Fields of gold

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Financial Decision-Making in the Wild

Inka Eberhardt

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Inka Eberhardt

Maastricht University School of Business and Economics

Maastricht, Netherlands

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Financial Decision- Making in the Wild

DISSERTATION

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by

Inka Eberhardt

Für meine Familie

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"Why do you go away? So that you can come back. So that you can
see the place you came from with new eyes and extra colors. And
the people there see you differently, too. Coming back to where you
started is not the same as never leaving." -Terry Pratchett

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0. ACKNOWLEDGEMENTS

Contents

List of Figures

List of Tables

Chapter 1

Introduction

> "Nobody has really tried to understand me or my behavior, which just reflected my decision to enjoy life to the full." - Princess Stéphanie of Monaco

At first sight, starting a thesis on financial behavior in the field with the quote above might seem counterintuitive. On the one hand, one could think that the explanation for the Princess' behavior is clear and easy to come by, as it is all about enjoying life. However, coming to this conclusion is only this simple in hindsight and due to the fact that Princess Stéphanie hands us an explanation on the silver platter. On the other hand, it sounds as if no one has tried to understand the Princess Stéphanie because of her behavior: it might have been deemed "irrational", "too spontaneous", or "too irregular" to be explainable or predictable. I nevertheless start this thesis with this quote because of two of its parts I find intriguing. First, trying to *really* understand someone and someone's behavior is a difficult undertaking. As I lay out later in this introduction, human behavior is influenced by many different factors, not all of which are observable or fit to be impacted by a researcher studying human behavior in the field. Second, the "decision to enjoy life to the full" does not need to lead to irrational behavior at all. In fact, most economists assume that individuals maximize their utility (i.e., well-being), and that maximizing society's utility should be the goal for policy-makers. Nevertheless, behavioral economists have found some instances

where individual's behavior contradicted what classical economists had predicted. For example, why do people not save for retirement by themselves, if it's in their interest? And why do people donate money to charities and other beneficiaries?

To provide more background on behavioral economics, I lay out interesting findings that behavioral economics has so far provided us with in this introduction. I primarily focus on behavior in the field as well as findings relating to the two main research areas of this dissertation: retirement planning behavior and giving behavior. Afterwards, I briefly summarize the remaining chapters of this thesis and discuss how they relate to the findings of behavioral economics.

In the 20th century, the biggest stream in economics was neo-classical economics. Neo-classical economists have often been said to have (hypothetically) invented a new human species, the homo economicus. The homo economicus is a rational, omniscient type of a human being that is similar to a super computer (Camerer and Loewenstein, 2003; Thaler, 2016a). The homo economicus has three main features. First, she has "well-defined preferences and unbiased beliefs and expectations" (Thaler, 2016a). She does not care whether her new car would be a BMW or an Opel, as long as the technical features of the car fit her preferences best. Second, she always chooses the best alternative given her beliefs and preferences. To do this, she has immense cognitive abilities and the strongest willpower. Knowing that industrial sugar is bad for her health, a homo economicus would always prefer fruit to chocolate-chip cookies. Third, a homo economicus only acts out of self-interest. She thus would help a friend move, but only when her short-term benefits (free pizza) and her long-term benefits (her friend helping in return) would be greater than her costs (Thaler, 2016a). Over time, behavioral and psychological literature increasingly influenced economics and finance (Camerer and Loewenstein, 2003).[1] In an article called "Psychology and Economics", Rabin (1998) defined three main breaches of neoclassical economics: non-standard preferences, non-standard beliefs, and non-standard decision-making.[2]

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[1]Before the neoclassical times, economists such as Adam Smith, Jeremy Bentham and Francis Edgeworth actually researched and discussed human psychology, too (Camerer and Loewenstein, 2003).

[2]Of course, given the widespread presence of these "non-standard" preferences, beliefs and decision-making processes, one can argue for renaming them "standard preferences", "standard

Non-standard preferences are risk preferences, time preferences, and social preferences (DellaVigna, 2009). They breach the first and third feature of homo economicus. Risk preferences describe how much risk a person is willing to accept for a specific gain. Prospect Theory, first theorized by Tversky and Kahneman (1979), suggests that people do not look at the absolute gain or loss they expect from an event, but rather at their subjective value of the outcome relative to a reference point (often the status quo). According to Prospect Theory, people overweight small probabilities, underweight large probabilities, avoid risks when a gamble is about gains, and accept risks when a gamble is about losses. In Chapter 2 we utilize these insights and analyze whether and which type of lottery can trigger most people to look up retirement information.

According to the neo-classical model, people have time-consistent preferences. This means that I would feel as excited about a pizza that I will eat in thirty minutes as about a pizza that I will eat in a year. Studies by Loewenstein and Prelec (1992) and Frederick, Loewenstein, and O'Donoghue (2002), however, have found that people are present-biased, meaning they place a higher value on anything in the present than in the future.[3] These time preferences have real life consequences, of course. Paired with the non-standard belief of overconfidence, they lead to more homework completion with intermediate deadlines, but also to the fact that self-set deadlines are not optimally set (Ariely and Wertenbroch, 2002). If people value current consumption higher than future consumption, they will not save for retirement by themselves. Self-aware individuals who nevertheless want to save to smooth consumption over their lifetime might use a commitment device so as to not spend their savings before reaching retirement. An example for such a commitment device is a savings account that restricts the number of withdrawals within a time period or from which one can only withdraw after a certain period of time or at a certain age (Ashraf, Karlan, and Yin, 2006; Beshears, Choi, Harris, Laibson, Madrian, and Sakong, 2020). Self-control problems can

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beliefs" and "standard decision-making". I use "non-standard" here to keep in line with Rabin (1998)'s and DellaVigna (2009)'s wording.

[3]Laibson (1997) and O'Donoghue and Rabin (1999) extended the standard, neoclassical utility model and introduced a parameter which captures self-control problems aka present-bias.

1. INTRODUCTION

also explain default effects in retirement savings decisions. People display inertia to enroll in a pension fund, so changing the default from actively having to enroll to automatically enrolling new employees with a certain contribution rate and investment portfolio often sticks and increases account balances (Choi, Laibson, Madrian, and Metrick, 2004; Madrian and Shea, 2001). Note that in these studies, individuals can opt out of the savings funds, change their contribution rate and/or investment portfolio if they wanted.

Thaler and Benartzi (2004) used another type of commitment device to increase retirement savings, also taking into account the concept of loss aversion from Prospect Theory. In the Save More Tomorrow™ program, individuals agree to save a portion of their future salary increases in their retirement savings accounts. This way they do not feel the loss of foregoing current income. Thaler and Benartzi (2004) find that 78% of people who have been offered the program joined it; most remained in it; and on average, average saving rates increased from 3.5% to 13.6% over the course of 40 months.

Social preferences relate to preferences for fairness, reciprocity, and altruism (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999; Levine, 1998; Rabin, 1993). In a neo-classical world, people care only about their own well-being, so any of the above would not matter for decision-making. In the laboratory, the results of numerous experimental games have shown that many participants behave "non-rationally" in that they also care about others' utility. A clear example is the Dictator Game. In this game, one player (i.e. the dictator) receives an endowment, classically $10. She then decides whether she wants to give some of her endowment to a second, anonymous player. The game is played only once with someone she does not know. She cannot build a reputation with the other player nor can she ask for or expect a reward or punishment from the other player. If she does not care about the other player's happiness or financial well-being, she will not give any money away. However, Forsythe, Horowitz, Savin, and Sefton (1994) find that 60 % of participants give money to the other player, therefore acting altruistically. In the laboratory, these and other experiments on social preferences have been replicated several times and recapped in Charness and Ra-

bin (2002) and Fehr and Gächter (2000). Humans thus take into account the feelings of others when they make decisions.

In the field, altruism is a crucial factor in the area of charitable giving. Andreoni (1989, 1990) and Harbaugh, Mayr, and Burghart (2007) find that individuals do experience an increase in well-being from their own donations. However, altruism also has its limits. In fund-raising field experiments, people give more when the charity has already received more seed money (List and Lucking-Reiley, 2002). The authors reckon that seed money signals a charity's quality, and is rather a sign of impact of money given. In a door-to-door fund-raising experiment, people (especially men) gave also more if asked by an attractive female solicitor (Landry, Lange, List, Price, and Rupp, 2006). DellaVigna, List, and Malmendier (2012) wanted to find out whether giving is due to altruism or social pressure. One day before a door-to-door fundraising campaign, they send a randomly-selected group of households a flyer stating the time of the future visit. Another group receives a flyer stating the time and containing a "Do Not Disturb"-box. In accordance with the reasoning that people give because of social pressure, 10 to 25% less households open the door in the treatment groups, compared to the group that did not receive a notification the day before. The authors find that less smaller donations have been made in the flyer group with the "Do Not Disturb"-box, while larger donations (i.e. donations above $10) are not affected by the intervention. The larger donations seem to be given due to altruism, while smaller donations seem to be given due to social pressure. A study that combines previous results regarding time and social preferences is Breman (2011). In her study, Breman (2011) uses the Save More Tomorrow™ concept for donations. She finds that asking monthly donors to raise their contributions in a month or in two months increases donations, compared to asking donors to increase contributions immediately. In Chapter 3 we analyze which considerations influence giving behavior the most when selecting between beneficiaries: trying to increase overall well-being or giving to the candidate who is most deserving?

Non-standard beliefs about the different states of the world distort "rational" decision-making just as much as non-standard preferences. They breach

1. INTRODUCTION

the first feature of homo economicus. one example of a non-standard belief is overconfidence. Overconfidence is at play when 93 % of respondents think that their driving skills are above the median driving skills (Svenson, 1981); when the majority of respondents are overly optimistic about their chance to experience a positive life event and to forego a negative life event (Weinstein, 1980); or when people think they will finish a project faster than they actually do (Buehler, Griffin, and Ross, 1994). In the field, overconfident individual investors seem to overestimate their stock picking skills and trade too much (Odean, 1999). This is costly due to transaction costs, but also as the sold stocks overperform the purchased stocks in the year after the transaction, on average. Barber and Odean (2001) also find that men are more overconfident than women as the former trade 45 % more than the letter. This can severely hurt retirement investors.

Last, but not least, aspects of non-standard decision-making breach the second feature of homo economicus. Even when taking into account non-standard preferences and non-standard beliefs into a utility-maximizing model, the context in which decisions are taken matter for the outcome of the decision. The clearest example of this is framing, when the same situation is described in different ways. The classic example is the Asian disease example in Tversky and Kahneman (1981). In the experiment, an Asian disease might outbreak in the United States of America. There are two programs that mitigate the consequences of the disease. The programs and their outcomes are described in two different frames. In the experiment, only one frame is given to one group. The table below presents the outcome of the two programs framed in two ways for a group of 600 people in total. Experiment participants would either see the first two rows or the last two rows.

Program A	Program B
400 people die	2/3 probability that 600 people die
	1/3 probability that 0 people die
Program C	Program D
200 people are saved	1/3 probability that 600 people are saved
	2/3 probability that 0 people are saved

6

Program A and Program C are the same, as are Program B and D. However, in Programs A and B, outcomes are described in losses (death), while outcomes are described in gains (lives saved) in programs C and D. In the loss decision frame, 72 % preferred Program B, while Program C was preferred by most in the gain decision frame.

In the field, Benartzi and Thaler (2002) asked employees who actively opted out of a customized portfolio to rate their portfolio, the average portfolio, and the customized portfolio, given information about projected retirement income. Framed in terms of outcome, 61 % ranked the customized portfolio higher than their own portfolio. Framing is also important to mitigate the annuitization puzzle (in markets where people select their retirement income products, most underannuitize and thus are not insured against outliving ones savings. Describing the features of annuities in a consumption frame can increase demand compared to using an investment frame (Brown, Kling, Mullainathan, and Wrobel, 2008). Beshears, Choi, Harris, Laibson, Madrian, and Sakong (2020) use framing to increase retirement savings. In a field experiment, university employees received mailings asking them to increase their contribution to a savings plan in the future. In the treatment group, the date was put in relation to a fresh start such as a birthday or the beginning of spring. Participants who received the framed mail were more likely to increase their future contributions without decreasing contributions at the time of mailing.

Homo economicus is all-knowing, making decisions with every available, relevant piece of information. Homo sapiens, however, has limited attention. In the domain of finance, inattention to financial news leads to investors' underreaction to new information (Huberman and Regev, 2001). Distraction also leads to underreaction. Investors react less to news that is released shortly before the weekend, which can explain the fact that worse earnings are often made public on Fridays (DellaVigna and Pollet, 2009). Investors also react more slowly to news on days where there are more announcements than one (Hirshleifer, Lim, and Teoh, 2009). Inattention to information can also be strategic, as it can prevent feeling disappointment when news are negative (Golman, Hagmann, and Loewenstein,

1. INTRODUCTION

2017; Karlsson, Loewenstein, and Seppi, 2009; Sicherman, Loewenstein, Seppi, and Utkus, 2016). Another study that demonstrates that individuals do not use every piece of information at hand is Bateman, Dobrescu, Newell, Ortmann, and Thorp (2016). In a laboratory experiment around simplified investment disclosure, they find that choices in the experiment of a third of participants are unrelated to the disclosure content.

As Gigerenzer (2008) puts it, "most problems of any importance are computationally intractable" (p. 20). It therefore is not a surprise that people ask for advice when making financial decisions. However, many people ask for advice from their spouses, friends or colleagues rather than from sector's professionals. Financial illiteracy as well as financial adviser anxiety reduces advice seeking (Calcagno and Monticone, 2015; Gerrans and Hershey, 2017).

Another interesting factor involved in non-standard decision-making is social pressure. Asch (1951) and Milgram (1963) have prominently shown that people answer wrongly when the majority does so and that the majority of people also would inflict electric shocks up to a level of 450 volts on another person if they are told to do so. In the field, social pressure can explain that extra time in a football [4] match is twice as long when the local team is one goal behind than when the local team is one goal ahead (Garicano, Palacios-Huerta, and Prendergast, 2005). The effect is larger when the end of the season nears and when more people are in the stadium.

Social pressure might also be one reason why information treatments that use information about peers are effective in changing behavior (Allcott, 2011; Buunk and Gibbons, 2007; Goldstein, Cialdini, and Griskevicius, 2008; Schultz, Nolan, Cialdini, Goldstein, and Griskevicius, 2007). In the retirement research field, Duflo and Saez (2003) show that employees who worked in a department where other colleagues were incentivized to go to a retirement information fair were three times as likely to go to the fair than employees who worked in a department where noone was incentivized to go to the fair. In contrast, Beshears, Choi, Laibson, Madrian, and Milkman (2015) show that providing information

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[4]For American and Australian readers: soccer.

about the percentage of coworkers in a similar age range contributing at least 6% to their 401 (k) savings plan decreases savings and savings rates. In Chapter 2, we analyze whether peer information treatments can increase the likelihood to look up retirement information.

Last but not least, emotions and mood play a role in decision-making of people who are not the classical homo economicus (Loewenstein and Lerner, 2003). On days on which the sun shines longer, tips are higher in restaurants (Rind, 1996) and prices are higher for low priced paintings (De Silva, Pownall, and Wolk, 2012). On days on which clouds cover the sky, aggregate stock returns are lower (Hirshleifer and Shumway, 2003; Saunders, 1993). When the national team loses, the losing country's stock returns are lower than on a day without a match (Edmans, Garcia, and Norli, 2007). For giving behavior, emotions can be used to attract more donations. Small and Loewenstein (2003) find that using a specific person in an advertisement for a charity increases the likelihood that people give. Using a sad face in the advertisement also increases donations(Small and Verrochi, 2009). Emotions might also be why students do not maximize overall well-being, as utilitarianism would predict. On average, students do not give more to a charity that saves four bald eagles than to a charity that saves one bald eagle (Hsee and Rottenstreich, 2004). This phenomenon is called "scope insensitivity". Hsee and Rottenstreich (2004)'s findings are replicated by Hasford, Farmer, and Waites (2015). Additionally, they find that scope insensitivity and emotional intelligence are correlated: The lower people score on emotional intelligence, the more scope sensitive they are.

Given that decisions are context specific, what can policy makers do to maximize well-being? Well, they can try to increase well-being by "organizing the context in which people make decisions" (Thaler, Sunstein, and Balz, 2013, p.428). This is called choice architecture. Nudges are the intervention of the 2017 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, Richard Thaler. In his book with Cass Sunstein, he describes nudges as one tool set for choice architects. Nudges are "approaches that steer people in particular directions, but that also allow them to go their own way" (Sunstein, 2018;

1. INTRODUCTION

Thaler, 2016b; Thaler and Sunstein, 2008, p.61). Examples of nudges are a GPS device, a calories-telling app, text or e-mail reminders by GP practices about appointments, graphic warnings, labels about nutrition of food or energy efficiency, defaults, and messages containing social norms (Halpern, 2015; Jolls, 2015; Sibony, Helleringer, and Alemanno, 2016; Sunstein, 2017). Nudges should remind or push people to do something (or nothing, in the case of defaults) so that the social good is enlarged. Sunstein (2017) has surveyed when nudges fail: when the nudged have strong preferences against the nudge, when there are nudges that counter the intended nudge, when there is confusion, when nudges lead to reactance, when nudges do not work because they are badly-designed, and when they lead to compensating behavior. The effectiveness of nudges is usually tested with the use of experiments.

Experiments are helpful as "experimental control is exceptionally helpful for distinguishing behavioral explanations from standard ones" (Camerer and Loewenstein, 2003, p.7). Nevertheless, as we have seen with decision making, context is also important (Goldstein and Weber, 1995; Loewenstein, 2001). Findings in the lab do not always generalize to the field (at least to the extent as predicted by lab experiments), and sometimes it is difficult to test the exact same treatments in both research settings.[5]

Harrison and List (2004) provide six criteria that define field experiments. First, the nature of the subject pool. In clear-cut laboratory experiments, the experiment participants are students. Any participant who is not a student is deemed a field subject (of course, the setting of a field experiment can be a university, making students field subjects as well). Second, the nature of information the subject brings to the task. In the field, people mostly bring experience with the task to the "experiment". An example of this is that people have some knowledge about the commodity used in the experiment. Third, the nature of the commodity. In field experiments, the commodity of the experiment is a real service or consumption good. In laboratory experiments, commodities can be hypothetical.

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[5]See Camerer (2011) and Al-Ubaydli and List (2013) on a discussion "On the Generalizability of Experimental Results in Economics" as well as Al-Ubaydli, List, and Suskind (2017) on scalability.

Fourth, the nature of the task. In field experiments, the experimenter examines real choices, not hypothetical choices. Fifth, the nature of the stakes. Given that the experiment participant makes real choices, the stakes of the experiment are real as well. Sixth, the nature of the environment. As we have seen above in the discussion on context, the "environment can provide context to suggest strategies and heuristics that a lab setting might not" (p. 1013).

What works in the field, and why? In this dissertation, I look at two different economic behaviors, namely retirement planning and giving behavior. Table 1.1 gives an outline of Chapters 2 and 3.

In both chapters, we examine financial behavior "in the wild", making use of the gold of field data. In Chapter 2, we look at active pension fund members' decisions to look at personal pension information and to increase their retirement savings. In Chapter 3 we analyze giving behavior and considerations to give a maximum of €10,000 to candidates in a TV show.

1.1 Pension Communication, Knowledge, and Saving Decisions

In Chapter 2 we focus on behavior in the pension context. Undersaving in retirement is an issue in most developed countries (EU, 2018). Even in a more generous Defined Benefit system like the Dutch pension system, a third of the population is at risk of an inadequate pension, given the low interest rates of the last years and an ageing population (de Bresser and Knoef, 2015). We adapt Bateman, Louviere, and Thorp (2014)'s Decision States Model to conceptualize the voluntary retirement savings process. In this model, fund members go through different states consequentially. Each state is important, but the time in each state differs per individual (based on factors such as financial literacy). In the beginning, individuals are unaware of a potential savings gap. Pension fund communication can make them aware of a potential savings gap, so that they then look up personal retirement information. By learning about their financial situation,the pension

1. INTRODUCTION

Table 1.1: Dissertation Outline

	Chapter 2		Chapter 3
	Study 1	**Study 2**	**Study 3**
Research Question	Can peer information or financial incentives motivate pension fund members to look up pension information?	Can different financial incentives motivate pension fund members to look up pension information, increase pension knowledge, and increase self-reported savings?	Utilitarianism or equity and desert- why do people give to certain causes?
Independent Variable(s)	Looking up pension information online (binary)	Looking up pension information online (binary), pension knowledge (0-6), self-reported savings behavior (binary)	Percentage of audience members donating to a candidate in a Dutch TV show (0-100)
Research sample	Pension plan participants in the Netherlands (N=245,712)	Pension plan participants in the Netherlands (N=247,433)	Candidates of the TV show "Geld Maakt Gelukkig" (N=165)
Data	Logging data, administrative data	Logging data, administrative data, survey data	Aggregrate data on giving decisions in the TV show, survey with raters
Methodology	RCT, ordinary least squares regressions	RCT, ordinary least squares regressions, instrumental variable regressions	Ordinary least squares regressions
Main results	Peer information letters ineffective, the Financial Incentive-letter increased the likelihood to look at pension information by 1.1pp	Few large financial incentives increase information search more than many small incentives; looking at pension information does not increase pension knowledge nor self-reported savings behavior	One st. dev. increase in gift's impact increases % of people donating to beneficiary by 6.4pp; one st. dev. increase in beneficiary's personal responsibility decreases % of people donating to beneficiary by 4.4pp

system and how to fill a savings gap, they become knowledgeable. The last step is to save.

We use two studies with fund members from a large Dutch pension fund to test how to motivate pension plan participants to look at personal pension information online. In both studies we sent letters to pension fund members to

make them aware about a potential pension gap. All letters contain the text of the control letter, but we add one sentence each to the treatment letters. In Study 1 (n=245,712 participants), four letters contain an additional sentence about peer information. We use a 2x2 design to examine whether a focus on retirement income (i.e. the goal of saving) or a focus on savings (i.e. the means to get retirement income) motivates more people, and whether a fear appeal or a hope appeal works better. The financial-incentive letter informs participants about a raffle of 100 vouchers, each worth €25.

We observe who logs into a personal pension platform and who looks at the pension planner, a site where one can find out about how much pension one is to receive. We find that the peer information treatments are ineffective. Fund members who received the Saving-Fear or the Income-Hope letters are even 0.3 percentage points less likely than fund members who received the control letter to look at the pension planner. Fund members who received the Financial-Incentive letter, however, have an increased rate of looking up information of 1.1 percentage points, a 50% increase compared to the rate of the control group.

In Study 2 (n=257,433 participants), we test what type of financial incentive increases the likelihood to look at personal information online the most. We also analyze whether looking up information has downstream consequence. Do people who look at the pension planner increase their pension knowledge? Do they report more savings? We again use six letter types: one control group, and five financial incentive letters. Each financial incentive letter announces a raffle of €2,000, with varying number of vouchers and amounts won per voucher. We again observe who looks at the pension planner. Three weeks after our intervention, we invite fund members to a survey to measure pension knowledge with a quiz of 5 questions, and to measure whether fund members have changed their savings behavior after the intervention.

We find that participants in the lottery that raffled two large amounts are 86% more likely to look at the pension planner than are participants in the control group (an increase from 4.3% in the control group to 6.7%). While we find a positive correlation between looking at the pension planner and pension knowl-

edge, this relationship is not significant when we use an Instrumental Variable regression. We also do not find a positive effect of looking at the pension planner on self-reported savings behavior.

For both studies, we do not find heterogeneous effects. Financial incentives work for all sub-groups of our sample, while peer information treatments are ineffective or have a negative effect. We find robust effects of the financial incentives treatments in Study 2 for when we control for participation in Study 1, and find no spill-over effects of the financial incentives treatments in both studies.

Our results are in line with Beshears, Choi, Laibson, Madrian, and Milkman (2015) who find detrimental effects of peer information on retirement savings decisions. Wee see that peer information treatments also are ineffective for looking up pension information. Financial incentives, however, are effective, just as in Duflo and Saez (2003). We add to the literature of financial incentives that one does not need to increase the budget for lottery incentives to raffle more prize money. Raffling the same overall amount, but splitting the prize money into large amounts is also effective in increasing the likelihood to look up information. Nevertheless, we do not find an effect on pension literacy and savings behavior.

1.2 Deciding Between Beneficiaries to Donate To

Chapter 3 focuses on the topic of giving behavior. We are specifically interested in why people give to certain causes, but not to others. Once the decision to give has been made, what impacts the decision to give to a specific beneficiary? Are these decisions influenced by utilitarian thinking or considerations about equity and desert? Utilitarians would give to the cause that maximizes the impact of their pecuniary gift. People that follow considerations of equity and desert would give to the beneficiary who deserves it the most, based on the beneficiary's personal

control over her situation.[6]The decision setting of our study is closest to that of donation-based crowdfunding. One has decided that one wants to give to a cause via the platform, but one still needs to select the beneficiary to give to out of a given set of projects or beneficiaries.

We look at data from a Dutch TV show called *Geld Maakt Gelukkig* (literally translates to "Money Makes Happy" in English). In this TV show, three candidates each describe why she is in need and asks for an amount of up to €10,000. The audience consists of 100 people. At the end of the show, each audience member is equipped with €100 and decides which candidate receives her €100. We have aggregate data for 165 candidates of 55 episodes. In order to receive a measure of money's impact on the candidate and her surrounding as well as a measure of personal control, we asked raters to watch the episodes and provide us with respective scores. We match the data about which candidate received how much money with these scores from raters. We also obtained scores on candidates' attractiveness, and age.

We regress the percentage of money received by each candidate on these variables and the candidate's gender. An increase of impact of one standard deviation increases the percentage of how many audience members donated to the beneficiary by 6.4 percentage points, on average. A one standard deviation-increase in Personal Control decreases the percentage of audience members donating to a candidate by 4.4 percentage points. In a dominance analysis, we find that the impact variable can explain 50% of the variation explained of our regression model, and Personal Control can explain 30%. Both impact and equity and desert considerations are important for giving decisions. These findings are robust to restricting our rater sample to rates who have never seen the TV show before, different variable specifications, adding the requested amount as a control variable, and excluding candidates who represent someone else.

The extent to which utilitarian considerations influence giving behavior in our study is surprising, given that most studies find emotional factors to play a bigger

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[6]In the redistribution context, theories of equity and desert investigate which factors are important to restore justice (i.e., what determines desert) and how these factors determine just redistributions (i.e., equity) (Konow, 2003).

part than impact deliberations (examples are Andreoni (1990); Berman, Barasch, Levine, and Small (2018)). In our study, the audience has more detailed information about the candidate's situation than in usual advertisements of charities. The impact of the gift on beneficiaries is easily imaginable and discussed during the show. This specificity can explain why impact is so influential in our study. We confirm predictions by the theories of equity and desert as candidates who are perceived to be more in control of their situation's causes receive less money than candidates who are perceived to be in financial need because of bad luck (Rudolph, Roesch, Greitemeyer, and Weiner, 2004; Weiner, 1995, 2000).

1.3   Dissertation Outline

In short, this dissertation looks at two areas of financial decision-making in the field. First, we look at pension communication, knowledge and savings decisions in Chapter 2. We find that peer information treatments are ineffective in motivating pension plan participants to look at online pension information, but financial incentives are. A lottery with few but large prizes motivates the largest share of participants on the online platform. Looking at the pension information does not increase pension knowledge, nor does it increase reported savings for retirement. Second, we examine considerations to select between donation beneficiaries in Chapter 3. We find that candidates in a Dutch TV show receive more money if the gift is perceived to have more impact and if the candidates are less responsible for their situation than other candidates. Chapter 4 provides a short conclusion of the dissertation and Chapter 4.2 discusses the implications of this research for pension funds, charities, crowdfund investment projects, and researchers in the pension and giving contexts.

Chapter 2

A Fistful of Dollars - Financial Incentives, Peer Information, and Retirement Savings *

2.1 Introduction

Undersaving for retirement is a major economic and societal challenge for many economies. Benartzi and Thaler (2013) have diagnosed a "retirement savings crisis" in the United States, as more than half of the U.S. population is at risk of inadequate funding to maintain their current lifestyle. In Europe, more than 18% of people aged 65 and over are at risk of poverty or social exclusion (EU, 2018). However, awareness of a potential savings gap is generally low, and current pension communication is predominantly ineffective (Debets, Prast, Rossi, and van Soest, 2018; Prast and van Soest, 2014).

Effective communication is crucial for individuals to make adequate savings decisions. Evidence for the effectiveness of information provision and financial education is mixed. People often avoid information if the resulting insights are uncertain and potentially negative (Golman, Hagmann, and Loewenstein, 2017;

Karlsson, Loewenstein, and Seppi, 2009; Sicherman, Loewenstein, Seppi, and Utkus, 2016). Consequently, several overview studies find little evidence of causal effects of information provision and financial education programs to increase financial literacy or influence financial behavior (Fernandes, Lynch Jr, and Netemeyer, 2014; Hastings, Madrian, and Skimmyhorn, 2013). On the other hand, a recent meta-analysis finds that financial education programs do increase financial literacy, which in turn improves financial behavior (Kaiser, Lusardi, Menkhoff, and Urban, 2021).

In this paper, we perform and analyze two field studies in which we investigate whether peer information and financial incentives are effective in triggering people to look up information about their pension, to improve their knowledge, and to change their self-reported savings behavior. Our analysis is guided by an adapted version of the Decision States Model (DSM), which conceptualizes the savings-decision-making process according to several sequential steps (Bateman, Louviere, and Thorp, 2014). First, fund members are unaware of a potential savings gap. Second, communication from their pension fund can make members aware of a potential savings gap. Third, members can look up personal information to find out how much money they will receive at retirement. Fourth, they might become capable of making a decision about adjusting their savings behavior upon increasing their knowledge. Fifth, members decide whether to increase their savings.

One potential way to increase people's interest in their personal retirement situation is to use peer information. A large number of studies show that peer information has positive effects on desired behavior (e.g., Allcott, 2011; Bott, Cappelen, Sørensen, and Tungodden, 2020; Duflo and Saez, 2002, 2003; Hallsworth, List, Metcalfe, and Vlaev, 2017). For example, teachers are more likely to refinance their mortgage if they learn that other teachers in their peer group have refinanced their mortgage in the past three months (Maturana and Nickerson, 2019). Yet, other studies find no or negative effects of peer information in a different but related context (e.g., Beshears, Choi, Laibson, Madrian, and Milk-

man, 2015; Cranor, Goldin, Homonoff, and Moore, 2020; Lieber and Skimmyhorn, 2018).

Alternatively, financial incentives could be used to trigger looking at pension information. Incentives are key to economics and have been shown to work in various settings, from education to health care and beyond. Yet, for financial decision making, the evidence on the effectiveness of incentives is mixed (Choi, Laibson, and Madrian, 2011; Duflo, Gale, Liebman, Orszag, and Saez, 2006; Duflo and Saez, 2003). Karlan, McConnell, Mullainathan, and Zinman (2016) find that reminder messages that mention financial incentives increase the attainment of savings goals that individuals set in commitment savings accounts, whereas Choi, Laibson, and Madrian (2011) find no effect of a survey informing employees of employer-matched pension contributions. An open question is therefore whether peer information and financial incentives can also be used to increase individuals' likelihood of looking at their pension savings situation. Our field experiments allow us to answer this question and shed light on which types of peer information and financial incentives are most effective.

The two natural field experiments (Harrison and List 2004) are highly powered, with 226,946 participants in Study 1, and 257,433 participants in Study 2. We preregistered both studies at the AEA RCT Registry.[2] The participants all work in the retail sector in the Netherlands and are part of the same pension fund, Pensioenfonds Detailhandel. The Dutch retirement system consists of a flat-rate public pension (first pillar), a funded occupational pension system in which most employees are automatically enrolled (second pillar), and private retirement accounts (third pillar). In the second pillar, most individuals belong to occupational schemes that are hybrid defined-benefit plans with benefits based on lifetime average earnings. Pension contributions are compulsory, and individuals cannot choose their pension provider, because they are stipulated in industry-wide agreements. Pension benefits are not guaranteed and depend on the financial performance of the fund, interest rates, and longevity.

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[2]Study 1 has been registered on https://www.socialscienceregistry.org/trials/987, and Study 2 on https://www.socialscienceregistry.org/trials/3144.

2. A FISTFUL OF DOLLARS

Although the Netherlands has a well-regarded pension system (Mercer, 2020), one in three individuals is at risk of not being able to meet their own retirement expenditure goals, mostly because of long periods of low interest rates (de Bresser and Knoef, 2015). Consequently, undersaving is a problem even in this highly rated and relatively well-funded pension system. Individuals can fill this gap with additional private savings outside of their mandatory pension plan, but only a small fraction do. For example, 26.8% of our study participants privately save for retirement.

Before pension fund participants know whether they require additional savings for their retirement, they need to be aware of their potential savings gap. Yet, this awareness is low for the pension fund in our study. In a 2016 survey, we asked fund members whether they expected 70% of their final salary to be paid out as their retirement income. Of the 789 survey respondents, 25.6% answered "I do not know," and 46.5% incorrectly answered "yes." The pension fund aims to provide a retirement income of 70% of the average salary, considerably lower than 70% of the final salary in most cases.

As part of our field experiments, the pension fund sent out a letter inviting members to visit an online personal pension planner to gain insight into their personal pension situation. The pension planner first showed which retirement income (both flat-rate state pension and second-pillar occupational pension) members would receive at their standard retirement age if they worked in the same employment position until retirement. In the next steps, members could change the parameters of the calculation by, for example, retiring part-time, adding pension entitlements from another pension fund, or exchanging their partner's pension to receive more pension oneself.

In both studies, participants randomly received one of six letters. The control group received a letter in the regular communication style of the pension fund. In Study 1, four treatments contained a peer-information statement. We used a 2x2 between-subjects design. Two peer information letters included an additional sentence that focused on retirement income, whereas the other two letters focused on retirement savings. One of each letter focusing on income or sav-

ings highlighted a positive outlook, and the other highlighted a negative outlook. With this study design, we sought to elicit whether a focus on means or a focus on goals is better for goal pursuit in the retirement context (Freund and Hennecke, 2015; Ülkümen and Cheema, 2011) and whether a fear appeal motivates participants to change attention and behavior (Witte and Allen, 2000). Studies such as Eberhardt, Brüggen, Post, and Hoet (2021) have shown that framing information differently can have a large impact on participant's engagement behavior. In the financial-incentive treatment of Study 1, we announced that 100 people who logged into the pension website would receive a voucher worth €25 ($29.64, converted on November 22, 2020). The participants could spend this voucher in many different stores in the Netherlands. The probability of winning depended on how many people in each treatment group logged in to the website, a response rate that was ex ante unknown. Studies commonly use the raffling of a fixed number of prizes among survey respondents, with unknown ex ante probabilities of winning, to incentivize survey completion (Laguilles, Williams, and Saunders, 2011). In our setting, participants who were only motivated to win the lottery prize might have logged in and then immediately left the website, without looking at their personal pension information. In this case, the treatment would be effective in triggering people to visit the pension fund's website, but it would still fail to increase interest.

We found that participants who received any of the peer information letters were not more likely to look at the pension planner than participants who received the control letter. Participants who received the Saving-Fear or the Income-Hope letters were even 0.3 percentage points (pp) less likely to look at the pension planner. By contrast, the financial incentive increased the information rate by 1.1 pp, a 50% increase over the control group in Study 1 (2.2% of participants in the control group looked at the pension planner). A back-of-the-envelope calculation shows the average cost per additional person looking at pension information via the financial-incentive treatment was €6.05 ($7.17).

In Study 2, we explored whether splitting up the same financial incentive in many small prizes or in a few large prizes would be most effective in increasing par-

ticipants' likelihood of looking at information. On the one hand, prospect theory (Tversky and Kahneman, 1992) predicts people will overweight small probabilities, and they might therefore respond more strongly to the chance of winning a few large rewards rather than many small rewards.[3] Moreover, members might largely ignore the unknown probabilities of winning and thus focus on the large, more salient prizes. Loewenstein, Weber, Hsee, and Welch (2001) find that people tend to be insensitive to the probability of winning, which makes raffles with a few large prizes with small probabilities more attractive. On the other hand, a robust finding in the literature on state lotteries and casino gambling is that people are more likely to play lotteries and engage in games of chance that offer a lot of small prizes (for a review, see Grote and Matheson (2013). Moreover, slot machines, optimally designed to keep people gambling, reward with many small prizes and rarely a large prize (Edwards, 1956). Overall, which division of the prize money would work best was unclear.

In Study 2, we therefore split up the same amount of money for each treatment into a large number of small prizes, a few large prizes, or a combination of small and large prizes. We found that a few larger prizes were more motivating to get participants to look at retirement information than many small prizes or a combination of small and large prizes. Participants in the lottery that raffled two large prizes were 86% more likely to look at the pension planner than were participants in the control group (an increase from 4.3% in the control group to 6.7%). Here, the cost of the financial incentive to get one additional participant looking at the pension planner was only €1.27 ($1.51). We also found that larger incentives increased the likelihood of spending more time on the pension planner, even for the likelihood of spending more than 10 minutes. Hence, the way the prize money was split had a large influence on the effectiveness of the incentive.

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[3]Many studies find behavior according to prospect theory. However, Castleman, Patterson, and Skimmyhorn (2020), in a large field experiment, found no evidence for the implications of prospect theory. They analyzed whether gain-loss framing of letters providing information about interest rate protection for U.S. army soldiers decreased credit card balances and average credit card interest rates. They found no difference between servicemembers who received the letter emphasizing the lost money due to not invoking the interest rate protection and servicemembers who received letters emphasizing the benefits of that protection.

Perhaps the most important question is whether looking at the pension planner affects the knowledge of pension matters of fund members and their subsequent savings decisions. We tested this possibility with the help of a survey administered three weeks after Study 2.We found that visiting the online pension planner had no causal effect on participants' pension knowledge. Our instrumental-variable regressions showed that participants who looked at the pension planner performed as poorly on a pension-knowledge quiz three weeks after our intervention as participants who did not look at the pension planner. All answers to the quiz questions could have been found either in the pension planner or directly on the pension fund's website that contains the planner. Yet, participants did not retain this information three weeks after our intervention, regardless of whether we focused on all quiz questions or only those referring to the planner. Visiting the pension planner also did not causally increase the likelihood of reporting additional private savings for retirement three weeks after the intervention. Thus, although incentives increased the likelihood of looking up pension information, they did not improve retirement knowledge nor affect self-reported savings.

Our findings contribute to several streams of literature. First, we add insights to the literature on retirement decision-making. Lusardi and Mitchell (2007, 2008, 2011a,b, 2017) report that few people make financial plans for their retirement. How should policymakers respond to this fact? Our results show that peer-information provision does not increase the rate of fund members who look up pension information; some even decrease this rate. This is in line with findings by Beshears, Choi, Laibson, Madrian, and Milkman (2015), who discover that older members in particular feel discouraged by information on others' contribution behavior. Our findings are also consistent with those of Lieber and Skimmyhorn (2018), who conclude that the savings of U.S. soldiers' peers do not influence those soldiers' own savings.

Second, our findings add to prior literature on the effectiveness of providing financial incentives. Previous evidence is mixed. Choi, Laibson, and Madrian (2011) find that financial incentives in the form of matching contributions do not

motivate employees to take up a pension plan. On the other hand, Duflo and Saez (2003) use a financial reward for attending a retirement information fair and find that it increases fair attendance and enrollment in a pension plan. Duflo, Gale, Liebman, Orszag, and Saez (2006) document that matching incentives increase the amounts individuals save in tax-deferred retirement accounts. Our two field experiments show that even a small financial incentive in the form of a lottery can substantially increase the likelihood of looking at retirement information. We also show that offering a few large incentives is more effective than offering many small incentives, while keeping the total budget constant. Previous studies that show larger incentives work better than smaller incentives did not keep the budget between treatment groups constant, and larger incentives were thus more expensive (see Charness and Gneezy (2009) for gym attendance; Volpp, Loewenstein, Troxel, Doshi, Price, Laskin, and Kimmel (2008) for adherence to medication plans; Björkman Nyqvist, Corno, De Walque, and Svensson (2018) in the context of HIV prevention). Our results show pension funds can increase the effectiveness of financial incentives to look at information, without increasing the required budget. However, this effectiveness is limited. Incentives do not improve pension knowledge or affect self-reported savings.

Third, our findings complement prior studies on financial literacy. Lusardi and Mitchell (2011b) demonstrate that financial literacy is an important factor for retirement planning. Van Rooij, Lusardi, and Alessie (2012) and Alessie, Van Rooij, and Lusardi (2011) show that financially literate people in the Netherlands are more likely to plan for their retirement and have higher household wealth than financially illiterate individuals. A key advantage of our study design is that we measured pension knowledge three weeks after we conducted our field experiment and find no effect. Given the importance of financial literacy and the mixed evidence on the effectiveness of financial education programs, more work is needed to understand how to improve financial knowledge.

2.2   Background of the Study

2.2.1   The Dutch Retirement System

The Dutch retirement system consists of three pillars. The first pillar is a pay-as-you-go state pension. The monthly flat-rate payment is linked to the minimum wage in the Netherlands. The full amount is paid out to individuals who have lived in the Netherlands for 50 years before the state retirement age. Otherwise, the amount depends on how many years an individual has stayed in the country. Singles receive 70% of the minimum wage, whereas each member of a couple receives 50%. If pensioners are still below a certain minimum of income and wealth, they can receive social benefits. The second pillar contains a funded occupational pension system, in which most employees are automatically enrolled. Ninety percent of the plans are (predominantly) so-called hybrid defined-benefit pension plans, whereas the remaining 10% are defined-contribution or collective-defined-contribution plans. Hybrid defined-benefit pension plans offer conditional indexation. If the funding level of the pension plan is below a certain threshold, full indexation (adjusting pension payments to the price or wage inflation rate) does not take place, resulting in a pension cut in real terms (Kortleve and Ponds, 2010). Since the 2008 financial crisis, many pension funds have had to lower indexation, some even to zero (Dreger and Heemskerk, 2016). The third pillar consists of private retirement savings accounts, which have become increasingly important due to the recent financial crisis and the long period of low interest rates (Knoef, Been, Alessie, Caminada, Goudswaard, and Kalwij, 2016).

In 2020, the Netherlands was ranked highest in the Mercer CFA Institute Global Pension Index (Mercer, 2020). The index assigns scores to 39 national retirement income systems, based on questions relating to the adequacy of a country's pension system, its sustainability, and its integrity. Despite its high ranking, the adequacy and sustainability of the Dutch pension system is suffering from the consequences of an aging population, the low-interest-rate environment, and an increasing number of people not being covered by the well-developed

second-pillar pension. The pension system could be improved with an increase in household savings and a reduction of household debt (Mercer, 2020).

By giving fund participants information on the first- and second-pillar pensions, they can decide on their third-pillar savings. Through the "Wet Pensioencommunicatie" ("Law Pension Communication", effective from July 1, 2015, onward), the Dutch government acknowledges the importance of pension communication, by requiring funds to communicate correctly, balanced, and according to the needs and characteristics of their participants. The law's objectives are for participants to know what income they can expect in retirement, to learn whether this pension provides an adequate lifestyle at retirement, and to be aware of any risks around their pension, and for pension communication to show participants' possible actions to improve their financial situation in retirement. Since 2008, pension funds have been obliged to send a Uniform Pension Overview (UPO) to active participants by regular mail once a year. The UPO informs participants about how much annual retirement income they have accrued and how much they will accrue if they continue working in the same job until retirement. Because the UPO is standardized across pension funds, participants can compare and add up their pension rights accrued from different pension funds.

2.2.2 The Pension Fund for the Retail Sector

We conduct our studies with participants in one of the 10 largest Dutch pension funds: the pension fund for the retail sector. The pension fund is a defined benefit plan and has approximately €29 billion assets under management and 1,225,000 participants (retired, active, and passive).[4] Active workers in the retail sector make mandatory monthly contributions to the pension fund. Table 2.1 shows demographics of those active participants of the pension fund for the retail sector at the time of Study 1 and Study 2 for whom we observe full information (226,946 active participants in Study 1 and 257,433 in Study 2). In Study 1, 69% of the participants are female, and in Study 2, 66% are. The average participant

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[4]Information on the pension fund's assets under management and participant base are as of August 10, 2020.

is 37 years old in Study 1 and 38 years old in Study 2. The average FTE is 0.67 in Study 1 and 0.69 in Study 2. In both studies, 39% of the participants report having a partner. The average nominal salary in Study 1 is €18,358, and in Study 2, €22,417. Because particularly low-educated women are at risk of poverty in old age (EU, 2017, 2018; OECD, 2017), we study a sample of the Dutch population that is more exposed to financial hardship than the general population.

Most of the time, the pension fund for the retail sector communicates with fund participants after participants contact the fund. Unsolicited communication takes place when a new fund member starts working in the retail sector or when the pension fund sends out the UPO between May and July every year. The pension fund additionally publishes a quarterly magazine called, *Jij & Wij* ("You and We"). The magazine covers personal stories on employees and store owners in the retail sector, articles about retail trends, and Q&As about the pension fund's policy and implementation.

2. A FISTFUL OF DOLLARS

Table 2.1: Mean Statistics: Active Pension Fund Participants

This table shows descriptive statistics for the active participants of the pension fund for the retail sector in Study 1 and Study 2. The pension administrator provided the data on the active pension fund participants for Study 1 in February 2016 and for Study 2 in July 2018. Except for the full-time equivalent (FTE), age, and income, all variables present rates. The variables are defined in Section 2.4.

	Study 1 N=226,946		Study 2 N=257,433	
	Mean	SD	Mean	SD
Male	0.31	0.46	0.34	0.48
Age (in years)	37.44	12.98	37.96	13.11
FTE	0.67	0.29	0.69	0.29
Partner	0.39	0.49	0.39	0.49
Income (in €)	18,358	12,994	22,417	16,254
Provinces				
Noord-Holland	0.18	0.39	0.18	0.39
Flevoland	0.03	0.16	0.03	0.16
Utrecht	0.08	0.27	0.07	0.26
Zuid-Holland	0.20	0.40	0.20	0.40
Gelderland	0.12	0.32	0.11	0.32
Noord-Brabant	0.14	0.35	0.16	0.36
Zeeland	0.02	0.15	0.02	0.15
Limburg	0.07	0.25	0.07	0.25
Overijssel	0.07	0.25	0.07	0.25
Drenthe	0.03	0.17	0.09	0.17
Friesland	0.03	0.18	0.03	0.18
Groningen	0.03	0.17	0.03	0.17

The fund also offers an online personal platform for each fund participant, called "Mijn Omgeving" ("My Environment").[5]. Participants can log in to the platform to see what information the pension fund has about them and whether it is correct. They can look at past communication with the pension fund, download forms for requests (e.g., to inform the pension fund of a partner), and look at their personal pension planner. The pension planner shows how much salary the participant currently receives and compares this amount with the projected

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[5]Screenshots of the personal platform and the pension planner as well as translations of the platform and the planner are provided in Appendix 2.10

pension payments (from both the state pension and the pension accrued at the pension fund). Participants can also change parameters to see how retiring later or earlier, for example, would affect the pension level. Compared to the UPO, the pension planner lets the participant experience what decisions one has and what they mean for the projected retirement income. In addition, participants might receive the UPO at a time when they are not thinking about their pension and might not need it. The pension planner shows information that is always accessible, whenever the participant needs it.

2.2.3 The Way from Unawareness to Savings: A Multi-step Approach

We investigate what motivates individuals to look up information about their personal retirement situation so they have sufficient knowledge to make adequate savings decisions. This decision-making process consists of several stages. We follow frequently used consumer behavior models, which describe consumer behavior with "hierarchy-of-effects" or "consumer funnels" (Barry, 1987; Kireyev, Pauwels, and Gupta, 2016; Murray and Vogel, 1997). The idea behind these models is that consumers go through different stages before they make a purchase, starting from being unaware of the product/brand, continuing with acquiring more information and knowledge about the product/brand, and eventually making the purchase. Each stage is crucial for the final outcome. Bateman, Louviere, and Thorp (2014) developed such a consumer funnel model in the context of financial decision making, the Decision States Model (DSM). How quickly consumers transition from one stage to the next depends on various individual and market factors (e.g., financial literacy, income, and information).

We make minor adjustments to the DSM model to fit our setting (Figure 1). In the first stage, members are unaware of their potential savings gap. In 2020, only 50% of a sample representative of the Dutch working population had thought about both income and expenses (Wijzer in Geldzaken, 2020). So, although a third of the Dutch are at risk of a savings gap, only half of the Dutch are likely to

know whether their savings level is adequate. Communication from the pension fund may make members aware of the possibility of a savings gap.
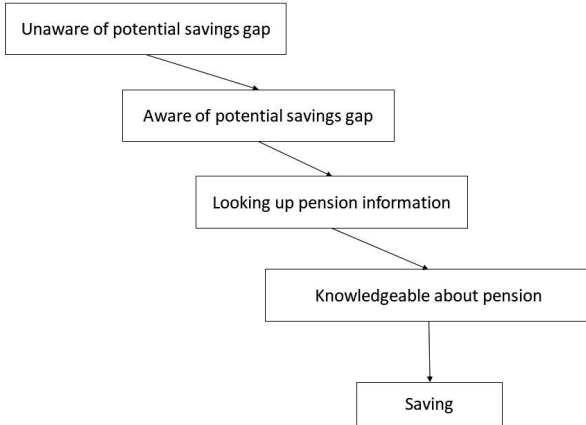
Figure 2.1. The visualization of the Decision States Model.

However, being aware of a potential savings gap does not automatically lead fund members to look up personal pension information. There are several reasons why people avoid information that could enhance their decision-making, such as anxiety, limited attention, regret aversion, and positivity maintenance (Golman, Hagmann, and Loewenstein, 2017; Sicherman, Loewenstein, Seppi, and Utkus, 2016). People may want to avoid seeing information on their savings that would lead them to the negative realization that they have too little savings. Pensions are also a complex matter and just one of many other issues people need to think about. Without an urgent incentive, looking up pension information is not easy. Providing financial incentives could trigger individuals to check their personal pension information.

Once pension members have looked up information, they might seek to improve their pension knowledge and find ways to decrease their potential savings gap. The importance of numeracy, financial knowledge, and capability in financial decision-making has been stressed by a broad literature (Hastings, Madrian,

and Skimmyhorn, 2013; Kaiser, Lusardi, Menkhoff, and Urban, 2021; Lusardi and Mitchell, 2014). More specifically, financial literacy is an important factor for retirement planning (Lusardi, 2009; Lusardi and Mitchell, 2007, 2011a, 2017). Financially literate people in the Netherlands are more likely to plan for their retirement and have larger household wealth than financially illiterate individuals (Alessie, Van Rooij, and Lusardi, 2011; Van Rooij, Lusardi, and Alessie, 2012). Furthermore, numeracy is positively linked with the decision to insure oneself against a high risk of adversity in retirement (Bateman, Eckert, Iskhakov, Louviere, Satchell, and Thorp, 2018).

Although robust correlations between financial knowledge and financial decisions have been reported, causal evidence has been weaker (Hastings, Madrian, and Skimmyhorn, 2013). For example, a meta-analysis showed that only 0.1% of the variance in behaviors studied is explained by interventions that aim to increase financial literacy (Fernandes, Lynch Jr, and Netemeyer, 2014). On the other hand, a recent meta-analysis of randomized controlled trials (RCTs) found that financial education programs do increase financial literacy and trickle through to financial behaviors (Kaiser, Lusardi, Menkhoff, and Urban, 2021). For the savings domain, a financial education program leads to an average increase in savings of 0.097 SD units.

In the final stage, members decide whether to increase their savings in the third-pillar private savings account. Several previous studies explore the effects of information provision and its trickle-down effect on savings. Carter and Skimmyhorn (2018) show that personalized, salient information on future retirement benefits does not affect current retirement savings. Mastrobuoni (2011) finds that informing workers about their estimated retirement benefits increases workers' knowledge about their benefits, but workers do not change their retirement behavior. Choi, Laibson, and Madrian (2011) analyze whether a survey including questions explaining foregone employer-matched savings would lead to higher contribution rates in 401(k) plans. Compared with survey respondents who received the control survey without these questions, respondents to the treatment survey did not increase tax-advantaged retirement savings. Most studies thus

find no effect of information provision on retirement savings behavior, but some find suggestive evidence for effects on financial knowledge.

Our field studies track the different stages of this decision-making process as follows. The goal of all the letters sent out by the pension fund, including the control letters, is to make participants aware of their potential savings gap ("With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate"). We design the treatment letters to increase the participants' interest in finding out about their own situation.We observe whether participants move to the third state as we know whether they look at their personal pension planner during the study periods. The next stage of the DSM is whether individuals are knowledgeable about pensions. With the pension knowledge quiz after Study 2, we test whether participants have acquired additional knowledge.

The final stage in the decision-making process is deciding whether to save additional funds. People who lack pension knowledge might simply not decide to put aside additional savings, and their savings gap would persist. In the follow-up survey after Study 2, we measure whether participants have saved additional funds by asking participants to report whether they have saved more for their pension over the past three weeks, the period between our field study and the survey.We thus observe whether triggering participants' interest in the pension planner trickles down and affects the decision to save more privately. In our study, we observe self-reported savings only, because the fund does not have information about savings outside of the pension fund.We do not observe whether members increase their savings after three weeks.

2.3 Study Design

In both field studies, we sent out different letter types to the active participants of the Dutch pension fund for the retail sector.[6] We target active participants specifically because they still contribute to the pension fund. All letters aimed

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[6]Because the pension fund did not have a database with e-mail addresses of a representative sample of the pension fund, we opted to send letters as the means of communication.

to motivate pension fund participants to go to the fund's website and log in to look at personalized retirement information. We measured who logged in to the platform and when. Furthermore, the data show what the participants clicked on within their personal platform and when they did so. We could thereby see which participants visited their personal pension planner and for how long.

In each experiment, we randomly allocated participants to one of six treatments. Randomization checks confirm successful randomization (see Appendix 2.10). We aimed to decrease the risk of a low response rate due to participants' fear of phishing in two ways. First, the letters were sent via the pension fund magazine, and thus were part of the pension fund's official communication. Second, the participants had to log in with their DigiD, a login system used by the Dutch government and such institutions as pension funds and health insurance providers. Nevertheless, because we could not verify who read the letters, all analyses of treatment effects based on the letters are intention-to- treat analyses. Given the features of our experiments, we classify them as natural field experiments (Harrison and List 2004).

The core text of the treatment letters in both studies was the text of the baseline letter. Each letter ended with the words "May we ask you to check your personal situation?" In every treatment letter, we added one sentence to the baseline text. With only one sentence varying between treatments, we could measure clean treatment effects, because we did not differ other variables such as the length of the baseline letter. Furthermore, studies such as Bhargava and Manoli (2015); Bott, Cappelen, Sørensen, and Tungodden (2020); Hallsworth, List, Metcalfe, and Vlaev (2017) and Goldin, Homonoff, Patterson, and Skimmyhorn (2020) show that adding one sentence is effective. The added sentence was printed in bold in order to be more salient. The next paragraphs described the messages in more detail. The (translated) wording of the letters can be found in Appendix 2.10.

2.3.1 Study 1

On December 22, 2015, we sent six different letter types to all 245,721 active participants at the time.We measured website behavior until February 1, 2016.

The Baseline. The baseline letter contained the text that all other letters displayed as well. It informed the pension fund participant about the pension fund's new website and the personal pension platform of the participant. The letter clarified the importance of retirement planning to be able to know the level of pension entitlements one has accrued so far.

Peer-Information-Treatment Letters. We tested four different peer information treatments that vary a positive or negative outlook as well as a focus on retirement savings or income:

1. Income-Fear: This letter focuses on the goal of retirement savings: retirement income. It includes a fear appeal because it emphasizes that people might not have adequate retirement income. The added sentence reads, "A large part of people in the Netherlands think that they will have a too low income to retain their current level of consumption in retirement. What about you?"

2. Income—Hope: This letter displays the goal focus and a positive description to focus on the positive, hopeful content: "A large part of people in the Netherlands think that they will have enough income to retain their current level of consumption in retirement. What about you?"

3. Savings—Fear: This letter focuses on the means to achieve retirement income, namely, saving for retirement. It also uses a fear appeal, as in the second letter: "A large part of people in the Netherlands think they save too little to retain their current level of consumption in retirement. What about you?"

4. Savings—Hope: This letter shows the means focus with a positive description of the peer information: "A large part of people in the Netherlands

34

think they <u>save enough</u> to retain their current level of consumption in retirement. What about you?"

The Financial Incentive. The financial-incentive letter included an additional sentence written in bold: "Among all participants who log in, we are raffling 100 VVV gift vouchers worth €25." VVV gift vouchers were vouchers from the tourism association in the Netherlands, usable in over 24,000 Dutch shops. Note that entering the prize draw was conditional on logging in to MyEnvironment, not on looking at the pension planner. We were interested in whether individuals have looked at the pension planner.

2.3.2   Study 2

On May 15, 2018, we sent six different letter types to all 274,279 active participants at the time. We investigated whether, given a fixed budget, a few large prizes, a larger number of small prizes, or a combination of both was most effective in increasing individuals' likelihood of looking at their pension situation.

The Baseline Letter. The baseline letters in the Study 1 and Study 2 showed the same text, in the standard communication form of the pension fund.

The Financial Incentive Letters Similar to the financial incentive letter in Study 1, we added one sentence to the baseline letter. The sentence announced a different lottery in each treatment letter.

1. 200*€10: We added the following sentence to the baseline letter: "Among all participants who log in, we are raffling 100 VVV gift vouchers worth €20."

2. 200*€10: The treatment sentence read: "Among all participants who log in, we are raffling 200 VVV gift vouchers worth €10."

3. 100*€10 and €1,000: We raffled several smaller amounts and one big amount. We added "Among all participants who log in, we are raffling 100 VVV gift vouchers worth €10 and one VVV gift voucher worth €1,000."

4. 4*€500: We increased the raffled amount and decreased the chance to win. The treatment sentence read: "Among all participants who log in, we are raffling four VVV gift vouchers worth €500."

5. 2*€1,000: We tested whether raffling two large amounts is the most effective treatment by adding "Among all participants who log in, we are raffling two VVV gift vouchers worth €1000."

The last day we measured website behavior was June 25, 2018. Because saving more than the default contribution rate via the pension fund is impossible, we do not have information on changed savings behavior from the administrative dataset. Three weeks after the letters were sent out, we invited participants for whom the pension fund had email addresses (49,552) to take an online survey. Of those invited, 4,395 initiated the survey, and 2,507 completed questions on financial knowledge and savings behavior. In the end, we matched 2,231 respondents to the experimental dataset.We describe the variables used in our analysis that are taken from the survey in Section 2.4.2 and present descriptive statistics of the survey sample in Section 2.5.

2.4 Data

In both studies, our final dataset stems from two data sources. Section 2.4.1 describes the administrative data source, and Section 2.4.2 describes the data obtained through the two studies.

2.4.1 Background Variables

The administrative datasets were provided by TKP, the delegated pension administrator of the pension fund for the retail sector. For Study 1, the data contained the full set of demographic statistics of 226,946 active pension fund participants at the beginning of February 2016. For Study 2, we had access to data on 257,433 active pension fund participants at the end of June 2018. Variables of interest are the following:

Gender is a binary dummy equal to 1 if the participant is male, and 0 if female. We used gender as a control in our regressions, but also for our heterogeneity analysis, to see whether men's reactions to a treatment letter differ from women's.

Age represents the participant's age in February 2016 and in July 2018, for Study 1 and Study 2, respectively. For the graphs in the heterogeneity analysis, we built a dummy that is equal to 1 if the participant's age is above the median age, and 0 otherwise. For most regressions, we used a factor variable with five levels: ages below 30 years, ages 30–39, ages 40–49, ages 50–59, and ages above 60 years.

Full-time equivalent (FTE) is the ratio of the participant's contract hours to the hours of a full-time contract. For the regressions, we used a categorization. We categorized participants who work full time (i.e., have an FTE of 1) as full-timers, and participants who work more than 0 hours, but less than full time, as part-timers. We dropped all observations of participants who work 0 hours, because they do not build up any pension benefits in the month of the study. This process resulted in dropping 16,490 observations in Study 1 and 16,192 in Study 2.[7]

Partner is a binary variable, with a value of 1 if the pension fund has registered the participant as having a partner, and 0 otherwise. Information on marriages and civil unions was automatically transferred to the pension fund, but fund members could also register a partner they had lived with for more than six months.

Income is the annual gross salary and was winsorized at the 1% and the 99% levels to replace extreme values. For the heterogeneity analysis, we built a dummy indicating whether a participant is above the median income (value =1) or below (value =0). For most regression analyses, we used a factor variable with five levels, where each level represents one quintile of the income distribution.

Province dummies indicate the province where the participant lives, by using the participant's postal code. We used a factor variable with 12 levels in the

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[7]Results are robust when we include observations with missing values. Results are available from the authors upon request.

regressions to check for regional effects. We dropped observations for which the information is missing (2,285 for Study 1 and 654 for Study 2).

2.4.2 Outcome Variables

The pension administrator of the pension fund provided administrative data on outcome variables of our study. For both studies, the datasets show the participants' website behavior within the 40-day period after the letters were sent out. We expect that the data spanning this period capture most of the behavior caused by the letters. Variables of interest are the following:

Pension Planner is a binary variable displaying whether the participant clicked on the pension planner at least once between December 22, 2015, and February 1, 2016, for Study 1, and between May 15, 2018, and June 25, 2018, for Study 2. The pension planner shows the current income as well as the pension payment that can be expected when the participant continues working until her retirement age. As one of our main dependent variables, it measures whether participants look at their personal pension information.

We also examined other variables related to total time spent on the pension planner. For Study 1, we observed the hour and minute a participant visited any page on MyEnvironment, or when they logged out. For Study 2, we received more precise data and observed the hour, minute, and second a participant clicked on the pages within MyEnvironment, or when they logged out. For both studies, we calculated how much time participants spent in total on the pension planner during the study periods mentioned above. We took the difference between the time a participant clicked on the pension planner and the time of her next click on the page (including clicking to log out). We then summed up the time spent on the pension planner for each visit over all visits during the study period.We winsorized at the 1%and the 99% levels and then created three binary variables. *More Than 1 Minute* indicates the participant spent more than 1 minute on the pension planner, *More Than 5 Minutes* indicates the participant spent more than 5 minutes on the pension planner, and *More Than 10 Minutes* indicates the participant spent more than 10 minutes on the pension planner. Because some

participants did not log out or visit another page on the personal platform after visiting the pension planner, we lack information about the time on the pension planner for 1,650 out of the 5,155 participants on the planner in Study 1 and for 4,461 out of 16,650 participants for Study 2. We interpret the time spent on the pension planner as an indicator of the intensity of this state of the DSM.

In the survey sent out three weeks after the experiment in Study 2, we asked six questions to test participants' capability in the domain of pension knowledge. We selected the questions so that we would have a balance of questions specific to the pension planner, the pension plan of the pension fund, and general knowledge of the Dutch pension system. In addition to creating our own questions, we took questions from Knoef, Been, and van Putten (2020). Appendix 2.10 lists where the answers to the questions can be found on the pension fund's website. The questions were as follows (* indicates the correct answer):

1. On Pensioenfonds Detailhandel's pension planner, you can enter the age at which you wish to retire. As it stands, can you see how the age of retirement affects the size of your pension? (Yes* / No, not in this version of the planner / I don't know)

2. You can tailor your own investment portfolio on Pensioenfonds Detailhandel's website. (True / False* / I don't know)

3. Who pays the pension contributions for employee pensions? (Usually only the employee / Usually only the employer / Usually both the employee and the employer* / I don't know)

4. In the past two years, product prices in the Netherlands have risen. Did your pension with Pensioenfonds Detailhandel grow in line with price increases in the last two years? (This process is called indexation.) (Pensions matched price increases / Pensions did not match price increases* / I don't know)

5. If you have accrued pension with Pensioenfonds Detailhandel and you get a new job, can you choose to transfer the accrued amount to another pension fund? (Yes* / No / I don't know)

6. When you retire, you can choose to receive a higher amount at the beginning of your pension followed by a slightly lower amount afterwards. (True* / False / I don't know)

The variable *Pension Knowledge* used in our regressions is constructed by adding up the number of correct answers and then subtracting one point for each incorrect answer. If participants answered "I don't know," they received zero points. The variable can range from –6 to 6. Our results are robust to using different scoring rules; see Appendix 2.10.

In the same survey we asked the following questions to elicit self-reported savings behavior:

1. Did you save for your retirement in the past three weeks (outside of your pension fund)? 26.8% answered yes.

If participants answered yes, they were asked:

2. Compared to the past months, did you save

   (a) More in the past weeks (12.4%)

   (b) The same in the past weeks (84.1%)

   (c) Less in the past weeks (3.5%)

The constructed variable *Saved More* displays whether the participant reported saving more in the weeks between Study 2 and the survey relative to not saving at all or saving the same or a lower amount.

2.5 Individual Characteristics and Website Behavior

This section describes the population of the pension fund's active participants as well as the subset of pension fund participants who looked at the pension planner. Table 2.2 compares the summary statistics of the administrative dataset between

pension fund participants who did and did not look at the pension planner at least once. Of the 226,946 participants who received a letter in Study 1, 5,155 (i.e., 2.3%) looked at the pension planner at least once. That number is 73% of the participants who logged in to MyEnvironment (Table 2.A.3 in the Appendix). Of the 257,433 participants who received a letter in Study 2, 16,650 (i.e., 6.5%) looked at the pension planner at least once. This number represents 30% of the participants who logged in to MyEnvironment in Study 2. In general, fund members spent more total time on the pension planner in Study 1 than in Study 2 (5.5 minutes vs 2.7 minutes), but more people visited MyEnvironment in Study 2 than in Study 1. This finding is not surprising, because the personal online platform was new to participants in Study 1 and had already been around for two years in Study 2. Participants thus spent more time in Study 1 to explore MyEnvironment and its sites. For example, 67% of participants in Study 1 clicked on My Profile, but only 35% did so in Study 2. The annual pension overview can be viewed online as well, and participants can opt to exclusively receive communication from the pension fund via the personal platform. Seventy-five percent of participants clicked on My Mail in Study 1, and 93% in Study 2. The click-through rates are close to those of other studies that used letters to invite people to visit websites (see, e.g., Service, Hallsworth, Halpern, Algate, Gallagher, Nguyen, Ruda, with Marcos Pelenur, Gyani, Harper, Reinhard, and Kirkman, 2014). A study on information search of investors found that only 0.8% of all participants clicked further to get more information (Døskeland and Pedersen, 2016).

Table 2.2: Comparison of Participants Who Visited the Pension Planner, Answered the Survey, and the Whole Pension Fund Population

This table shows descriptive statistics of the subset of participants who clicked on the pension planner for Studies 1 and 2, of participants who responded to the survey in Study 2, and of participants who did not click on the pension planner for Studies 1 and 2. Except for the full-time equivalent (FTE), age (in years), and income (in €), all variables present rates. The "Diff. Planner" column shows t-statistics of the differences between the characteristics of the participants of the pension fund who clicked on the pension planner and those who did not. The "Diff. Survey" column shows t-statistics of the differences between the characteristics of the participants of the pension fund who answered the survey and those who did not. The province names not fully spelled out are: N.-Holl.=Noord-Holland, Z.-Holland=Zuid-Holland, and N.-Brab.= Noord-Brabant. *** p<0.001, ** p<0.01, * p<0.05

| | Study 1 | | | | | Study 2 | | | | | | | |
| | Pension Planner (N=5,155) | | No Pension Planner (N=221,791) | | Diff. Planner | Pension Planner (N=16,650) | | No Pension Planner (N=240,783) | | Survey (N=2,231) | | Diff. Planner | Diff. Survey |
	Mean	SD	Mean	SD	t	Mean	SD	Mean	SD	Mean	SD	t	t
Male	0.39	0.49	0.31	0.46	11.96***	0.36	0.48	0.34	0.47	0.37	0.48	5.07***	2.34*
Age	43.53	15.13	37.30	12.89	34.17***	39.52	14.96	37.85	12.97	46.49	13.91	15.90***	30.93***
FTE	0.69	0.29	0.67	0.29	5.09***	0.67	0.30	0.69	0.29	0.71	0.28	-6.82***	3.23**
Partner	0.51	0.50	0.39	0.49	-2.23*	0.43	0.50	0.39	0.49	0.54	0.50	11.41***	14.77***
Income (in €)	21,360	15,636	18,288	12,918	16.79***	22,755	17,273	22,393	16,181	25,596	18,351	2.78**	9.28***
Provinces													
N.-Holl.	0.18	0.39	0.18	0.39	-0.48	0.17	0.37	0.18	0.39	0.16	0.37	-5.14***	-2.54*
Flevoland	0.02	0.15	0.03	0.16	-0.84	0.02	0.15	0.03	0.16	0.02	0.14	-1.86	-1.77
Utrecht	0.08	0.28	0.08	0.27	1.76	0.07	0.26	0.07	0.26	0.08	0.27	-0.83	0.44
Z.-Holland	0.18	0.39	0.21	0.40	-4.11***	0.20	0.40	0.20	0.40	0.18	0.38	-0.57	-2.35*
Gelderland	0.12	0.32	0.12	0.32	0.56	0.12	0.32	0.11	0.32	0.13	0.33	1.08	2.04*
N.-Brab.	0.15	0.36	0.14	0.35	1.24	0.17	0.37	0.16	0.36	0.16	0.37	3.20**	0.65
Zeeland	0.02	0.15	0.02	0.15	0.41	0.03	0.16	0.02	0.15	0.03	0.17	1.85	1.65
Limburg	0.07	0.26	0.07	0.25	1.17	0.08	0.27	0.07	0.25	0.08	0.27	4.27***	2.64**
Overijssel	0.07	0.26	0.07	0.25	1.19	0.07	0.25	0.07	0.25	0.07	0.26	-0.03	1.07
Drenthe	0.04	0.19	0.03	0.17	2.73**	0.03	0.17	0.03	0.17	0.03	0.18	0.84	1.74
Friesland	0.03	0.18	0.03	0.18	0.09	0.03	0.17	0.03	0.18	0.03	0.16	-1.74	-1.67
Groningen	0.03	0.16	0.03	0.17	-1.42	0.03	0.17	0.03	0.17	0.03	0.17	0.26	0.22

Comparing the demographics of the pension fund participants who looked at the pension planner with the demographics of those who did not, we find the following (Table 2.2). In Study 1, males were more likely to look at the pension planner. During Study 1, 31% of participants who did not look at the pension planner were male, whereas 39% of participants online were male. On average, the participants who did not look at the planner were 37.3 years old. Individuals who clicked on the pension planner were older, with an average age of 43.5 years. Participants who checked the pension planner worked more hours than those who did not, with a part-time factor of 0.69 compared with 0.67. Participants on the website earned more on an annual basis, on average (€21,360 vs. €18,288). All these differences are statistically significantly different from 0 at the 99% confidence interval.

As in Study 1, the average pension fund participant who looked at the pension planner in Study 2 worked more hours, earned more, was older, and was more likely to have a partner and to be male than the average participant who did not look at the pension planner. These differences are again statistically significant at the 99% confidence level. The differences can be explained by the fact that, on average, older individuals are more interested in the topics of pension and retirement, because they are topics more relevant to them (Alessie, Van Rooij, and Lusardi, 2011; Van Rooij, Lusardi, and Alessie, 2012).

Table 2.2 also shows descriptive statistics on the people who answered the pension-knowledge questions. Participants in this sample were older and more likely to be male and have a partner than those who did not answer the pension-knowledge questions.With an average income of €25,596, the sample participants also earned more income than participants who did not answer the pension-knowledge questions. In Appendix 2.10 in Table A2.A.12, we regress whether participants initiated the survey on the treatment indicators. We find a small positive effect of the 100*€10+€1,000 and 4*€500 treatment groups (0.2- and 0.3-pp increases). Participants in the 200*€10 and 4*€500 treatment groups were 0.1 and 0.2 pp more likely to answer the pension-knowledge questions.

A potential selection effect could go in two directions. On the one hand, fund members with more income might have less need to save additionally. On the other hand, saving more may be easier for fund members with more income. We later use instrumental variables to account for potential endogenous effects.

2.6 Study 1: How Can We Increase Individuals' Interest in Personal Retirement Information?

In this section, we analyze the effectiveness of peer information and financial incentives to increase the likelihood of looking at one's personal pension situation. Figure 2.2 shows that the financial-incentive letter was effective in heightening individuals' interest in looking at personal retirement information, whereas the peer information letters were not. In the control group, 2.2% of the pension fund participants visited their personal pension planner during the study period. On average, 2.2% of participants in the Saving—Hope group checked their personal planner as well, indicating no effect of the treatment letter on participants' interest. Participants in the Income—Fear, Income— Hope, and Saving—Fear groups were even less likely to look up information than participants in the control group: on average, 2.0% looked at the pension planner. These results are in line with studies that find no or negative effects of peer information (e.g., Beshears, Choi, Laibson, Madrian, and Milkman, 2015). In the financial-incentive treatment group, 3.3% looked at the pension planner. Given that a total of €2,500 was raffled in Study 1 and 413 additional people looked at the pension planner due to the financial-incentive treatment (1,258 in the financial-incentive treatment group minus 845 in the control group), the average cost of a participant looking at information was €6.05 ($6.82).

We run an OLS regression to control for demographics. The coefficients are presented in Table 2.3. These results confirm the univariate analysis. The *Financial Incentive* letter increased interest at the extensive margin by 1.1 pp

**Figure 2.2. Percentage of respondents who visited their personal
pension planner (per letter) in Study 1.**
The error bars represent the 95% confidence intervals. Table 2.A.7 in Appendix 2.10 shows the mean differences and multiplicity-adjusted p-values for the mean differences.We use the bootstrap-based procedure proposed by List, Shaikh, and Xu (2019) to adjust for testing multiple null hypotheses simultaneously using experimental data with random treatment assignment. For more detail, see List, Shaikh, and Xu (2019). Results for both experiments and all subgroups can be found in Appendix 2.10

(p<.001), representing a 50% increase relative to the baseline rate of 2.2%. The *Income—Hope* and *Saving—Fear* letters each decreased the likelihood of looking at the pension planner by 0.3 pp, a 13.64% decline compared to the baseline (p<.01). We also run pairwise comparisons across the treatment letters, adjusting for multiple comparisons with Bonferroni's method. Comparisons between the peer information treatment letters are all statistically insignificant. Comparisons between the peer information treatment letters and the *Financial Incentive* letter are all statistically significant (p<.001). Men were more likely to look at information than women (the baseline category). The effect of age is U-shaped. Participants between 30 and 50 years were less likely to look up their pension situation than participants younger than 30. Participants older than 49 were more likely to look at the pension planner than participants younger than 30. Being employed part time had a positive effect, as did belonging to the highest income quintile and having a partner.

We calculate the total time on the pension planner per participant. On average, participants who looked at the pension planner spent 5.51 minutes in total (SD =8.81). The minimum time is 1 minute and the maximum time 85 minutes. This variation shows that some participants seemed to click on the pension planner out of curiosity and then did not want to spend time clicking through it, whereas others spent a considerable amount of time on the pension planner to see how different factors would affect their future pensions.

We therefore examine whether participants in the treatment groups stayed longer on the pension planner than the control group. Columns (2) to (4) of Table 2.3 show coefficients from OLS regressions with variables indicating a participant spent at least 1 minute, 5 minutes, or 10 minutes on the pension planner as dependent variables. Because 1,650 participants did not log out, and we thus do not observe exactly when participants stopped looking at the pension planner, the number of observations for these columns is smaller than the number of observations for Column (1). Column (2) shows that participants in the *Financial Incentive* treatment were 0.8 pp more likely to look at the pension planner for more than 1 minute. As for the likelihood of looking at the pension planner, the

Saving—Fear letter decreased the likelihood of looking at the pension planner for more than 1 minute by 0.3 pp. The effect of the peer-information treatments is no longer statistically significant in Column (3), meaning that participants were as likely to look at the pension planner for more than 5 minutes in the peer-information treatment groups as in the control group. Participants in the *Financial Incentive* letter group were 0.1pp more likely to look at the pension planner for more than 5 minutes than the control group (p=0.008). In Column (4), we do not see an effect of any treatment group being more likely to look at the pension planner for more than 10 minutes.

Table 2.3: Financial Incentive Increases the Likelihood of Looking at Retirement Information (Study 1)

This table shows the coefficients of OLS regressions with different binary dependent variables for Study 1. *Pension Planner* indicates a participant clicked on the pension planner, *More Than 1 Minute* indicates the participant spent more than 1 minute on the pension planner, *More Than 5 Minutes* indicates the participant spent more than 5 minutes on the pension planner, and *More Than 10 Minutes* indicates the participant spent more than 10 minutes on the pension planner. *Income-Fear, Income-Hope, Saving-Fear, Saving-Hope*, and *Financial Incentive* indicate the treatment letter the participant received. The control letter is the baseline. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income quintile rows indicate to which income quintile the participant belongs. The first income quintile is the baseline category. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. The number of observations is lower in Columns (2) to (4) as we do not have information on time spent on the planner from 1,650 participants. We control for province dummies. Baseline mean shows the mean of the respective dependent variable for the baseline group. Robust standard errors are in parentheses. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

	Pension Planner (1)	More Than 1 Minute (2)	More Than 5 Minutes (3)	More Than 10 Minutes (4)
Financial Incentive	0.011***	0.008***	0.001**	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Peer Information				
Income-Fear	-0.002*	-0.002*	-0.001	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Income-Hope	-0.003**	-0.002*	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Saving-Fear	-0.003**	-0.003***	-0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Saving-Hope	-0.001	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Male	0.007***	0.005***	0.002***	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 30-39	-0.006***	-0.004***	-0.001***	-0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 40-49	-0.003**	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 50-59	0.015***	0.012***	0.008***	0.003***
	(0.001)	(0.001)	(0.001)	(0.000)
Age: 60-66	0.064***	0.048***	0.035***	0.021***
	(0.003)	(0.002)	(0.002)	(0.001)
Income: 2nd Quintile	0.000	0.000	0.001**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Income: 3rd Quintile	0.001	0.000	0.001**	0.001*
	(0.001)	(0.001)	(0.000)	(0.000)
Income: 4th Quintile	0.002	0.002	0.002***	0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
Income: Highest Quintile	0.014***	0.011***	0.007***	0.004***
	(0.001)	(0.001)	(0.001)	(0.000)
Part-timer	0.006***	0.004***	0.002***	0.001***
	(0.001)	(0.001)	(0.001)	(0.000)
Partner	0.004***	0.003***	0.002***	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Constant	0.006***	0.002	-0.004***	-0.002***
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	226,946	225,296	225,296	225,296
Adj. R-Squared	0.013	0.010	0.014	0.011
Province Dummies	YES	YES	YES	YES
Baseline Mean	0.022	0.016	0.005	0.002

Overall, the peer-information treatments were not effective, but our financial incentive treatment in Study 1 was. In the remainder of the paper, we focus on the effect of financial incentives to motivate people to look at personal retirement information.

2.7 Study 2: Are a Few Large Incentives or Many Small Incentives More Effective?

We now focus on financial incentives in order to examine whether we can improve the positive effect of the financial-incentive treatment found in Study 1. We thus analyze the effects of varying financial-incentive letters, holding fixed the budget per treatment group. In the next section, we look at the consequences of the experiment on pension knowledge and self-reported savings.

As in Study 1, we observe whether an individual visited their personal pension planner during the study period. Figure 2.3 shows that the two letters raffling a few large prizes were the most effective. Approximately 8% of participants receiving the larger-prize lotteries letter visited the pension planner. The baseline letter triggered 4.3% of participants to look at their personal pension information. The other three letters resulted in between 5.8% and 6.7% of participants looking at the pension planner, being significantly more effective than the control letter but less effective than the lotteries with larger prizes.

We again run an OLS regression to control for demographics. The coefficients are presented in Column (1) of Table 2.4. All our treatments are statistically significantly different from the baseline letter (p<.001). The 2*€1,000 letter increased interest by 3.7 pp, and the 4*€500 letter by 3.5 pp. This amount represents an increase of 86% and 81%, respectively, relative to the baseline rate of 4.3%. The other three lotteries increased interest by 1.5pp (35%), 2.1pp (49%), and 2.4pp (56%). We also perform pairwise comparisons across the treatment letters and adjust for multiple comparisons with Bonferroni's method. The differences in the treatment effects between treatment letters are all statistically significant (p<.001) except for three comparisons. The 0.57- pp difference between

% respondents clicking on pension planner
at least once
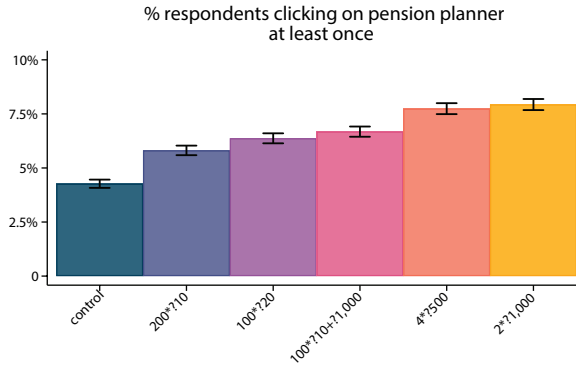
Figure 2.3. Percentage of respondents who visited their personal pension planner (per letter) in Study 2.
The error bars represent the 95% confidence intervals. Table 2.A.8 in Appendix 2.10 shows the mean differences and p-values for the differences.

the 100*€20 and the 200*€10 letter is statistically significant with a p-value of .007. The differences in effect between the 100*€10+€1,000 and the 100*€20 letters and between the 2*€1,000 and the 4*€500 letters are statistically insignificant.

Given that we use a budget of €2,000 for this financial-incentive treatment and that 1,570 more people looked at the pension planner due to the 2*€1,000 treatment (3,402 in the 2*€1,000 treatment group minus 1,832 in the control group), the average cost of a participant checking the pension planner was €1.27 ($1.43) for the most effective financial incentive.

Similar to Study 1, men were more interested in their pension situation, and age had a U-shaped effect. Being a part-timer had a positive effect on looking up pension information, as did having a partner. In contrast to Study 1, participants in the second-, third-, and fourth-income quintiles were less likely to look at the pension planner than participants in the lowest quintile. Participants in the highest quintile were again more likely to look at the pension planner than participants in the lowest quintile.

2.7 Study 2: Are a Few Large Incentives or Many Small Incentives More Effective?

In Study 2, participants spent an average of 2.72 minutes in total on the pension planner (SD = 3.77). The minimum time is 0.08 minutes, and the maximum time is 25.37 minutes. Again, some participants seem to have clicked on the pension planner out of curiosity and then did not want to spend time to click through it, whereas others spent a considerable amount of time on the pension planner to see how different factors would affect their future pensions.

In columns (2) to (4) of Table 2.4, we show the coefficients of three additional OLS regressions to examine whether participants just quickly looked at the pension planner or spent more time on the planner.We again use binary variables as the dependent variables that indicate whether the participants spent more than 1, 5, and 10 minutes on the planner. In Column (2), the 2*€1,000 letter increased the likelihood of looking at the pension planner for more than 1 minute by 1.8 pp (p<.001), and the 4*€500 letter increased it by 1.7 pp (p<.001). Both of the larger lotteries (p<.001) increased by 0.4 pp the likelihood

Table 2.4: Larger and Fewer Financial Incentives Work Better than Smaller and Many Incentives (Study 2)

This table shows the coefficients of OLS regressions with different binary dependent variables for Study 2. *Pension Planner* indicates a participant clicked on the pension planner, *More Than 1 Minute* indicates the participant spent more than 1 minute on the pension planner, *More Than 5 Minutes* indicates the participant spent more than 5 minutes on the pension planner, and *More Than 10 Minutes* indicates that the participant spent more than 10 minutes on the pension planner. *200*€10, 100*€20, 100*€10+€1,000, 4*€500*, and *2*€1,000* indicate which letter a participant received. The control letter is the baseline. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile rows indicate the income quintile to which the participant belongs. The first income quintile is the baseline category. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. The number of observations is lower in Columns (2) to (4) as we do not have information on time spent on the planner from 4,461 participants. Baseline Mean shows the mean of the respective dependent variable for the baseline gorup. Robust standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05

	Pension Planner	More Than 1 Minute	More Than 5 Minutes	More Than 10 Minutes
	(1)	(2)	(3)	(4)
200*€10	0.015***	0.008***	0.001*	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
100*€20	0.021***	0.010***	0.002***	0.000
	(0.002)	(0.001)	(0.001)	(0.000)
100*€10+€1,000	0.024***	0.012***	0.003***	0.001**
	(0.002)	(0.001)	(0.001)	(0.000)
4*€500	0.035***	0.017***	0.004***	0.001***
	(0.002)	(0.001)	(0.001)	(0.000)
2*€1,000	0.037***	0.018***	0.004***	0.001***
	(0.002)	(0.001)	(0.001)	(0.000)
Male	0.006***	0.003***	0.001***	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 30-39	-0.019***	-0.004***	0.000	-0.000*
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 40-49	-0.021***	0.000	0.002***	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Age: 50-59	0.005***	0.016***	0.010***	0.004***
	(0.002)	(0.001)	(0.001)	(0.000)
Age: 60-66	0.069***	0.061***	0.038***	0.018***
	(0.003)	(0.003)	(0.002)	(0.001)
Income: 2nd Quintile	-0.006***	-0.001	0.001	0.001*
	(0.002)	(0.001)	(0.001)	(0.000)
Income: 3rd Quintile	-0.009***	-0.000	0.002***	0.001**
	(0.002)	(0.001)	(0.001)	(0.000)
Income: 4th Quintile	-0.007***	0.000	0.002***	0.001**
	(0.002)	(0.001)	(0.001)	(0.000)
Income: Highest Quintile	0.004*	0.005***	0.004***	0.002***
	(0.002)	(0.001)	(0.001)	(0.000)
Part-timer	0.007***	0.002*	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Partner	0.009***	0.007***	0.002***	0.001**
	(0.001)	(0.001)	(0.000)	(0.000)
Constant	0.034***	0.004**	-0.003***	-0.002***
	(0.002)	(0.002)	(0.001)	(0.000)
Observations	257,433	252,972	252,972	252,972
Adj. R-Squared	0.010	0.010	0.012	0.007
Province Dummies	YES	YES	YES	YES
Baseline mean	0.043	0.017	0.005	0.002

of looking at the pension planner for more than 5 minutes. When examining the likelihood of looking at the pension planner for more than 10 minutes, the larger lotteries are still statistically significant, although the effect is small: an increase of 0.1 pp over the baseline group (p=0.001 for 4*€500, p<.001 for 2*€1,000).

2.8 Effects on Pension Knowledge and Self-Reported Savings Behavior

With the responses to the survey sent out three weeks after Study 2, we can investigate whether people who looked at the pension planner were more capable of making a savings decision. In contrast to the intention-to-treat analyses above, we can observe who actually looked at the pension planner. We measure capability with the score on a pension-knowledge quiz. The average survey respondent scored 1.98 out of 6 (SD =1.91). Table 2.5 shows the rates of participants who answered the questions correctly, wrongly, or answered "I don't know," overall and per treatment group. The question most people answered correctly was Question 3: 73% of participants knew that both the employer and the employee pay pension contributions; 66% knew they could take their accrued pension with them to another pension fund; 48% answered correctly that no indexation (i.e., compensation for inflation) occurred in the two years prior to the survey; 41% knew they could enter their desired pension age in the pension planner and see how it affected their pension level; and 37% knew they could decide to receive a higher pension level for the first years in retirement and then switch to a lower level for the remaining years. Only 19% answered correctly that they could not change their investment portfolio in the pension planner, whereas 73% answered "I don't know." We only find a statistically significant difference between treatment groups for the rate of participants who answered incorrectly for Question 2 (p=.01): 4% of participants in the 100*€20 treatment group answered that question incorrectly, whereas 12% in the 2*€1,000 treatment group did. As stated in Section 2.4.2, to calculate the pension-knowledge score, we sum the number of

correct answers and detract one point for each incorrect answer. As we show, the results are robust when we use a different scoring procedure.

Table 2.5: Statistics on Pension Knowledge Quiz: Overall and per Treatment (Study 2)

This table shows descriptive statistics of how many participants answered the six quiz questions correctly or "don't know" per treatment group. All variables present rates. The "Difference" column show the F-statistics of the differences between the characteristics of the participants between treatment groups. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

	Overall (N=2,231)		Baseline (N=334)		200*€10 (N=392)		100*€20 (N=383)		100*€10+€1,000 (N=360)		4*€500 (N=410)		2*€1,000 (N=352)		Difference Treatments F-statistic
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
Age Influences Pension Level															
Correct	0.41	0.49	0.38	0.49	0.43	0.50	0.40	0.49	0.42	0.49	0.44	0.50	0.40	0.49	0.54
Incorrect	0.13	0.34	0.13	0.34	0.15	0.36	0.11	0.32	0.14	0.35	0.12	0.33	0.14	0.35	0.67
Don't Know	0.45	0.50	0.48	0.50	0.42	0.49	0.49	0.50	0.44	0.50	0.44	0.50	0.46	0.50	1.01
Change Investment Portfolio															
Correct	0.19	0.39	0.19	0.39	0.18	0.38	0.18	0.39	0.20	0.40	0.22	0.41	0.18	0.38	0.61
Incorrect	0.08	0.27	0.08	0.26	0.07	0.26	0.04	0.21	0.08	0.27	0.10	0.29	0.12	0.32	2.90*
Don't Know	0.73	0.44	0.74	0.44	0.75	0.43	0.78	0.420	0.72	0.45	0.69	0.46	0.71	0.46	1.95
Paying Contribution															
Correct	0.73	0.44	0.75	0.44	0.72	0.45	0.72	0.45	0.76	0.43	0.73	0.44	0.70	0.46	0.91
Incorrect	0.13	0.33	0.13	0.33	0.14	0.34	0.14	0.35	0.12	0.33	0.14	0.35	0.14	0.34	0.25
Don't Know	0.13	0.33	0.13	0.33	0.14	0.35	0.14	0.34	0.12	0.32	0.12	0.33	0.17	0.37	0.89
Indexation															
Correct	0.48	0.50	0.47	0.50	0.50	0.50	0.46	0.50	0.48	0.50	0.48	0.50	0.50	0.50	0.41
Incorrect	0.06	0.24	0.06	0.24	0.06	0.24	0.07	0.25	0.07	0.26	0.06	0.24	0.04	0.20	0.54
Don't Know	0.46	0.50	0.47	0.50	0.44	0.50	0.48	0.50	0.45	0.50	0.45	0.50	0.46	0.50	0.28
Taking Pension to Other PF															
Correct	0.66	0.47	0.67	0.47	0.66	0.48	0.65	0.48	0.69	0.46	0.67	0.47	0.65	0.48	0.43
Incorrect	0.05	0.21	0.05	0.21	0.06	0.24	0.03	0.17	0.05	0.21	0.04	0.20	0.05	0.22	1.03
Don't Know	0.29	0.45	0.29	0.46	0.28	0.45	0.32	0.47	0.26	0.44	0.29	0.45	0.30	0.46	0.69
High-Low Construction															
Correct	0.37	0.48	0.36	0.48	0.40	0.49	0.35	0.48	0.39	0.49	0.35	0.48	0.35	0.48	0.69
Incorrect	0.10	0.30	0.10	0.30	0.09	0.29	0.11	0.32	0.10	0.30	0.10	0.29	0.11	0.31	0.28
Don't Know	0.53	0.50	0.54	0.50	0.51	0.50	0.54	0.50	0.52	0.50	0.55	0.50	0.54	0.50	0.45
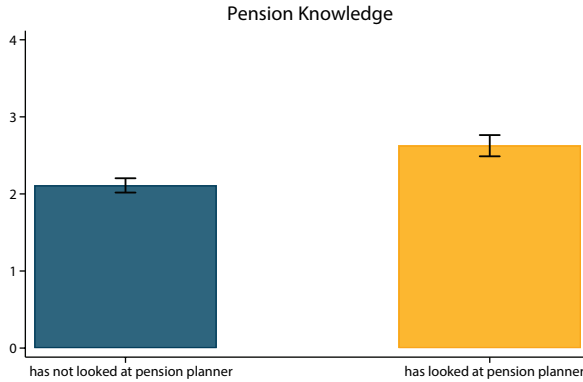
Figure 2.4. Pension knowledge by whether participants looked at the pension planner (Study 2).
The quiz questions can be found in Appendix 2.10. The error bars represent the 95% confidence intervals.

Figure 2.4 shows the average score from the pension-knowledge quiz for the participants who looked at the pension planner and for those who did not. Participants who viewed their planner scored significantly higher on the pension quiz (M =2.26, SD =0.07) than participants who did not (M =1.83, SD =0.05; t(2,229) =-5.25, p<.001). To see whether our results are robust to a different scoring procedure, Figure 2.A.11 shows participants who looked at the pension planner answered more questions correctly (without being punished for an incorrect answer).

Although the correlational evidence shows better pension knowledge for people who looked at the pension planner, looking at the pension planner might be endogenous. If participants with better pension knowledge are more likely to look at the pension planner, a simple OLS regression could be prone to reverse-causality issues. Given the selection biases of who looked at the planner and who answered the survey after Study 2, we use a two-stage least squares instrumental-variable approach in Table 2.6. We show the first stage in Column (1) and the second in Column (2). In the first stage, we regress the treatment dummies (as instrumental variables) and the standard control variables on Pension Planner.

As shown in Table 2.4 and as we can see in Table 2.6, Column (1), the treatment dummies and *Pension Planner* are positively correlated. The instrumental variables are exogenous because the assignment to a treatment group was random. Given the exogeneity, we should observe no direct correlation between treatment assignment and pension knowledge. The F-statistic of the test of the excluded instruments is F(5, 2203) =8.87 (p<.001).

Column (2) shows that the coefficient of *Pension Planner* is positive but not statistically significant in the second-stage regression. The weak-instrument robust Anderson-Rubin test also yields the same conclusion ($\mathcal{X}^2$ (5, N = 2,231) =3.01, p=0.70, 95% confidence interval of the *Pension Planner* coefficient [-1.90, 1.93]). In Table 2.A.13 in Appendix 2.10, we run the two-stage least squares instrumental-variable regressions using a revised pension-knowledge score. In this score, we use only the two questions that directly test knowledge about the pension planner. We find similar coefficients and conclude the treatment letters did not affect pension knowledge. Table 2.A.14 shows the same analysis with the pension score as the simple sum of correct answers to all six questions. We again conclude that the treatment letters did not affect pension knowledge.

We also use the two-stage least-squares instrumental-variable approach to see whether looking up retirement information affects the decision to save more.[8] Again, the relationship between having looked at the pension planner and our self-reported savings measure, *Saved More*, could be endogenous. We once more use the random-treatment assignment in the field experiment to argue for the satisfaction of the exogeneity condition and that no direct correlation exists between treatment assignment and the likelihood of reporting additional savings. Column (3) shows the second-stage regression. The first-stage regression is the same as for the previous instrumental-variable regression, thus depicted by Column (1). Similar to the results of Column (2), we see that looking at the pension

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[8]Because the dependent variables in the first stage and the second stage are binary variables, we also show marginal effects from a recursive simultaneous bivariate probit in Table 2.A.15 in Appendix 2.10. The conclusions are the same. We focus on the two-stage least-squares regression in the main text as Angrist and Pischke (2009) argue the two-stage least-squares regression is a more robust estimator.

planner has no statistically significant effect on self-reported savings behavior for retirement.

Table 2.6: Instrumental Variable Regression: Pension Knowledge and Self-Reported Savings Behavior (Study 2)

This table shows coefficients of two different two-stage least squares instrumental variable regressions. The first stage for both regressions is the same and has the dependent variable *Pension Planner*, indicating whether the participant looked at the pension planner. *Pension Planner* is instrumented by the treatment letters: *200*€10, 100*€20, 100*€10+€1,000, 4*€500*, and *2*€1,000*. The second stage is a regression on either *Pension Knowledge*, measured by a pension quiz score of six questions, or *Saved More*, equal to 1 if the participant has saved more in the three weeks before the survey, and 0 otherwise. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile rows indicate the income quintile to which the participant belongs. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group for Column 1 and for participants who have not looked at the pension planner for Columns 2 and 3. Robust standard errors are in parentheses. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

	Pension Planner	Pension Knowledge	Saved More
	(1)	(2)	(3)
Pension Planner		0.168	-0.053
		(0.628)	(0.063)
200*€10	0.100**		
	(0.033)		
100*€20	0.148***		
	(0.034)		
100*€10+€1,000	0.144***		
	(0.035)		
4*€500	0.200***		
	(0.034)		
2*€1,000	0.172***		
	(0.035)		
Male	0.027	0.125	0.012
	(0.024)	(0.089)	(0.010)
Age: 30-39	-0.135***	0.276	-0.016
	(0.038)	(0.156)	(0.017)
Age: 40-49	-0.186***	0.608**	-0.022
	(0.037)	(0.175)	(0.020)
Age: 50-59	-0.176***	1.149***	-0.030
	(0.034)	(0.165)	(0.017)
Age: 60-66	-0.146***	1.776***	-0.010
	(0.037)	(0.166)	(0.019)
Income: 2nd Quintile	-0.032	0.196	0.004
	(0.038)	(0.137)	(0.014)
Income: 3rd Quintile	-0.013	0.085	0.013
	(0.039)	(0.140)	(0.015)
Income: 4th Quintile	-0.034	0.202	0.008
	(0.039)	(0.146)	(0.015)
Income: Highest Quintile	-0.001	0.592***	0.024
	(0.039)	(0.143)	(0.016)
Part-timer	0.028	0.030	0.018
	(0.029)	(0.109)	(0.012)
Partner	0.024	0.051	0.000
	(0.023)	(0.088)	(0.009)
Constant	0.269***	0.983**	0.034
	(0.055)	(0.308)	(0.030)
Observations	2,231	2,231	2,231
Province Dummies	YES	YES	YES
Baseline Mean	0.043	2.110	0.031

2.9 Robustness of Our Results

This section examines the robustness of our results with respect to five aspects: heterogeneity, spillover effects, frequency of interest in retirement information, shifting planned behavior to the study period, and interaction effects between Study 1 and Study 2.

Treatment effects could differ depending on the participant's gender, age, and income. In Appendix 2.10 we describe the heterogeneity analyses we conduct in more detail. Concerning heterogeneous effects of gender, we find that men were less receptive than women to the lottery with the smallest prizes in Study 2 (a decrease of 0.9 pp). All other differences are nonsignificant. For age, we do not find heterogeneous treatment effects in Study 1. For Study 2, the interaction coefficients between age and 4*€500 and age and 2*€1,000 are statistically significant at the 95% significance level, with an extra increase of 0.3 pp per additional 10 years. Regarding effects depending on income, we find no statistically significant interactions for Study 1. For Study 2, we find that an extra €10,000 in income in the 2*€1,000 letter treatment group increased the likelihood of looking at the pension planner by 0.3 pp (p<.05). Overall, we find little evidence for heterogeneous treatment effects. The heterogeneity analysis yields null effects for the peer information treatments for all groups.

Financial incentives increased the likelihood of looking at personal-pension information for all studied subgroups. Financial incentives might create spillover effects (see Banerjee, Banerji, Berry, Duflo, Kannan, Mukerji, Shotland, and Walton (2017) for a discussion of spillover effects in RCTs). For example, Duflo and Saez (2003) find that employees who have received a monetary reward for attending a benefits information fair are as likely to enroll in a retirement plan as employees of the same department who have not received the incentive. In our experiment, employees in a branch could have talked about the possibility of receiving a financial reward when logging in, thus leading to a higher number of logins across treatments and washing out the treatment effects. Three aspects make spillover effects unlikely to explain our findings. First, spillover effects would attenuate the measured treatment effect, because participants in

the control group would also have been more likely to visit the pension planner, which would make our documented results rather conservative. Second, participants in the financial-incentive group had a disincentive to talk about the letter: the more people who logged in, the lower their chances of receiving the VVV gift voucher. Third, we test whether the fraction of participants in the financial-incentive group affects our results. In Table 2.A.16 in Appendix 2.10, we add the fraction of employees who work for the same employer and receive the same financial-incentive treatments as a control. The addition of the fractions does not affect the treatment effects of the financial incentives, nor are five out of six coefficients of the fraction variables statistically significant.[9] Spillover effects therefore do not explain our main results.

More than 90% of participants did not look at the pension planner in either study. To look at whether some participants never looked at the pension planner, we use additional logging data collected after Study 1, covering the period from February 2016 until and including December 2016. Because we do not have monthly administrative data, we need to assume the number of fund members stayed the same throughout the year. Of the fund members in Study 1, we find 5.12% looked at the pension planner at least once in the period between December 22, 2015, and December 31, 2016. In the Study 1 period (December 22, 2015, until February 1, 2016), 2.27% of participants looked at the pension planner at least once, meaning 44% of the participants who looked at the pension planner in the extended year of 2016 at least once did so during the study period. Looking at the rate of people logging in to the pension planner per month (Figure A10), we see that the month in which Study 1 took place is the month in which most participants looked at their personal retirement information. Our treatments were thus effective in motivating participants to look at the pension planner.

Another concern is that participants shifted their planned website visits forward into the treatment period. Gneezy, Meier, and Rey-Biel (2011) describe this decision as a crowding-out effect after incentives have been removed. This effect would mean we do not see an overall increase in looking at information,

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[9] *Fraction of 200*€10 letters per employer* is statistically significant at the 10% level (0.97 pp).

but rather a shift within the year for the participants in the financial-incentive treatment group. For Study 1, we do have logging data after the study period, but no monthly administrative data. Figure 2.5 shows the number of clicks per treatment for the period of January 2016 until and including December 2016. We see the increase in website visits of participants who received the financial-incentive letter only in the experimental period. Furthermore, participants in the financial-incentive treatment group were as likely to look at the planner during 2016 as participants who received any other letter. This finding shows that the financial-incentive treatment resulted in people looking at the pension planner at a rate that normally would not have taken place in a later period.[10]
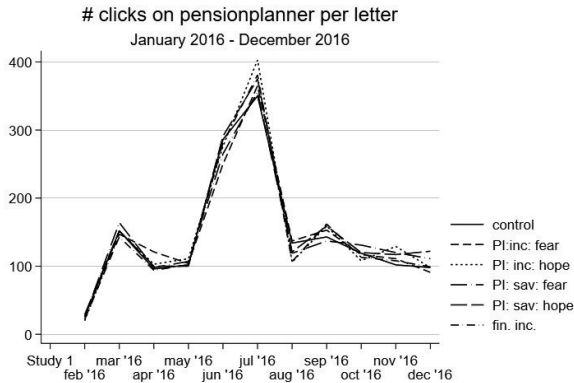
Figure 2.5. Number of clicks on the pension planner per letter, Study 1 and 11 months after.

A subset of participants in Study 2 were also participants in Study 1. During our study period, the average participant stayed active in the fund for about four to five years, which means an average turnaround in the population of 25%. Consequently, 47% of participants in Study 2 did not participate in Study 1: 120,838 of the 257,433 participants in Study 2 are not in the Study 1 data. This setup gives us the opportunity to examine whether participants who received the financial-incentive letter in Study 1, participants who received a different letter in

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[10]The data provider unfortunately did not record website behavior after Study 2. We can therefore not do a similar analysis for the behavior after Study 2.

Study 1, and participants who were not a member in Study 1 reacted differently to the letters. Column (2) in Table 2.A.17 in the Appendix shows a linear regression of Pension Planner in Study 2 on the treatment letters in Study 2. The regression controls for whether participants were not a fund member in Study 1 and whether they received the financial-incentive letter in Study 1. Participants who received any other letter in Study 1 are the baseline group.

We find that not being a member in Study 1 increased the likelihood of looking at the pension planner in Study 2 by 0.6 pp. Receiving the financial-incentive letter in Study 1 did not affect the likelihood of looking up personal information in Study 2. The treatment effects of the financial-incentives letters in Study 2 are the same as when we do not control for participation in Study 1. In column (3) in Table 2.A.17, we interact the treatments in Study 2 with the variable indicating whether participants received the financial-incentive letter in Study 1 or whether they were not a member in Study 1. Participants who received any other letter in Study 1 are the baseline group. *Not a member in Study 1* is no longer statistically significant. We find positive effects significant at the 95% confidence level for the interaction between Not a member in Study 1 and 100*€20 (0.6 pp) and the interaction between *Fin. Inc. in Study 1* and *100*€20* (1.3 pp). Our main results of Study 2 are thus robust to including controls for prior engagement in Study 1. Framed differently, there is no interaction or learning effect from additional incentives in the future for Study 1 members.

2.10   Conclusion and Discussion

Benartzi, Beshears, Milkman, Sunstein, Thaler, Shankar, Tucker-Ray, Congdon, and Galing (2017) recommend ways for improving behavioral science's power for policymakers. First, they recommend comparing the effects of different interventions on the same population. To our knowledge, our study is the first to compare the effects of different peer information and different financial incentives on savings decisions within the same pension fund population. Second, Benartzi, Beshears, Milkman, Sunstein, Thaler, Shankar, Tucker-Ray, Congdon, and Gal-

ing (2017) recommend reporting cost effectiveness. Our results indicate the financial incentive costs between €1.27 and €6.05 per additional person looking at the pension planner. Third, Benartzi, Beshears, Milkman, Sunstein, Thaler, Shankar, Tucker-Ray, Congdon, and Galing (2017) stress that "tracking failures is as important for knowledge creation as tracking successes" (p. 12). We document no effects, or slightly negative effects, of using peer information. This result is in line with that from an overview study on peer information (John, Sanders, and Wang, 2014). We also find no effects of financial incentives on pension knowledge or on self-reported savings behavior. Our two-stage instrumental-variable approach for the analysis on pension knowledge shows that correlational studies can be misleading. If we were just using correlations, we would falsely conclude that looking at the pension planner leads to more knowledge. This finding is in line with Fernandes, Lynch Jr, and Netemeyer (2014), who conclude that study designs that consider selection into treatment find a smaller effect of financial education on financial behavior than studies that ignore selection.

Why do we find that peer information is ineffective and sometimes even backfires? One explanation could be that people do not read the letters carefully, so one sentence was not enough to be treated.We find three arguments against this explanation. First, the one sentence about the financial incentive did have a positive effect, which indicates participants did notice the treatment sentence. Second, some of the peer-information sentences did have a significant effect, but a negative one rather than the expected positive effect. For example, the income-hope treatment significantly decreased the login rate. Third, Bhargava and Manoli (2015); Bott, Cappelen, Sørensen, and Tungodden (2020); Choi, Haisley, Kurkoski, and Massey (2017); Hallsworth, List, Metcalfe, and Vlaev (2017) and Goldin, Homonoff, Patterson, and Skimmyhorn (2020) also add only one sentence to the usual text and find the desired effects.

A second explanation for why our peer information treatments were ineffective could be that the particular statements we used are suboptimal. In our peer-information treatments, we referred to "a large part of people in the Netherlands." An advantage of this type of peer information is that we could credibly

make our statements in the context of the pension fund, without deceiving pension fund participants.We think that communicating in an honest manner to foster trust in the pension fund is crucial. We chose this type of peer information because a large number of previous studies successfully changed behavior this way (de Groot, Abrahamse, and Jones, 2013; Hallsworth, List, Metcalfe, and Vlaev, 2017; Sanders and Smith, 2016). For example, Hallsworth, List, Metcalfe, and Vlaev (2017) use the phrasing "a great majority of the people in the UK" to increase payment rates for overdue tax. Studies by Wenzel (2004, 2005) show that adding the country reference to the peer information in tax letters increases tax compliance. Based on the existing evidence, we therefore expected this type of peer information to be effective in triggering participants to look at their retirement information. However, if participants in our study perceive themselves to be disadvantaged compared with the general Dutch population, a discouragement effect to the one discovered by Beshears, Choi, Laibson, Madrian, and Milkman (2015) could have arisen. Another explanation could be that providing peer information is not as effective as previously found. DellaVigna and Linos (2020) show that the effect of nudges is overestimated in the academic literature due to publication bias and file-drawer bias.

Our study opens several avenues for future research. First, an important question is whether other types of peer information do work effectively to change behavior in the context of retirement. For example, Johnson, Shu, Dellaert, Fox, Goldstein, Häubl, Larrick, Payne, Peters, Schkade, Wansink, and Weber (2012) point out that more personalized nudges might increase the power of peer information. Peer information referring to specific characteristics of pension fund participants could be more effective. One could think of peer information relating to the specific employer at which the individual is employed or the neighborhood in which she lives.

Second, our study took place in the Netherlands, the country ranked first in the Mercer CFA Institute Global Pension Index (Mercer, 2020). The need for retirement information might be smaller in the world's best pension system. Moreover, the context of our study is a hybrid defined-benefit pension fund, where

the need for retirement information might be lower than in a defined-contribution system.Yet, de Bresser and Knoef (2015) estimate that a third of the Dutch population has a savings gap. Moreover, the planner shows participants what happens to their pension when they retire later or retire part-time, for example. Many participants make use of these options. In 2018, 66.1% of participants chose to move their retirement income to an earlier age than 67, the age at which they would receive full retirement benefits. The planner gives participants insights into the financial consequences of doing so. In addition, people can save for retirement privately, as 26.8% of our participants report doing. The pension planner can help individuals decide whether and how much to save for their retirement privately to complement their pension payments to achieve their desired level of consumption upon retirement.

Third, other approaches to improving pension knowledge and encouraging additional savings can be explored. The decision to save is complex, which might explain why we do not find any effects of peer information and financial incentives on self-reported savings behavior. Future research can focus on different avenues for increasing people's savings. Perhaps individuals need more time to decide on additional pension savings than the three weeks we observe. On the other hand, Fernandes, Lynch Jr, and Netemeyer (2014) document that the effect of financial education programs fades rather quickly. This finding suggests that individuals make savings decisions either rather quickly or not at all. Furthermore, simply providing individuals with information might not be enough, and more intensive financial education could be necessary. Kaiser, Lusardi, Menkhoff, and Urban (2021) find the most positive effects of financial education programs with longer duration. Moreover, using choice architecture to simplify the savings process might be important. Daminato, Filippini, and Haufler (2020) show that the availability of a pension app simplifying the process to directly apply for tax benefits increases tax-favored contributions. Goldin, Homonoff, Patterson, and Skimmyhorn (2020) emphasize that highlighting a specific contribution rate simplifies the savings decision and consequently leads to more savings.

Another interesting avenue for research is to look at the effects of financial incentives that are related to other stages in the Decision States Model. We have provided incentives to look at information. One could also incentivize actual learning in the pension-knowledge quiz or incentivize additional savings. An example for the latter is a lottery bond, in which the coupon rate is lower but bondholders have a chance to win a large amount of money, as long as they hold the bond (Lobe and Hölzl, 2007; Tufano, 2008). Another example is a prize-linked savings account. When one uses such an account, one automatically enters lotteries for which the number of eligible tickets depends on the account balance. Because incentives are key to economics, further exploring these avenues is fruitful. Moreover, Duflo, Gale, Liebman, Orszag, and Saez (2006) show that financial incentives do work if a professional advisor is helping individuals make decisions. Future studies can investigate whether a general pattern exists in which incentives work best in combination with financial advice. Examining the effect of lotteries with explicitly stated probabilities and whether offering even larger prizes than ours does increase effectiveness also seems promising.

Appendix

Appendix A. Summary Statistics per Treatment

Table 2.A.1: Summary Statistics per Treatment, Study 1

This table shows descriptive statistics for the active participants of the pension fund for the retail sector per treatment group for Study 1. Except for the full-time equivalent (FTE), age (in years), and income (in €), all variables present rates. The "Difference" column show the F-statistics of the differences between the characteristics of the participants between treatment groups. The province names not fully spelled out are: N.-Holl.=Noord-Holland, Z.-Holland =Zuid-Holland, and N.-Brabant= Noord-Brabant. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	Baseline (N=37,855)		Income: Fear (N=37,993)		Income: Hope (N=37,806)		Saving: Fear (N=37,748)		Saving: Hope (N=37,798)		Fin. Incentive (N=37,806)		Difference
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	F-statistic
Male	0.31	0.46	0.31	0.46	0.31	0.46	0.31	0.46	0.31	0.46	0.31	0.46	0.35
Age (yrs.)	37.42	12.96	37.34	12.91	37.44	12.98	37.50	12.96	37.46	13.04	37.51	13.00	0.89
FTE	0.67	0.29	0.67	0.29	0.67	0.29	0.67	0.29	0.67	0.29	0.67	0.29	1.11
Partner	0.39	0.49	0.39	0.49	0.40	0.49	0.40	0.49	0.39	0.49	0.40	0.49	0.76
Income (€)	18,433	13,021	18,343	12,951	18,331	12,975	18,340	13,031	18,320	13,018	18,379	12,971	0.39
Provinces													
N.-Holl.	0.18	0.39	0.18	0.39	0.19	0.39	0.18	0.39	0.18	0.39	0.19	0.39	0.06
Flevoland	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.02
Utrecht	0.08	0.27	0.08	0.27	0.08	0.27	0.08	0.27	0.08	0.27	0.08	0.27	0.06
Z.-Holland	0.20	0.40	0.21	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.05
Gelderland	0.12	0.32	0.12	0.32	0.12	0.32	0.12	0.32	0.12	0.32	0.12	0.32	0.04
N.-Brabant	0.14	0.35	0.14	0.35	0.14	0.35	0.14	0.35	0.14	0.35	0.14	0.35	0.09
Zeeland	0.02	0.15	0.02	0.15	0.02	0.15	0.02	0.15	0.02	0.15	0.02	0.15	0.06
Limburg	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.03
Overijssel	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.01
Drenthe	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03
Friesland	0.03	0.18	0.03	0.18	0.03	0.18	0.03	0.18	0.03	0.18	0.03	0.18	0.04
Groningen	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.16

Table 2.A.2: Summary Statistics per Treatment, Study 2

This table shows descriptive statistics for the active participants of the pension fund for the retail sector per treatment group for Study 2. Except for the full-time equivalent (FTE), age, and income, all variables present rates. The "Difference" column show the F-statistics of the differences between the characteristics of the participants between treatment groups. The province names not fully spelled out are: N.-Holl.=Noord-Holland, Z.-Holland =Zuid-Holland, and N.-Brab.= Noord-Brabant. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

| | Baseline (N=42,900) | | 200*€10 (N=42,896) | | 100*€20 (N=42,917) | | 100*€10+€1,000 (N=42,953) | | 4*€500 (N=42,881) | | 2*€1,000 (N=42,886) | | Difference |
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	F-statistic
Male	0.34	0.48	0.35	0.48	0.34	0.47	0.34	0.48	0.34	0.48	0.34	0.48	0.63
Age (yrs.)	37.92	13.06	37.98	13.14	37.93	13.15	37.90	13.06	38.05	13.15	37.97	13.10	0.69
FTE	0.69	0.29	0.69	0.29	0.69	0.29	0.69	0.29	0.68	0.29	0.69	0.29	0.22
Partner	0.39	0.49	0.39	0.49	0.39	0.49	0.39	0.49	0.39	0.49	0.39	0.49	0.32
Income (€)	22,413	16,270	22,452	16,261	22,386	16,206	22,469	16,324	22,388	16,247	22,393	16,218	0.21
Provinces													
N.-Holl.	0.18	0.38	0.18	0.39	0.18	0.39	0.18	0.39	0.19	0.39	0.18	0.38	1.24
Flevoland	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.03	0.16	0.47
Utrecht	0.07	0.26	0.07	0.26	0.07	0.26	0.07	0.26	0.07	0.26	0.07	0.26	0.16
Z.-Holland	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.98
Gelderland	0.11	0.32	0.11	0.32	0.12	0.32	0.11	0.316	0.12	0.32	0.11	0.32	0.43
N.-Brab.	0.16	0.36	0.16	0.36	0.16	0.36	0.16	0.37	0.16	0.36	0.16	0.37	1.37
Zeeland	0.02	0.15	0.02	0.15	0.03	0.15	0.02	0.15	0.02	0.15	0.02	0.15	0.18
Limburg	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	1.05
Overijssel	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.25	1.22
Drenthe	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.16	0.03	0.16	0.03	0.16	2.47*
Friesland	0.03	0.18	0.03	0.18	0.03	0.18	0.03	0.18	0.03	0.18	0.04	0.19	1.44
Groningen	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.17	1.92

Table 2.A.3: Website Behavior in Study 1 and Study 2

This table shows data on website behavior. Panel A shows the fraction of participants who, conditional on having logged in to MyEnvironment, have clicked on different sites within MyEnvironment. My Request was not an available site in Study 1, and there was no Survey available in Study 2. Panel B shows summary statistics on the total time spent on the pension planner, conditional on having spent time on the pension planner.

	Panel A			
	Study 1: 7,092 logged in Mean		Study 2: 54,747 logged in Mean	
Conditional on Having Logged In, Fraction of Participants Clicking On:				
Pension Planner	0.73		0.30	
My Request	NA		0.17	
My Profile	0.67		0.35	
My Mail	0.75		0.93	
Logout	0.52		0.36	
Survey (Study 1)	0.21		NA	
	Panel B			
	Study 1: 3,505 tracked		Study 2: 50,286 tracked	
	Mean	SD	Mean	SD
Conditional on Having Looked at the Pension Planner:				
Total Time on Pension Planner	5.51	8.81	2.72	3.77

Appendix B. The Letters

The Baseline Letter: Studies 1 and 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

The Income-Fear Letter: Study 1

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

A large part of people in the Netherlands think that they will have a <u>too low income</u> to retain their current level of consumption in retirement. What about you?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

2. A FISTFUL OF DOLLARS

The Income-Hope Letter: Study 1

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

A large part of people in the Netherlands think that they will have <u>**enough income**</u> **to retain their current level of consumption in retirement. What about you?**

On <u>www.pensioenfondsdetailhandel.nl</u> you check- in less than a minute - what you will be able to spend soon.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

The Savings-Fear Letter: Study 1

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

A large part of people in the Netherlands think they <u>save too little</u> to retain their current level of consumption in retirement. What about you?

On <u>www.pensioenfondsdetailhandel.nl</u> you check- in less than a minute - what you will be able to spend soon.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

The Savings-Hope Letter: Study 1

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

A large part of people in the Netherlands think that they will <u>save enough</u> to retain their current level of consumption in retirement. What about you?

On <u>www.pensioenfondsdetailhandel.nl</u> you check- in less than a minute - what you will be able to spend soon.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?
Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

The Financial Incentive Letter: Study 1

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 100 VVV gift vouchers worth €25.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?
Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

2. A FISTFUL OF DOLLARS

200*€10: Study 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 200 VVV gift vouchers worth €10.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

100*€20: Study 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 100 VVV gift vouchers worth €20.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

2. A FISTFUL OF DOLLARS

100*€10+€1,000: Study 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 100 VVV gift vouchers worth €10 and one VVV gift voucher worth €1,000.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

4*€500: Study 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 4 VVV gift vouchers worth €500.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

2. A FISTFUL OF DOLLARS

2*€1,000: Study 2

Dear participant,

Have you ever thought about your future income? Your pension? The money which you will receive monthly from age 67 onwards?

On www.pensioenfondsdetailhandel.nl you check- in less than a minute - what you will be able to spend soon.

Among all participants who log in, we are raffling 2 VVV gift vouchers worth €1,000.

This is how you do it:

- Go to "Mijn Omgeving" on pensioenfondsdetailhandel.nl;

- Log in with your personal ID number;

- Look at your personal pension accrual and pension planner.

With this new insight you can decide whether you want to set aside additional money or whether you want to keep on accruing your pension at your current rate.

"Mijn Omgeving" also has a helpful tool to see what it means for your pension if you change your job, start living together, get married or divorced... Topics you also find back in the Jij&Wij Magazine, the journal you receive with this letter.

May we ask you to check your personal situation?

Thanks in advance,
Best wishes,
Henk van der Kolk
President of the Pensioenfonds Detailhandel

Appendix C. Screenshots of the Website
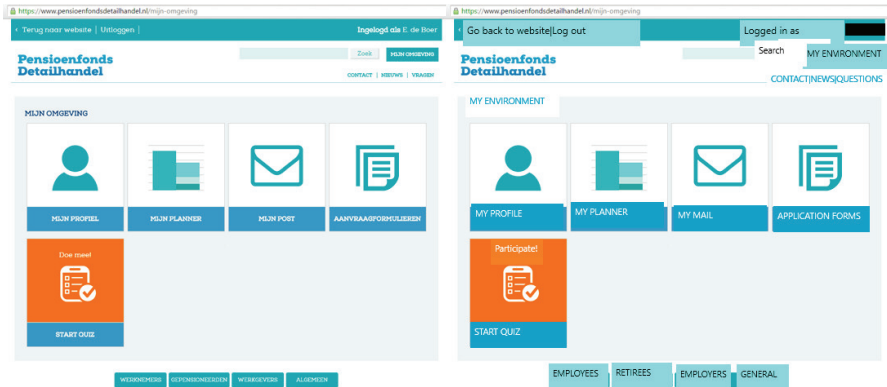
Figure 2.A.1. Screenshot of the first page the participant sees when she logs in to the "My Environment", plus translations.
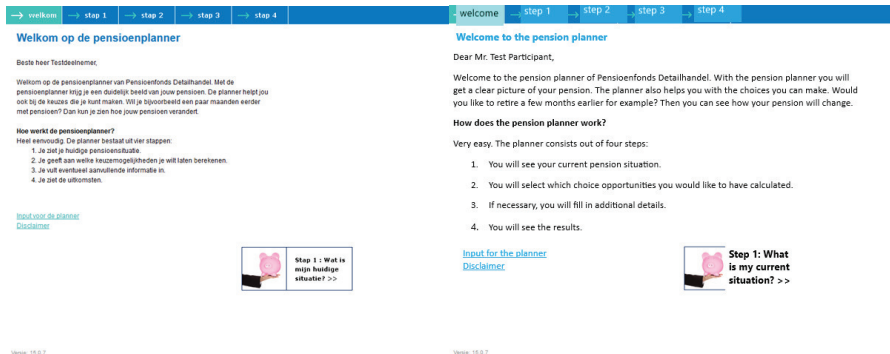
Figure 2.A.2. Screenshot of the welcome page on the pension planner.

It welcomes the participant and explains the pension planner.
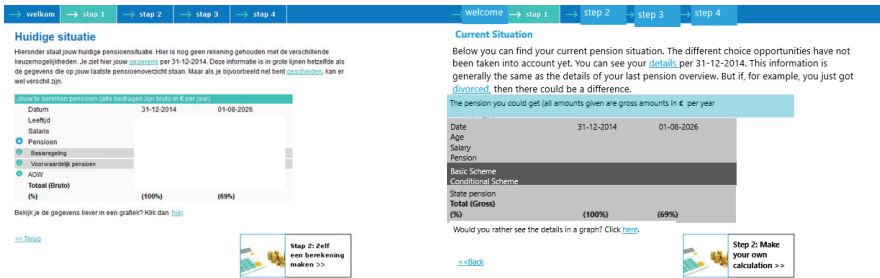
Figure 2.A.3. Screenshot of the page of step 1 of the pension planner.
While this profile is empty, a participant would see the age, salary, pension, and state pension at the end of 2014 and on the date the participant will retire (here, 1 August 2026).
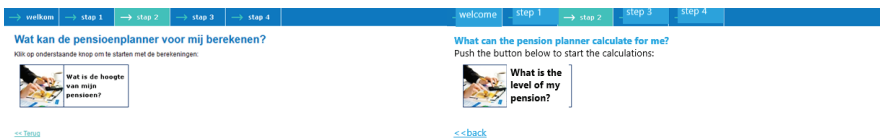
Figure 2.A.4. Screenshot of the page of step 2 of the pension planner.
If the participant clicks on the picture, one can change assumptions of the planner.

Figure 2.A.5. Screenshot of the first page of step 3 of the pension planner.
The participant can insert the desired pension age and tick on boxes for stopping to work in the sector, retiring part-time, and having accumulated a pension somewhere else.
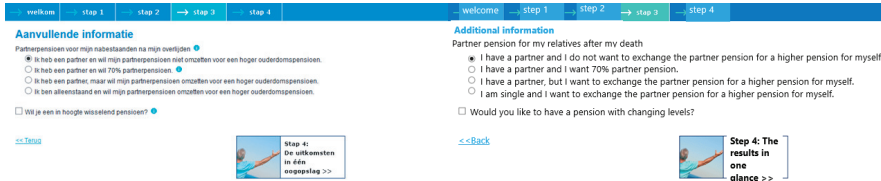
Figure 2.A.6. Screenshot of the second page of step 3 of the pension planner.
The participant can choose 1) she has a partner and does not want to exchange the partner pension for a higher pension for herself; 2) she has a partner and wants 70% partner pension; 3) she has a partner, but wants to exchange the partner pension for a higher pension for herself; 4) she is single and wants to exchange the partner pension for a higher pension for herself. The participant can also tick the box if she wants to have a pension that is either lower first and then higher, or higher first and then lower.

Figure 2.A.7. Screenshot of the page of step 4 of the pension planner.
The participant again sees the same information items as in step 1, but now the changes and information given in steps 2 and 3 are taken into account.

Appendix D. Survey Questions on Pension Knowledge and Where to Find the Answers

1. On *Pensioenfonds Detailhandel's* pension planner, you can enter the age at which you wish to retire. As it stands, can you see how the age of retirement affects the size of your pension?

   - Yes

   - No, not in this version of the planner

   - I don't know

   The correct answer is "Yes" and could be found on the pension planner at Stap 3 (Step 3, see Figure 2.A.5 in Appendix 2.10). The pension planner states "my desired retirement age is 67 years and 0 months." (the participants can change the 67 and 0; in Dutch: "Mijn gewenste pensioenleeftijd is 67 jaar en 0 maanden.")

2. You can tailor your own investment portfolio on Pensioenfonds Detailhandel's website.

   - True

   - False

   - I don't know

   The correct answer is "False". You cannot change the investment portfolio in the pension planner and changing the investment portfolio is not listed in the Pensioen 1-2-3 under "Which choices do you have?" ("Welke keuzes heb je zelf?).

3. Who pays the pension contributions for employee pensions?

   - Usually only the employee

   - Usually only the employer

- Usually both the employee and the employer

- I don't know

The correct answer is "Usually both the employee and the employer". This information could be found in the Pensioen 1-2-3 under "How do you accrue your pension?" ("Hoe bouw je pensioen op?"). The section reads:

You and your employer both contribute to your pension

Each month you pay contribution for your pension. Your employer does the same. The contribution rate at Pensioenfonds Detailhandel is 21.6%. Ask your employer how much you pag and how much your employer pays. The contribution that you pay can also be found on your salary statement.

*(“**Jij en je werkgever betalen allebei voor jouw pension***

Je betaalt elke maand premie voor je pensioen. Je werkgever doet dat ook. Bij Pensioenfonds Detailhandel is de premie 21.6%. Vraag bij je werkgever na hoeveel jij betaalt en hoeveel je werkgever betaalt. De premie die je zelf betaalt, staat ook op je loonstrook.”)

4. In the last two years, product prices in the Netherlands have risen. Did your pension with Pensioenfonds Detailhandel grow in line with price increases in the last two years? (This process is called indexation)

   - Pensions have matched price increases

   - Pensions did not match price increases

   - I don't know

   The correct answer is "Pensions did not match price increases". The Pensioen 1-2-3 contains the information in the section "How safe is your pension?" ("Hoe zeker is je pensioen?"):

Index-linked Pension

We try to let your pension grow annually with the price increases of that year. This is called indexation. this is only possible if the financial situation of our pension fund is good enough. The last years we have adjusted the pensions of participants like this:

*(**Waardevast pensioen***
Wij proberen jouw pensioen elk jaar mee te laten groeien met de prijzen. Dit heet indexatie. Dit kan alleen als de financiele situatie van ons pensioen-

2. A FISTFUL OF DOLLARS

	Indexation	Change in Prices
2017	0.0%	1.4% increased
2016	0.0%	0.3% decreased
2015	0.0%	0.8% increased
2014	0.3%	0.6% increased
2013	0.0%	1.7% increased

fonds goed genoeg is. De afgelopen jaren hebben wij de pensioenne voor deelnemers zo geindexeerd:)

	Indexatie	Verandering van de prijzen
2017	0.0%	1.4% increased
2016	0.0%	0.3% decreased
2015	0.0%	0.8% increased
2014	0.3%	0.6% increased
2013	0.0%	1.7% increased

5. If you have accrued pension with Pensioenfonds Detailhandel and you get a new job, can you choose to transfer the accrued amount to another pension fund?

   - Yes

   - No

   - I don't know

   The correct answer is "Yes". The answer can be found in *Pensioen 1-2-3* in the section "When do you have to act?" ("Wanneer moet je in actie komen?"). The sub-section "If you change your job" ("Als je van baan verandert") reads: "If you have accrued pension with us, then you can take it with you to your new pension fund or insurer." ("Heb je bij ons pensioen opgebouwd, dan kun je dat meenemen naar je nieuwe pensioenfonds of –verzekeraar.")

6. When you retire, you can choose to receive a higher amount at the beginning of your pension followed by a slightly lower amount afterwards.

- True

- False

- I don't know

The correct answer is "True". In *Stap 3*, (Step 3, see Figure 2.A.6 in Appendix 2.10) the pension planner asks "Do you want a pension that varies in amounts?" ("Wil je een in hoogte wisselend pensioen?")

Appendix E. Heterogeneity Analyses

Gender

Figure 2.A.8 shows the effect of the different treatment groups for women and men separately. In both studies, women were less likely than men to look at the pension planner. Interestingly, Study 1 shows that when we offered a financial incentive, the rates of women and men looking at their pension planner converged (Panel (a)). Also in Study 2, women and men looked at the pension planner equally often in the financial-incentive treatments. Although women looked more often at the pension planner in the lottery groups with smaller prizes, men did so more often in the 4*€500 and 2*€1,000 lottery groups.

To analyze heterogeneity in treatment effects, we run OLS regressions and add interactions between the male dummy and the treatments. Table 2.A.4 shows the coefficients. In Study 1, the financial-incentive letter had a negative effect on the likelihood of looking at the pension planner for men, but the difference is not statistically significant. In Study 2, men were less receptive than women to the lottery with the smallest prizes (a decrease of 0.9 percentage points (pp)). All other differences are nonsignificant.

Age

Figure 2.A.9 splits the sample into participants above and below the median age of 36. Panel (a) shows the results from Study 1. On average, younger participants were less likely to look for information on their retirement situation. A financial incentive increased their average rate of looking at the pension planner to a level similar to the rate of the older participants in the control group. Panel (b) presents the results from Study 2. Most people in both age groups reacted to the lottery raffling two packs of vouchers worth €1,000. As with Study 1, the younger participants looked at pension information less than the older participants.

We add interactions of continuous age (in steps of 10 years) with the treatment dummies in Table A5. In Study 1, no heterogenous treatment effect is detectable. For Study 2, the interaction coefficients between age and 4*€500 and age and 2*€1,000 are statistically significant at the 95% significance level, with an extra increase of 0.3 pp per additional 10 years.
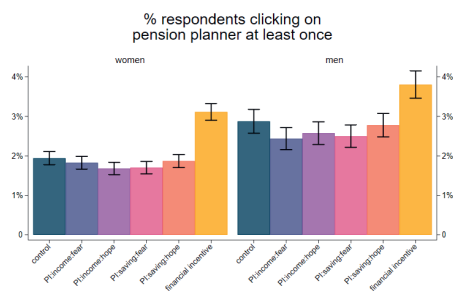
(a)
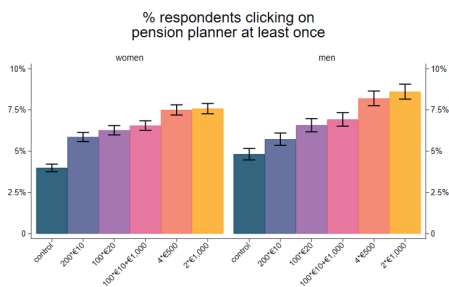
(b)

Figure 2.A.8. Percentage of respondents who visited their personal pension planner (per letter, by gender).

Panel (a) shows the results of Study 1, and Panel (b) shows the results of Study 2. The samples are split by gender. The error bars represent the 95% confidence intervals. Table 2.A.9 in Appendix 2.10 shows the mean differences and p-values for the differences.

Table 2.A.4: Website Behavior in Study Less than Women to Smaller Financial Incentives (Study 1 and Study 2)

This table shows the OLS regressions with the dependent variable *Pension Planner* (binary variable), for Study 1 and Study 2. *Financial incentive in Study 1, 200*€10, 100*€20, 100*€10+€1,000, 4*€500*, and *2*€1,000* indicate which letter a participant received. Participants who received the control letter are in the baseline category. We control for the covariates from Table 2.4. In Columns (1) and (2) we control for the peer-information treatment letters. In Column (2) we control for the interactions between Male and the peer-information/comparison treatment letters. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group. Robust standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05

	Pension Planner (Study 1)		Pension Planner (Study 2)	
	(1)	(2)	(3)	(4)
Financial incentive in Study 1	0.011***	0.012***		
	(0.001)	(0.001)		
Male x Financial		-0.003		
Incentive in Study 1		(0.003)		
Male	0.007***	0.009***	0.006***	0.009***
	(0.001)	(0.002)	(0.001)	(0.002)
Financial incentives in Study 2				
200*€10			0.015***	0.018***
			(0.001)	(0.002)
100*€20			0.021***	0.023***
			(0.002)	(0.002)
100*€10+€1,000			0.024***	0.026***
			(0.002)	(0.002)
4*€500			0.035***	0.035***
			(0.002)	(0.002)
2*€1,000			0.037***	0.036***
			(0.002)	(0.002)
Male x 200*€10				-0.009**
				(0.003)
Male x 100*€20				-0.005
				(0.003)
Male x 100*€10+€1,000				-0.004
				(0.003)
Male x 4*€500				-0.001
				(0.003)
Male x 2*€1,000				0.002
				(0.003)
Constant	0.006***	-0.000	0.025***	0.033***
	(0.002)	(0.002)	(0.0022)	(0.002)
Observations	226,946	226,946	257,433	257,433
Adj. R-squared	0.013	0.014	0.010	0.010
Other Covariates	YES	YES	YES	YES
Other Treatments	YES	YES		
Other Interactions		YES		
Baseline Mean		0.022		0.043

(a) Study 1

(b) Study 2

Figure 2.A.9. Percentage of respondents who visited their personal pension planner (per letter, by median age).

Panel (a) shows the results of Study 1, and Panel (b) shows the results of Study 2. The samples are split by the median age (36 years). The error bars represent the 95% confidence intervals. Table 2.A.10 in Appendix 2.10 shows the mean differences and p-values for the differences.

Table 2.A.5: Older Participants React Slightly More to (Larger) Incentives than Younger Participants (Study 1 and Study 2)

This table shows the OLS regressions with the dependent variable *Pension Planner* (binary variable). *Financial incentive in Study 1, 200*€10, 100*€20, 100*€10+€1,000, 4*€500,* and *2*€1,000* indicate which letter a participant received. The control letter is the baseline. We now use *Age (in steps of 10 years)* instead of the age categories. We control for the covariates from Table 2.4. In Columns (1) and (2) we control for the peer-information treatment letters. In Column (2) we control for the interactions between *Age (in steps of 10 years)* and the peer-information treatment letters. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group. Robust standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05

	Pension Planner (Study 1)		Pension Planner (Study 2)	
	(1)	(2)	(3)	(4)
Financial incentive in Study 1	0.011***	0.003		
	(0.001)	(0.004)		
Age x Financial incentive in Study 1		0.002		
		(0.001)		
Age (in steps of 10 years)	0.008***	0.009***	0.006***	0.004***
	(0.000)	(0.001)	(0.000)	(0.001)
Financial Incentives in Study 2				
200*€10			0.015***	0.010*
			(0.001)	(0.005)
100*€20			0.021***	0.013**
			(0.002)	(0.005)
100*€10+€1,000			0.024***	0.015**
			(0.002)	(0.005)
4*€500			0.035***	0.023***
			(0.002)	(0.005)
2*€1,000			0.037***	0.026***
			(0.002)	(0.005)
Age x 200*€10				0.001
				(0.001)
Age x 100*€20				0.002
				(0.001)
Age x 100*€10+€1,000				0.002
				(0.001)
Age x 4*€500				0.003*
				(0.001)
Age x 2*€1,000				0.003*
				(0.001)
Constant	-0.014***	-0.017***	0.017***	0.025***
	(0.002)	(0.003)	(0.003)	(0.004)
Observations	226,946	226,946	257,433	257,433
Adj. R-squared	0.008	0.008	0.005	0.005
Other Covariates	YES	YES	YES	YES
Other Treatments	YES	YES		
Other Interactions		YES		
Baseline Mean		0.022		0.043

Income

Previous studies have found that low-income individuals react more strongly to financial incentives (cf. John, MacDonald, and Sanders (2015), Haisley, Volpp, Pellathy, and Loewenstein (2012)). Because most of the interactions in our regressions are nonsignificant, we do not find this effect. Within our sample, participants with higher incomes reacted slightly more than low-income individuals to large incentives. This finding suggests the effects of financial incentives are not limited to low-income individuals.

This subsection shows we find little evidence for heterogeneous treatment effects. Financial incentives increased the likelihood of looking at personal-pension information for all studied subgroups. The money spent by the pension fund thus has a positive effect on looking up retirement information for all people in the fund.

Table 2.A.6: High-Income Participants React Slightly More to Large Incentives (Study 1 and Study 2)

This table shows the OLS regressions with the dependent variable *Pension Planner* (binary variable). *Financial incentive in Study 1, 200*€10, 100*€20, 100*€10+€1,000, 4*€500,* and *2*€1,000* indicate which letter a participant received. The control letter is the baseline. The interactions between income (in €10,000) and the treatment letters are indicated in rows 13–23. We control for the covariates from Table 2.4. In Columns (1) and (2), we control for the peer-information treatment letters. In Column (2), we control for the interactions between *Age* and the peer-information treatment letters. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group. Robust standard errors are in parentheses. $p<0.001$, ** $p<0.01$, * $p<0.05$

	Pension Planner (Study 1)		Pension Planner (Study 2)	
	(1)	(2)	(3)	(4)
Financial incentive in Study 1	0.011***	0.009***		
	(0.001)	(0.002)		
Income x Financial incentive in Study 1		0.001		
		(0.001)		
Income	0.005***	0.005***	0.002***	0.002*
	(0.000)	(0.001)	(0.000)	(0.001)
Financial incentives in Study 2				
200*€10			0.015***	0.014***
			(0.001)	(0.003)
100*€20			0.021***	0.021***
			(0.002)	(0.003)
100*€10+€1,000			0.024***	0.024***
			(0.002)	(0.003)
4*€500			0.035***	0.033***
			(0.002)	(0.003)
2*€1,000			0.037***	0.030***
			(0.002)	(0.003)
Income x 200*€10				0.001
				(0.001)
Income x 100*€20				-0.000
				(0.001)
Income x 100*€10+€1,000				0.000
				(0.001)
Income x 4*€500				0.001
				(0.001)
Income x 2*€1,000				0.003*
				(0.001)
Constant	0.000	-0.000	0.025***	0.027***
	(0.002)	(0.002)	(0.002)	(0.003)
Observations	226,946	226,946	257,433	257,433
R-squared	0.014	0.014	0.010	0.010
Other Covariates	YES	YES	YES	YES
Other Treatments	YES	YES		
Other Interactions		YES		
Baseline Mean		0.022		0.043

Appendix F. Multiple Hypotheses Testing

Table 2.A.7: Multiple Hypotheses Adjustments According to List, Shaikh, and Xu (2019), Study 1

This table shows the multiple hypotheses adjusted p-values according to List, Shaikh, and Xu (2019). We refer to their remark and theorem. The first column presents the treatment group. *DI* stands for the differences in means between treatment and control group. *** p< 0.01, ** p<0.05, * p<0.1

Letter	*DI*	*p*-values			
		Unadjusted	Multiplicity Adjusted		
		Remark 3.1	Theorem 3.1	Bonferroni	Holm
Fin. inc.	0.0110	0.0003***	0.0003***	0.0017***	0.0017***
SN:inc:fear	0.0022	0.0393**	0.0687*	0.1967	0.0787*
SN:inc:hope	0.0028	0.0087***	0.0220**	0.0433**	0.0260**
SN:sav:fear	0.0029	0.0077***	0.0243**	0.0383**	0.0031***
SN:sav:hope	0.0008	0.4657	0.4657	1.0000	0.4657

Table 2.A.8: Multiple Hypotheses Adjustments According to List, Shaikh, and Xu (2019), Study 2

This table shows the multiple hypotheses adjusted p-values according to List, Shaikh, and Xu (2019)). We refer to their remark and theorem. The first column presents the treatment group. *DI* stands for the differences in means between treatment and control group. *** p<0.01, ** p<0.05, * p<0.1

Letter	*DI*	*p*-values			
		Unadjusted	Multiplicity-Adjusted		
		Remark 3.1	Theorem 3.1	Bonferroni	Holm
200*€10	0.0154	0.0003***	0.0003***	0.0017***	0.0003***
100*€20	0.0210	0.0003***	0.0003***	0.0017***	0.0010***
100*€10+€1,000	0.0241	0.0003***	0.0003***	0.0017***	0.0013***
4*€500	0.0347	0.0003***	0.0003***	0.0017***	0.0017***
2*€1,000	0.0366	0.0003***	0.0003***	0.0017***	0.0007***

Table 2.A.9: Multiple Hypotheses Testing: Gender, Study 1 and Study 2

This table shows the multiple hypotheses adjusted p-values according to List, Shaikh, and Xu (2019). Panel A presents the results from Study 1, Panel B, from Study 2. The first column shows the gender, and the second column shows the treatment group. *DI* stands for differences in means between treatment and control group. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

| Subgroup | Letter | DI | Unadjusted | | Multiplicity Adjusted | |
			Remark 3.1	Theorem 3.1	Bonferroni	Holm
				p-values		
Panel A: Study 1						
Men	Fin.inc.	0.0093	0.0003***	0.0003***	0.0033***	0.0030***
Men	SN:inc:fear	0.0044	0.0367**	0.1953	0.3667	0.2567
Men	SN:inc:hope	0.0030	0.1610	0.4683	1.0000	0.6427
Men	SN:sav:fear	0.0038	0.0753*	0.2837	0.7533	0.3767
Men	SN:sav:hope	0.0010	0.6490	0.6490	1.0000	0.6490
Women	Fin.inc	0.0117	0.0003***	0.0003***	0.0033***	0.0033**
Women	SN:inc:fear	0.0012	0.3233	0.6687	1.0000	0.9700
Women	SN:inc:hope	0.0026	0.0233**	0.1437	0.2333	0.1867
Women	SN:sav:fear	0.0024	0.0380**	0.1813	0.3800	0.2280
Women	SN:sav:hope	0.0007	0.5350	0.7790	1.0000	1.000
Panel B: Study 2						
Men	200*€10	0.0091	0.0003***	0.0003***	0.0033***	0.0013***
Men	100*€20	0.0176	0.0003***	0.0003***	0.0033***	0.0020***
Men	100*€10+€1,000	0.0211	0.0003***	0.0003***	0.0033***	0.0030***
Men	4*€500	0.0339	0.0003***	0.0003***	0.0033***	0.0010***
Men	2*€1,000	0.0379	0.0003***	0.0003***	0.0033***	0.0017***
Women	200*€10	0.0187	0.0003***	0.0003***	0.0033***	0.0007***
Women	100*€20	0.0228	0.0003***	0.0003***	0.0033***	0.0023***
Women	100*€10+€1,000	0.0257	0.0003***	0.0003***	0.0033***	0.0027***
Women	4*€500	0.0351	0.0003***	0.0003***	0.0033***	0.0003***
Women	2*€1,000	0.0359	0.0003***	0.0003***	0.0033***	0.0033***

Table 2.A.10: Multiple Hypotheses Testing; Age

This table shows the multiple hypotheses adjustments according to the procedure proposed by List, Shaikh, and Xu (2019). Panel A presents the results for Study 1, and Panel B for Study 2. The first column shows the subgroup. The second column presents the treatment group. *DI* stands for the differences in means between the treatment and control group. *** p<0.01, ** p<0.05, * p<0.1

| Subgroup | Letter | DI | Unadjusted | Multiplicity Adjusted | | |
			Remark 3.1	Theorem 3.1	Bonferroni	Holm
Panel A: Study 1						
Above median age	Fin.inc.	0.012	0.0003***	0.0003***	0.0033***	0.0030***
Above median age	SN:inc:fear	0.0031	0.0637*	0.2863	0.6367	0.3820
Above median age	SN:inc:hope	0.0031	0.0740*	0.2847	0.7400	0.3700
Above median age	SN:sav:fear	0.0056	0.0010***	0.0057***	0.0100**	0.0080***
Above median age	SN:sav:hope	0.0017	0.3293	0.6853	1.0000	0.9880
Below median age	Fin.inc.	0.0096	0.0003***	0.0003***	0.0033***	0.0033***
Below median age	SN:inc:fear	0.0013	0.3047	0.7303	1.0000	1.0000
Below median age	SN:inc:hope	0.0025	0.0397**	0.2080	0.3967	0.2777
Below median age	SN:sav:fear	0.0003	0.8490	0.9740	1.0000	1.0000
Below median age	SN:sav:hope	0.0001	0.9617	0.9617	1.0000	0.9617
Panel B: Study 2						
Above median age	200*€10	0.0181	0.0003***	0.0003***	0.0033***	0.0007***
Above median age	100*€20	0.0225	0.0003***	0.0003***	0.0033***	0.0027***
Above median age	100*€10+€1,000	0.0267	0.0003***	0.0003***	0.0033***	0.0017***
Above median age	4*€500	0.0372	0.0003***	0.0003***	0.0033***	0.0023***
Above median age	2*€1,000	0.0389	0.0003***	0.0003***	0.0033***	0.0033***
Below median age	200*€10	0.0124	0.0003***	0.0003***	0.0033***	0.0003***
Below median age	100*€20	0.0193	0.0003***	0.0003***	0.0033***	0.0030***
Below median age	100*€10+€1,000	0.0212	0.0003***	0.0003***	0.0033***	0.0010***
Below median age	4*€500	0.0319	0.0003***	0.0003***	0.0033***	0.0013***
Below median age	2*€1,000	0.0341	0.0003***	0.0003***	0.0033***	0.0020***

Table 2.A.11: Multiple Hypotheses Testing: Income

This table shows the multiple hypotheses adjusted *p*-values according to List, Shaikh, and Xu (2019). Panel A presents the results for Study 1, and Panel B for Study 2. The first column shows the subgroup. The second column presents the treatment group. *DI* stands for differences in means between treatment and control group. *** p<0.01, ** p<0.05, * p<0.1

Subgroup	Letter	DI	*p*-values			
			Unadjusted	Multiplicity Adjusted		
			Remark 3.1	Theorem 3.1	Bonferroni	Holm
Panel A: Study 1						
Above median income	Fin.inc.	0.0112	0.0003***	0.0003***	0.0033***	0.0033***
Above median income	SN:inc:fear	0.0030	0.0533*	0.2483	0.5333	0.3200
Above median income	SN:inc:hope	0.0030	0.0520*	0.2697	0.5200	0.3640
Above median income	SN:sav:fear	0.0026	0.0980*	0.3150	0.9800	0.3920
Above median income	SN:sav:hope	0.0011	0.4820	0.7323	1.0000	0.9640
Below median income	Fin.inc.	0.0106	0.0003***	0.0003***	0.0033***	0.0030***
Below median income	SN:inc:fear	0.0014	0.3513	0.7050	1.0000	1.0000
Below median income	SN:inc:hope	0.0025	0.0790*	0.3017	0.7900	0.3950
Below median income	SN:sav:fear	0.0031	0.0240**	0.1467	0.2400	0.1920
Below median income	SN:sav:hope	0.0005	0.7360	0.7600	1.0000	0.7600
Panel B: Study 2						
Above median income	200*€10	0.0162	0.0003***	0.0003***	0.0033***	0.0013***
Above median income	100*€20	0.0195	0.0003***	0.0003***	0.0033***	0.0017***
Above median income	100*€10+€1,000	0.0232	0.0003***	0.0003***	0.0033***	0.0003***
Above median income	4*€500	0.0331	0.0003***	0.0003***	0.0033***	0.0030***
Above median income	2*€1,000	0.0391	0.0003***	0.0003***	0.0033***	0.0027***
Below median income	200*€10	0.0146	0.0003***	0.0003***	0.0033***	0.0007***
Below median income	100*€20	0.0227	0.0003***	0.0003***	0.0033***	0.0033***
Below median income	100*€10+€1,000	0.0251	0.0003***	0.0003***	0.0033***	0.0010***
Below median income	4*€500	0.0365	0.0003***	0.0003***	0.0033***	0.0023***
Below median income	2*€1,000	0.0338	0.0003***	0.0003***	0.0033***	0.0020***

Appendix G. Further Graphs
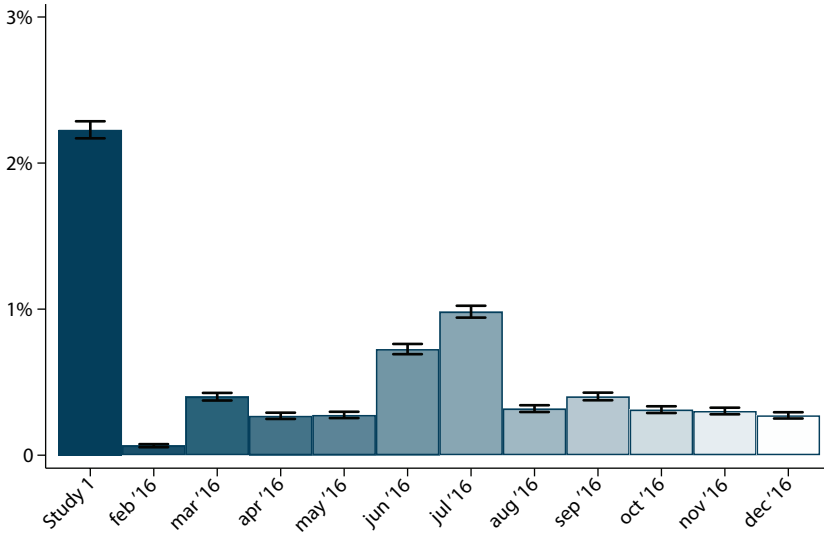
Figure 2.A.10. Percentage of respondents who visited their personal pension planner (per month), Study 1 and 11 months after.
The error bars represent the 95% confidence intervals. Study 1 covers the period of 22 December 2015 until 1 February 2016.
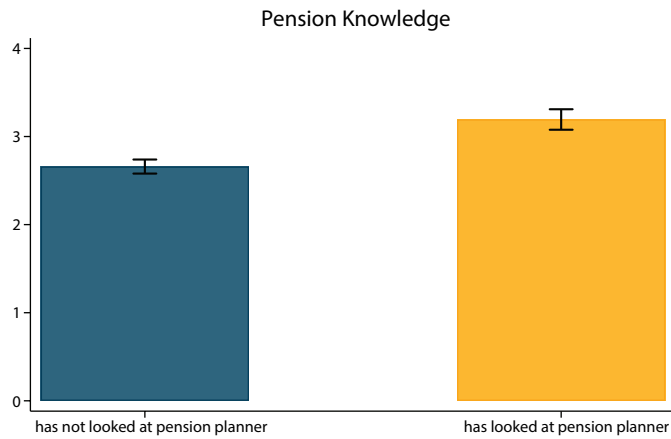
Figure 2.A.11. Number of correct questions by whether participants looked at the pension planner, Study 2.
The quiz questions can be found in Appendix 2.10. The error bars represent the 95% confidence intervals.

Appendix H. Further Regressions

Table 2.A.12: The Effect of Treatments on Survey Initiation and Completion (Study 2)

This table shows the OLS regressions with the dependent variables *Initiated Survey* (binary variable) and *Answered Pension Knowledge Questions*. *200*€10, 100*€20, 100*€10+€1,000, 4*€500*, and *2*€1,000* indicate which letter a participant received. The control letter is the baseline. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile rows indicate to which income quintile to which the participant belongs. The first income quintile is the baseline category. *Part-timer* indicates the participants who worked less than full-time. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group. Robust standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05

	Initiated Survey (1)	Answered Pension Knowledge Questions (2)
200*€10	0.001	0.001**
	(0.001)	(0.001)
100*€20	0.001	0.001*
	(0.001)	(0.001)
100*€10+€1,000	0.002**	0.001
	(0.001)	(0.001)
4*€500	0.003***	0.002***
	(0.001)	(0.001)
2*€1,000	0.001	0.000
	(0.001)	(0.001)
Male	-0.001	0.001**
	(0.001)	(0.000)
Age: 30-39	0.000	0.001**
	(0.001)	(0.000)
Age: 40-49	0.003***	0.002***
	(0.001)	(0.000)
Age: 50-59	0.019***	0.011***
	(0.001)	(0.001)
Age: 60-66	0.050***	0.026***
	(0.002)	(0.002)
Income: 2nd Quintile	-0.000	-0.000
	(0.001)	(0.001)
Income: 3rd Quintile	0.001	0.000
	(0.001)	(0.001)
Income: 4th Quintile	0.003**	0.002**
	(0.001)	(0.001)
Income: Highest Quintile	0.003***	0.003***
	(0.001)	(0.001)
Part-timer	0.000	0.001
	(0.001)	(0.001)
Partner	0.002***	0.001*
	(0.001)	(0.000)
Constant	0.005***	0.001
	(0.001)	(0.001)
Observations	257,433	257,433
Adj R-Squared	0.010	0.005
Province Dummies	YES	YES
Baseline Mean	0.016	0.008

Table 2.A.13: IV Regression: Pension Planner and Alternative Pension Knowledge Measure (Study 2)

This table shows coefficients of the two-stage least squares instrumental variable regression to analyze whether looking at the planner increases the likelihood to answer correctly to questions specifically about the pension planner. The first stage is shown in Table 2.6, Column (1). *Pension Planner* is instrumented by the treatment letters. The second stage is a regression on *Alternative Pension Knowledge*, measured by a pension quiz score of only the two questions about the pension planner (i.e., the first two questions). *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile levels indicate the income quintile to which the participant belongs. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the dependent variable for participants who have not looked at the pension planner. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

	Alternative Pension Knowledge
Pension Planner	0.231
	(0.309)
Male	0.041
	(0.043)
Age: 30-39	0.081
	(0.071)
Age: 40-49	0.217*
	(0.083)
Age: 50-59	0.372***
	(0.078)
Age: 60-66	0.625***
	(0.080)
Income: 2nd Quintile	0.046
	(0.064)
Income: 3rd Quintile	0.102
	(0.066)
Income: 4th Quintile	0.145*
	(0.066)
Income: Highest Quintile	0.216**
	(0.068)
Part-timer	-0.058
	(0.053)
Partner	0.016
	(0.042)
Constant	-0.065
	(0.151)
Observations	2,231
Province Dummies	YES
Baseline Mean	0.270

Table 2.A.14: IV Regression: Pension Planner and Pension Knowledge as Simple Sum (Study 2)

This table shows coefficients of the two-stage least squares instrumental variable regression. The first stage is shown in Table 2.6, Column (1). *Pension Planner* is instrumented by the treatment letters. The second stage is a regression on *Pension Knowledge as Sum*, measured by the number of correct answers in a pension quiz score of six questions. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile levels indicate the quintile to which the participant belongs. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the dependent variable for participants who have not looked at the pension planner. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	Pension Knowledge as Sum
Pension Planner	0.271
	(0.527)
Male	0.238**
	(0.075)
Age: 30-39	0.241
	(0.135)
Age: 40-49	0.554***
	(0.146)
Age: 50-59	1.013***
	(0.138)
Age: 60-66	1.636***
	(0.135)
Income: 2nd Quintile	0.160
	(0.116)
Income: 3rd Quintile	0.133
	(0.118)
Income: 4th Quintile	0.205
	(0.121)
Income: Highest Quintile	0.522***
	(0.120)
Part-timer	0.013
	(0.094)
Partner	0.126
	(0.073)
Constant	1.538***
	(0.256)
Observations	2,231
Province Dummies	YES
Baseline Mean	0.270

Table 2.A.15: Marginal Effects from a Recursive Simultaneous Bivariate Probit Regressions

This table shows the marginal effects from a bivariate probit regression. In the first regression, we regress *Pension Planner* on the treatment dummies and the covariates from Table 2.4. In the second regression, we regress *Saved More* on *Pension Planner* and the covariates from Table 2.4. *Pension Planner* indicates whether the participant looked at the pension planner. *Saved More* is equal to 1 if the participant has saved more in the three weeks before the survey, and 0 otherwise. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile levels indicate the quintile to which the participant belongs. *Part-timer* indicates the participants who worked less than a full-time position but more than 0 hours a month. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the dependent variable for respondents who have not looked at the pension planner. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

	Saved More
Pension Planner	-0.023
	(0.028)
Male	0.009
	(0.008)
Age: 30-39	-0.019
	(0.017)
Age: 40-49	-0.025
	(0.018)
Age: 50-59	-0.031
	(0.020)
Age: 60-66	-0.017
	(0.016)
Income: 2nd Quintile	-0.000
	(0.009)
Income: 3rd Quintile	0.007
	(0.010)
Income: 4th Quintile	0.005
	(0.010)
Income: Highest Quintile	0.014
	(0.012)
Part-timer	0.011
	(0.008)
Partner	0.002
	(0.006)
200*€10	0.005*
	(0.002)
100*€20	0.008*
	(0.003)
100*€10+b,1,000	0.007*
	(0.003)
4*€500	0.010**
	(0.003)
2*€1,000	0.008**
	(0.003)
Observations	2,231
Province Dummies	YES
Baseline Mean	0.031

Table 2.A.16: Fraction of Participants in Financial-Incentive Treatments per Employer (Study 1 and Study 2)

This table shows the coefficients from the OLS regressions with the dependent variable *Pension Planner* (binary variable). *200*€10, 100*€20, 100*€10+€1,000, 4*€500, 2*€1,000* and *Financial incentive in Study 1* indicate which letter a participant received. The control letter is the baseline. We control for the fraction of fund members who received a treatment letter per employer. We control for the covariates from Table 2.4. Baseline Mean shows the mean of the respective dependent variable for participants in the baseline group. Robust standard errors are in parentheses. *** $p<0.001$, ** $p<0.01$, * $p<0.05$

	Pension Planner	
	Study 1	Study 2
	(1)	(2)
Financial incentive in Study 1	0.010***	
	(0.001)	
Fraction of fin. inc. letters per employer	0.003	
	(0.003)	
200*€10		0.014***
		(0.002)
100*€20		0.020***
		(0.002)
100*€10+€1,000		0.024***
		(0.002)
4*€500		0.034***
		(0.002)
2*€1,000		0.037***
		(0.002)
Fraction of 200*€10 letters per employer		0.009
		(0.005)
Fraction of 100*€20 letters per employer		0.004
		(0.005)
Fraction of 100*€10+€1,000 letters per employer		0.002
		(0.005)
Fraction of 4*€500 letters per employer		0.005
		(0.005)
Fraction of 2*€1,000 letters per employer		-0.003
		(0.005)
Constant		0.031***
		(0.004)
Observations	226,946	257,433
Adj. R-Squared	0.013	0.010
Other Treatment Dummies	YES	
Other Covariates	YES	YES
Baseline Mean	0.022	0.043

Table 2.A.17: Interaction Effects Between Study 1 and Study 2

This table shows the coefficients of OLS regressions with the dependent variable *Pension Planner* (binary variable). *200*€10, 100*€20, 100*€10+€1,000, 4*€500*, and *2*€1,000* indicate which letter a participant received in Study 2. The control letter is the baseline. *Not a member in Study 1* indicates whether the participant was not pension fund member during Study 1. *Fin. inc in Study 1* indicates the participant received the financial-incentive letter in Study 1. Participants who have received either the control letter or a peer-information letter are the baseline. *Male* indicates the gender of the participant; women are the baseline. *Age: 30-39, Age: 40-49, Age: 50-59*, and *Age: 60-66* are levels indicating the participant's age group. Participants aged below 30 are the baseline category. The income-quintile rows indicate the income quintile to which the participant belongs. The first income quintile is the baseline category. *Part-timer* indicates the participants who worked less than a full-time position. Full-timers are the baseline category. *Partner* indicates whether the member had a partner registered with the pension fund. We control for province dummies. Baseline Mean shows the mean of the dependent variable for participants in the baseline group. Robust standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05

	Pension Planner in Study 2	
	(1)	(2)
200*€10	0.015***	0.013***
	(0.001)	(0.002)
100*€20	0.021***	0.017***
	(0.002)	(0.002)
100*€10+€1,000	0.024***	0.024***
	(0.002)	(0.002)
4*€500	0.035***	0.034***
	(0.002)	(0.002)
2*€1,000	0.037***	0.035***
	(0.002)	(0.002)
Not a member in Study 1	0.006***	0.004
	(0.001)	(0.002)
Fin. inc. in Study 1	0.001	-0.003
	(0.002)	(0.003)

Not a member in Study 1 x 200*€10		0.003
		(0.003)
Not a member in Study 1 x 100*€20		0.006*
		(0.003)
Not a member in Study 1 x 100*€10+€1,000		-0.000
		(0.003)
Not a member in Study 1 x 4*€500		0.002
		(0.003)
Not a member in Study 1 x 2*€1,000		0.003
		(0.003)
Fin. inc. in Study 1 x 200*€10		0.011
		(0.005)
Fin. inc. in Study 1 x 100*€20		0.013*
		(0.006)
Fin. inc. in Study 1 x 100*€10+€1,000		0.003
		(0.006)
Fin. inc. in Study 1 x 4*€500		0.002
		(0.006)
Fin. inc. in Study 1 x 2*€1,000		0.000
		(0.006)
Male	0.005***	0.005***
	(0.001)	(0.001)
Age: 30-39	-0.018***	-0.018***
	(0.001)	(0.001)
Age: 40-49	-0.019***	-0.019***
	(0.001)	(0.001)
Age: 50-59	0.007***	0.007***
	(0.002)	(0.002)
Age: 60-66	0.072***	0.072***
	(0.003)	(0.003)
Income: 2nd Quintile	-0.005**	-0.005**

	(0.002)	(0.002)
Income: 3rd Quintile	-0.008***	-0.008***
	(0.002)	(0.002)
Income: 4th Quintile	-0.006**	-0.006**
	(0.002)	(0.002)
Income: Highest Quintile	0.004*	0.004*
	(0.002)	(0.002)
Part-timer	0.007***	0.007***
	(0.001)	(0.001)
Partner	0.009***	0.009***
	(0.001)	(0.001)
Constant	0.030***	0.031***
	(0.003)	(0.003)
Observations	257,433	257,433
R-squared	0.010	0.010
Other Covariates	YES	YES
Baseline Mean	0.043	

Chapter 3

Impact or Responsibility? Giving Behavior in a Televised Natural Experiment[*]

3.1 Introduction

In 2020, US American individuals donated $324.1 billion to charity, representing 1.6% of the nation's GDP (Giving USA, 2021). Individuals have an enormous choice set to choose from when selecting a beneficiary. 1.54 million charitable organizations are registered with the IRS. In addition, donation-based crowdfunding platforms offer a large number of projects (f.ex., as of 26 October 2022, Global Giving has provided 33,038 projects a platform since 2002 (GlobalGiving, n.d.)). How do people choose between these options?

Previous studies have mostly answered the question of why people give money to charities or crowdfunding platforms. They find that the decision to donate off-line can be based on drivers such as altruism, reciprocity, kinship, affect, and impact (Aknin, Dunn, Whillans, Grant, and Norton, 2013; Andreoni, 1990; Berman, Barasch, Levine, and Small, 2018; Karlan and Wood, 2017; Lindauer,

3. IMPACT OR RESPONSIBILITY?

Mayorga, Greene, Slovic, Västfjäll, and Singer, 2020; List and Lucking-Reiley, 2002; Ottoni-Wilhelm, Vesterlund, and Xie, 2017; Small and Loewenstein, 2003). Next to these reasons to give, platform- and project-specific factors have been analyzed for donations through crowdfunding platforms. Reasons to give online are being part of a community, having a closer tie with the beneficiary, ease of use, and credibility of the platform and the beneficiary (Choy and Schlagwein, 2016; Liu, Suh, and Wagner, 2018; Salido-Andres, Rey-Garcia, Alvarez-Gonzalez, and Vazquez-Casielles, 2021).

In this study we look at how people choose between beneficiaries once they have made the decision to donate to any beneficiary. We use data from a televised natural experiment where people in need request money from people in the audience, to examine why people prefer to give to one cause over another. We analyze how people weight two fundamental considerations against each other: giving for impact and giving for equity and desert (personal responsibility). To illustrate, imagine two people who ask for money. Person A is a young person from an abusive home who wants to study. They are highly indebted. Because of previous arrestations and unpaid fines from the government, they cannot start a debt restructuring program and could go to jail if they cannot pay. Receiving money would have a high impact, as it could change a life with time in prison to a potentially high-income life. However, they are also responsible for their debt. Person B asks for money to pay for a special colon cancer treatment for their mother. Receiving money would have a lower impact, given that success of the treatment is unsure and the mother is already considerably older than Person A (56 vs 19 years old). However, Person B is not responsible for the situation. A donor driven by impact might give to Person A as they deem that the money increases well-being of the person and others most. A donor driven by considerations of equity and desert thinks about Person A's responsibility for their situation and thus gives to Person B, who is not responsible for the sickness. Generally, if people give for impact, the most effective charities that also advertise their effectiveness will receive most money and societal benefits will be highest.

However, if people give due to considerations of equity and desert, fundraising needs to address these concerns to attract donations.

People who give money to a cause because doing so generates the highest impact think in line with the school of consequentialism in moral philosophy. The most prominent consequentialist theory is utilitarianism, formulated by thinkers such as Bentham (1789) and John Stuart Mill (2015). It prescribes that "one should act so as to produce the greatest possible balance of good over bad" (Konow, 2003, p.1200). Utilitarian thinking is important in economics, with welfare economists seeking to maximize the utility of society. In the giving domain, the growing Effective Altruism movement is based on utilitarian thinking, which aims to ensure that donations flow to charities that create the highest impact per dollar (MacAskill, 2015; The Life You Can Save, n.d.). As of February 2022, effective altruists have donated about $264 million and have pledged to donate nearly $3billion; the 280 groups spread across the world make it a global movement (Effective Altruism Hub, n.d.; Giving What We Can, n.d.). Many charities also aim to raise funds by showing their impact and efficiency through ratings by initiatives such as the Charity Navigator.

In contrast to utilitarian thinking, people might give to causes because they deem the beneficiaries as deserving and worthy of their help. We call these "equity and desert deliberations", in line with Konow's (2003) wording. Theories about equity and desert consider the personal responsibility of people and its effect on fair allocations. This class of theories originates from Aristotle's distributive justice theory and John Locke's natural law/desert theory (Konow, 2003). They imply that views on fairness are often affected by whether the person involved is either in their situation because of luck or factors under their control (such as effort) (Cappelen, Hole, Sørensen, and Tungodden, 2007; Cappelen, Konow, Sørensen, and Tungodden, 2013; Konow, 2001, 2003, 2009, 2012). For giving behavior, the predictions from these theories are in line with empirical findings of preferences for redistribution. People who think that the poor are responsible for their situation are less willing to help them than people who think that the poor are not responsible for their situation (Cohn, Jessen, Klasnja, and Smeets,

3. IMPACT OR RESPONSIBILITY?

2021; Fong, 2001; Osborne and Weiner, 2015). Studies analyzing crowdfunders' campaign descriptions found that many crowdfunders try to increase donations through storytelling. They present beneficiaries as worthy and deserving of help by describing them as hard-working and members of communities, and the causes of their need as not within the beneficiaries' control and due to luck (Berliner and Kenworthy, 2017; Body and Breeze, 2016; Paulus and Roberts, 2018).

We study giving behavior in a Dutch television show to assess the relative importance of impact and personal responsibility considerations. In each episode, three candidates each describe their situation and the reasons why they ask for an amount of money up to €10,000 (i.e., \$11,743). 100 people sit in the audience and are allocated with €100 (i.e., \$117). At the end of the show, each audience member decides to which candidate she gives her €100. Complementary to both conventional laboratory experiments and field research, this semi-controlled real-world setting allows the study of giving behavior in a well-defined choice task where choices affect the wellbeing of real recipients facing real financial problems.[2] Would we have run this giving experiment ourselves, the total cost of donations to candidates alone would have been more than half a million euros.

We ask raters to watch short video clips of the show, and to assess the potential impact of donating to a candidate and the degree to which a candidate is responsible for her situation. For impact deliberations, we consider how wellbeing would be affected by a gift of €100 (the marginal contribution of a single audience member) and by a gift of €10,000 (the total amount to be divided). For equity and desert deliberations, we employ a modified version of the Revised Causal Dimension Scale (CDSII), developed by McAuley, Duncan, and Russell

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[2]Our study connects to a broader literature that uses TV shows to investigate individual economic decision making. Such an approach has been used to study, for example, decision making under risk (Beetsma and Schotman, 2001; Gertner, 1993; Jetter and Walker, 2018; Metrick, 1995; Post, Van den Assem, Baltussen, and Thaler, 2008), strategic decision making (Bennett and Hickman, 1993; Berk, Hughson, and Vandezande, 1996; Tenorio and Cason, 2002), discrimination (Antonovics, Arcidiacono, and Walsh, 2005; Levitt, 2004), cooperative behavior (Belot, Bhaskar, and van de Ven, 2010; List, 2004, 2006; Turmunkh, Van den Assem, and Van Dolder, 2019; Van den Assem, Van Dolder, and Thaler, 2012), bargaining (van Dolder, van den Assem, Camerer, and Thaler, 2015), and competitiveness (Antonovics, Arcidiacono, and Walsh, 2009; Buser, van den Assem, and van Dolder, 2023; Hogarth, Karelaia, and Trujillo, 2012).

(1992) as a way to measure how individuals perceive causes. Personal control is one of the four dimensions of this scale. If the cause of a negative event is under the person's control, then that person is perceived as responsible for the situation and not deserving of help (Weiner, 1995, 2000). Various studies have indeed found that such judgments of responsibility determine help giving and co-operative behavior (Gurevich and Kliger, 2013; Rudolph, Roesch, Greitemeyer, and Weiner, 2004; Tscharaktschiew and Rudolph, 2016; Weiner, 1995, 2006).

We find that both the candidate's personal responsibility and the gift's impact influence giving-behavior. Impact is positively correlated with the amount received by a candidate. It explains 50% of the variation explained in our main model. Next to utilitarianism, people also follow the theories of equity and desert, as they give less money to candidates with more personal control over their situation's cause. Personal control explains 30% of the variation explained. We find no evidence for discrimination based on age, gender, and attractiveness.

Our study contributes to the literature on giving behavior in three ways. First, our finding differs from findings of previous studies that conclude that impact considerations do not matter for giving behavior. For example, Berman, Barasch, Levine, and Small (2018) find that information about effectiveness of different charities does not affect participants' choice to donate, but an emotional connection to a charity does. The Giving USA 2021 report demonstrates that, only 5% of all US donations in 2020 are donated to international affairs, even though many Effective Altruism websites argue that giving internationally is the most good you can do per dollar (Effective Altruism, 2013; Giving USA, 2021; The Life You Can Save, n.d.). Three factors can explain why our findings differ from those of previous literature. First, the cases of the individual candidates are shown in video clips and discussed by the jury. This process makes the candidate's situation concrete and describes the situation and the potential impact of giving vividly. It is in line with Aknin, Dunn, Whillans, Grant, and Norton (2013) that impact is more important in a setting in which it is better imaginable than in studies making use of more abstract marketing solicitations via letters or quality certificates (as in Caviola, Schubert, and Nemirow (2020); Karlan and Wood

(2017)). As the TV show setting resembles donating via a crowdfunding platform more than donating to a charity, this implies that crowdfunding choices might be motivated more by impact than giving to charities. A second factor is that all giving in the TV show is targeted towards beneficiaries in financial need. This is different in giving to charities, where people often support subjective causes they like, e.g. arts and sports (Berman, Barasch, Levine, and Small, 2018). A third reason for the difference could be that audience members feel they need to give the money to the candidate for which it has the most impact. The title of the show, translating to "Money Buys Happiness", could nudge the audience to focus on increasing the candidate's well-being rather than on desert. If this nudge works, our finding would be in line with Lindauer, Mayorga, Greene, Slovic, Västfjäll, and Singer (2020) who show that a philosophical argument against preferring helping close beneficiaries to distant ones is just as effective as an emotional appeal in increasing charitable donations.

Second, we contribute by examining giving behavior in a setting with high stakes and real payouts in a study whose findings are not driven by common method bias (Campbell and Fiske, 1959; Podsakoff, MacKenzie, and Podsakoff, 2012). With the audience's giving behavior and the raters' scores, our data stems from two different sources. The link between the raters' scores and the audience's giving behavior cannot be explained by the audience trying to justify their behavior or by the raters being influenced by the audience's giving decisions. Furthermore, most studies use vignettes to test predictions about equity and desert (e.g., Konow (2001, 2003, 2009); Weiner (1980b, 2006). An exception is Gurevich and Kliger (2013)who use the TV show "The Manipulation" to examine cooperative behavior in a strategic setting with interactions between the players. Our setting, however, is purely non-strategic as the audience does not directly interact with the beneficiaries.

Third, we examine the influence of impact and responsibility considerations in the same giving context. The motives for selecting one cause over the other are difficult to observe in the field. Participants in more controlled experiments might prefer giving to a charitable goal that the experimenter cannot observe, which

can lead to false conclusions about giving decisions and behavior. For example, instead of giving during the experiment, participants could save their earnings to give to their preferred cause. The television show determines the choice set in our study. Our results show that people do consider the impact of their gift when choosing whom to give to, but personal control by the beneficiary remains important.

The paper proceeds as follows. Section 3.2 discusses impact and the beneficiary's personal responsibility as two major ethical considerations people may be concerned with when deciding to give to one cause and not another. Section 3.3 describes the TV show and our data. In Section 3.4, we present our analyses and results. We conduct robustness analyses in Section 3.5. Section 3.6 discusses and concludes.

3.2   Background of the Study

This section outlines the two ethical considerations we distinguish between. First, we describe utilitarianism, a school of moral philosophy. We illustrate how this school is still relevant for altruism today. In the second subsection, we discuss theories of equity and desert, and document findings of empirical research into the role of these considerations in the fields of charitable giving and helping behavior.

3.2.1   Impact and Utilitarianism

According to utilitarianism, a deed that leads to higher well-being is preferred to a deed that leads to lower well-being. Utilitarians aim to maximize the well-being of all living beings, including humankind, animals, and plants (Bentham, 1789; de Lazari-Radek and Singer, 2017; Mill, 2015). Utilitarianism belongs to the consequentialist strand of moral philosophy. Consequentialists think that the outcome of an action is most important, not how the outcome was achieved. An underlying assumption of utilitarianism is that well-being is quantifiable. It is an idea that is also present in welfare economics, where the utility of goods and services is measured in utils. Effective altruists want to maximize the well-being

of all by giving to charities that are proven to be most effective. The aim is to create as much impact as possible per dollar (Ng and Singer, 1981).

Do people adhere to utilitarianism in the field of help-giving and donations? Many studies find mixed evidence on the influence of impact considerations on giving decisions. They utilize that utilitarian donors should give to charities that they know the effectiveness of. According to Karlan and Wood (2017), only donors who have given a large amount of money before donate more when they have information on effectiveness of the charity, compared to when that information is missing. Donors who have previously given small amounts give less with effectiveness information. Similarly, Yörük (2016) presents evidence that quality ratings only increase donations for small charities, but not for larger charities.

Adena, Alizade, Bohner, Harke, and Mesters (2019) conduct a classroom experiment and find that participants give more to a charity when they know of a quality certificate than participants who do not know of that certificate. However, this effect is only significant for the first time people make the giving decision. In the second round, participants who get to know about the quality certificate do not increase contributions to the charity anymore. The results suggest that new donors are prone to look for impact maximization, but previous donors are anchored on their previous donation decisions.

A smaller strand of the literature concludes that (perceived) impact is an important factor in people's decision-making. Hannikainen, Machery, and Cushman (2018) find that younger generations think more utilitarian than older generations when facing hypothetical dilemmas. Aknin, Dunn, Whillans, Grant, and Norton (2013) find that donors who spent money on a specific goal express higher well-being than donors who gave to a charity that did not state specifically for what money is used. Grant, Campbell, Chen, Cottone, Lapedis, and Lee (2007) find that face-to-face contact with beneficiaries increases crowdfunding effort because the perceived impact of the effort is higher. For fundraising via crowdfunding, Kuppuswamy and Bayus (2017) demonstrate that people are more likely to give when the perceived impact is higher. This is the case when the campaign nearly

raised the required money, is close to its deadline, or has a small (interpreted as reachable) goal amount.

A study by Gneezy, Keenan, and Gneezy (2014) shows that people want to know that the money they gave reached the beneficiaries of the charity. The authors frame the coverage of overhead costs in two different ways. One treatment group is told that the overhead costs have been paid for by another donor. The other treatment group is told that the same amount of money is used as seed money. Compared to the other treatment group, the treatment group where overhead costs had been paid for by another donor were 80% more likely to give and donated 75% more money in total. Donors thus dislike overhead costs, even though these are necessary to ensure that the charity continues to receive donations and operates efficiently. Gneezy, Keenan, and Gneezy (2014) conclude that perceived personal impact drives giving behavior; however, given most people's incorrect perceptions about overhead costs, this leads to giving behavior that is not actually driven by real impact. Caviola, Faulmüller, Everett, Savulescu, and Kahane (2014) name this phenomenon "evaluation bias". People use overhead costs as a proxy to evaluate charities' effectiveness because it is often easier to evaluate than charities' actual effectiveness. However, when people choose between two charities at the same time, cost effectiveness (defined as greater number of saved lives per dollar) becomes the driving factor of donation behavior.

Caviola, Schubert, and Nemirow (2020) find that lay donors often do not give effectively because of misconceptions around the effectiveness of charities and because they do not know the most effective charities. Once study participants unlearn these misconceptions and learn about effective charities, effective donations are considerably higher. The authors find a group of self-reported effective altruists who donate to maximize impact, but also a group of donors who continue to give ineffectively after receiving information.

The latter finding is in line with the several studies showing that giving behavior is often driven by warm glow, a good feeling one feels because one has donated time or money to a good cause (Andreoni, 1990). Null (2011) learns that warm glow prevents people to give in a utilitarian way as giving to many

charities brings more warm glow than giving to only one. In Null's experiment, participants donate to several charities that have the same goal, but different levels of effectiveness. Participants thereby give up social benefit by giving to a cause that is not the most effective. Ottoni-Wilhelm, Vesterlund, and Xie (2017) show further evidence for the predominance of impure altruism, meaning that donors receive a private benefit from giving (i.e., warm glow). For pure altruists, the source of the money does not matter for the increase in the altruist's well-being due to the donation. For impure altruists, the increase of well-being due to a gift is higher when the altruist herself donated than when the donation stems from another donor.

Berman, Barasch, Levine, and Small (2018) present evidence that people focus more on personal values and emotional connections towards charities than on effectiveness information when they give as a private person. Yet, people are perceived as more altruistic when they donate more to the effective charity. People also give more to the effective charity when they pose as an agent of an organization and must make a donation decision. In these lab and online experiments, participants do not adhere to utilitarianism when making private giving decisions but seem to generally think that impact maximization is altruistic and more important in a non-private setting.

Other factors that can be related to warm glow giving are the following. First, people give more to causes when they have been primed with the identity of a previous donor or as members of a community (Kessler and Milkman, 2018). Second, people are more likely to give to a cause that they are familiar with because it has played a role within their direct social environment earlier (Small and Simonsohn, 2008). Another factor is the so-called identifiable victim effect: people give and help more when the beneficiary ("victim") is identified than when they are to be identified from a selected list of beneficiaries (Genevsky, Västfjäll, Slovic, and Knutson, 2013; Small and Loewenstein, 2003). This means that donors do not value lives or needs equally, but prefer identified beneficiaries to statistical beneficiaries. Small, Loewenstein, and Slovic (2007) show that teaching people about this effect decreases giving to identifiable victims, but does not increase

giving to statistical victims. More recently, however, Lindauer, Mayorga, Greene, Slovic, Västfjäll, and Singer (2020) find that a rational appeal arguing for caring for nearby individuals as much as for distant ones increases donations as much as an emotional appeal based on an identifiable child in need. Another factor leading people to give ineffectively is scope insensitivity. On average, students do not give more to a charity that saves four bald eagles than to a charity that saves one bald eagle (Hsee and Rottenstreich, 2004). Hasford, Farmer, and Waites (2015) replicate these findings and show that scope insensitivity is related to emotional intelligence. The lower people score on emotional intelligence, the more scope sensitive they are. Last but not least, people give because of self-interest. They expect to benefit from their gift in the future (Chuan, Kessler, and Milkman, 2018; Flynn, 2003; Flynn, Reagans, Amanatullah, and Ames, 2006; Hardy and Van Vugt, 2006; Perlow and Weeks, 2002).

In conclusion of this section, there is mixed evidence that donors give to maximize society's well-being. While donors like to see that their money has an impact on beneficiaries, many donors seem to give mainly in line with their personal values and/or to receive an additional "warm glow" from giving.

3.2.2 Personal Responsibility and Theories of Equity and Desert

A second line of thought of why people give to certain causes is related to questions of fairness and justice. Donations to charitable causes and beneficiaries can be seen as acts of redistribution from the wealthy to people in need. In the redistribution context, theories of equity and desert investigate which factors are important to restore justice (i.e., what determines desert) and how these factors determine just redistributions (i.e., equity) (Konow, 2003). While originally different groups of theories, Konow (2003) groups theories that examine equity considerations and theories that discuss desert together. The philosophical idea behind the theories of equity and desert stem from Aristotle's distributive justice theory and the natural law/desert theory of John Locke (Konow, 2003). In this group of theories individual actions matter for a fair distribution of money

and other resources. Specifically, the fairness of a distribution is judged by the level of each recipient's personal responsibility for her situation (Konow, 2001, 2003). Konow (2003) conflates the theories to an Equity Principle, which states that "fair allocations across individuals are proportionate only to the inputs they control" (p.1214).

In economics, the extent to which concerns about equity and desert influence decision-making have been tested in several studies. Konow (2001) conducts survey research with varying hypothetical scenarios. Survey participants must decide either whether a distribution of resources is fair or unfair, or what a fair distribution would look like. While Konow finds that people often judge based on whether the allocation between people is proportionate to the value of factors in people's control, participants sometimes think considerations of efficiency or basic needs to be more important. Cappelen, Konow, Sørensen, and Tungodden (2013) demonstrate that most participants in their experiment favor redistribution of resources with desert in mind. The experiment had two phases. In the first phase, stakeholders chose between two alternatives. With the risky alternative, the participants had an equal chance to win money or to win nothing. The safe alternative varied in value over four settings; the highest value was equal to the expected value of the risky alternative, other values were lower. After the first phase, stakeholders were paired together, and the outcome of the choices were made available to the stakeholders and the spectators. Stakeholders then had the chance to re-distribute. Spectators also made choices about how a fair distribution would look like between four pairs of stakeholders. When participants re-distributed, they distinguish between inequalities that are caused by luck or by choice. Trhal and Radermacher (2009) also use a two-phased experiment. In one treatment group participants are equally likely to receive zero money, while in the other treatment group, participants can choose between a secure payment and a lottery. Participants are randomly grouped together and select how much money they would give to losers in their group. On average, participants distribute less money to losers who are responsible through choosing the lottery than to losers who did not have the chance to take the safe alternative.

Some experimental studies also conclude that participants do not act in line with the predictions from the theories of equity and desert. Buitrago, Gueth, and Levati (2009) find that participants who are rich in the experiment, do not give less help to poor participants, even when the rich can verify whether the poor person is in her situation because of less effort or bad luck. This contradicts equity and desert considerations. Mollerstrom, Reme, and Sørensen (2015) use a similar experiment as Cappelen, Konow, Sørensen, and Tungodden (2013), where a spectator can redistribute outcomes that generate from bad, unavoidable luck and bad, avoidable luck. They find that many spectators redistribute in a way that even bad, unavoidable luck is not compensated when the agent chose to risk bad, avoidable luck. This finding also runs counter to the predictions of equity and desert theories.

A prominent theory within the theories of equity and desert is Attribution Theory (AT), a theory from social psychology that is categorized within the theories of equity and desert by Konow (2003). Attribution Theory stems from social psychology and generally theorizes that people try to explain other people's behavior by attributing causes to it. In the help-giving domain, people deduce causes of situations and attribute different levels of responsibility to agents (e.g., Heider (1958); Rotter (1966); Weiner and Kukla (1970). It is in line with the idea that actions of the person in need matter extensively for the decision to help that person. If a potential helper is asked for help by the person in need, she will get to know the cause of the person's situation. The potential helper then attributes dimensions to that cause. Generally, the dimensions used are personal control, that is the extent to which the situation's cause is controllable by the person in need; external control, that is the extent to which the situation's cause is controllable by someone or something other than the person in need; locus of causality, that is the extent to which the cause is internal to the person in need; and stability, that is the extent to which the cause is constant in time.[3] As

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[3]To illustrate the difference between the first three dimensions, consider: When the cause of having a good grade in a group project can be attributed to motivation, this can rank high on internal locus of causality, high on personal control, but also high on external control, when the other students' motivation was a reason for the student's motivation. When sickness is the

help-giving includes two people (a giver and a receiver), we use the interpersonal AT specified by Weiner (2000). A lack of controllability by the person in need leads to sympathy by the helper and, eventually, to help-giving. The opposite is assumed when the cause is controllable by the person in need. The potential helper is then angry at the person in need and will not help her. This is called the theory of responsibility (Weiner, 1995).

Most studies test the predictions of AT by letting survey participants read vignettes and then rate perceived controllability, sympathy, and anger towards the person in need, and the likelihood of helping (e.g., Betancourt (1990); Greitemeyer and Rudolph (2003); Reisenzein (1986); Schmidt and Weiner (1988); Weiner (1980a,b)). Reisenzein (1986), Betancourt (1990) and Greitemeyer and Rudolph (2003) use structural equation modelling and confirm that the thought-affect-action sequence, generally proposed for AT by Weiner (1985), fits their vignette survey data the best. In their meta-analysis, Rudolph, Roesch, Greitemeyer, and Weiner (2004) confirm the moderating effect of sympathy and anger for helping behavior. Osborne and Weiner (2015) examine the willingness to help the poor in the framework of AT. They categorize people into attributional profiles and find that those who think that poverty is due to causes of high personal control, high internal locus, moderate external control, and moderate stability are less willing to help the poor than people who think poverty is caused by factors with low internal locus, moderate stability, low personal control, and high other control. A study that analyses non-hypothetical data is Gurevich and Kliger (2013), who use AT to explain behavior in a TV game show called The Manipulation. In the final round of that show, participants play a one-shot, high-stakes Prisoner's Dilemma. Perceived controllability of the opponent affects the decisions to cooperate in this strategic setting.

In the crowdfunding literature, theories of equity and desert are related to how crowdfunders present the beneficiary and the cause for which people should donate to. Berliner and Kenworthy (2017) as well as Paulus and Roberts (2018) analyze how different campaigns deviate from the crowdfunding platforms' rec-

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cause of a bad mark, on the other hand, this can rank low on personal control, high on internal locus of causality, and low on external control.

ommended storytelling to include descriptions of the beneficiaries as worthy and the causes of financial need as unlucky. This is in line with Body and Breeze (2016) finding that causes that are perceived as worthy and deserving attract more money from donors than causes that are perceived as unworthy and undeserving.

In summary, there is some evidence that considerations of equity and desert drive help-giving and crowdfunding behavior, but some studies also find behavior that contradicts the theories. In the remainder, we look into the relative prevalence of impact considerations and considerations of equity and desert when donors choose between beneficiaries.

3.3 TV Show and Data

In this section we describe the TV show, explain how we obtained our data, and present summary statistics.

3.3.1 The TV Show

The TV show *Geld Maakt Gelukkig* ("Money Buys Happiness") was developed and produced by the Dutch media company Talpa. It aired on the Dutch channel SBS6 in 2014 and 2015 and ran for two seasons. The 20 episodes from the first season aired between July 7, 2014 and August 1, 2014. The second season aired in two parts. The first 20 episodes of the second season ran between January 5, 2015 and February 6, 2015. The last 15 episodes aired between July 6 and July 17, 2015. Sandra Schuurhof hosted the show in Season 1, and Dominique Rijpma van Hulst hosted the show in Season 2.

In each episode, three candidates ask for money to resolve some form of financial difficulty. The audience consists of one hundred people, who are each endowed with €100. Their task is to donate this amount to one of the three candidates. The audience thus distributes €10,000 in total. Based on impressions from watching the episodes, people in the audience seem to resemble a cross section of the general Dutch population in terms of gender, age, and ethnicity.

Each candidate's situation is presented in a short video clip. In this clip, which is shot at location, the candidate explains why she needs financial help and how much money she needs. After the clip, an expert panel of two people, lawyer Prem Radhakishun and financial coach Eef van Opdorp, question the candidate and discuss her situation in the studio. This procedure is repeated for all three candidates. At the end of the show, the audience allocates the money to the candidates by voting with a device attached to their seat.

The reasons why candidates ask for money are diverse. For example, candidates ask for money to pay for medical treatments that are not covered by health insurance, to rent a place to live, to buy a mobility-adapted car or to convert a car for wheelchair access, to go on a last vacation before they will die of a terminal illness, or to pay for their debts. In most cases the candidates themselves have financial problems, but in some cases, candidates ask for money for a beneficiary who is not the candidate and outside of the candidate's own family or household, such as sports, cultural, or societal associations. In 20 out of 165 cases, a pair of two people ask for money together. In that case, only one candidate is questioned by the expert panel.

For each episode, we collected data on the relevant observables in the show, such as the order in which candidates appear, the amount each candidate requests, and the amount each candidate receives. We also coded the gender of the candidate, whether the candidate is white or non-white, and whether the candidate represents another beneficiary than herself. In the cases where two people ask money for a cause together, we coded the gender and whiteness of the candidate who is questioned by the panel. If stated, we also collected the candidate's age.

3.3.2   Ratings

To analyze the extent to which impact and personal responsibility affect giving behavior, we need a measure of the impact on well-being that a gift to the candidate would have as well as a measure of the degree to which the cause is something over which the candidate has power. We rely on ratings to obtain these

measures as well as measures of candidates' attractiveness and age. We recruited 80 Dutch-speaking raters from the student population of Maastricht University in the Netherlands. Each rater participated in one of the eleven sessions that were held at the Behavioral and Experimental Economics Laboratory of Maastricht University. Raters were on average 19.9 years of age, 44 percent were female, and 85 percent were studying at the School of Business and Economics. Most raters declared that they had never seen the game show before (82%). A minority of 14 percent of the raters declared that they had seen one or more episodes before, and 4 percent was not sure.

In the lab, the raters first were welcomed by the organizer. The organizer then read the instructions to the raters (see appendices A and B for the wording of the welcome message and the instructions) and asked whether anything was unclear to the raters. No questions were asked in any session. Each rater drew a card to determine which cubicle the rater was allocated to. The raters then entered their individual cubicles and started the rating process on the computer. Raters watched one video clip per candidate. Each video clip was comprised of the segment in which the candidate's situation is shown and the questioning of the candidate by the expert panel. Raters could only continue to the next screen after the video had played for at least four minutes, the minimum length of a clip. After each video clip, the raters answered the questions relating to the candidate.

Each rater watched nine video clips from three episodes. The clips were on average 5 minutes and 23 seconds long (shortest: 4 minutes; longest: 11.5 minutes). Raters watched the three clips belonging to one episode in the same order as the audience in the studio. We semi-randomly allocated episodes to raters so that each episode was rated at least four times and each episode should be at least once the first, second, and third episode watched by a rater. With 80 raters each rating 9 of the 165 candidates, 105 candidates were rated four times and 60 candidates were rated five times. At the end of the questionnaire, the raters stated their own age, gender, and faculty at which they study. Once they answered all questions, they received a show-up fee of €13. After all sessions were completed, we raffled an amount of €100 among the participants. The winning

participant was contacted by e-mail and the money was transferred to her bank account.

Impact

We asked the raters two questions on the impact of a gift on the candidate's well-being. The questions differed in terms of the monetary amount of the gift. We asked these two questions as it was unclear ex-ante which amount is the most appropriate one. First, we asked them to rate the impact of a gift of €100. This is the actual amount each audience member decides upon. Second, we asked the raters to rate the impact of the maximum amount a candidate could receive and ask for the perceived impact of a gift of €10,000. Normally this maximum amount should resolve the financial problem for which the candidate asks money.

More specifically, we asked the following questions:

In general, gifts increase the well-being of the recipient and people who are indirectly involved. Assume that this impact can be measured, and that the overall impact of a gift is the sum of the increases in well-being for all people involved. Imagine a group of 100 people, consisting of the candidate and 99 people who are randomly drawn from the Dutch population. Assume that people can be ranked in terms of the overall impact from receiving a monetary gift.

- *What would the rank of the candidate be if those 100 people are ordered from lowest overall impact (1) to highest overall impact (100) of a gift of €100?*

- *What would the rank of the candidate be if the 100 people are ordered from lowest overall impact (1) to highest overall impact (100) of a gift of €10,000? (Note that this rank can be lower than, higher than, or equal to your previous answer where the gift was €100.)*

The wording of the impact question reflects utilitarian principles (Bentham, 1789; de Lazari-Radek and Singer, 2017; Mill, 2015): well-being can be quantified and aggregated across people, and the overall impact of a gift is not only the change in the candidate's well-being, but also the change in well-being of everyone involved. We used a percentile scale with the Dutch general population as the reference

group to contextualize and standardize the endpoints of the scale. We visualized the scale as a line by letting the raters use a slider to score impact.

Personal Responsibility

As outlined in Section 3.2.2, a beneficiary's personal responsibility for her situation is related with helping behavior (f.ex., Rudolph, Roesch, Greitemeyer, and Weiner (2004); Weiner (1980b)). We measure personal responsibility by means of the Personal Control dimension of AT, one of the core theories of the theories of equity and desert. We used the CDSII by McAuley, Duncan, and Russell (1992) to obtain a measure of Personal Control. The CDSII has been frequently used and has been used by Osborne and Weiner (2015) to study willingness to support the poor.

In line with McAuley, Duncan and Russell's (1992) procedure, we first asked the raters to briefly describe why the candidate is in financial need. Raters thus needed to focus on and think about the cause of the situation in order to evaluate whether a beneficiary was personally responsible for the situation. Second, we asked to rate the situations' cause on the items of the CDSII. Raters gave their scores on a nine-point Likert scale. An example of an item for Personal Control is: "Is the cause, or are the causes, something over which the candidate has power (1) or over which the candidate has no power (9)".

The CSDII also measure three other dimensions of AT, Locus of Control, Stability, and External Control. Locus of Control measures the extent to which the cause is internal or external to the candidate, Stability the extent to which the causes are stable or unstable over time, and External Control the extent to which the cause can be controlled by a factor outside of the candidate. Each of the four dimensions is measured by three items. According to Reisenzein (1986), Weiner (2006), and Rudolph, Roesch, Greitemeyer, and Weiner (2004), Personal Control is the principal dimension that predicts helping behavior. For the sake of completeness and to control for possible correlations between the different explanatory variables, we include all four dimensions in our analyses.

Attractiveness and Age

133

3. IMPACT OR RESPONSIBILITY?

After the ratings of impact and personal responsibility, the raters assessed each candidate's age and attractiveness. We use the scale of Landry, Lange, List, Price, and Rupp (2006) to obtain a physical attractiveness measure. The wording of our request is: "Please rate the candidate's physical appearance on a scale from 1 to 10, where 1 means 'extremely unattractive' and 10 means 'model beautiful or handsome'."

Variable Standardization Procedures

We standardize the impact variables, the items of the CDSII, and Attractiveness because these variables and items are based on ordinal scales. Raters can interpret these differently, which can lead to different averages and variations in the variables. To facilitate interrater comparability, we use the same procedure as Landry, Lange, List, Price, and Rupp (2006). We obtain a standardized score SC_{ijs}^{ST} of questionnaire item s for each candidate i across different raters by generating $\text{SC}_{ijs}^{ST} = \frac{\text{SC}_{ijs} - \overline{\text{SC}_{js}}}{\sigma \text{SC}_{js}}$, where SC_{ijs} is the score given by rater j to candidate i on questionnaire item s, $\overline{\text{SC}_{js}}$ is the mean of all scores on item s by rater j across all candidates she rated, and σSC_{js} is the standard deviation of rater j's scores on item s. To obtain one value for each candidate for the impact variables and Attractiveness, we take the average of a given variable SC_{ijs}^{ST} across raters $(\overline{\text{SC}_{is}^{ST}} = \sum_{j=1} \text{SC}_{ijs}^{ST})$. To generate one value for each candidate for the AT dimension variables, we first take the average of a given CDSII-item SC_{ijs}^{ST} across raters. We then average the three items belonging to one AT dimension. For Age, we use the stated age in the TV show, when available. When a candidate's age was not stated, we take the median score the raters gave the candidate to replace the missing value. We standardize all independent variables (except for gender) so that they are distributed standard normal.

3.3.3   Summary Statistics

Table 3.1 shows summary statistics of the received and requested amounts, the main candidates' characteristics, and the obtained ratings. On average, candidates request €5,087 (median=€4,900), with a minimum of €1,200 and a maximum of €10,000. The amount any candidate received ranges from €0 to €8,300.

As the audience in each episode divides €10,000, the average amount received is €3,333. 28.5% of candidates received at least their requested amount.

Table 3.1: Summary Statistics

This table shows summary statistics of our data. For the variables based on ratings, we present median ratings on the candidate-level. Impact of €100 and Impact of €10,000 are measured on a percentile scale. Personal Control, Locus of Control, Stability, and External Control present the score for the dimensions that we obtained by averaging the dimension-specific three items scored on the CDSII-scale. The items were rated on a scale from 1 to 9. Attractiveness was measured on a scale from 1 to 10, where 1 means "extremely unattractive" and 10 means "model beautiful or handsome". Female is a dummy variable that takes the value one if the candidate is female. White is a dummy variable that takes the value one if the candidate is perceived as white.

	N	Mean	Median	St. Dev.	Min.	Max.
Amounts						
Amount Requested, in €	165	5,087	4,900	2,295	1,200	10,000
Amount Received, in €	165	3,333	3,200	1,767	0	8,300
Impact						
Impact of €100	165	35.63	34.00	17.96	7.00	83.00
Impact of €10,000	165	71.52	74.50	16.87	22.50	100.00
Attribution Dimensions						
Personal Control	165	4.89	5.00	1.75	1.00	8.33
External Control	165	4.95	5.00	1.42	1.50	8.33
Locus of Control	165	5.22	5.33	1.77	1.50	9.00
Stability	165	4.59	4.33	1.51	1.67	8.67
Candidate Characteristics						
Age	165	38.16	37.00	13.25	18.00	70.00
Attractiveness	165	5.17	5.00	1.49	2.00	9.00
Female	165	0.66	1.00	0.47	0.00	1.00
White	165	0.94	1.00	0.24	0.00	1.00

For all variables based upon raters' scores we show the summary statistics of the median for ease of interpretation. To obtain the candidate-level score on a variable, we take the median of the four or five scores a candidate received by the raters. For the medians of the variables stemming from the CDSII-scale, we first add up the values of the three items that correspond to one dimension and were given by one rater to the candidate, and then take the median of those sums. For the Impact of €100-variable, the mean is 35.6. The average candidate is thus situated in the 36th percentile of an improvement in well-being. The impact of

€100 on the candidate's situation is deemed to be lower than the Impact of the €10,000-variable (mean=71.5). This seems logical, as the average candidate asks for an amount 50 times as large as €100, and receiving the full requested amount supposedly solves the contestant's financial problem. Furthermore, candidates are arguably financially less capable than the average person in the wider Dutch population, implying that €100 has less than an average impact on the candidates' situations. In our analyses, we are not interested in the absolute impact levels, but in the relative impact between the candidates.

The means of the AT dimensions are all close to 5, the middle value of the used scale. The average score for Personal Control is 4.89, with a standard deviation of 1.75. The Cronbach's alphas of the standardized, averaged dimensions are 0.91 for Personal Control, 0.88 for External Control, 0.87 for Locus of Control, and 0.78 for Stability. As a Cronbach's alpha close to 0.8 is appropriate for applied research (Nunnally and Bernstein, 1994), the coefficients suggest that the dimensions have relatively high internal consistency.

The average candidate is 38 years old, ranging from 18 years to 70 years. Raters' median scores of the main candidate's Attractiveness range from 2 to 9. The average median attractiveness score is 5.17. 66% of the candidates are female. 94% of the candidates are white, so we do not use this variable in the remainder of our analysis.

Correlations between independent variables

In Table 3.A.1 in the Appendix, we show correlations between our independent variables. The correlation between Impact of €100 and Impact of €10,000 is 0.37. While the correlation is positive and statistically significant at the 1% level, the two impact variables are not perfectly correlated. Personal Control is negatively correlated with both impact variables, but the correlation with Impact of €10,000 is stronger than with Impact of €100 (-0.43 and -0.26, respectively). People perceive the impact of money lower for a candidate who is in her situation because of her own doing than the impact for a candidate who has had bad luck. The negative correlation between Personal Control and the impact variables shows the importance of using both the concept of impact and of desert in one

analysis. With a negative correlation between receiving help and desert, we would have overestimated the impact of desert if we had only studied the relationship between desert and the allocated share of money. With a positive correlation between receiving help and impact, we would have overestimated the effect of impact if we had only studied impact.

Most of the correlations between the AT dimensions we find are in line with findings by McAuley, Duncan, and Russell (1992), and Osborne and Weiner (2015). The most notable exception is that we do not find a correlation between the two control dimensions, while the other two studies find a negative correlation.

3.4 Analyses and Results

In the first subsection we describe the variables we use in our analyses. Second, we show univariate analyses to describe the relationships between the variables in our models. Third, we present multivariate analyses to show whether the gift's impact or the beneficiary's personal responsibility is more influential on giving behavior.

3.4.1 Variables Used in the Analyses

The dependent variable in the analyses is the percentage of the €10,000 being distributed in a given episode to a specific candidate, which in theory runs from 0 to 100. The independent variables of our analyses are the two impact variables, Impact of €100 and Impact of €10,000, as well as the dimension of AT measuring personal responsibility, Personal Control. We include External Control, Locus of Control, and Stability as well as Age, Attractiveness, and Female as control variables in all regressions.

3.4.2 Univariate Analysis

To visually explore how impact and personal responsibility influence giving behavior, we look at ternary plots. Ternary plots show how a variable is distributed between three agents in an equilateral triangle. Each dot within the triangle represents one observation and shows the proportion that each agent receives. In our case, the three sides of the triangle show each candidate's share of the €10,000 distributed per episode. We rank the three candidates within each episode along the three independent variables of interest, Impact of €100, Impact of €10,000, and Personal Control. Figure 3.1 shows the ternary plots, where each black dot represents one episode.[4]
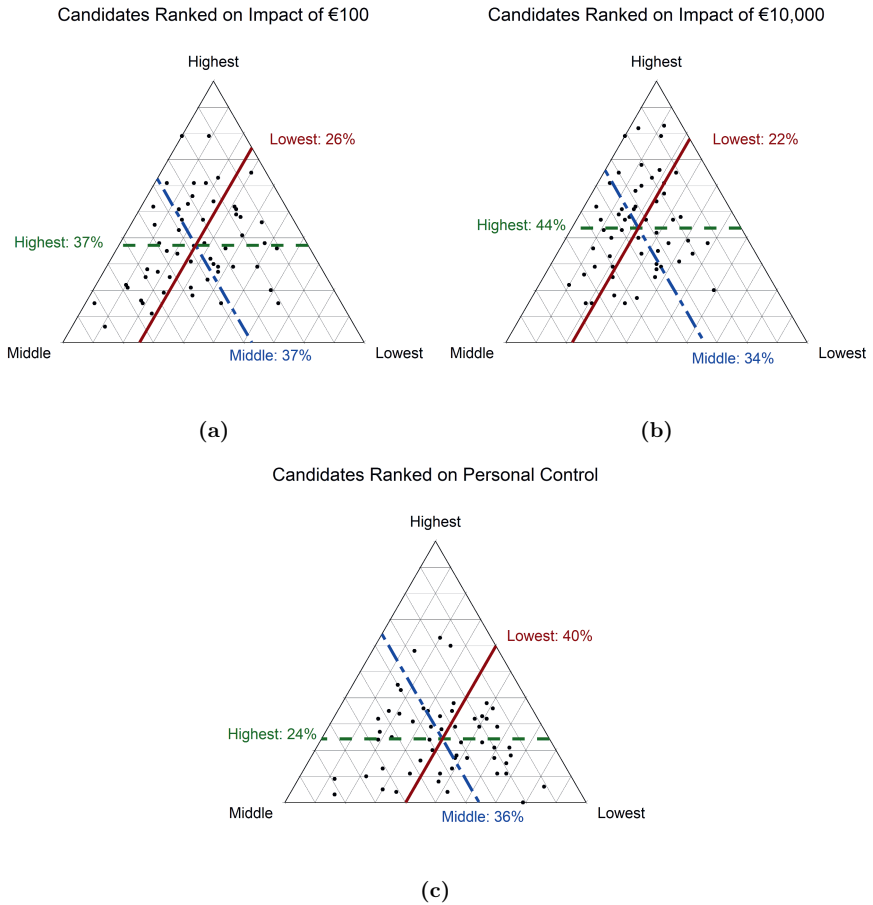
In Panel (a), candidates are ranked on the scores for Impact of €100, and in Panel (b) on the scores for Impact of €10,000. We see that the candidates who are ranked lowest on impact in their episodes also receive less money than the other two candidates. This pattern is more pronounced for Impact of €10,000 than for Impact of €100. For the latter, the lowest ranked candidate received 26% of the money on average, the middle candidate received 37%, and the highest ranked candidate received 37%. For Impact of €10,000 the lowest ranked candidate earns 22%, the middle candidate, 34%, and the highest ranked 44%. This suggests a positive relation between impact and giving behavior. In Panel (c), candidates are ranked on the scores for Personal Control. In line with theories of equity and desert, candidates with the highest Personal Control in the episode receive less money than the other two candidates. They receive 24% of the money, on average. Candidates who rank in the middle earn an average of 36%. Candidates who rank lowest on Personal Control get an average of 40%.

Figure 3.A.1 in the appendix shows the ternary plots where we rank the candidates according to External Control, Locus of Control, Stability, Age, and Attractiveness. The ternary plots do not show such a clear-cut ranking as the plots for Personal Control and Impact of €10,000 show. The average male candidate receives 29.59% of €10,000 (st. dev.= 18.44%, N=56) and the average

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[4]We use R (R Core Team, 2020) and the packages ggtern (Hamilton and Ferry, 2018) and ggplot2 (Wickham, 2016) to construct and plot the graphs.

(a)

(b)

(c)

Figure 3.1. Share of Money Received Per Candidate, Ranked by Impact and Personal Control Variables

The panels show ternary plots of each episodes' distribution of the €10,000 among three candidates. The candidates within each episode are ranked by the respective score they received. For Panel (a), candidates are ranked on the scores for Impact of €100; for Panel (b) on the scores for Impact of €10,000; and for Panel (c) on the scores for Personal Control. The grey lines are reference lines. The dashed green line shows the average percentage the candidate ranked highest received, the two dashed blue line shows the average percentage the candidate ranked second received, and the solid red line shows the average percentage the candidate ranked lowest received.

139

female candidate receives 35.26% (st. dev. 17.03%, N=109). Applying a two-sided t-test, the difference between men and women of 5.67% (std. error=2.88%) is only weakly statistically significant (t=-1.97, p=0.05).

3.4.3 Regression Results

We next turn to our regression analysis. We run Ordinary Least Squares regressions with standard errors clustered by episode.

Table 3.2 presents the regression results. In Models 1-3, we look at the effect of impact on the percentage share of money the candidate has received. Model 1 uses Impact of €100, Model 2 uses Impact of €10,000, and Model 3 uses both Impact of €100 and Impact of €10,000 to measure impact. In all three models we find that impact is positively correlated with giving behavior. Consistent with the ternary plots in Section 3.4.2, the effect is strongest when impact is measured with the Impact of €10,000-variable. Both impact coefficients in Models 1 and 2 are statistically significantly different from 0 at the 1% significance level. When one regresses the allocated share on both impact variables, only Impact of €10,000 is statistically significant at the 1% significance level. Impact of €100 is then statistically significant at the 5% level. When the Impact of €10,000 increases by one standard deviation, the share of money the candidate receives increases by 7.37 percentage points. Utilitarian considerations thus matter for the decision to choose between beneficiaries.

Table 3.2: Regression Analysis

This table reports the coefficients from regression analyses of the percentage of audience members donating to a given candidate. Except for Female and Age, all variables are double-standardized scores (standardized within rater and candidate). Age is the median of the scores for each candidate, and then standardized. Female is a dummy variable that takes the value one if the candidate is female. Standard errors (in parentheses) are corrected for clustering at the episode level. Asterisks denote significance at the one ***, five **, and ten * percent level, respectively.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Impact of €100	5.24***		2.62**		3.87***		2.23*
	(1.23)		(1.30)		(1.18)		(1.26)
Impact of €10,000		8.36***	7.37***			6.40***	5.62***
		(1.38)	(1.44)			(1.49)	(1.58)
Personal Control				-6.59***	-5.64***	-4.39***	-4.12**
				(1.80)	(1.68)	(1.64)	(1.61)
External Control				-2.26	-2.25	-2.10	-2.12
				(1.84)	(1.73)	(1.60)	(1.55)
Locus of Control				-3.07	-3.17	-1.99	-2.18
				(1.98)	(1.96)	(1.87)	(1.88)
Stability				-0.33	-0.29	-0.15	-0.15
				(1.81)	(1.84)	(1.73)	(1.77)
Age	2.02	-0.17	0.19	0.45	0.76	-0.51	-0.22
	(1.65)	(1.49)	(1.53)	(1.70)	(1.79)	(1.48)	(1.57)
Attractiveness	2.18	0.25	0.95	0.19	1.11	-0.09	0.47
	(1.85)	(1.66)	(1.69)	(1.69)	(1.75)	(1.58)	(1.64)
Female	3.67	0.50	0.11	1.34	0.34	-1.22	-1.48
	(3.21)	(3.20)	(3.13)	(3.28)	(3.10)	(3.29)	(3.21)
Constant	30.91***	33.01***	33.26***	32.45***	33.11***	34.14***	34.31***
	(2.12)	(2.24)	(2.16)	(2.20)	(2.04)	(2.27)	(2.18)
Observations	165	165	165	165	165	165	165
R-squared	0.113	0.227	0.245	0.198	0.240	0.294	0.306
Adj. R-squared	0.0909	0.207	0.221	0.162	0.201	0.257	0.266

3. IMPACT OR RESPONSIBILITY?

In Model 4 we look at the effect of personal responsibility on giving behavior. We regress the percentage share the candidate has received on Personal Control and control for the other three AT dimensions, External Control, Locus of Control, and Stability. In line with the theories of equity and desert, we find that Personal Control influences giving behavior statistically significantly, but the other three AT dimensions do not. A one standard deviation increase in Personal Control reduces the share allocated towards a candidate by 6.59 percentage points. The absolute size of the effect is thus close to the effect of Impact of €10,000 in Models 2 and 3.

To see whether considerations of utilitarianism and of equity and desert remain important when we control for both simultaneously, Models 5-7 incorporate impact variables and Personal Control. We also control for the other three AT dimensions for consistency. Model 5 uses Impact of €100, Model 6 uses Impact of €10,000, and Model 7 uses both Impact of €100 and Impact of €10,000 to measure impact. We find that the effects of impact and personal responsibility on giving behavior remain similar to the effects found in Models 1-4: impact is positively correlated with giving behavior and personal responsibility is negatively correlated. The effects of personal responsibility and impact are more balanced-out when Impact of €10,000 is used to proxy impact. An increase of one standard deviation in impact means additional 6.40 percentage points of money received, an increase of one standard deviation in Personal Control results in 4.39 percentage points less for the candidate. In Model 7, the coefficient of Impact of €100 is only weakly statistically significant (p=0.08). Impact of €10,000 is statistically significant at the 1% significance level. Personal Control is statistically significant at the 5% significance level. Compared with Models 1 – 4, we see a slight reduction in the size of the coefficients in Models 5-7. We can explain this by the correlations between Personal Control and the impact variables.

Overall, Table 2 shows that both impact and personal responsibility are concepts that influence the allocation of money. A candidate whose situation is due to personal responsibility receives less money, while people decide to give money to people where it is perceived to be impactful. Impact of €100 seems to be less

important for giving behavior than Impact of €10,000. Other included variables from AT and the candidate's age, attractiveness and gender do not affect giving behavior.

In the remainder of the paper, we use Model 6 as our core model for further analyses and robustness checks. We use Model 6 because it uses the Impact of €10,000 as an impact measure. Impact of €10,000 is a more meaningful measure than Impact of €100 as €10,000 solve most candidates' problems. Empirically, Impact of €10,000 is stronger correlated with giving behavior than Impact of €100.

Out of the considerations of utilitarianism and equity and desert, which consideration is more important for selecting a beneficiary? In the following analysis, we perform a dominance analysis (see, for example, Budescu (1993)) to check which of the variables contributes the most to the variation explained (i.e., the R-squared) in Model 6.

The algorithm used with dominance analysis runs all possible regressions with the variables of a model, thus excluding and including variables repeatedly. It then compares the variance explained of the different regressions. For example, the R-squared of possible regressions with Personal Control is compared to the R-squared of possible regressions without Personal Control. Table 3.3 shows the results of the dominance analysis. We present the total variance explained by the regression in the bottom row. The other rows present the relative contribution of each variable in the model. The relative contributions per regression add up to 100%.

We find that utilitarian considerations are most important for selecting a beneficiary. Impact of €10,000 is the most important variable in the model. It explains 50.33% of the variation explained by Model 6. Considerations of equity and desert follow utilitarian considerations, as Personal Control is the second most important variable and explains 30.12%. The statistically insignificant Locus of Control-variable explains 7.51% of the R-squared of Model 6. Stability, External Control, Female, Age, and Attractiveness follow in descending order of relative contribution.

Table 3.3: Dominance Analysis

This table reports the relative contribution of each explanatory variable to the variance explained in Model 6 (Table 3.2). The relative contributions are calculated by dominance analysis. The relative contribution percentages add up to 100%. The bottom row shows the percentage of total variance explained by all variables (i.e. the R-squared).

	Model 6
	Relative contribution
Impact of €10,000	50.33%
Personal Control	30.12%
External Control	2.29%
Locus of Control	7.51%
Stability	6.72%
Age	0.52%
Attractiveness	0.25%
Female	2.26%
Total Variance Explained	29.37%

Overall, our analyses result in three key findings. First, the higher the perceived impact of help on wellbeing, the higher the amount the candidate receives. Second, a candidate who is believed to have a lot of responsibility over the cause of her financial situation receives less money from the audience than candidates who are believed to have little responsibility. Third, we find that impact is more important than personal responsibility when it comes to giving behavior.

3.5 Robustness Analyses

In this section we check the robustness of our results in four ways. Similar to the dominance analysis in Section 3.4, we use Model 6 of Table 3.2. This model tests both the effects of impact and personal responsibility on giving behavior. It uses Impact of €10,000 as the impact variable, given that this is a more meaningful measure of impact than Impact of €100.

As a first robustness check, we only take the scores of raters who have never watched the TV show before. While we do not have a clear hypothesis about the direction of a potential bias, having watched the TV show before might influence raters' scores. We restrict the sample of raters to the raters who have never

watched any episode of Geld Maakt Gelukkig before. In the new dataset, 18 candidates are rated twice, 54 are rated three times, 78 are rated four times, and 15 are rated five times. The results are presented in Table 4. As previously, the allocated share is positively correlated with Impact and negatively correlated with Personal Control. The R-squared is lower than in Table 4, due to the noisier measures being based on fewer ratings. Overall, our results are robust to previous experiences of the raters with the show.

Second, we test whether variable specification matters for our results. Specifically, we change how we aggregate the raters' scores on the candidate level. Instead of using the standardization procedure, we take the median of the scores for each candidate, and standardize them for the regression. Using the median scores as variables in Model 2 of Table 3.4, we find that while the direction of the coefficients of Personal Control and Impact of €10,000 are the same as in the other regressions, the absolute effects are smaller than when we use standardized scores. Both the R-squared and the adjusted R-squared are the lowest compared to the other models. By taking the median scores instead of the standardization procedure, we lose information from the different scores per candidate. This potentially leads to mismeasurement and attenuation of variables, which in turn might explain the smaller coefficients and drop in R-squared.

Third, we add the requested amount as a control variable. The requested amount is a focal amount which signals financial need and thus might influence the perception of impact. In Model 3 of Table 3.4, the coefficient of Amount Requested is statistically insignificant. The coefficients of Personal Control and Impact of €10,000 are nearly the same as in Model 6 of Table 3.2. Adding the requested amount as a covariate therefore does not change our conclusions.

Table 3.4: Robustness Analysis

The table reports the coefficients from regression analyses of the percentage of audience members donating to a given candidate. All columns show a variation of Model 6 from Table 3.2. Model 1 is run only with ratings by raters who have not watched an episode of the TV show before. Model 2 uses median scores instead of double-standardized scores for all variables (median scores are standardized). In Model 3 we include Amount Requested as a control variable. Amount Requested is the amount requested by the candidates, as a percentage of €10,000. In Model 4 we only use candidates who do not represent someone else. Standard errors (in parentheses) are corrected for clustering at the episode level. Asterisks denote significance at the one ***, five **, and ten * percent level, respectively.

	Model 1: Only Non-Viewers' Ratings	Model 2: Median Scores	Model 3: Amount Requested Added	Model 4: Excluding Candidates Representing Someone Else
Impact of €10,000	9.40***	4.55***	6.36***	6.91***
	(2.43)	(1.45)	(1.48)	(1.55)
Personal Control	-4.20***	-2.98**	-4.17**	-5.26***
	(1.35)	(1.42)	(1.61)	(1.72)
External Control	-1.50	-2.04	-2.13	-1.52
	(1.61)	(1.56)	(1.62)	(1.63)
Locus of Control	-0.98	-2.66	-1.79	-2.43
	(1.82)	(1.77)	(1.86)	(1.88)
Stability	0.29	1.22	-0.17	-0.94
	(1.96)	(1.72)	(1.72)	(1.88)
Age	0.24	-0.03	-0.39	-0.09
	(1.60)	(1.63)	(1.45)	(1.33)
Attractiveness	0.06	0.15	-0.10	0.54
	(1.58)	(1.56)	(1.55)	(1.55)
Female	0.02	2.55	-1.13	-1.22
	(3.29)	(3.32)	(3.33)	(3.21)
Amount Requested			0.05	
			(0.06)	
Constant	33.29***	31.65***	31.35***	34.92***
	(2.30)	(2.28)	(4.18)	(2.32)
Observations	165	165	165	153
R-squared	0.245	0.197	0.298	0.358
Adj. R-squared	0.206	0.156	0.257	0.322

Fourth, we check whether representing oneself or another beneficiary has an influence on the relationships between personal responsibility and the amount received. The effect of the candidate's personal responsibility on how much money she receives will likely be smaller if the candidate asks for money for another beneficiary than if she asks for money for herself. Therefore, our results in Section 3.4 possibly underestimate the importance of equity and desert considerations. We exclude the twelve candidates who represent another beneficiary and show the regression coefficients in Model 4. Excluding those candidates increases the adjusted R-squared to 32.2%, the highest R-squared for all models. As hypothesized above, the coefficients of Impact of €10,000 and Personal Control are indeed slightly larger in absolute size than in Model 6 of Table 3.2. If we only look at candidates who ask for money for themselves, especially equity and desert considerations have a larger effect.

To see whether changes to the variable specifications or models have impacted the explanatory power of impact and personal responsibility, we show dominance analyses of the four robustness tests in Table 3.5. We find that Impact of €10,000 contributes the most to all models, ranging between explaining 41.75% and 49.37% of the variances. Personal responsibility as measured with Personal Control contributes between 25.72% and 31.95% of the R-squared. The robustness checks in Table 3.4 and 3.5 seem to confirm the broad picture of our main analyses: Impact increases giving behavior, while a personally responsible person receives less help. Both variables remain important for giving behavior, in the same order of magnitude.

Table 3.5: Dominance Analysis of Table 3.4

This table reports the relative contribution of each explanatory variable to the variance explained in the models of Table 3.4. The relative contributions are calculated by dominance analyses. The relative contribution percentages add up to 100%. The bottom row shows the percentage of total variance explained by all variables in the different Models (i.e. the R-squared).

	Model 1: Only Non-Viewers' Ratings	Model 2: Median Scores	Model 3: Amount Requested Added	Model 4: Excluding Candidates Representing Someone Else
	Relative Contribution	Relative Contribution	Relative Contribution	Relative Contribution
Impact of €10,000	49.37%	41.75%	48.75%	49.32%
Personal Control	31.95%	25.72%	28.22%	30.72%
External Control	2.15%	3.48%	2.32%	0.95%
Locus of Control	4.29%	11.99%	6.74%	10.36%
Stability	8.14%	10.15%	6.38%	5.03%
Age	1.10%	0.72%	0.51%	0.49%
Attractiveness	0.22%	0.55%	0.22%	0.65%
Female	2.78%	5.65%	2.17%	2.47%
Amount Requested			4.68%	
Total Variance Explained	24.51%	19.69%	29.81%	35.79%

3.6 Discussion and Conclusion

In this paper we analyze how people make trade-offs between two main considerations when deciding to which cause to give. Using a televised natural experiment, we examine the extent to which giving behavior can be explained by considerations for impact of the gift and considerations for personal responsibility and deservedness of the beneficiary. Our study yields three main findings. First, the proportion of money a candidate received is positively correlated with impact. In our main model, impact explains half of the variation explained. Second, giving behavior is negatively correlated with Personal Control, our measure of considerations for personal responsibility. Personal Control explains 30% of the R-squared in our main model. Third, we find that impact and personal responsibility are correlated concepts. In order not to overestimate each concept's explanatory power, it is thus important to include both in the analysis of giving behavior.

Given the empirical results of most studies so far, the first finding is rather surprising. As described in Section 3.2, many studies find that people do not consider the impact and scope of their giving behavior (Berman, Barasch, Levine, and Small, 2018; Genevsky, Västfjäll, Slovic, and Knutson, 2013; Hsee and Rottenstreich, 2004; Small and Loewenstein, 2003), or even reduce giving when receiving information on charities' effectiveness and efficiency (Exley, 2020; Karlan and Wood, 2017). We, however, conclude that people care about the impact of money given to the candidate. Real world inefficiencies might not be caused by people not wanting to be effective, but by other inefficiencies. A difference between most studies on utilitarianism in giving behavior and ours is that other studies use charities as the goal of the donation.

In our study the goal of the donation is beneficiaries to which audience members can select to give money to directly in 93% of the cases.[5] In the show, audience members see the candidates and their living situations. They can vividly imagine the concrete impact the donation would have on the candidates' lives. This is often not the case with a donation to a charity. Aknin, Dunn, Whillans, Grant, and Norton (2013) show that donors' wellbeing is higher when they give

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[5]In the other 7% of cases, candidates ask for money for another beneficiary, such as charities.

to a charity that describes how the money is used rather than to a charity with a more abstract goal. It might be that charities display the goal of the possible donation in a too abstract manner. This results in people not properly understanding the importance and the impact of the charities on people's lives. Vividly and concretely describing the impact of help on beneficiaries is more common for crowdfunding campaigns, where platforms such as GoFundMe recommend to "tell how these funds will help you or your loved ones" (Paulus and Roberts, 2018). In our study, the candidates' situation is vividly shown in video clips. Furthermore, the situation is also discussed in the studio. In other studies, participants did not have that much detail on specific cases. It seems that vivid examples presented in detail trigger people to think about impact.

Previously, the identifiable victim effect has been used to argue for giving decisions being made out of sympathy and against the prevalence of utilitarian giving (Genevsky, Västfjäll, Slovic, and Knutson, 2013; Small and Loewenstein, 2003). However, our findings suggest that impact considerations are still important to people who select between beneficiaries who are all identifiable. Future research can analyze how impact considerations and sympathy relate to and interact with giving decisions with identifiable victims. Which other emotions and primes lead people to think about impact? Our findings imply that charities need to be careful about how to explain impact to possible donors. Using rankings and stars to suggest impact might be too abstract for donors. Fundraising material that clearly shows the impact of a charity on one specific person's life might be most effective to reach donors.

Our second result is in line with the predictions of theories of equity and desert. We use the CDSII to measure dimensions of AT, one of the theories of equity and desert. We find that the importance of equity and desert for giving behavior holds in our semi-controlled real-life setting with high stakes. In line with the previous literature, we do not find an effect of the other three dimensions of AT. When deciding between causes to help, perceived deservedness of the beneficiaries play a big role. Importantly, we add impact to the model on giving behavior. We observe that impact is a separate concept that the literature on AT

has neglected so far. In our study, impact is more crucial for explaining giving decisions than considerations about equity and desert. Our results are robust to different specifications of variables. They are also robust to using only the score of the raters who saw the television show for the first time for our study, using only candidates who do represent themselves, and adding the requested amount as control variable.

A limitation of our study is that we do not have information about the audience members and their selection behavior. Because of the aggregate nature of our data, we cannot categorize audience members into different giving types. Do some people only give in line with utilitarianism? Do some only give based on equity and desert? Does the importance of the gift's impact and the importance of the beneficiary's personal responsibility differ between individuals, and if yes, is that based on characteristics such as income, education, and gender? In this paper, we examined the aggregate effect of impact and personal responsibility for a sample of the Dutch population. Future research can help answering questions about the heterogeneity of effects.

As candidates are always identifiable in our case, we cannot look at other factors that have been found to impact giving behavior, such as the identifiable victim effect. Given that all audience members have the same information about the candidates' situations and their causes, we cannot look at context effects either. Do impact or personal responsibility considerations dominate in different situations? Does giving online make people more prone to select a beneficiary based on impact or based on the candidate's responsibility? These are questions for future research.

On our aggregate level, we do not find any evidence for discrimination against candidates based on their age, gender, or attractiveness. It might be surprising that attractiveness does not play a role in giving behavior in our study. Other studies find effects based on beauty in different domains. Attractive people are more likely to be hired (Hamermesh and Biddle, 1994), receive higher wages because they are deemed more competent (Mobius and Rosenblat, 2006), are offered loans to better conditions (Duarte, Siegel, and Young, 2012), have more en-

trepreneurial success (Brooks, Huang, Kearney, and Murray, 2014), and are more likely to be elected (Hamermesh, 2006). In the donation domain, Sargeant (1999) models that attractive people do receive more donations. Results by Landry, Lange, List, Price, and Rupp (2006) confirm this hypothesis. They discover that a one standard deviation increase in female solicitor's attractiveness leads to an increase of donations that is as large as the effect of the lottery incentive in their study. Park, Kim, and Hong (2019) find that attractive women receive more money on a prosocial lending platform from men, while they receive less from women. Given the available data, we cannot analyze similar relations between the audience members' characteristics and giving to beneficiaries. The studies on attractiveness differ from our research in two ways. First, Landry, Lange, List, Price, and Rupp (2006) use the attractiveness of the solicitor. We use the attractiveness of the candidate. In most cases in our study, the candidate functions as both the solicitor and the direct beneficiary. This might attenuate the effect of attractiveness on giving decisions found by Landry, Lange, List, Price, and Rupp (2006). Second, the study by Park, Kim, and Hong (2019) uses profile pictures to measure attractiveness. Superficial traits such as attractiveness might be important in settings where there is limited engagement with the actual person who profits from the charity. Related to the effect of vivid impact descriptions on the importance of impact considerations discussed above, attractiveness might become less important in scenarios in which donors receive detailed information about the beneficiaries' situation and the potential impact of help.

Overall, our findings suggest that people decide to give to one cause over another because of both considerations studied. First, people give money to a goal on which money has the highest impact. Second, people prefer to choose to give to causes over which the affected people have little to no personal control. Marketing campaigns of charities can use this finding to vividly showcase people who benefit from the charity the most. Our findings are in line with recommendations to crowdfunders to describe the beneficiary's situation and impact of donations in detail. Given our findings, video clips similar to the ones used in this study could induce people to give to a cause over another. The video clips would need to

present the beneficiary's non-responsibility as well as the impact of a donation for the beneficiary. Future studies should test whether these video clips are a means to help donors reach their aim of donating to worthy causes with high impact.

Appendix

Appendix A. Welcome Message (read out in the waiting room)

Good morning/afternoon, I welcome you to this session in the BEElab. I am [FILL IN NAME HERE] and I am the organizer. This session is part of a research project on giving behavior. The approximate duration of the session is 90 minutes.

You see here a number of cards. I will ask you to draw one of these cards. On the card you will find a number. In the lab the computer cubicles are also numbered. Each number corresponds to one computer in the lab. After all have drawn a number I'll ask you to enter the computer room and take a seat in the computer cubicle that corresponds to your number. We use these numbers for the data analysis. The number will not be linked to your name. Your anonymity is thus secured.

You will receive detailed instructions about the tasks on paper. It is important for our research that you understand and follow the instructions carefully. As soon as you have entered the lab, I ask you not to communicate with anyone else but me.

Talking to other participants and using mobile phones or any other electronic devices is strictly prohibited. Mobile phones and other electronic devices should be left in the waiting room. Also, you are not allowed to use the computer in the BEElab for any other purpose.

If you are found violating these rules, you will forfeit any earnings from this session, and may be excluded from future sessions as well. Please leave your mobile phone here. [WAIT UNTIL ALL MOBILE PHONES ARE PUT DOWN.]

All your decisions have to stay private. In the laboratory, if you have a question, please raise a hand. I will come to you to answer your question in private.

The show-up fee of €13 will be paid out to you confidentially and privately in cash at the end of this session. By participating, you also take part in a lottery of €100. The winner will be contacted by e-mail after all sessions are finished.

This room will be locked. So you can leave your stuff here.

[HAND OUT INSTRUCTIONS]

I will read out the questions now.

[READ INSTRUCTIONS]

I now ask you to draw one of the cards.

Appendix B. Instructions on paper

Dear participant,

This session in the BEElab is about giving behavior in a game show. After you have read these instructions, you will go to your cubicle. On the computer screen in your cubicle you will see a questionnaire. A total of nine videos from the Dutch TV show "Geld Maakt Gelukkig" are embedded in the questionnaire.

In each video clip, a candidate explains why he or she needs financial help. After each video clip, we will ask you questions about the candidate and the candidate's situation. We are interested in your honest opinion. Your answers are important for our research and will be stored anonymously.

Talking to other participants, using mobile phones or any other electronic devices is strictly prohibited. Mobile phones and other electronic devices should be left in the waiting room. Also, you are not allowed to use the computer in the BEElab for any other purpose than the questionnaire. If you are found violating these rules, you will forfeit any earnings from this session, and may be excluded from future sessions as well.

This session is finished when everybody is done with the survey, so there is no need to hurry. In total, the session is expected to last about 90 minutes. After everybody is done, the researcher in the BEElab will give you your payment of €13. By participating, you also automatically take part in a lottery of €100. We will contact the winner via e-mail after all sessions are finished.

After each video, we will ask you the following questions about the candidate and the candidate's situation:

Question 1a

In general, gifts increase the well-being of the recipient and people who are indirectly involved. Assume that this impact can be measured, and that the overall impact of a gift is the sum of the increases in well-being for all people involved.

Imagine a group of 100 people, consisting of the candidate and 99 people who are randomly drawn from the Dutch population. Assume that people can be ranked in terms of the overall impact from receiving a monetary gift.

- What would the rank of the candidate be if those 100 people are ordered from lowest overall impact (1) to highest overall impact (100) of a gift of €100?

Question 1b

- What would the rank of the candidate be if the 100 people are ordered from lowest overall impact (1) to highest overall impact (100) of a gift of €10,000? (Note that this rank can be lower than, higher than, or equal to your previous answer where the gift was €100.)

Question 2a

- Please give a brief description of the cause or the causes of the candidate's situation.

Question 2b

Think about the cause or causes you have just described. The items below concern your impressions or opinions of this cause or these causes.

- Is the cause, or are the causes, something. . .

that reflects an aspect of the candidate	1 - 9	that reflects an aspect of the situation
manageable by the candidate	1 - 9	not manageable by the candidate
permanent	1 - 9	temporary
the candidate can regulate	1 - 9	the candidate cannot regulate
over which others have control	1 - 9	over which others have no control
inside of the candidate	1 - 9	outside of the candidate
stable over time	1 - 9	variable over time
under the power of other people	1 - 9	not under the power of other people
about the candidate	1 - 9	about others
over which the candidate has power	1 - 9	over which the candidate has no power
unchangeable	1 - 9	changeable
other people can regulate	1 - 9	other people cannot regulate

3. IMPACT OR RESPONSIBILITY?

<u>Question 3</u>

- What is your estimate of the candidate's age?

<u>Question 4</u>

- Please rate the candidate's physical appearance on a scale from 1 to 10, where 1 means "extremely unattractive" and 10 means "model beautiful or handsome".

In some video clips, two people ask for money. In those cases, please answer the questions about the candidate's age and the physical appearance for each person separately.

You can start the questionnaire after taking your seat in the cubicle. If you have any questions, please raise your hand and the researcher will come to you.

Table 3.A.1: Correlations between Independent Variables

This table shows the pair-wise correlations between the independent variables used in Table 3.2. Asterisks denote significance at the one ***, five **, and ten * percent level, respectively.

	Impact of €100	Impact of €10,000	Personal Control	External Control	Locus of Control	Stability	Age	Attractiveness
Impact of €10,000	0.37***	1						
Personal Control	-0.26***	-0.43***	1					
External Control	-0.01	0.08	0.02	1				
Locus of Control	-0.01	-0.28***	0.22***	-0.51***	1			
Stability	0.16**	0.22***	-0.58***	-0.33***	0.12	1		
Age	0.04	0.16**	-0.12	0.15*	-0.12	0.16**	1	
Attractiveness	-0.18**	0.03	0.00	-0.09	-0.11	-0.06	-0.45***	1
Female	0.14*	0.29***	-0.26***	-0.02	-0.13	0.23***	-0.10	0.19**

(a)

(b)

(c)

(d)

(e)
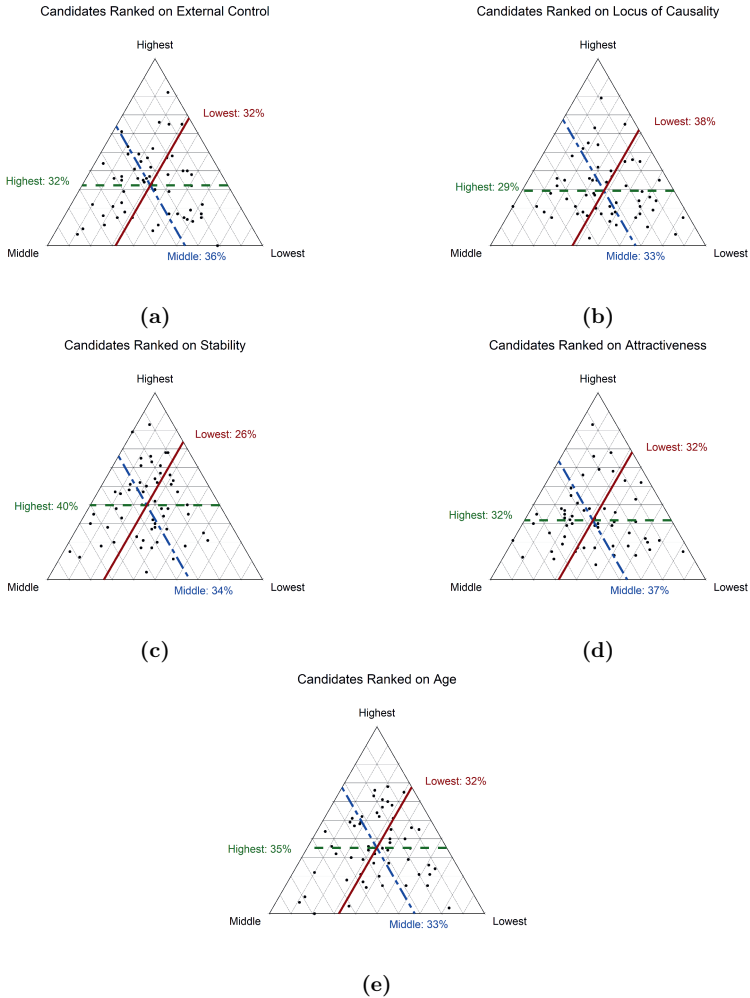
Figure 3.A.1. Share of Money Received Per Candidate, Ranked by External Control, Locus of Causality, Stability, Attractiveness, and Age

The panels show ternary plots of each episodes' distribution of the €10,000 among three candidates. The candidates within each episode are ranked by the respective score they received. For Panel (a), candidates are ranked on the scores for External control; for Panel (b) on the scores for Locus of Control; for Panel (c) on the scores for Stability; for Panel (d) on the scores for Attractiveness; and for Panel (e) on the scores for Age. The grey lines are reference lines. The dashed green line shows the average percentage the candidate ranked highest received, the two dashed blue line shows the average percentage the candidate ranked second received, and the solid red line shows the average percentage the candidate ranked lowest received.

Chapter 4

Conclusions

In this dissertation I look at financial behavior in the field in two contexts: saving for retirement (Chapter 2), and giving decisions (Chapter 3).

4.1   Chapter 2

Retirement undersaving is one of the most pressing problems in the developed world on a socio-economic level. Population ageing leads to the de-greening and greying of societies who need to finance the lives of the non-working elderly population. The decreasing ratio of workers to retirees means that Defined Benefit systems that are financed by Pay-As-You-Go schemes are increasingly under pressure. But also retirees in funded Defined Contribution schemes will face hardships if the accrued pension needs to finance a longer time in retirement. Chapter 2 focused on private savings as a mechanism to increase retirement funds. For fund members to make appropriate decisions, they need to know how big a potential savings gap is and how they can fill that gap. We use two studies to test how one can best motivate people to look up personal retirement information, and whether looking up personal retirement information leads to more pension knowledge and more private savings.

The two field experiments combined provide six key findings. First, peer information treatments do not increase a fund member's likelihood to look at

retirement information. Members who receive peer information statements are not more likely to look at their pension planner than members in the control group. Participants who receive the Saving-Fear or the Income-Hope letters are 0.3 percentage points less likely to look at the pension planner. Second, financial incentives increase the likelihood to look at retirement information. A member who receives the financial incentive treatment in Study 1 is 50% more likely to look at the pension planner than a member who received the control letter. Third, raffling few large prizes triggers more participants to inform themselves than a lottery of many small prizes. For example, in Study 2 participants in the lottery which raffled two large prizes are 86% more likely to inform themselves about their pension situation than the control group. Fourth, we find that participants in the Financial Incentive treatment that raffle few large prizes are significantly more likely to look at the pension planner for more than 1 minute, 5 minutes and 10 minutes. Smaller incentives only affect spending shorter amounts of time on the planner. Fifth, looking at the pension planner does not causally increase pension knowledge. Sixth, looking at the pension planner does not causally affect self-reported savings behavior. Taken together, financial incentives can be used to motivate people to look at pension information, but do not increase actual knowledge, nor affect savings behavior, while peer information treatments have no positive effects altogether.

4.2   Chapter 3

Large sums of money are being donated to charities every year. US Americans, for example, donated $324.1 billion in 2020 to charities. Depending on the charities' goals, they provide public goods and redistribute wealth within and between countries. Donation-based crowdfunding has increased for the past years and provides a less bureaucratic, more direct form of funding public goods, financing projects in developing countries or private health costs (Salido-Andres, Rey-Garcia, Alvarez-Gonzalez, and Vazquez-Casielles, 2021). For charities and crowdfunders, it is important to find out how (potential) donors select between

beneficiaries. In our study we use aggregate data from a Dutch TV show as well as information from raters scoring the show's candidates on (1) how much impact a monetary gift would have on the well-being of the candidate and anyone involved in the candidate's situation and (2) how much personal control they had over the cause of their financial need.

We find that a gift's impact increases the share of money a candidate receives, while the candidate's personal control over the cause of their financial need decreases the share of money they receive. A one standard deviation increase of impact increases their share of money distributed by 6.4 percentage points. A one standard deviation increase of personal control decreases the candidate's share by 4.39 percentage points. Dominance analysis shows that impact explains most of the variation explained in our regression model (50.33%), with personal control following with 30.12%. We do not find evidence for discrimination based on age, attractiveness, or gender. Our results are robust to different model and variable specifications as well as to restricting the rater sample.

Do people give to the person who deserves it or to the person who needs it most? Both considerations are important, but the dominance analysis points to impact having the higher explanatory power of giving behaviour. In our TV show setting, where candidates' need for financial help is vividly shown and described, the impact of a gift is more concrete to the donor than when imagining the impact of a donation to many charities. If charities want to attract donations, they need to not only show abstract impact ratings to convince donors, but also vividly describe the impact of a donation to the final beneficiaries of the charity.

4. CONCLUSIONS

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Research Impact

"We can't do evidence-based policy without evidence." - Richard
Thaler

Generally speaking, the research areas of behavioral economics and behavioral finance examining people's economic and financial behavior are growing. The rise of networks and teams that follow the "Nudge Unit"'s approach to use behavioral science in public policy and test its effect is a testament to this, and the list of those networks and teams is long.[1] The design and effect of nudges are context-specific, so trialling them in the field remains important. Knowing what works in which contexts highly increases the success rates of new policies, ultimately decreasing money spent on ineffective policies and campaigns. This thesis analyses financial behavior in the field in two decision areas- retirement and giving.

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[1] To name just a few: the original Nudge Unit, now the Behavioural Insights Team, started in London and opened offices in Toronto (Canada), New York (USA), Sydney (Australia), Wellington (New Zealand), and Singapore (Singapore)); Ontario's Behavioural Insights Unit in Canada; iNudgeyou and the Danish Nudging Network in Denmark, the Behavioural Insights Network Netherlands in the Netherlands, MineduLab in Peru, and the Behavioural Economics Team of the Australian Government.

.1 Chapter 2 - Nudges and Incentives in Pension Communication and Their Effect on Retirement Savings Behavior

In most of the developed world, the population is ageing. For retirement financing, the growing number of people in old-age can pose a problem. For Pay-As-You-Go schemes, an ageing society leads to an increasing share of the population (the retired) needing to be financed by a decreasing share of the population (the working population). Even for funded schemes, an ageing society can pose problems: with increasing time in retirement, retirees can run out of money on their accumulated funds. If governments aim to fulfill the UN Sustainable Development Goals 1 (No poverty) & 10 (Reducing inequality) at home, the question of how to finance an ageing society is on the forefront of problems of developed economies (UN, 2015).

In Chapter 2, we focus on how to increase private savings for retirement. We test the possible effects of nudges and incentives in the context of pension communication. In Study 1, we test the effect of peer information and financial incentives on pension fund members' likelihood to look up pension information online. Marketers often employ statements about other people's behavior to showcase the quality of a product and to invoke a feeling of scarcity. Financial incentives in the form of lotteries are used to increase savings; an example is the Premium Bond issued by the UK government. We show that peer information statements are not effective in increasing members' likelihood to look up pension information. Financial incentives, however, are effective. In Study 2, we then look at which financial incentive structure works best to engage fund members in looking up information online. We also analyze whether looking up information increases pension knowledge and, ultimately, savings for retirement. We find no such effects.

The findings of Chapter 2 show that it is difficult to change consequential behavior in the retirement savings context with financial information or education. This is in line with findings by Fernandes, Lynch Jr, and Netemeyer (2014) as well

as ASIC & AFM (2019). Regarding pension communication, law requires pension funds to activate fund members and guide them in accordance with the members' best interest. This is not a trivial task, given the low interest in pension matters.[2] Our study shows how fund members can be activated to look up personal information. Nevertheless, our study does not show that looking up information leads to further action. We thus do not see an effect of our interventions with respect to guiding members to evaluate their financial behavior in line with their own best interest and to change financial behavior.

Future research could thus look more deeply into how fund members use online platforms that show them personal information. How can these platforms engage fund members so that they increase their knowledge and make appropriate choices in their interest? Can story telling methods or better visualizations engage pension fund members more? In addition, future research could test whether more personalized peer information statements are more effective than the ones tested in Chapter 2. Another avenue would be to use financial incentives to engage fund members in increasing pension literacy, or in other retirement planning activities such as receiving targeted advice, saving, or retiring later or earlier.

If smaller changes in the choice architecture (such as nudges) are insufficient to increase retirement savings, more structural changes are needed. One major structural change in the Dutch pension industry follows from the "Wet toekomst pensioenen", essentially switching DB funds to DC funds. This tightens the relation between the economy and pension levels and puts investment risk in the hands of pension fund members. While employers and employees agree upon the new scheme and compensation/transition arrangements, any changes to a pension fund members' rights and wealth have to be communicated to them. For the new system to be successful, pension communication has to explain how the system functions. An effective pension communication people have trust in thus is more important than ever in the Dutch context. While Dutch funds still have the possibility to have a single collective investment policy under the solidarity contribution scheme, a personal investment strategy is possible under

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[2]35% have not followed the discussions around the new Dutch pension system at all, 45% have followed them a little bit (van Dalen and Henkens, 2021).

the flexible contribution scheme. Arguably, only pension funds that think their member base can take these decisions themselves will allow them to decide on their own investment strategy. But communication will become essential for all pension funds, as the investment risk is borne by fund members. The findings of Chapter 2 can be a crucial first part of concluding on which type of pension communication is effective.

.2 Chapter 3 - How Can Beneficiaries Attract Funding?

In Chapter 3, we analyze giving behavior in a Dutch TV show in order to examine whether and to what extent people distinguish between two ethical considerations when they choose a beneficiary. Do people choose a beneficiary to increase the impact of their money, or do they give money to a beneficiary who deserves their help? In our setting, we find that both considerations are important and correlate with giving in the expected directions: people give to beneficiaries when the gift has more impact, but refrain from giving to beneficiaries who caused their financial situation.

While a beneficiary is mostly a person in financial need in our setting, it can also be a charity or a crowdfunding project. US American individuals donated $324.1 billion in 2020 and $47.91 billion were given by bequest, in addition to $88.55 billion given by foundations. Overall, giving increased by 5.1% compared to 2019 (Giving USA, 2021).

For charities, knowing why people choose a certain beneficiary over another is important to attract funds and, consequently, be able to finance their help. Donations are crucial, especially in areas where governments seem to be too slow to act. By working to reduce poverty, improve public health, and fight against climate change and for animal welfare, charities also help to fulfill various UN Sustainable Development Goals.[3]

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[3]For example, charities such as Oxfam aim for the reduction of poverty (SDG #1 (No Poverty), #2 (Zero Hunger), #9 (Industry, Innovation, and Infrastructure), and #10 (Reducing

Our findings show that charities can increase their donations by explaining their impact on people's lives vividly and clearly to potential donors. Efficiency or effectiveness rankings or scores might be too abstract for donors who want to receive a "warm glow" from giving. Future research can explore in what ways charities can describe their impact in a meaningful way to donors. Next to charities, our findings also matter for donation-based crowdfunding projects. They emphasize that detailed and vivid descriptions of the beneficiary's situation are important to show the impact of a donation and to convince the donor that the cause of the situation is outside of the beneficiary's control.[4]

It remains to be seen whether our findings also hold for crowdfund-investment projects such as Kiva or Zidisha. Kiva facilitates loans to entrepreneurs, businesses and NGO's in developing countries, while Zidisha is a peer-to-peer microlending platform. For the World Bank, "Crowdfunding has emerged as a multibillion-dollar global industry" and the developing world can gain much from promoting crowdfunding as a financing tool for entrepreneurs and small businesses (World Bank, 2013). Future research could look at whether impact considerations and deliberations around equity and desert also matter for providers of microloans and crowdfunding platforms.

Inequality)); not-for-profit organizations such as Partners in Health improve health systems (SDG #1 (No Poverty), #3 (Good Health and Well-Being), #6 (Clean Water and Sanitation)); and organizations such as Greenpeace or WWF fight against climate change and for animal welfare (SDG #7 (Affordable and Clean Energy), #11 (Sustainable Cities and Communities), #13 (Climate Action), #14 (Life Below Water), #15 (Life on Land).

[4]An example can be the YOU GO 100KM fundraiser of the Danish scleroseforeningen.

Biography

Inka Eberhardt was born on May 29th, 1991 in Ahaus, Germany. She earned a B.A. in European Studies from Maastricht University in 2013. In 2014 she received a M.Sc. in Economics of Public Policy and Management cum laude from Utrecht University.

After graduating, Inka joined the Department of Finance at Maastricht University to pursue a doctorate degree under the supervision of Prof. Dr. Rob Bauer and Prof. Dr. Paul Smeets. During her PhD she was a Visiting Fellow at Harvard Graduate School of Arts and Sciences, visiting Dr. John Beshears and Prof. Dr. Michael Norton in 2017.

The results of this effort are collected in this thesis. Inka has presented this research at several international conferences and seminars. After her time in Maastricht, she was a Research Fellow at the University of New South Wales-node of the ARC Centre of Excellence in Population Ageing Research in Sydney, Australia. She is now a Visiting Fellow there.