.84) (8.36%)	275.92 (154.57) (11.04%)
Sandwiches/bread/wraps (14)	235.26 (136.46) (9.52%)	199.33 (29.37) (11.61%)	140.23 (70.06) (10.30%)	280.35 (143.14) (11.21%)
Candy (14)	79.96 (40.87) (3.24%)	110.77 (22.81) (6.44%)	91.38 (43.37) (6.71%)	73.10 (36.69) (2.92%)
Snacks savoury cold (5)	42.06 (25.84) (1.70%)	44.33 (15.37) (2.58%)	49.96 (36.26) (3.67%)	40.37 (23.24) (1.61%)
Drinks with zero added sugar (7)	39.54 (31.88) (1.60%)	32.00 (12.49) (1.86%)	31.23 (27.83) (2.29%)	62.46 (55.67) (2.50%)
Rest (10)	33.12 (21.57) (1.34%)	22.33 (8.33) (1.30%)	20.12 (13.69) (1.48%)	46.37 (25.72) (1.85%)
Ice cream (4)	15.34 (46.31) (0.62%)	2.33 (4.04) (0.14%)	17.08 (35.36) (1.25%)	19.11 (40.00) (0.76%)
Fruits (2)	-	2.67 (4.62) (0.16%)	3.96 (6.42) (0.29%)	1.98 (3.21) (0.08%)
Dairy products (1)	-	1.00 (1.00) (0.06%)	0.08 (0.39) (0.01%)	0.15 (0.78) (0.01%)
Total (119)	2470.85 (1038.47) (100%)	1717.33 (216.31) (100%)	1362.04 (584.76) (100%)	2500.10 (1102.94) (100%)

Note: The number of nudged products is set at nine instead of eight, since the two different kinds of flavours of popcorn were registered separately in the cash register.

SUPPLEMENTARY FILE 2: PRODUCTS SALES OF CANTEEN B

Table S2.1: Mean frequencies and percentages of sales and revenue per category per week per phase of canteen B

Category (number of products)	Phase 0 (16 weeks) Revenue: *M* (%)	Baseline phase (15 weeks) Sales: *M* (%)	Intervention phase (16 weeks) Sales: *M* (%)	Revenue: *M* (%)
Nudged products (combined) (8)	-	11.80 (0.52%)	28.19 (1.29%)	39.54 (0.93%)
Alcoholic beverages (26)	1605.51 (38.30%)	878.73 (38.63%)	820.31 (37.57%)	1684.48 (39.42%)
Hot drinks (6)	405.84 (9.68%)	445.40 (19.58%)	324.50 (14.86%)	382.30 (8.95%)
Rest (31)	954.58 (22.77%)	125.27 (5.51%)	112.06 (5.13%)	726.03 (16.99%)
Sugary drinks (20)	411.01 (9.81%)	267.40 (11.76%)	267.50 (12.25%)	430.06 (10.06%)
Deep fried foods/snacks from the grill (48)	370.82 (8.85%)	291.60 (12.82%)	273.63 (12.53%)	439.41 (10.28%)
Sandwiches/bread/wraps (13)	207.62 (4.95%)	104.00 (4.57%)	117.19 (5.37%)	253.80 (5.37%)
Drinks with zero added sugar (6)	141.56 (3.38%)	98.20 (4.32%)	151.63 (6.94%)	247.10. (5.78%)
Candy (7)	82.69 (1.97%)	43.67 (1.92%)	67.50 (3.09%)	63.95 (1.50%)
Ice-cream (14)	9.84 (0.23%)	9.00 (0.40%)	31.38 (1.44%)	34.73 (0.81%)
Fruits (2)	2.32 (0.06%)	6.13 (0.27%)	7.00 (0.32%)	3.58 (0.08%)
Snacks savoury cold (3)	-	5.27 (0.23%)	10.57 (0.48%)	7.70 (0.18%)
Dairy products (1)	-	0.07 (0.00%)	-	-
Total (177)	4191.78 (100%)	2274.73 (100%)	2183.25 (100%)	4273.14 (100%)

SUPPLEMENTARY FILE 3: PERCEIVED INFLUENCE OF PRESELECTION

Perceived influence of default nudge on own behaviour

We hypothesized that the extent to which participants thought that the nudge influenced their own decision would moderate the temporal spillover effect, especially when attitude towards the behaviour is a strong mediator for the temporal spillover effect. When people are affected by a transparent nudge, but do not believe that the nudge actually influenced them, internal attribution may still occur. Such internal attribution would give way for a changed attitude, resulting in a temporal spillover. However, when they do believe that the nudge affected them, internal attribution may not occur, negating the temporal spillover effect. For exploratory purposes then, at the end of day two, we asked participants to which extent the preselection influenced their decision to choose the normal or longer version of the survey.

Study 1

At the beginning of the survey on day two participants were asked which version of the survey they chose the previous day (normal/longer), whether one of the options was already preselected (yes/no) and if so, to what extent this influenced their decision to choose a particular version of the survey on a 7-point Likert scale ranging from 'not at all' to 'a great extent'.

Results

Participants who did not correctly identify which version of the survey they chose on day one were excluded from these analyses ($n = 10$). Significantly more participants in the disclosed nudge condition (59.4%) noticed that one of the options was preselected on day one than participants in the non-disclosed nudge condition (29.2%) (Chi-square test: $\chi^2(1) = 78.094$, $p < .001$, $\varphi = -.304$). (Note: in the control condition, still 8.5% mentioned to notice the preselection, which was not there.) Conducting the manipulation analyses only including those participants in the non-disclosed and disclosed nudge conditions that noticed the preselection, significant differences were found between the control and non-disclosed nudge conditions ($\chi^2(1) = 10.116$, $p = .001$, $\varphi = .137$) and between the control and disclosed nudge conditions ($\chi^2(1) = 20.871$, $p < .001$, $\varphi = .177$), but not between the non-disclosed and disclosed nudge conditions ($\chi^2(1) = .134$, $p = .715$). A similar pattern was observed for the temporal spillover effect, with differences on survey choice day two between the control and non-disclosed nudge conditions ($\chi^2(1) = 9.046$, $p = .003$, $\varphi = .130$) and between the control and disclosed nudge conditions ($\chi^2(1) = 17.429$, $p < .001$, $\varphi = .162$) but not between the non-disclosed and disclosed nudge conditions ($\chi^2(1) = .062$, $p = .803$). This means that, for participants who noticed the preselection, effects of both the non-disclosed and disclosed nudge were found on survey choice for

both days when compared to the control condition. However, these effects were not more pronounced in any of the two nudge conditions.

We also asked participants to which extent they thought the preselection influenced them, hypothesizing that the temporal spillover effect may not be present or less pronounced for those participants who attributed their choice on the first day to the preselection. Surprisingly, the extent to which participants thought that the preselection influenced their decision was significantly higher in the non-disclosed nudge condition ($M = 3.09$, $SD = 1.78$) than in the disclosed nudge condition ($M = 2.66$, $SD = 1.69$) (ANOVA: $F(1) = 5.063$, $p = .025$, $\eta^2 = .013$). Conducting the manipulation analyses with the extent to which participants were influenced by the preselection as possible moderator (only including participants who noticed the preselection) we did not find a significant moderation effect when comparing the control and non-disclosed nudge conditions ($B(1) = -.312$, $p = .221$), but we did find a significant moderation effect when comparing the control and disclosed nudge conditions ($B(1) = -.606$, $p = .014$) and when comparing the non-disclosed and disclosed nudge conditions ($B(1) = -.294$, $p = .025$). This means that the effect of the nudge is stronger for participants in the disclosed nudge condition that mentioned that they were more influenced by the preselection than those that mentioned that they were less influenced by the preselection. With regards to the temporal spillover effect we did not find significant moderation effects when comparing the control and non-disclosed nudge conditions ($B(1) = -.241$, $p = .357$), when comparing the control and disclosed nudge conditions ($B(1) = -.403$, $p = .110$) and when comparing the non-disclosed and disclosed nudge conditions ($B(1) = -.162$, $p = .201$). This means the temporal spillover effect is not more pronounced when participants felt more influenced by the preselection than when they felt less influenced by the preselection

Study 2

Results

Participants who did not correctly identify which version of the survey they chose on day one were excluded from these analyses ($n = 18$). We asked participants whether they noticed the preselection. Significantly more participants in the disclosed nudge condition (57.5%) noticed that one of the options was preselected on day one than participants in the non-disclosed nudge condition (35.8%) (Chi-square test: $\chi^2(1) = 40.771$, $p < .001$, $\varphi = .047$). (Note: in the control condition, still 13.6% mentioned to notice the preselection, which was not there.) Conducting the manipulation analyses only including those participants in the non-disclosed and disclosed nudge condition that noticed the preselection, significant differences were found between the control and non-disclosed nudge conditions ($\chi^2(1) = 22.535$, $p < .001$, $\varphi = .200$) and between the control and disclosed nudge conditions ($\chi^2(1) = 25.537$, $p < .001$, $\varphi = .198$), but not between the non-disclosed and disclosed nudge conditions ($\chi^2(1) =$

.159, $p = .690$). A similar pattern was observed for the temporal spillover effect, with differences on survey choice day two between the control and non-disclosed nudge conditions ($\chi^2(1) = 8.673$, $p = .003$, $\varphi = .124$) and between the control and disclosed nudge conditions ($\chi^2(1) = 17.712$, $p < .001$, $\varphi = .165$) but not between the non-disclosed and disclosed nudge conditions ($\chi^2(1) = .356$, $p = .551$). This means that, for participants who noticed the preselection, effects of both the non-disclosed and disclosed nudge were found on both days compared to the control condition. However, the temporal spillover effect was not more pronounced in any of the two nudge conditions.

We also asked participants to which extent they though the preselection influenced them, hypothesizing that the temporal spillover effect may not be present or less pronounced for those participants who attributed their choice on the first day to the preselection. Similar to Study 1, the extent to which participants thought that the preselection influenced their decision was significantly higher in the non-disclosed nudge condition ($M = 3.20$, $SD = 1.80$) than in the disclosed nudge condition ($M = 2.71$, $SD = 1.65$) (ANOVA: $F(1) = 7.741$, $p = .006$, $\eta^2 = .020$). Conducting the manipulation analyses with the extent to which participants were influenced by the preselection as possible moderator (only including participants who noticed the preselection) we did not find significant moderations effect when comparing the control and non-disclosed nudge conditions ($B(1) = .245$, $p = .201$) and not when comparing the control and disclosed nudge conditions ($B(1) = -.066$, $p = .724$). However, we did find a significant effect when comparing the non-disclosed and disclosed nudge conditions ($B(1) = -.311$, $p = .013$). This means that the effect of the nudge on survey choice day one is stronger for participants in the disclosed nudge condition versus the non-disclosed nudge condition that mentioned that they were more influenced by the preselection than those that mentioned that they were less influenced by the preselection. With regards to the temporal spillover effect we did not find significant moderation effects when comparing the control and non-disclosed nudge conditions ($B(1) = .045$, $p = .819$) and when comparing the control and disclosed nudge conditions ($B(1) = -.207$, $p = .273$). However, we did find a significant moderation effect when comparing the non-disclosed and disclosed nudge conditions ($B(1) = -.252$, $p = .041$). This means that the temporal spillover effect when comparing the non-disclosed and disclosed nudge condition is more pronounced when participants felt more influenced by the preselection than when they felt less influenced by the preselection.

SUPPLEMENTARY FILE 4: INSTRUCTIONS AND MATERIALS

Manipulation

In the previous study you indicated that you have the intention to increase your fruit consumption. To help yourself do this, we kindly ask you to use one of the following strategies during the following two months:

1. Put a fruit basket within sight of where I spend much of my time
2. Place fruit in the refrigerator in a clearly visible place
3. Place a reminder that I have to eat fruit in a clearly visible place
4. Prepare and cut fruit I want to eat in advance
5. Set an alarm that reminds me to eat a piece of fruit
6. Place fruit within reach of where I spend much of my time

These strategies are called nudges. Nudges are small changes in the environment to help you stick to your goals. For example, you are more likely to choose something when it is in sight or within reach. You may recognise this from supermarkets where products on eye level are chosen more often. You can also implement these strategies yourself to help you with your current goals, such as your intention to eat more fruit! In this study, you will select one of the 'nudges' (strategies) to help you increase your fruit consumption.

Which of these nudges would you like to use the coming 2 months to help you increase your fruit consumption? Please select a nudge that is new to you. In other words: **Choose a nudge that you are currently not using** (for example, if you currently already place fruit in the refrigerator in a clearly visible place, don't select that option.) **The nudge should also be feasible for you to implement. All nudges require that you have fruit in your house and some also require some preparation. It is important that you, every day, use the same nudge for the coming two months.**

- Put a fruit basket within sight of where I spend much of my time
- Place fruit in the refrigerator in a clearly visible place
- Place a reminder that I have to eat fruit in a clearly visible place
- Prepare and cut fruit I want to eat in advance
- Set an alarm that reminds me to eat a piece of fruit
- Place fruit within reach of where I spend much of my time

Thank you for choosing a nudge to help you increase your fruit intake! **We kindly ask you to start using the nudge after you finished this questionnaire.** For example, if you chose to set an alarm to help you remember to eat fruit, set it as soon as possible after you finished this questionnaire. In one of the last questionnaires, you will be asked to remove it again. It is important that you use the nudge every day from now on. The remainder of this questionnaire will be used to ask you some general questions.

You are nearing the end of the questionnaire. Do not forget to install your chosen nudge. Please write down which nudge (strategy) you chose to help you increase your fruit consumption.

Which nudge (strategy) did you choose to help you?

Nudge strategy given In case you don't remember the nudge you are going to use, here it is: {Put a fruit basket within sight of where I spend much of my time; Place fruit in the refrigerator in a clearly visible place; Place a reminder that I have to eat fruit in a clearly visible place; Prepare and cut fruit I want to eat in advance; Set an alarm that reminds me to eat a piece of fruit; Place fruit within reach of where I spend much of my time}

Demographics

What is your age (in years)?

What is your gender?

- ○ Male
- ○ Female
- ○ Other

Please indicate the highest degree or level of school you have completed. If you are currently enrolled in school, please indicate the highest degree you have received.

- ○ Less than high school
- ○ High school degree or equivalent
- ○ Bachelor's degree (e.g. BA, BS)
- ○ Master's degree (e.g. MA, MS, Med)
- ○ Doctorate (e.g. PhD, EdD)
- ○ Other, namely_____

How many days per week do you work?

▼ 1; 2; 3; 4; 5; 6; 7

How many days per week do you currently work from home?

▼ 1; 2; 3; 4; 5; 6; 7

How many people currently live in your household (including yourself)?

- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ More, namely_____

To what extent do you agree or disagree with the following statements:

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
I intend to increase my fruit consumption in the upcoming week	○	○	○	○	○	○	○
I plan to increase my fruit consumption in the upcoming week	○	○	○	○	○	○	○
I want to increase my fruit consumption in the upcoming week	○	○	○	○	○	○	○

Fruit intake

We would now like to ask you to take a moment to think back to the past two days: {Tuesday; Thursday} (yesterday) and {Monday; Wednesday} (the day before yesterday).

Please try to remember as accurately as possible whether and how many fruits you consumed (including dried fruits, canned fruit and fruit juice). Please use the diaries below to fill in what and how many fruits you consumed. **It is important that you use 1 diary for each day.** Please use care when filling in the diaries since this is important for the results of our study.

Every diary represents 1 day. In every diary, you specify which type of fruit you ate (first column), the number of portions ('Number' column) and the unit ('Unit' column). You can choose between pieces, hands, slices or glasses. If you ate any fruit that is not on the list, use the 'other' options. If you indicate 'other', it is very important to specify what and how much fruit you consumed! Moreover, if you consumed only part of a fruit, please indicate this in the 'number' column (e.g. ½ if you only ate half of specific fruit). If you did not eat any fruits, you can indicate this in the comment box under the diary.

Example: On (Tuesday; Thursday), you ate two slices of pineapple and drank a glass of apple juice. You would then fill in the (Tuesday; Thursday) diary as follows:

What and how many **fruits** did you consume ({**yesterday; the day before yesterday**}) ({**Monday; Tuesday; Wednesday; Thursday**})?

	Number	Unit
FRUIT: Apple	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Banana	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Pear	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Mango	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Strawberries	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Grapes	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Tangerine	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Pineapple	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Clementine	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Blackberries	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Nectarine	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Other, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Other, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT: Other, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
DRIED FRUIT: Raisins	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
DRIED FRUIT: Dried dates	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
DRIED FRUIT: Other, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
DRIED FRUIT: Other, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
CANNED FRUIT, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
CANNED FRUIT, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT JUICE, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
FRUIT JUICE, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
SMOOTHIE, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses
SMOOTHIE, namely	▼ 0; 1/8; 1/4; 1/2; 1-15	▼ Pieces; Hands, Slices, Glasses

If you have any comments (e.g. if you did not eat any fruits), you can write them down below.

_____

_____

_____

_____

Fruit intake habit strength

Eating fruit at home is something...

	Strongly disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly agree
I do frequently.	○	○	○	○	○	○	○
I do automatically.	○	○	○	○	○	○	○
I do without having to consciously remember.	○	○	○	○	○	○	○
that makes me feel weird if I do not do it.	○	○	○	○	○	○	○
I do without thinking.	○	○	○	○	○	○	○
that would require effort not to do it.	○	○	○	○	○	○	○
that belongs to my (daily, weekly, monthly) routine.	○	○	○	○	○	○	○
I start doing before I realize I'm doing it.	○	○	○	○	○	○	○
I would find hard not to do.	○	○	○	○	○	○	○
I have no need to think about doing.	○	○	○	○	○	○	○
that's typically 'me'.	○	○	○	○	○	○	○
I have been doing for a long time.	○	○	○	○	○	○	○

Fruit availability and Self-nudge adherence at Tbaseline-T4 and Tspillover

In the first questionnaire you were asked to choose a nudge to help you increase your fruit intake. You chose to {**Put a fruit basket within sight of where you spend much of your time; Place fruit in the refrigerator in a clearly visible place; Place a reminder that you have to eat fruit in a clearly visible place; Prepare and cut fruit you want to eat in advance; Set an alarm that reminds you to eat a piece of fruit; Place fruit within reach of where you spend much of your time**} to help you. We would now like to ask you to think back to the past two days: **Tuesday** (yesterday) and **Monday** (the day before yesterday).

On {**Tuesday; Thursday**} (yesterday), did you have any fruit in your home?

- ○ Yes
- ○ No

On {**Tuesday; Thursday**} (yesterday), was {a fruit basket present within sight of where you spend much of your time; fruit kept in the refrigerator in a clearly visible place; a reminder present that you have to eat fruit in a clearly visible place; fruit prepared and cut that you wanted to eat in advance; an alarm in place that reminded you to eat a piece of fruit; fruit available within reach of where you spend much of your time}? In other words: Did you use the nudge?

- ○ Yes
- ○ No

On {**Monday; Wednesday**} (the day before yesterday), did you have any fruit in your home?

- ○ Yes
- ○ No

On {**Monday; Wednesday**} (the day before yesterday), was {a fruit basket present within sight of where you spend much of your time; fruit kept in the refrigerator in a clearly visible place; a reminder present that you have to eat fruit in a clearly visible place; fruit prepared and cut that you wanted to eat in advance; an alarm in place that reminded you to eat a piece of fruit; fruit available within reach of where you spend much of your time}? In other words: Did you use the nudge?

- ○ Yes
- ○ No

If you have any comments, you can write them down below

_____

_____

_____

_____

In the previous questionnaire you were asked to undo the change (nudge) you made in your environment at the start of this study. This means that we asked you to no longer {Put a fruit basket within sight of where you spend much of your time; Place fruit in the refrigerator in a clearly visible place; Place a reminder that you have to eat fruit in a clearly visible place; Prepare and cut fruit you want to eat in advance; Set an alarm that reminds you to eat a piece of fruit; Place fruit within reach of where you spend much of your time}. We would now like to ask you to think back of the last two days: {Tuesday; Thursday} (yesterday) and {Monday; Wednesday} (the day before yesterday).

On {**Tuesday; Thursday**} (yesterday), was {fruit basket indeed NOT put within sight of where you spend much of your time; fruit indeed NOT placed in the refrigerator in a clearly visible place; a reminder that you have to eat fruit indeed NOT placed in a clearly visible place; fruit you wanted to eat indeed NOT prepared and cut in advance; there indeed NO alarm set that reminds you to eat a piece of fruit; fruit indeed NOT placed within reach of where you spend much of your time}? In others words: Did you indeed NOT use the nudge to help you with your intention to eat more fruit?

- ○ Yes, I did NOT use the nudge
- ○ No, I did use the nudge

On {Monday; Wednesday} (the day before yesterday), was {fruit basket indeed NOT put within sight of where you spend much of your time; fruit indeed NOT placed in the refrigerator in a clearly visible place; a reminder that you have to eat fruit indeed NOT placed in a clearly visible place; fruit you wanted to eat indeed NOT prepared and cut in advance; there indeed NO alarm set that reminds you to eat a piece of fruit; fruit indeed NOT placed within reach of where you spend much of your time}? In others words: Did you indeed NOT use the nudge to help you with your intention to eat more fruit?

- ○ Yes, I did NOT use the nudge
- ○ No, I did use the nudge

You indicated that on one or more days you did use the nudge. Could you indicate why you still {put a fruit basket within sight of where you spend much of your time; place fruit in the refrigerator in a clearly visible place; place a reminder that you have to eat fruit in a clearly visible place; prepare and cut fruit you want to eat in advance; set an alarm that reminds you to eat a piece of fruit; place fruit within reach of where you spend much of your time} even though we asked you not to do so in the previous week (more options possible)?

- ○ I did it without thinking
- ○ I forgot the instructions
- ○ I did not want to change back to the old situation
- ○ I did not read the instructions
- ○ I did not it automatically
- ○ Other, namely _____
- ○ Other, namely _____
- ○ Other, namely _____

Could you please explain your answer?

_____

_____

_____

_____

End questions

In the previous questionnaire, we asked you to no longer put a fruit basket within sight of where you spend much of your time. However, in the other weeks of the study you were encouraged to use the nudge to help you with your intention to eat more fruit.

You have almost reached the end of the study. In the remainder of this questionnaire, we will ask you some questions about the 8 weeks that you were asked to {put a fruit basket within sight of where you spend much of your time; place fruit in the refrigerator in a clearly visible place; place a reminder that you have to eat fruit in a clearly visible place; prepare and cut fruit you want to eat in advance; set an alarm that reminds you to eat a piece of fruit; place fruit within reach of where you spend much of your time}. This means that the questions are about the entire study with the exception of the last week.

Please indicate to which extent you agree with the following statements.

	Strongly disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Strongly agree
It was easy for me to consistently {Insert self-nudge}	○	○	○	○	○
{Insert self-nudge} helped me to eat more fruit	○	○	○	○	○

Please indicate to which extent you agree with the following statement.

	Strongly disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Strongly agree	Not applicable
{Insert self-nudge} helped others in my household to eat more fruit	○	○	○	○	○	○

If you have any comments, you can write them down below

_____

_____

_____

_____

SUPPLEMENTARY FILE 5: UNHEALTHY SNACK INTAKE

Methods

Unhealthy snack consumption was measured to explore whether an increase in fruit consumption would affect unhealthy snack consumption. It was measured similar to fruit consumption for the two days before participants received the questionnaire. Unhealthy snacks were defined as snacks that were consumed in addition to breakfast, lunch or dinner. In the questionnaire, a list of common consumed unhealthy snacks was presented.[24] Moreover, multiple 'other' options were presented to participants to list unhealthy snacks they had consumed that were not on the list. Standard units were given as default for each unhealthy snack (e.g. 'bowls' for crisps), but participants could choose between 'pieces', 'hands' and 'bowls'. To calculate the number of calories consumed, the average number of calories in the mentioned snacks was added together.

Results

We explored whether the self-nudge affected unhealthy snack intake. Generalized linear mixed models comprising of both fixed and random effects were formulated to assess the effect of the self-nudge snack intake for Tbaseline-Tspillover. After visual inspection of the normality of residuals, log-transformations were performed for all dependent variables and 10 was added to account for the number of participants who did not consume any snacks.

In the overall model including both the self-nudge and control condition and intention to increase fruit intake only a significant effect of time was observed ($F(5, 1426) = 9.106$, $p < .001$, $\eta_p^2 = .031$). Estimated marginal means of unhealthy snack calorie intake on the different timepoints for the control and self-nudge condition can be seen in Figure S5.1.

Post-hoc tests were then conducted comparing every timepoint with the previous timepoint. These showed no significant differences between Tbaseline and T1 ($p = .231$), between T1 and T2 ($p = .330$), between T2 and T3 ($p = .998$), between T3 and T4 ($p = .963$), and between T4 and Tspillover ($p = .404$). However, a significant difference was found when comparing Tbaseline with the average of T1-T4 ($p < .001$, $\eta_p^2 = .049$). This shows that unhealthy snack intake decreased during the study period, but that this was not noticeable between subsequent timepoints.

[24] A pilot study ($N = 30$) was conducted to list the most commonly consumed unhealthy snacks by asking participants what unhealthy snacks they had consumed last week and what unhealthy snacks others they know eat.

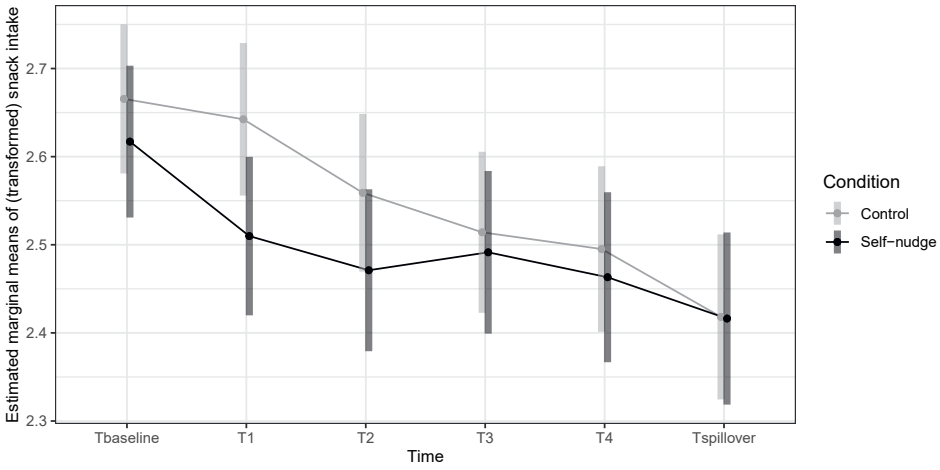
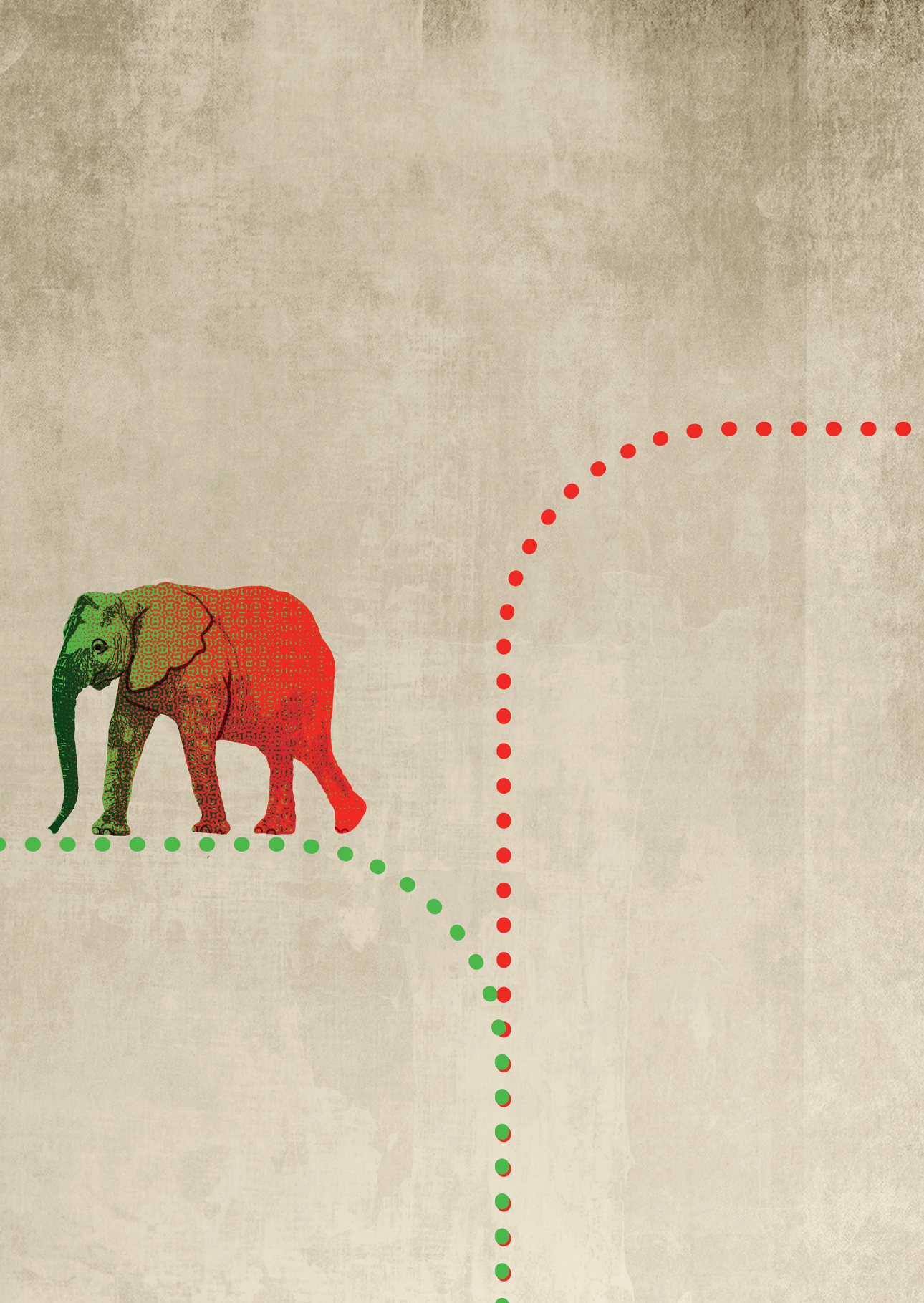


Figure S5.1: Estimated marginal means of unhealthy snack intake at Tbaseline-Tspillover for the control and self-nudge condition.
Note. Error bars represent 95% confidence intervals.

Discussion

We did not find any indication that the increase in fruit consumption backfired into an increase in unhealthy snack consumption. Surprisingly, a decrease in unhealthy snack consumption was found for both the control and self-nudge condition, which may reflect a form of response bias in which (the prospect of) filling in unhealthy snack diaries affected unhealthy snack intake.



SUMMARY

Since the term 'nudging' was coined by Richard Thaler and Cass Sunstein in 2008, the behaviour change technique has had an enormous impact on both research and policy. Nudging refers to subtle changes in the choice architecture that predictably steer people's behaviour in a certain direction. It inspired the establishment of the Behavioural Insights Team (BIT) in the United Kingdom and related organisations worldwide with, amongst others, the aim to generate and apply behavioural insights to overcome the alarming trend in the increase in unhealthy eating patterns, leading to associated health problems such as cardiovascular diseases and type 2 diabetes (GBD 2017 Obesity Collaborators, 2019).

Despite their immense popularity, however, there has been little research examining nudges' potential to have lasting impact on eating behaviour. Most research into the effectiveness of nudges solely focusses on their effects on one single food choice. In some instances, nudging a single choice can have a large impact, such as with the choice to become an organ donor. However, affecting one single food choice will not be sufficient to establish healthy eating patterns.

Therefore, the aim of this dissertation was to investigate the extent to which nudges can have a lasting impact on eating behaviour by examining 1) the process and effects of implementing nudges over a longer period of time and 2) whether, and under which circumstances, nudges have the potential to extend their influence once they are removed (i.e. temporal spillover effect).

Chapter 1 describes the background to this question and discusses the possible ways through which nudges can have a lasting impact on eating behaviour.

Chapter 2 describes a case study in which the effect and feasibility of long-term implementation of nudges in two Dutch football canteens were examined. In the first canteen, healthier products were added to the product range for 3 weeks, after which a nudge intervention consisting of salience, scarcity, availability and default nudges was implemented to promote the sale of these products for 26 weeks. This canteen was regularly visited by the researcher to ensure that the healthier products were available and that the nudge intervention remained correctly implemented during the intervention period. In the second canteen, the healthier products were added to the product range for 15 weeks, after which their sale was stimulated through the nudge intervention for 16 weeks. Contrary to the other canteen, this canteen was only regularly visited to observe whether the healthier products were available and whether the nudge intervention was implemented as originally planned, without interference when deviations from the protocol were observed. In addition to collecting data on intervention adherence and sales, intervention reach, acceptance, and applicability were measured using observations, questionnaires and interviews. Results showed an initial rapid increase in the percentage of sales of the nudged products from the total sales, which quickly decreased with time. The nudge intervention had a large reach and was accepted by both visitors and canteen personnel. More importantly, however, we found that adhering to the intervention by the nudgers was difficult in both canteens. Multiple factors of the nudge intervention, target group and organisational practices, as well as their interplay, were identified that could explain these results.

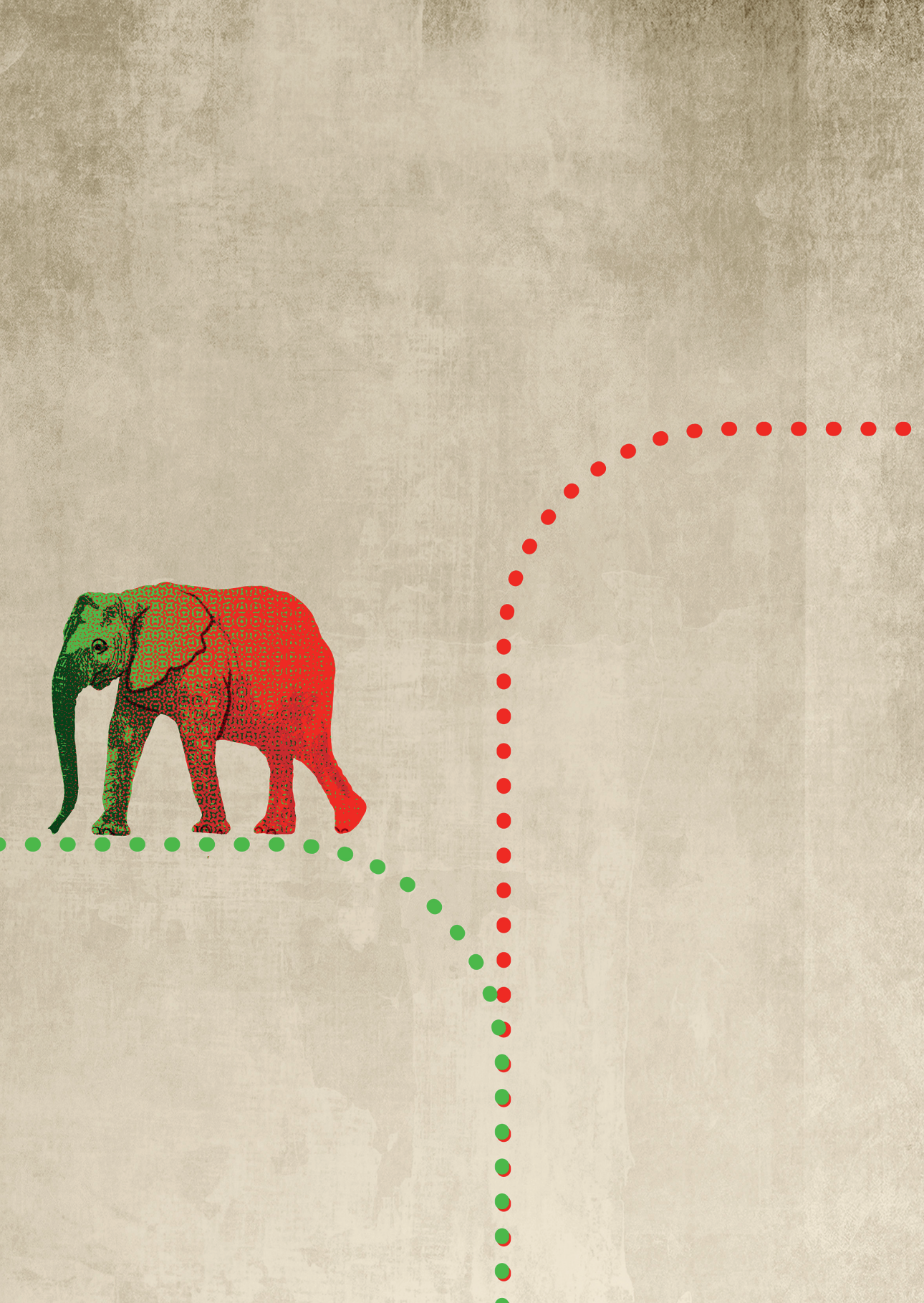
Since it proved difficult to implement a nudge intervention for a longer period of time in Chapter 2, in Chapter 3 three studies are described that investigated whether such maintenance is a prerequisite for any prolonged effects of nudging or whether nudges' effects can persist after removal (i.e. temporal spillover effect). Studies one and two served as proof-of-principle studies, in which the temporal spillover effect of a default nudge (preselection) on the decision to voluntarily complete a longer questionnaire was studied. Participants were asked to choose between a normal and a longer version of a questionnaire on two consecutive days. On the first day, the longer version of the questionnaire was preselected with half of the participants. On the second day, however, this default nudge was removed. As expected, the results showed that participants in the default nudge condition more often chose the longer version of the questionnaire on the first day than participants in the control condition. More importantly, on the second day, when the nudge was no longer present, this difference between the conditions persisted. Results also showed that this temporal spillover effect could be (partially) attributed to a change in attitude towards the nudged behaviour. The third study investigated whether this temporal spillover effect would also occur when a combined salience and default nudge is used to promote healthier food choices in a hypothetical food choice task. Results showed that the nudge did affect food choice on the first day, but that this effect did not spill over to the second day.

Chapter 4 describes a study which investigated whether the temporal spillover effect is affected by transparency manipulations. The relevance of this question was brought forward by rising ethical concerns about nudging. One of the proposed ways to (partly) mitigate these concerns is to explain the presence and/or purpose of the nudge to the nudgee (Schmidt & Engelen, 2020). Therefore, two studies were conducted using the same design as the proof-of-principle studies discussed in Chapter 3, with the addition of a transparent nudge condition. Taken together, the results showed no negative effect of the transparency manipulation on the temporal spillover effect of the default nudge.

Chapter 5 describes a study in which the prolonged effect and temporal spillover effect of self-nudging promoting fruit intake were investigated. Participants in the experimental condition had to choose one out of six accessibility, salience or reminder self-nudges promoting fruit intake and had to implement this self-nudge for eight weeks. After that, participants were asked to remove the self-nudge for one week. Results showed an increase in fruit intake in the experimental condition relative to the control condition during the eight weeks the self-nudges were implemented. This increase was accompanied by an increase in fruit intake habit strength. However, mixed results were found for the temporal spillover effect and the results did not support the hypothesis that the temporal spillover effect might be explained through an increased habit strength.

Chapter 6 provides a theoretical and methodological reflection on the various chapters. Moreover, it discusses implications for both research and practice. It can be concluded that nudges have the potential to influence behaviour beyond a single food choice. However, the results also exemplify that

eating behaviour is complex and multifaceted and that more research is needed to uncover which food choices in which context, and for whom, are especially suited for which types of nudge interventions to promote lasting effects. In this way, we can slowly uncover the role of nudging in addition to other measures to alter unhealthy eating patterns and associated health problems.



SAMENVATTING

Sinds de term 'nudging' in 2008 werd bedacht door Richard Thaler en Cass Sunstein, heeft de gedragsveranderingstechniek een enorme impact gehad op zowel onderzoek als beleid. Nudging verwijst naar subtiele veranderingen in de keuzearchitectuur die het gedrag van mensen voorspelbaar in een bepaalde richting sturen. Het inspireerde de oprichting van het Behavioural Insights Team (BIT) in het Verenigd Koninkrijk en verwante organisaties wereldwijd met onder meer als doel gedragsinzichten te genereren en toe te passen om de zorgwekkende trend in de toename van ongezonde eetpatronen, die kunnen leiden tot gezondheidsproblematiek zoals hart- en vaatziekten en diabetes type 2, aan te pakken (GBD 2017 Obesity Collaborators, 2019).

Ondanks de enorme populariteit van nudging is er echter weinig onderzoek gedaan naar de vraag hoe blijvend het effect van nudging op eetgedrag is. Het meeste onderzoek naar de effectiviteit van nudging richt zich uitsluitend op de effecten van nudging op één enkele voedselkeuze. In sommige gevallen kan het nudgen van een enkele keuze een enorme impact hebben, zoals bij de keuze om orgaandonor te worden. Maar om gezonde eetpatronen tot stand te brengen is het beïnvloeden van één enkele voedselkeuze niet voldoende.

Het doel van dit proefschrift was daarom om te onderzoeken in hoeverre nudges een blijvende invloed op eetgedrag kunnen hebben door 1) het proces en de effecten van de implementatie van nudges over een langere periode te onderzoeken en 2) te onderzoeken of, en onder welke omstandigheden, nudges gedrag kunnen beïnvloeden wanneer ze niet langer aanwezig zijn (d.w.z. het temporele spillover effect).

In Hoofdstuk 1 wordt de achtergrond van deze vraag beschreven en wordt ingegaan op de mogelijke manieren waarop nudges blijvende effecten kunnen hebben op eetgedrag.

In Hoofdstuk 2 wordt een casus beschreven waarin het effect en de haalbaarheid van langdurige implementatie van een nudge interventie in twee Nederlandse voetbalkantines werd onderzocht. In de eerste kantine werden gedurende 3 weken gezondere producten aan het assortiment toegevoegd, waarna de verkoop van deze producten gestimuleerd werd gedurende 26 weken middels een interventie waarin gebruik werd gemaakt van opvallendheids-, schaarste-, beschikbaarheids- en standaardnudges (in het Engels: salience, scarcity, availability en default nudges). Deze kantine werd regelmatig door de onderzoeker bezocht om ervoor te zorgen dat de gezondere producten beschikbaar waren en de nudges gedurende de interventieperiode op de juiste manier geïmplementeerd bleven. In de tweede kantine werden de gezondere producten gedurende 15 weken aan het assortiment toegevoegd, waarna de verkoop van deze producten middels de nudge interventie werd gestimuleerd voor 16 weken. Anders dan bij de eerste kantine, werd in deze kantine alleen geobserveerd of de gezondere producten nog beschikbaar waren en de nudges gedurende de interventieperiode op de juiste manier geïmplementeerd bleven, zonder dat de onderzoeker hier actief ingreep wanneer dit niet zo bleek te zijn. Naast het verzamelen van gegevens over de interventietrouw en verkoopcijfers, werden het bereik,

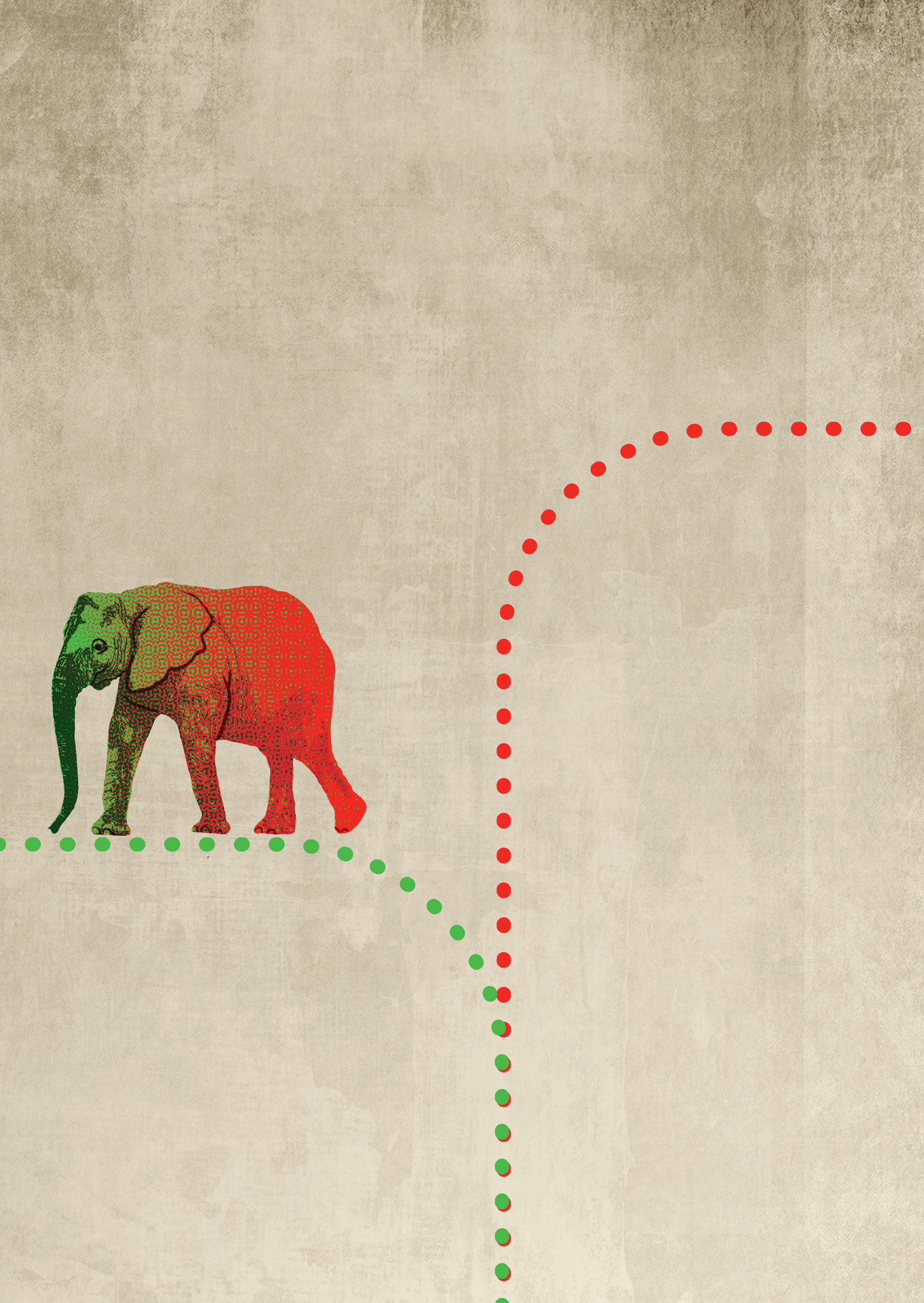
de acceptatie en de toepasbaarheid van de interventie gemeten door middel van observaties, vragenlijsten en interviews. De resultaten toonden een aanvankelijk snelle stijging van het verkooppercentage van de genudgde producten ten opzichte van de totale verkoop, welke daarna snel afnam. De nudge interventie had een groot bereik en werd door zowel bezoekers als kantinepersoneel geaccepteerd. Een belangrijkere bevinding was echter dat in beide kantines het opvolgen van de nudge interventie lastig bleek te zijn. Meerdere elementen van de nudge interventie, doelgroep en organisatie, alsmede hun interactie, werden geïdentificeerd die deze resultaten zouden kunnen verklaren.

Omdat het in Hoofdstuk 2 moeilijk bleek om een nudge interventie voor langere tijd te implementeren, wordt in Hoofdstuk 3 een onderzoek beschreven waarin bekeken werd of langdurige implementatie wel nodig is voor blijvende effecten, of dat de effecten van nudges kunnen aanhouden wanneer zij zijn verwijderd (d.w.z. het temporele spillover effect). Studies één en twee dienden hier als proof-of-principle studies, waarin het temporele spillover effect van een standaardnudge (preselectie) op de beslissing om vrijwillig een langere vragenlijst in te vullen werd onderzocht. Deelnemers werd gevraagd om op twee achtereenvolgende dagen te kiezen tussen een normale en een langere versie van een vragenlijst zonder dat zij een extra vergoeding zouden krijgen voor het invullen van de langere vragenlijst. Op de eerste dag werd de langere versie van de vragenlijst voorgeselecteerd bij de helft van de deelnemers. Op de tweede dag werd deze standaardnudge echter weggehaald. Zoals verwacht lieten de resultaten zien dat deelnemers waarbij de langere vragenlijst op dag één was voorgeselecteerd vaker kozen voor de langere versie van de vragenlijst op de eerste dag dan deelnemers in de controle conditie. Verrassender is echter de bevinding dat ook op de tweede dag, toen de nudge niet meer aanwezig was, dit verschil tussen de condities aanhield. Uit de resultaten bleek tevens dat dit temporele spillover effect (gedeeltelijk) kon worden toegeschreven aan een verandering in de attitude ten opzichte van het genudgde gedrag. In studie drie werd onderzocht of dit temporele spillover effect ook optreedt wanneer een gecombineerde opvallendheids- en standaardnudge wordt gebruikt om gezondere voedselkeuzes te stimuleren in een hypothetische voedselkeuzetaak. Uit de resultaten bleek dat de nudge op de eerste dag wel invloed had op de voedselkeuze, maar dat dit effect niet oversloeg naar de tweede dag.

In Hoofdstuk 4 wordt een onderzoek besproken waarin werd onderzocht of het temporele spillover effect beïnvloed wordt door transparantie manipulaties. De aanleiding voor deze vraag kwam voort uit de toenemende zorgen omtrent de ethische kwesties rondom nudging. Om deze zorgen (deels) weg te nemen, is voorgesteld om nudges transparant te maken, wat betekent dat de aanwezigheid en/of het doel van de nudge wordt uitgelegd aan degene die genudged wordt (Schmidt & Engelen, 2020). Daarom werden twee studies uitgevoerd, die dezelfde opzet gebruikten als de proof-of-principle studies zoals besproken in Hoofdstuk 3, met als toevoeging een conditie met daarin een transparante nudge. Samengenomen toonden de resultaten geen negatief effect van de transparantie manipulatie op het temporele spillover effect van de standaardnudge.

In Hoofdstuk 5 wordt een studie besproken waarin het effect van zelf-nudging om fruitinname te bevorderen over langere tijd alsmede het temporele spillover effect werden onderzocht. Deelnemers in de experimentele conditie moesten één uit zes opvallendheids- toegankelijkheids- (in het Engels: accessibility), of herinnerings- (in het Engels: reminder) zelf-nudges die de inname van fruit stimuleren kiezen en gedurende acht weken implementeren. Daarna werd de deelnemers gevraagd om de zelf-nudge gedurende één week te verwijderen. De resultaten toonden een toename in fruitinname in de experimentele conditie aan ten opzichte van de controle conditie gedurende de acht weken dat zelf-nudge geïmplementeerd was. Deze toename ging gepaard met een toename van de gewoonte om fruit te eten. Er werden echter gemengde resultaten gevonden wat betreft het temporele spillover effect en in de resultaten werd geen ondersteuning gevonden voor de hypothese dat het temporele spillover effect verklaard zou kunnen worden door een toegenomen gewoontesterkte.

Hoofdstuk 6 biedt een theoretische en methodologische reflectie van de verschillende hoofd-stukken. Bovendien worden implicaties voor zowel onderzoek als praktijk besproken. Er kan worden geconcludeerd dat de effecten van nudges verder kunnen reiken dan het beïnvloeden van een eenmalige voedselkeuze. Hierbij moet echter niet uit het oog worden verloren dat eetgedrag complex is en dat meer kennis nodig is over welke nudges onder welke omstandigheden, voor wie en gericht op welke eetkeuzes het beste langdurig werken. Zulke kennis geeft inzicht in de rol die nudging in aanvulling op andere beleidsmaatregelen kan hebben bij het aanpakken van ongezonde eetpatronen en daaruit voortvloeiende gezondheidsproblematiek.



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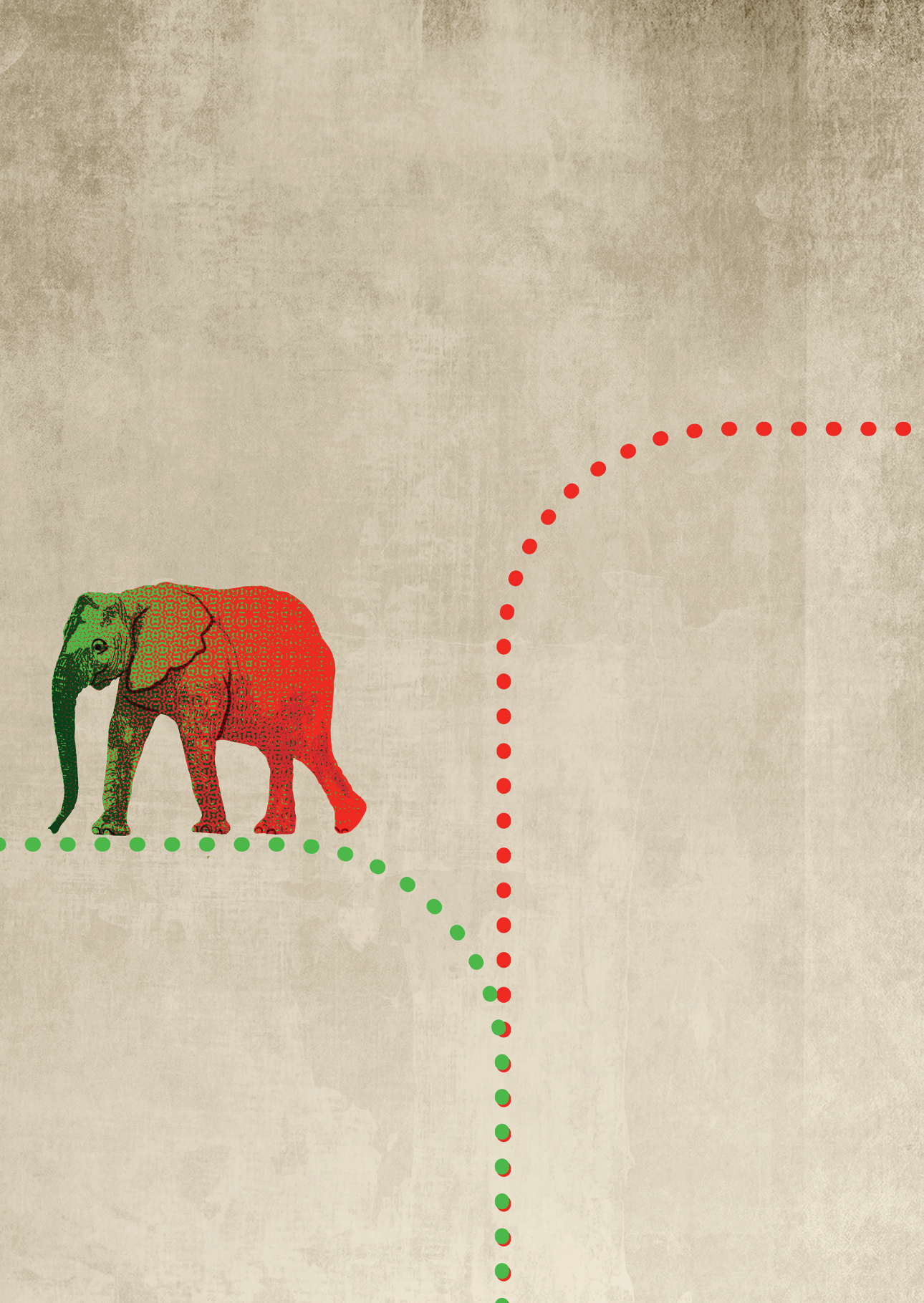

ABOUT THE AUTHOR

Wilhelmina Elisabeth Adriana Metje (Merije) van Rookhuijzen was born on 21 January 1992 in Wageningen, the Netherlands. After she received her secondary school diploma from the Stedelijk Gymnasium Nijmegen, she completed the Bachelor's programme Psychology at Radboud University, Nijmegen (2010-2013, *cum laude*). During this time she took part in the interdisciplinary Honours Programme. She then completed the Master's programme Health-Care Psychology at the Radboud University in Nijmegen with a special focus on clinical neuropsychology (2013-2014). She was an intern at rehabilitation centre Groot Klimmendaal in Arnhem and wrote her thesis on the effect of errorless learning on learning ability in brain-injured patients with executive dysfunction. She then completed the Master's programme Health and Society at Wageningen University (2014-2016), where she also worked as a student assistant. She was an intern at Kennisinstituut Bier and wrote her thesis on the effect of social norms on healthy eating behaviour.

In 2016, Merije started her PhD trajectory as member of the Strategic Communication group and later as member of the Consumption and Healthy Lifestyles group at Wageningen University. Her PhD project was part of the Health Improvement Through Nudging Techniques (HINTS) project, a collaboration between Wageningen University and Utrecht University. Here, she investigated the potential of nudges to impact eating behaviour. During her PhD trajectory, she followed numerous courses, was a member of several councils and committees, was involved in teaching, supervised BSc and MSc thesis students, and presented her work at national and international conferences.

As of 2021, Merije is working as education coordinator and teacher at the Consumption and Healthy Lifestyles group at Wageningen University.

Wilhelmina Elisabeth Adriana Metje van Rookhuijzen
Wageningen School of Social Sciences (WASS)
Completed Training and Supervision Plan

Name of the learning activity	Department/Institute	Year	ECTS*
A) Project related competences			
Writing PhD proposal	COM	2016-2017	4
CREATE workshop	University of Padua, Italy	2017	1
'Social norms: Can we use them to promote healthy eating?'	EHPS, University of Padua, Italy	2017	0.5
Symposium: Food labels	Museum Speelklok, Utrecht	2017	0.3
Theorizing consumers and consumption: A canon of classics	SCH/WUR	2017	4
Introduction to programming in R for social sciences	WASS/WUR	2018	2
Linear models	PE&RC and SENSE/WUR	2018	0.9
Mixed linear models	PE&RC and SENSE/WUR	2018	0.6
Oral presentation: 'Nudges, easy, cheap and effective, are they?'	ARPH 2018, Tilburg University	2018	1
Winterschool on bounded rationality	TAPMI-MPIB-SOTON, Manipal, India	2018	1.5
Symposium: Nudging healthy food choice	Winkel van Sinkel, Utrecht	2018	0.5
Masterclass Food environments and public health: Interdisciplinary perspectives and prospects	KNAW Amsterdam	2019	0.5
'Nudging healthy eating in Dutch football canteens: A multi-method case study'	WINK 2019, University Hall, Utrecht	2019	1
'Nudges: One hit wonders or a sustainable tool for healthy eating?'	ARPH 2019, Hotel Zuiderduin, Egmond aan Zee	2019	1
'Nudges: One hit wonders or sustainable tools? Exploring the aftereffect of a default nudge promoting prosocial behaviour'	WASS PhD Day	2020	1
'The aftereffect of nudges'	ARPH 2020, Hotel Zuiderduin, Egmond aan Zee	2020	1
'Nudges, spillover effects and transparency'	ARPH 2021, Hotel Zuiderduin, Egmond aan Zee	2021	1
'Nudging, meer dan een éénmalig duwtje in de rug?'	WeVo 2022, WUR	2022	1
'When nudgees become nudgers: Exploring the use of self-Nudging to promote fruit intake'	ARPH 2022, Hotel Zuiderduin, Egmond aan Zee	2022	1

Name of the learning activity	Department/Institute	Year	ECTS*
B) General research related competences			
WASS Introduction course	WASS/WUR	2016	1
Brain training	WGS/WUR	2017	0.3
Communicating with children	Wetenschapsknooppunt	2017	0.6
Data management planning	WGS/WUR	2017	0.4
PhD workshop carousel 2017	WGS/WUR	2017	0.3
Philosophy and ethics of food science & technology	WGS/WUR	2017	1.5
PhD workshop carousel 2018	WGS/WUR	2018	0.3
PhD workshop carousel 2019	WGS/WUR	2019	0.3
Reviewing a scientific manuscript	WGS/WUR	2019	0.1
C) Career related competences/personal development			
Competence Assessment	WGS/WUR	2017	0.3
Membership WASS PhD Council	WASS/WUR	2017-2019	2
Supervising BSc thesis (4) and MSc thesis (5) students and teaching in various courses	COM/CHL	2017-2022	4
Start to Teach	WGS/WUR	2019	0.3
Supervising BSc & MSc students	Education Support Centre	2019	0.1
Career orientation	WGS/WUR	2020	1.5
Total			**36.8**

*One credit according to ECTS is on average equivalent to 28 hours of study load.

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