STOCKHOLM SCHOOL OF ECONOMICS
Department of Economics
5350 Master's Thesis in economics
Academic Year 2019-2020

Sudden Death Aversion: Does It Exist and Can Something Be Done About It?

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Abstract. This thesis investigates a bias termed sudden death aversion, focusing on the existence of the bias and dealing with the problems it causes. Sudden death aversion is defined as the tendency to avoid options which include a higher likelihood of immediate loss, e.g. due to an excessive focus on the likelihood of immediate loss instead of focusing on the ultimate objective, even if it would be payoff superior compared to other options. Thus, sudden death aversion can cause individuals to be worse off. Sudden death aversion is investigated using a simple card game experiment with real-world financial incentives to choose the payoff superior option. Two interventions aiming to mitigate sudden death aversion are considered. It is found that individuals do exhibit sudden death aversion. It is not found that being explicit about the payoff superiority of the payoff superior option is effective at mitigating sudden death aversion. However, it is found that being explicit about the proposed bias is effective at mitigating sudden death aversion. Thus, this thesis has two main findings. Firstly, it finds that sudden death aversion is exhibited in this context. Thus, it is not simply an artefact of prior experiments lacking financial incentives to choose the payoff superior option. Secondly, it finds evidence in favour of the efficacy of an intervention to somewhat deal with the problems sudden death aversion causes.

Keywords: Sudden Death Aversion, Decision Making Under Risk, Perceived Risk, Myopic Loss Aversion, Preregistered Experiment

JEL: D03, D90, Z29

Supervisor: Magnus Johannesson
Date Submitted: 15 May 2020
Date Examined: 25 May 2020
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I Introduction

[1]The purpose of this thesis is to firstly investigate if people choose a payoff inferior option in a simple card game experiment. Secondly, this thesis investigates if numerically informing participants of this payoff inferiority or if being explicit about the proposed bias leading to choosing the payoff inferior option reduces the likelihood of choosing the payoff inferior option. These two goals are investigated using a preregistered randomized experiment[2].

The card game used in the experiment is identical to one used by Walker *et al.* (2018). It consists of participants choosing between playing one of two card games. The first involves attempting to win a single less likely random draw of a card from a deck of playing cards, while the second involves attempting to win two identical, i.e. with replacement, more likely random draws of a card from a deck of playing cards. Thus, the first game can be described as a so-called "fast" option, while the second game can be described as a so-called "slow" option. The experiment is designed such that the "fast" option has a higher likelihood of ultimate victory, but also a higher likelihood of immediate defeat for the participant. Walker *et al.* (2018) term the bias causing participants to choose the "slow" inferior option as *Sudden Death Aversion* (SDA). They propose that SDA is, at least in part, caused by myopic loss aversion. Of note here is that the myopic loss aversion explanation does not involve a misestimation of probabilities.

The experiment this thesis conducts has three separate treatment conditions. In each condition, participants are asked to choose and play one of the two games with monetary incentives incentivizing participants to choose the option that yields the highest probability of winning. The differences between the conditions come from the information presented to the participants. In each case, the game is explained in full. In one condition, participants are explicitly told the ultimate probabilities of winning and in another they are told about the proposed bias. The final condition is a control condition where neither of these pieces of information are presented. The control condition is termed Condition 1 and is essentially a reproduction of an experiment conducted by Walker *et al.* (2018). The condition where participants are explicitly told the probability of ultimate success is termed Condition 2 and differs from the closest experiment conducted by Walker *et al.* (2018) by including a financial incentive to choose the payoff superior game[3]. The condition where the proposed bias is told to participants is termed Condition 3 and Walker *et al.* (2018) do not conduct a similar experiment.

The phenomenon under investigation was originally motivated by observations of seemingly suboptimal choices made in sports settings. One such example comes from basketball. In the National Basketball Association[4] (NBA), a win in regulation or a win in overtime and a loss in regulation or loss in overtime are of equal value in terms of standings. As such, there is no difference in the standings if

[1]I would like to thank my supervisor, Professor Magnus Johannesson, for all of the helpful discussions we had throughout this project. His guidance has been of immense value. Additionally, I would also like to thank everyone else who supported me in any manner during this project. All of the support has been greatly appreciated. All errors are my own.

[2]The study was preregistered at the Open Science Framework (preregistration available from `https://osf.io/h56tq`). The content of the preregistration is also presented in Exhibit 5 in the appendix. The study was preregistered to increase the credibility of the results, by making it clear to the reader that the findings were not a consequence of, e.g., p-hacking. This is particularly important in light of recent evidence that a substantial portion of published scientific findings fail to replicate in many fields (Ioannidis, 2005), including financial economics (Harvey *et al.*, 2015; Hou *et al.*, 2018) and psychology (Open Science Collaboration, 2015).

[3]Walker *et al.* (2018) included financial incentives to choose the payoff superior option in their version of Condition 1, but not in their version of Condition 2.

[4]This is the most prestigious basketball league in the world.

the result is determined in regulation or in overtime. SDA comes into play when a team with possession is losing by two points with very little time remaining. This essentially means that the losing team has to decide between attempting a three-point shot to win or lose the game right away or attempting a two-point shot to force overtime and then trying to win in overtime. The three-point shot is less likely to be successful than the two-point shot. However, if the two-point shot is successful, the team must still win in overtime. As such, the three-point shot option is analogous to the "fast" option and the two-point shot option is analogous to the "slow" option. In this setting, going for the three-point shot seems to be the option with the higher likelihood of winning the game, but teams choose it only roughly 30% of the time (Walker *et al.*, 2018)[5].

Another example comes from the National Football League[6] (NFL). Similarly to the NBA, a win in regulation or win in overtime and a loss in regulation or loss in overtime are of equal value in terms of standings. After a touchdown, a team can choose to attempt a one-point conversion or a two-point conversion. If a team is initially losing by seven points and scores a touchdown, this team is now losing by one point. If very little time is remaining, choosing to attempt a one-point conversion is an attempt to force overtime while attempting a two-point conversion is attempting to win or lose right away. As such, the two-point conversion is analogous to the "fast" option and the one-point conversion is analogous to the "slow" option. In this setting, it is less clear if the "slow" option is inferior, thus SDA may not cause a problem in this context. This is due to the situation being uncommon and the aversion to attempting a two-point conversion being large. Consequently, there is a lack of power when investigating which option is superior (Walker *et al.*, 2018). However, the point estimate suggests that choosing the "fast" option is superior (Walker *et al.*, 2018). Similar situations exist in other sports such as golf or tennis, where rather than there being little time remaining, the choice is the last action before a potential playoff or tiebreak, respectively.

However SDA is not confined to only sports domains[7]. This is because there are any number of areas in which there are negative effects if people shun options which are optimal in terms of a stated goal and instead choose options which have both a lower likelihood of immediate loss and a lower ultimate expected payoff. Essentially, this is relevant to any context in which people have to make choices between options which can be characterized as "fast" or "slow" in a similar manner as the card game which is used. These include sports, the military, and business as highlighted by Walker *et al.* (2018). In business, Walker *et al.* (2018) highlight that SDA can effect a firm deciding between continuing with their current operations or taking a substantial risk by innovating. They suggest that this could be a partial explanation for the fall of Blockbuster and Kodak[8]. Further examples could be thought of in any number of areas such as health care. As there are substantial real damages from making poor choices, it is of interest to study when people may behave poorly in this manner and how this behaviour could be

[5]Thus, from a perspective of what is the best option to win the game, the teams are seemingly paying a price for their reluctance to choose the three-point shot. This ignores off-court factors, such as social stigma or criticism one may face due to opting for one choice over the other.

[6]This is the most prestigious American football league in the world.

[7]However, even if sports settings were the only applicable context, SDA would arguably still be of interest. This is because there are a large number of emotionally invested fans who want their favoured sports team to make the right decisions and a large number sports organizations which benefit from better results, all else equal. As such, even in this domain, there are substantial number of individuals who want better decisions to be made by at least of subset of decision makers and this research could be beneficial in informing these better decisions.

[8]It is not claimed here that one can highlight an event like the fall of Blockbuster and say that SDA explains this event. Rather the claim here is that the story is plausible and that there likely exist cases of firms failing to adapt due to SDA, even if we can't necessarily point to them.

modified to make people choose the best options available to them.

To the best of my knowledge, Walker *et al.* (2018) wrote the only published paper[9] which experimentally investigates SDA. This thesis extends their research in three main ways. Firstly, Condition 1 is a reproduction of an experiment conducted by Walker *et al.* (2018).

Secondly, when they investigated whether informing participants of the ultimate probabilities of success reduced the likelihood of choosing the inferior game, the payoff to the participants was not tied to the choice made, instead participants were paid a flat rate for answering the survey. As a result, there was no clear incentive for participants to choose the game, which if played, would yield the greater expected payoff. This lack of an incentive to choose the superior option calls into question the reliability of the prior finding. This thesis includes financial incentives to choose the payoff superior option. The ex-ante expectation of this thesis was that SDA was driven by a lack of a clear incentive to choose the payoff superior option, at least when there is no ambiguity of the payoff superiority of the payoff superior option.

Thirdly, this thesis introduces a new nudge to attempt to mitigate SDA. This nudge informs participants of the proposed bias prior to them making their decision. To the best of the my knowledge, this nudge is novel in this context. Furthermore, it has an attractive feature. In most cases where this research could be relevant, the true probabilities of success are not known for certain. As a result, informing people about the true probabilities of success may not be a practical nudge. This is because the information required can be too uncertain for the designer of the choice architecture to use or decision makers may be overly sceptical about the information due to perceived uncertainty. As a result, informing decision makers about the proposed bias may be a more practical nudge in some cases[10].

Roughly speaking, it was expected that in the control condition a substantial portion of participants will choose to play the payoff inferior game. It was also expected that giving participants explicit information about the probabilities of ultimate success will substantially reduce the proportion of choices to play the payoff inferior game. Furthermore, it was expected that the choice to play the payoff inferior game will be less common than in the control case when information about the proposed bias is presented. Finally, it was also weakly expected that explicit information about the likelihoods of ultimate success will be more effective in reducing the proportion of those choosing the payoff inferior game than being explicit about the proposed bias.

The perspective taken in this thesis is that once a bias has been identified, one ought to consider several questions. These include if the bias causes a problem, if anything can be done to mitigate this problem, and if anything should be done to mitigate this problem. However, addressing all of these questions is beyond the scope of this thesis. As such, this thesis investigates if a bias exists in a setting where it is quite clear that a problem is caused if it does. In addition to this, this thesis investigates whether two interventions can resolve this problem. It is beyond the scope of this thesis to rigorously investigate if these interventions increase the value of an appropriate objective function. However, it is argued that if either of these interventions is effective, they are also likely increasing the value of an appropriate objective function. A substantial portion of the motivation for this thesis is to investigate what can be done to improve the decision making of individuals if they suffer from SDA. Thus, in the

[9]There also exists an unpublished working paper by Richard Thaler originally from the 1980s, revised in 2000, which introduced SDA (Walker *et al.*, 2018), but I could not find it.

[10]E.g., these cases include cases where there is a substantial amount of uncertainty regarding the probabilities of success of various options.

background of this thesis is always the goal of improving outcomes for individuals. This is particularly important due to the large potential for damages to individuals caused by SDA as highlighted above. As such, this thesis has motivations similar to papers concerned with improving retirement savings systems (Thaler & Shlomo, 2004), improving financial outcomes through financial literacy education (Hastings *et al.*, 2013), and addressing individual-level health issues (Volpp *et al.*, 2008, 2009; Charness & Gneezy, 2009).

To summarize the findings of this thesis, it is found that SDA is exhibited even when there are monetary incentives to choose the payoff superior option. However, at a substantially lower level than found by Walker *et al.* (2018). Additionally, this thesis finds statistically significant evidence suggesting that the nudge informing participants of the proposed bias is effective at decreasing the proportion of participants choosing the payoff inferior game. This thesis does not find statistically significant evidence suggesting that the nudge informing participants of the ultimate probabilities of success is effective at decreasing the proportion of participants choosing the payoff inferior game.

This thesis proceeds with Section II containing a discussion of relevant literature. This is followed by Section III describing the design of the experiment and the analysis that is conducted. Then, Section IV presents the empirical results of the experiment. Next, Section V places these results into context and discusses the interpretations of these results. Following this, Section VI presents and discusses several limitations of this research and avenues for further research. Finally, Section VII concludes.

II Literature Review

The experiment used in this thesis is motivated by Walker *et al.* (2018). The experiment is explained in detail in Section III.I[11]. For the purposes of this section, it is essential to understand that Game 1 in the experiment is a one-stage game providing the participant with the same payoff in the event of winning the game as the two-stage Game 2 does, Game 1 gives the participant a greater probability of winning than Game 2, and Game 1 has a higher probability of immediate loss than Game 2. As it is argued that choosing to play Game 2 instead of Game 1 is a poor choice, the reasons for why participants might opt to play the payoff inferior Game 2 should be laid out. The tendency to avoid options that include a relatively large probability of immediate loss (sudden death) is referred to as SDA. In this thesis, SDA is associated with choosing expected payoff inferior options, but this need not be the case in all settings. This is because the "slow" option can be the payoff superior option in some settings.

This thesis highlights five main reasons for SDA. The first three are highlighted and the fourth is implicitly acknowledged by Walker *et al.* (2018). The five reasons are: 1. incorrectly calculated/believed probabilities, 2. (quasi-)magical thinking, 3. myopic loss aversion, 4. misunderstanding the game, and 5. deriving sufficiently more enjoyment from playing the two-stage card game compared to the one-stage card game. These mechanisms are now explained in more detail.

Considering the first reason, it is clear that if the participant is using incorrect probabilities of winning the two games when deciding which game to play in such a way that the participant thinks that Game 2 has a larger probability of success than Game 1, the participant can justifiably choose to play Game 2 given their beliefs. This reason driving Game 2 selections is effectively a lack of simple mathematical ability or effort on the part of the participant. It is already known that people tend to overestimate

[11] For clarity to the reader, it may be of use to read Section III.I prior to reading Section II.

the likelihood of conjuctive events and underestimate the likelihood of disjunctive events (Bar-Hillel, 1973; Tversky & Kahneman, 1974). Thus, if one wants to point to SDA as a new bias rather than a different interpretation for people overestimating the likelihood of conjunctive events occurring, SDA should be driven by more than just incorrect beliefs about probabilities. Walker *et al.* (2018) find that incorrect beliefs about the probabilities of winning are not the only nor the primary reason participants choose to play Game 2. They find this by conducting experiments using conditions where the probabilities of success are left to be calculated by participants and where they are explicitly told to participants. By explicitly informing participants about the probabilities of winning, one can control for incorrect beliefs about the probabilities of winning. Walker *et al.* (2018) find that there is something more than just overestimating the likelihood of conjunctive events occurring going on here[12].

The second reason presented is magical or quasi-magical thinking. Shafir & Tversky (1992) define magical thinking as an incorrect belief of being able to influence the outcome of an event with an irrelevant action. The example they highlight is believing that thinking about a particular number prior to rolling a die influences the outcome of the roll of the die. They then define quasi-magical thinking as behaving as if they hold this incorrect belief even though they do not hold this incorrect belief. Magical and quasi-magical thinking have been used to attempt to explain why people vote and why people cooperate in labour unions (Shiller, 1998).

This is relevant in the context of this thesis if, as Walker *et al.* (2018) highlight, people believe that opting for a "fast" option instead of a "slow" option lowers the probability that the "fast" option leads to success as it is a form of "tempting fate"[13]. As the game played here is a card game, it is arguably unlikely that participants fall victim to magical thinking, but they may fall victim to quasi-magical thinking when choosing between Games 1 and 2. However, similarly to the first reason for Game 2 selections, this is a mistake in estimating probabilities. Thus, when the probabilities of winning are explicitly laid out for participants it is arguably unlikely that this would drive choices to play Game 2.

The third reason for SDA is myopic loss aversion. This is the reason favoured by Walker *et al.* (2018). Myopic loss aversion combines both loss aversion and narrow framing. Loss aversion is the phenomenon where people weigh losses more than equally sized gains. In prospect theory, this is represented using a value function which is steeper in the domain of losses than it is in the domain of gains (Tversky & Kahneman, 1991). Typically, it is found that a multiplicative coefficient for loss aversion in the value function is between two and three (Tversky & Kahneman, 1991, 1992). Myopia refers to the short-sighted behaviour of narrow framing (Kahneman & Lovallo, 1993) or narrow bracketing (Read *et al.*, 1999). Both essentially refer to the behaviour of decision makers making choices in isolation and without considering decisions to be made in the future.

[12] In the control condition of Walker *et al.* (2018), roughly 61% of the participants choose the payoff inferior "slow" option. In the condition where they explicitly inform participants of the probabilities of success, roughly 54% of the participants choose the payoff inferior "slow" option. However, these were conducted as different experiments, with different contexts and similar but different likelihoods of success. As such, they do not and cannot conduct a clean test between these conditions, while this thesis can and does. The important takeaway is that they find that a substantial amount of SDA is exhibited, even when presenting the likelihoods of success.

[13] Consider the basketball example where one is losing by two points with little time remaining and compare it to a case where one is losing by three points. If one is losing by two points, both the two-point and the three-point shot are on the table and as such people may view choosing the three-point shot as "tempting fate". If one is losing by three points, only the three-point shot is on the table and as such it seems unlikely that choosing the three-point shot would be seen as "tempting fate" in this context.

Myopic loss aversion is then the tendency to use a short evaluation period when making a choice causing a loss to be more likely to be observed (Benartzi & Thaler, 1995; Thaler, 1999). Myopic loss aversion has been used to explain, e.g., the equity premium puzzle (Benartzi & Thaler, 1995) and that investors are less willing to accept risks if they have shorter evaluation periods (Thaler *et al.*, 1997; Gneezy & Potters, 1997). These examples show how myopic loss aversion can arguably cause decision makers to make poor decisions.

Walker *et al.* (2018) provide a simple example attempting to show how myopic loss aversion can lead to SDA. The example is unsatisfactory, but is included here for the sake of context. In the example, the decision maker is presented with a choice between two options. The first option gives a one-third chance of winning and a two-thirds chance of losing immediately. The second option gives a one-half chance of proceeding to a second round and a one-half chance of losing immediately. In the second round, there is a one-half chance of winning and a one-half chance of losing. The value function a decision maker uses in this example takes a value of 1 for a win, a value of -2.5 for a loss, consistent with equal magnitude monetary gains and losses in the events of winning and losing with a loss aversion parameter of 2.5, and a value of 0 for proceeding to the second round, attempting to represent shortsighted behaviour. Using simple calculations, they then show that both loss aversion and shortsightedness are required to justify choosing the second option, i.e. exhibiting SDA, if the goal is maximizing the expectation of the value of the choice.

The above example is contrived, but it is used by Walker *et al.* (2018) to justify SDA being caused by myopic loss aversion. However, to be clear, it does not really apply to the card game used in this thesis. As the same card game is used by Walker *et al.* (2018), it does not really apply to their setting either. This is because in this card game experiment no participant incurs a monetary loss, but rather loses the game receiving no monetary payoff and loses the opportunity to win a monetary payoff. Consequently, the loss aversion portion of the myopic loss aversion explanation used by Walker *et al.* (2018), in the context of this card game experiment, is somewhat different from what loss aversion is typically used to mean. Furthermore, the myopic portion of myopic loss aversion is not exactly the same as in prior literature. This is because, in this context, it is not a shortsighted focus on one decision when multiple decisions should be considered, but rather a shortsighted focus on the aspects of one decision. Thus, in the context of this experiment myopic loss aversion refers to a different phenomenon than it typically does. Specifically, it is used to refer to a disproportionate focus on the potential immediate loss rather than focusing on the end outcome. The reason this thesis refers to this as myopic loss aversion is to use the same terminology as Walker *et al.* (2018), but it is useful to keep in mind that it is not the typical definition of myopic loss aversion[14]. However, the definition used in this context has a similar feel to it in that it considers a shortsighted focus on losing.

The fourth reason for SDA is that participants may choose the payoff inferior option if they misunderstood the instructions presented to them in ways that make Game 2 more attractive than Game 1. It is common for experiments to attempt to minimize this problem by including questions designed to ensure that participants understood the instructions. This thesis did so as did Walker *et al.* (2018). However, one can never be fully certain that every participant has really understood the instructions.

The fifth reason for SDA is that participants may gain utility from the act of playing a game. This utility gain may be larger in Game 2 than in Game 1, and this difference may be large enough to make

[14]The fact that a different definition for myopic loss aversion was used is not highlighted by Walker *et al.* (2018).

up for the fact that Game 2 is payoff inferior. If this is the reason for Game 2 selections, these selections are utility-maximizing and the choice is not a poor choice. Of note here is that it is not clear that Game 2 should be more enjoyable to participates than Game 1. If there is such a difference in utility derived from playing the games, it could hypothetically favour either game. Furthermore, the way in which playing Game 2 physically differs from Game 1 in the experiment conducted in this thesis is that at most one additional click of a button asking the participant to proceed to the next page of the survey occurs in Game 2. There are no animations or other potential attractions besides being potentially able to click to proceed to the next page once more than in Game 1. Thus, the experiment is structured in a "boring" manner in an attempt to minimize this motivation to influence the choice of which game to play[15].

To conclude this discussion about the reasons for SDA, it is useful to note that because the first two reasons involve incorrect probabilities of success, informing participants of the true probabilities of success controls for these motivations. The fourth motivation is arguably controlled for using questions designed to ensure that participants understand the instructions. Thus, once one informs participants of the true probabilities of success, of the reasons presented above, only the third and fifth reasons for SDA remain.

While the experiment conducted in this thesis is motivated by Walker *et al.* (2018), the experiment is not identical, as highlighted in Section I. Specifically the experiment conducted in this thesis improves upon the prior experiment in two ways. Firstly, it introduces a monetary incentive to choose the game with the higher probability of winning[16]. Secondly, it introduces an alternative nudge to improve decision making. These extensions are discussed in more detail below.

The first extension of introducing a monetary incentive to choose the game with the higher probability of winning is done by entering everyone who wins in the experiment into a lottery to win a real-world sum of money. This extension has the flavour of increasing stakes in experiments. It has been shown before that increasing stakes in an experiment may influence behaviour to become more in line with economic theory in the ultimatum game. The extent of this effect varies from a modest effect (Cameron, 1999) to a quite large effect (Andersen *et al.*, 2011).

However, the introduction of the monetary incentive in this experiment is conceptually more than just increasing the stakes in the experiment. This is because to make the argument that SDA leads to poor choices, the resulting choices have to be credibly argued to be poor choices. Without a clear incentive to choose to play Game 1, it is difficult to argue that choosing to play Game 2 is a poor choice. This is because without the monetary incentive to attempt to win in the experiment, it is not clear that a higher probability of winning in the experiment is what increases utility for participants. It is plausible that without monetary incentives, participants simply do not care about winning or losing in the experiment. Furthermore, it is possible that participants could view proceeding to the second round in Game 2 as a win and as such without monetary incentives it could be utility maximizing to choose

[15]Strictly speaking, participants do not know that this is how the experiment is constructed when deciding which game to play. However, no indication otherwise is presented to the participants and the alternative to inform the participants of the "boring" nature of the game could have decreased the number of people completing the experiment. This hypothetical decrease need not be driven by decreasing the expected utility from the fun of playing the game, but rather from the reactions people would have to the explanation that would then use words like bland or boring to describe what the participant will be doing. Furthermore, it would have increased the amount of reading the participant would have to do, which could also decrease the number of participants completing the experiment. As such, it was decided that no such explanation ought to be included. This is motivated by the idea, often expressed by Richard Thaler in various forms, that if you want to get someone to do something, you should make it easy for them to do it.

[16]To be clear, Walker *et al.* (2018) included this incentive in their control condition, but not elsewhere.

Game 2. This thesis posits that this plausible motivation or similar motivations are likely small and dwarfed by introducing non-trivial monetary incentives into the experiment.

In parts of their research, Walker *et al.* (2018) paid a constant sum to participants, i.e. the monetary payoff to participants was not a function of any decision made in their experiment. This does not apply to their control experiment. However, it does apply to the condition when they inform participants about the ultimate probabilities of success. As such, their results are arguably not convincing. In this thesis it was chosen that participants would be entered into a lottery to win a monetary payoff if they win in the experiment. Doing this makes the expected monetary payoff a function of the probability to win in the experiment. This introduces a clear incentive to choose the game with a higher probability of winning the game. This extension adds credibility to the results of this thesis, credibility which was absent in the results found by Walker *et al.* (2018).

The second extension is the introduction of an alternative nudge to explicitly informing participants of the probabilities to win in the games. It is useful to define a nudge here. Thaler & Sunstein (2008) define a nudge as a part of the choice architecture which predictably changes behaviour while not removing options or changing economic incentives substantially. The introduction of an alternative nudge may be beneficial as, in the field, the probabilities of success required for the first nudge are not necessarily known in the way they are in a card game experiment. As highlighted in Section I, this may make the nudge impossible to implement or decision makers may disbelieve estimated probabilities. Both of these could cause that nudge to be ineffective. However, one still must know which option is the superior option to use this alternative nudge well. Thus, the alternative nudge still has an informational requirement, but a lesser one than the first nudge.

However, an alternative nudge does not have value just because another nudge is impractical. Here it is useful to distinguish between a nudge being effective and a nudge being efficient. The following definitions are rough simplifications which obscure some details, but they will suffice for this discussion. A nudge is effective if it alters behaviour as predicted. A nudge is efficient if it is the best alternative to increase the value of the objective function. Thus, nudges can be welfare improving without being efficient as another nudge which improves welfare by a greater amount may exist.

There has been substantial discussion about policy interventions in behavioural settings. Shleifer & Summers (1990), in the context of noise trading in financial markets, highlight that it is important to consider if anything ought to be done to attempt to fix behavioural biases either for the sake of the agent exhibiting the bias or for the sake of others. Thus, it is not enough to find an effective nudge, one may want to attempt to find a nudge that is efficient or at least a nudge that increases the value of an appropriate objective function. An example of a welfare evaluation of a nudge has been conducted by Allcott & Kessler (2019). They highlight that nudges are often deemed to be successful if they substantially alter behaviour at a low implementation costs, but that this thinking misses critical costs. They evaluate the welfare implications of a nudge designed to reduce energy consumption. They find that whilst it improves welfare, these welfare gains are less than previously estimated for that nudge once one considers costs that those affected by the nudge incur. Glaeser (2006) further highlights several reasons why a nudge that is effective at changing behaviour may not be desirable. These include that nudges can cause emotional costs to those affected and that in a dynamic setting nudges may increase the probability that policies which decrease freedom are implemented in the future.

The above has highlighted that a nudge which is effective need not be worth implementing. This is

done to motivate the nudges considered whilst acknowledging the incomplete nature of this thesis. The second nudge informs the participants of the proposed bias. This is a low cost nudge as it is essentially just one additional paragraph for the participant to read. However, this thesis does not conduct any welfare analysis and as such this thesis essentially only considers if this nudge is effective at changing behaviour, not if it is efficient nor if it increases the value of any objective function. Consequently, no definitive claim saying that one should implement either nudge considered in this experiment is made. This said, the two nudges considered in this thesis are designed to be very low costs nudges, in terms of both implementation and expected emotional costs, and as such it is argued that if they do substantially change behaviour in the direction to lessen SDA, they are likely, but not necessarily, increasing the value of some appropriate objective function.

III Research Design

III.I Experimental Design

The experiment was conducted using a Qualtrics survey[17].

The experiment consists of participants choosing between playing one of two possible games. These games are referred to as Game 1 and Game 2. These two games are described below:

Game 1: The participant draws a card from a shuffled regular 52-card deck of playing cards. If the drawn card is a Jack, Queen, King, or Ace, the participant wins. Otherwise the participant loses. If the participant wins, they are entered into a lottery and the experiment is over. If the participant loses, the experiment is over.

Game 2: The participant draws a card from a shuffled regular 52-card deck of playing cards. If the drawn card is an 8, 9, 10, Jack, Queen, King, or Ace, the participant proceeds to another round. Otherwise the participant loses. In the next round, the participant draws a card from a shuffled regular 52-card deck of playing cards. If the drawn card is an 8, 9, 10, Jack, Queen, King, or Ace, the participant wins. Otherwise the participant loses. If the participant wins, they are entered into a lottery and the experiment is over. If the participant loses, the experiment is over.

The lottery used in this experiment[18] is a lottery wherein each winner in the experiment has an equal chance of winning 1000 SEK. Thus, winning in Game 1 is identical to winning in Game 2. Three win-

[17] Qualtrics is a survey tool that allows the creation of an anonymous survey with randomization between respondents and within the survey. These surveys can be spread using a conventional link online.

[18] Using a lottery as a payment method was chosen in an attempt to provide a strong motivation for students to participate in the experiment given a budget constraint in running the experiment. The lottery achieves this by taking advantage of the probability weighting function in prospect theory. Even though in general people are risk-averse for gains according to prospect theory, this does not apply for very low probability gains. This is due to the probability weighting function overweighting very low uncertain probabilities. As a result, prospect theory gives rise to a fourfold pattern of risk-attitudes where people are risk-averse for high probability gains and low probability losses and risk-seeking for low probability gains and high probability losses (Tversky & Kahneman, 1992). This risk-seeking behaviour for low probability gains can explain preferences for holding assets with positively skewed expected returns (Barberis & Huang, 2008). Similarly, it can explain some gambling behaviour (Tversky & Kahneman, 1979). Consequently, people may have a preference for gambles where with a high probability there is no gain nor loss and with a low probability there is a large gain when compared to a choice where you receive the low expected value of the aforementioned gamble. Thus, using a kind of lottery as used here can provide potential participants a relatively strong incentive to participate in the experiment.

ners are randomly drawn in this lottery.

These games are explained to the participants in detail. The exact information presented to the participants is presented as Exhibit 1[19] in the appendix.

The probability of winning in Game 1 is $4/13$. The probability of winning a round in Game 2 is $7/13$. Thus, the probability of winning Game 2 is $49/169$. As $4/13 = 52/169 > 49/169$, the likelihood of ultimate victory is higher in Game 1 than in Game 2. The probabilities of immediate defeat, i.e. defeat after the first draw, are $9/13$ and $6/13$ in Games 1 and 2 respectively. Thus, Game 1 is called a so-called "fast" game where the probabilities of both ultimate victory and immediate loss are larger than in Game 2. Conversely, Game 2 is called a so-called "slow" game where the probabilities of both ultimate victory and immediate loss are smaller than in Game 1.

Participants are randomly assigned into one of three conditions. These conditions are referred to as Condition 1, Condition 2, and Condition 3. The random assignment to any one of these conditions occurs immediately when the participant opens the survey on their device. All participants are equally likely to be assigned to any of the three conditions. The only differences between the conditions are that Conditions 2 and 3 each have one additional paragraph of information given to the participant. The conditions are described below:

Condition 1[20]: Condition 1 is the control condition. The participants are welcomed and introduced to the experiment. They are then asked for their email information in case they win and are selected to be paid. The experiment is then described to them. The games are then described and the participants answer a few understanding and clarifying questions. These questions must be answered correctly to proceed. The participant is then asked to choose between playing Game 1 and Game 2. The chosen game is then played and the participant learns if they have won or lost. The experiment is then concluded.

Condition 2[21]: Condition 2 is identical to Condition 1 with the sole exception that a paragraph informing participants that playing Game 1 yields a 31% chance of winning and playing Game 2 yields a 29% chance of winning. This paragraph is presented just above the choice of playing either Game 1 or Game 2.

Condition 3[22]: Condition 3 is identical to Condition 1 with the sole exception that a paragraph informing participants of the suggested bias at play is presented. This paragraph is presented just above the choice of playing either Game 1 or Game 2.

The exact wording that participants see in Condition 1 is displayed in Exhibit 1 in the appendix.

[19]To be explicit, Exhibit 1 shows everything that a participant in the control condition would see. The explanation participants in other conditions would see is identical except that there is an additional informative paragraph in the other two conditions.

[20]Condition 1 is a reproduction of an experiment conducted by Walker *et al.* (2018). The same game is used and the most notable difference is the payoff structure, but in both Condition 1 and the analogue conducted by Walker *et al.* (2018) there was a financial incentive to choose the payoff superior game.

[21]The main difference between Condition 2 and the analogue conducted by Walker *et al.* (2018) is that in Condition 2 there is a financial incentive to choose the payoff superior option, while in the analogue conducted by Walker *et al.* (2018) there was not.

[22]Condition 3 introduces an intervention that was not considered by Walker *et al.* (2018).

The additional paragraph that participants see in Condition 2 is displayed in Exhibit 2 in the appendix. The additional paragraph that participants see in Condition 3 is displayed in Exhibit 3 in the appendix.

The choice to display the additional paragraphs just above the choice to play Game 1 or Game 2 was chosen to minimize the distance between the nudge and the action taken by participants. This means that the questions to ensure that participants have understood the games come in between the description of the games and the nudge. There was a worry that having a large distance between the nudges and the choice may negatively impact the ability of the nudges to influence the participants' decisions. Initially, it may seem that the choice of putting the questions that ensure participants understand the games in between the description and the nudge may cause a lack of clarity. However, as the questions ensuring understanding of the games among participants are framed as clarifying questions, it is arguable that they are likely not separated from the description of the games in the participants' minds. As such, the contention here is that this choice does not cause a lack of clarity and that it does minimize the distance between the nudge and the action taken by participants, which may improve the effectiveness of the nudges.

It has been shown that there are cases where the framing of a situation can influence actions. Kahneman *et al.* (1986) show how increasing the list price of a car is perceived as more unfair than removing an equivalent discount. Tversky & Kahneman (1981) highlight that framing choices in terms of lives saved or people dying influences what choices people make. More generally, framing is important in prospect theory as value is measured using gains and losses relative to a reference point (Tversky & Kahneman, 1979, 1992). Consequently, the location of the reference point is critical and this can be influenced using framing. Thus, it is important to consider the framing choices made in this experiment.

Generally, the framing choices made in this experiment were made in a way that would make the findings comparable to Walker *et al.* (2018), however there are two decisions which warrant explicit discussion.

Firstly, Exhibit 2 shows the additional paragraph presenting the probabilities of success to the participants. It is arguable that the portion of this paragraph which is in parentheses is attempting to get participants to think about the chance of immediate loss and exhibit SDA. In a sense, this is similar to trying to get people to exhibit SDA. The only reason this portion is in the paragraph is because Walker *et al.* (2018) included this portion in their paragraph. Generally speaking, the paragraph presented in Condition 2 of the experiment conducted in this thesis is similar to the analogous paragraph used by Walker *et al.* (2018).

Secondly, the instructions used by Walker *et al.* (2018) in their card game experiment refer to the participant playing the game and attempting to beat a dealer. While there is no indication that this dealer is someone who stands to gain from any outcome, using the word "dealer" may cause participants to think of playing against another human. This could influence decisions. For example, this may bring up fairness considerations if participants think that them winning means that someone else loses. This is a consideration which is distinct from SDA and thus not of interest here. As such, the decision was made to remove all references to a dealer in this experiment. Admittedly, this does make the results found in this thesis somewhat less comparable to those found by Walker *et al.* (2018). However, if the results differ and even if this difference entirely is due to removing the references to a dealer[23], this

[23]Given the structure of the experiment conducted here, knowing if potential differences in findings are due to the removal of the references to a dealer is impossible. It would have been possible to have an additional treatment investigating if using the word "dealer" is important to the results. This was not done because it would lead to a loss in power as the sample sizes

experiment would lead to valuable information. This is because it would mean that, at the very least, the SDA explanation presented by Walker *et al.* (2018) is inappropriate.

III.II Data

The survey was emailed to all of the bachelor's and master's students at the Stockholm School of Economics. The email that was sent out to the students is presented as Exhibit 4 in the appendix.

Prior to emailing the survey to potential participants, this research was preregistered at the Open Science Framework (OSF). This preregistration is available from `https://osf.io/h56tq` and is displayed in Exhibit 5 in the appendix. The preregistration contains information about the context of the experiment, the experimental design, the hypotheses made, and the tests used to test these hypotheses. The study was preregistered to increase the credibility of this experiment by preventing issues relating to the analysis conducted being contingent upon the data.

The desired sample size was determined using power calculations. The cutoff for statistical significance used in this experiment is 0.005. With a power of 80% and an effect size corresponding to a Cohen's d of 0.5[24], the calculation yields a desired minimum sample size of roughly 109 participants per group. This calculation is for a two-tailed difference in two independent means from different groups. As such, the aim was to get at least approximately 109 participants in each group. However, as the survey is emailed to students, prior to collecting data it was conceivable that more observations could be achieved. It was decided that the survey would be open until a prespecified date[25] in the preregistration, all observations prior to that date would be used, and data collection would be extended if and only if the sample sizes were smaller than the desired minimum sample sizes. The achieved sample sizes were larger than the desired minimum sample sizes prior to this first prespecified date. As such, data collection was not extended.

By 15.01.2020, the survey had 618 total responses. Of these 618 responses, those which did not complete the survey or had the same email address as a prior response were removed from the data used to conduct the preregistered analysis. This ultimately led to a final usable sample size of 501 observations. Most of the unused observations were incomplete responses rather than responses from the same respondent[26].

As the participants were randomized, with equal chance, into the condition they were allocated to, the number of participants in each condition are not exactly equal to each other. In Condition 1, there were 180 participants. In Condition 2, there were 146 participants. In Condition 3, there were 175 participants.

III.III Hypotheses and Analysis

This thesis conducts six tests in total. Thus, this thesis suffers from the multiple testing problem. As mentioned in Section III.II, this study was preregistered. Furthermore, all of the statistical analysis

per condition would then be smaller.

[24]Corresponding to a "medium" effect size.

[25]The prespecified date was 15.01.2020.

[26]Of the 117 unusable observations, three categories can be identified. These are entirely blank responses, responses that put their emails in and nothing else, and complete responses from an email address which had already responded. Here it is highlighted that the first two categories are greater in number than the third category.

conducted is presented in this thesis[27]. Both of these are done to increase the credibility of the findings of this thesis. One further step taken with the same motivation is that the threshold for considering a finding as statistically significant is set to 0.005 following the recommendation of Benjamin *et al.* (2018)[28]. Results with p-values between 0.005 and 0.05 are referred to as suggestive evidence rather than being statistically significant findings.

All six of the tests conducted in this thesis are z-tests[29]. The first tests are tests which test if the proportions of Game 2 selections by participants are statistically significantly greater than zero. These are conducted for all three conditions. Thus, these tests constitute three of the six tests conducted in this thesis. These tests are conducted as one-sided tests, as specified in the preregistration, as the proportion of Game 2 selections cannot be less than zero.

The remaining tests are tests which test if the proportions of Game 2 selections by participants are statistically significantly different by condition. These tests are conducted for every pairwise combination of the three conditions, thus three tests are conducted. These tests are conducted as two-sided tests even though there are expectations about the expected directions of the effects.

The hypotheses for these tests are now laid out. Let p_i denote the proportion of participants choosing to play Game 2 in Condition i.

Hypothesis 1: The null hypothesis which is being tested is that $p_1 = 0$ and the alternative hypothesis is that $p_1 > 0$. The expectation is that the null hypothesis will be rejected. This means that it is expected that the proportion of participants choosing to play Game 2 will be statistically significantly greater than zero in Condition 1. This is in line with the findings by Walker *et al.* (2018), where 61% of participants chose to play Game 2 in their analogue of Condition 1.

Hypothesis 2: The null hypothesis which is being tested is that $p_2 = 0$ and the alternative hypothesis is that $p_2 > 0$. The expectation is that the null hypothesis will not be rejected. This means that it is expected that the proportion of participants choosing to play Game 2 will not be statistically significantly greater than zero in Condition 2. This is not in line with the findings by Walker *et al.* (2018) as in their nearest analogue to Condition 2 roughly 54% of participants chose to play the equivalent Game 2 and this was statistically significantly greater than zero. This thesis expects that such a large proportion of participants choosing the payoff inferior game is due to the lack of (monetary) incentives to choose the payoff superior game. Thus, when being explicit about the probabilities of success in the two games, it is expected that SDA will not be exhibited. This would be seen as the proportion of participants choosing to play the payoff inferior game being lower and in this thesis it is expected that it will not be statistically distinguishable from zero[30].

[27] Due to the nature of the research conducted here, this is relatively easy to do.

[28] This is similar to using a 0.05 threshold for statistical significance and then applying the Bonferroni correction for multiple testing using six tests.

[29] The sample sizes are large enough to invoke the central limit theorem, thus the means (or proportions in this case) of the outcome variable follow an (approximately) normal distribution. Thus, z-tests were used. This is identical to using Pearson's chi-squared tests. One could have used t-tests invoking the fact that the population standard deviations are unknown, but practically speaking this would make little difference.

[30] As one never accepts a null hypothesis, but rather fails to reject a null hypothesis, it is not claimed that the proportion is equal to zero if it cannot be statistically significantly distinguished from zero. Rather that it is just expected that the proportion will be low enough to the extent that one cannot statistically significantly find that it is greater than zero.

Hypothesis 3: The null hypothesis which is being tested is that $p_3 = 0$ and the alternative hypothesis is that $p_3 > 0$. Consistent with the preregistration, this thesis has no expectation of the outcome of this test. It is expected that informing participants about the suggested bias reduces the proportion of Game 2 selections (see Hypothesis 5), but this thesis does not have an ex-ante expectation on whether this will reduce the proportion of Game 2 selections to such an extent that it statistically indistinguishable from zero.

Hypothesis 4: The null hypothesis which is being tested is that $p_1 = p_2$ and the alternative hypothesis is that $p_1 \neq p_2$. The expectation is that the null hypothesis will be rejected. This means that it is expected that the proportion of Game 2 selections in Condition 1 will be statistically significantly different from the proportion of Game 2 selections in Condition 2. Directionally, it is expected that the proportion of Game 2 selections will be lower in Condition 2 than in Condition 1, although this test is conducted as a two-sided test. This expectation is driven by the expectation that informing participants about the probabilities of success in the two games will be effective in getting people to choose the payoff superior game.

Hypothesis 5: The null hypothesis which is being tested is that $p_1 = p_3$ and the alternative hypothesis is that $p_1 \neq p_3$. The expectation is that the null hypothesis will be rejected. This means that it is expected that the proportion of Game 2 selections in Condition 1 will be statistically significantly different from the proportion of Game 2 selections in Condition 3. Directionally, it is expected that the proportion of Game 2 selections will be lower in Condition 3, although this test is conducted as a two-sided test. This contrasts with the lack of an expectation in Hypothesis 3. This is because it is expected that the proportion of Game 2 selections in Condition 3 will be reduced compared to Condition 1 as it is believed that being explicit about the suggested bias will reduce Game 2 selections[31], but this thesis does not ex-ante take a stand if this will reduce the proportion of Game 2 selections to a level indistinguishable from zero.

Hypothesis 6: The null hypothesis which is being tested is that $p_2 = p_3$ and the alternative hypothesis is that $p_2 \neq p_3$. It is weakly expected that the null hypothesis will be rejected. This means that it is weakly expected that the proportion of Game 2 selections in Condition 2 will be statistically significantly different from the proportion of Game 2 selection in Condition 3. Directionally, it is expected that the proportion of Game 2 selections will be lower in Condition 2, although this test is conducted as a two-sided test. This expectation is driven by a belief that SDA is more driven by incorrect probabilities, owing from, e.g., a lack of mathematical effort or ability, than by a form of myopic loss aversion.

The above discussion of the hypotheses has implicitly assumed that the differences between the conditions are, in some sense, causal effects of the nudges. To identify the causal effects of the nudges, this experiment relies on the randomization. Angrist & Pischke (2009), using the potential outcomes framework, explain well how randomization of treatment can allow an experiment to get an unbiased estimate of the average treatment effect of a treatment in the trial population. The unbiased estimate of

[31] It is not true that knowing about a bias necessarily reduces the effects of said bias. Barberis & Thaler (2003) highlight that it is the case that once a bias is explained to and understood by individuals and a new choice is immediately presented, individuals often will not correct their behaviour. However, this need not be the case in all settings and it is conceivable that knowing about a bias will reduce the effect of said bias, particularly if the learning and decision making are not separated by a long period of time.

the average treatment effect is the difference in means of the outcome variable between the groups.

It is possible that the three conditions end up with participants who differ from each other in meaningful ways. This could cause a false positive result. It would have been possible to conduct a test attempting to test if participants in different conditions are similar on some observable characteristics. This would have involved the survey including questions asking about some observable characteristics of the participants, such as the sex of the participant or degree program the participant is enrolled in. This information could then be used to conduct a test attempting to test if the participants in each condition match each other in terms of their observable characteristics. However, this was not done for three main reasons which are explained below.

Firstly, these tests would have been meaningless. Begg (1990) highlights that such a test is essentially a test of a null hypothesis that is known to be true as we know that treatment is randomly assigned and, as a result, any rejection of the null hypothesis would by definition be a false positive result. This sentiment is echoed by others (Mutz *et al.*, 2017; Deaton & Cartwright, 2018). This alone is reason enough not to include these tests. However, this only shows that there is no benefit to doing so. The next two reasons highlight that there could also be a substantial cost to including tests like these.

Secondly, it was deemed important that the survey be quick and easy to fill out in order to get a large enough sample size in order to be able to find meaningful results. Adding questions about observable characteristics of the participants would have lengthened the survey and consequently likely decreased the sample size achieved.

Thirdly, the experiment was conducted in a somewhat anonymous[32] manner. If questions asking about observable characteristics of the participants would have been asked, the experiment would have been less anonymous. This may change the behaviour of participants. That would not be desirable as it may change the behaviour of participants to, e.g., behaviour that the participants think the experimenter would like to observe rather than what they would more naturally do. In the context of this thesis, behaviour intended to please the experimenter is not of interest and consequently was not desired.

As such, including questions that would make these tests possible would bring no benefit at a potentially substantial cost. Furthermore, if there is no post-randomization selection, the samples are more likely to be to provide appropriate counterfactuals for each other the larger the sample size is (Deaton & Cartwright, 2018). Thus, it was decided that it is better to focus on increasing the sample size achieved. Finally, these tests would essentially only be appropriate for investigating if participants systematically broke the random allocation mechanism[33] (Deaton & Cartwright, 2018). Based on the above reasons, it was determined that it would be best to not conduct these tests.

This discussion is included here to justify the choice not to conduct these tests even though they are not uncommon in the literature. These tests are conducted in economics literature (Banerjee *et al.*, 2015) and elsewhere, e.g., in the medical literature as Assmann *et al.* (2000) find that they are conducted in half of the medical trials they investigate. The prevalence of these tests in the medical field has prompted guidelines specifying that these tests are not valuable (Moher *et al.*, 2010). To be clear, this is not necessarily researchers themselves wanting to conduct these tests, these kinds of meaningless balance

[32] As can be seen from Exhibit 1, emails were collected, so the experiment was not fully anonymous. However, care was taken to convince participants that these emails would not be used for any purpose other than payment purposes.

[33] In its most simple form, this would be participants somehow knowing what condition they are in and then restarting the survey to get into the condition they desired. This would appear as incomplete surveys that could be identified using email addresses. Looking at the data, there is no suggestion that this is a problem.

tests can be required by some journals if a researcher wants to publish an experimental paper (Mutz *et al.*, 2017).

If there were some covariate which was thought to be important to the treatment effect, e.g. due to prior literature, one can argue that one should include that covariate in the analysis to increase the precision of the results[34], however the choice of including it in the analysis should not be dependent upon whether or not the groups seem balanced (Begg, 1990; Berk *et al.*, 2013; Mutz *et al.*, 2017). It is not clear that any such control variable is relevant for this experiment[35].

IV Results

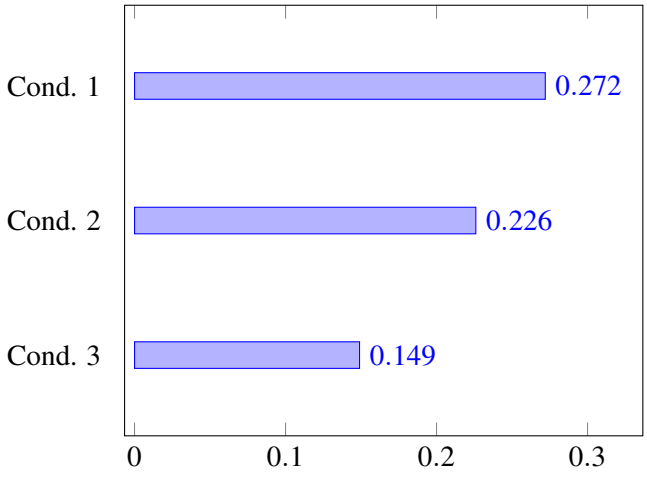
To begin the results section, Table 1 provides an overview of the data obtained in this experiment by each condition. Table 1 is presented below:

Table 1. Overview of the Data

	Condition 1	Condition 2	Condition 3
Number of Participants	180	146	175
Number of Game 1 Selections	131	113	149
Number of Game 2 Selections	49	33	26
Proportion of Game 2 Selections	0.272	0.226	0.149

From Table 1, it can be seen that the observed number of Game 1 selections exceeds the observed number of Game 2 selections in all conditions. To better visualize the data, the proportions of Game 2 selections in each condition are presented in Figure 1 below. From Table 1 and Figure 1, it can be seen that the observed proportion of Game 2 selections is largest in Condition 1 and smallest in Condition 3.

Figure 1. Proportion of Game 2 Selections by Condition



Compared to Condition 1, Condition 2 has an approximately 4.6 percentage points lower point estimate for the proportion of Game 2 selections. This is an approximately 17% lower point estimate. Compared to Condition 1, Condition 3 has an approximately 12.4 percentage points lower point estimate

[34] Or equivalently, to increase the power of the study.

[35] This is not to say that such a control variable does not exist, just that it is not obvious that such a control variable does exist.

for the proportion of Game 2 selections. This is an approximately 45% lower point estimate. Compared to Condition 2, Condition 3 has an approximately 7.7 percentage points lower point estimate for the proportion of Game 2 selections. This is an approximately 34% lower point estimate.

While looking at the observed frequencies and proportions is useful, it is also useful to proceed with more statistical analysis. The results from the hypothesis tests outlined in Section III.III are shown in Table 2 below:

Table 2. Results from the Hypothesis Testing

Test #	Null Hypothesis	Alternative Hypothesis	Z-statistic	p-value	Test Outcome
Test 1	$p_1 = 0$	$p_1 > 0$	8.21	<0.0001	Reject H_0
Test 2	$p_2 = 0$	$p_2 > 0$	6.53	<0.0001	Reject H_0
Test 3	$p_3 = 0$	$p_3 > 0$	5.53	<0.0001	Reject H_0
Test 4	$p_1 = p_2$	$p_1 \neq p_2$	0.96	0.3391	Fail to Reject H_0
Test 5	$p_1 = p_3$	$p_1 \neq p_3$	2.85	0.0043	Reject H_0
Test 6	$p_2 = p_3$	$p_2 \neq p_3$	1.78	0.0744	Fail to Reject H_0

From Table 2, it can be seen that in all of Tests 1, 2, and 3 the null hypothesis that the proportion of Game 2 selections is equal to zero is rejected in Conditions 1, 2, and 3, respectively.

Test 4, which tests if the proportions of Game 2 selections are statistically significantly different between Conditions 1 and 2, fails to reject the null hypothesis that they are equal to each other. Similarly, Test 6, which tests if the proportions of Game 2 selections are statistically significantly different between Conditions 2 and 3, fails to reject the null hypothesis that they are equal to each other. In contrast, Test 5, which tests if the proportions of Game 2 selections are statistically significantly different between Conditions 1 and 3, rejects the null hypothesis that they are equal to each other.

Thus, Tests 1, 2, and 3 find that the proportions of Game 2 selections in Conditions 1, 2, and 3 are statistically significantly greater than zero. Tests 4 and 6 indicate that there are no statistically significant differences in the proportions of Game 2 selections between Conditions 1 and 2 and Conditions 2 and 3, while Test 5 indicates that the proportion of Game 2 selections between Conditions 1 and 3 are statistically significantly different.

V Discussion of Results

The point estimates found here indicate that the proportion of Game 2 selections in Conditions 1 and 2 are 0.272 and 0.226, respectively. These point estimates are substantially smaller than the comparable results found by Walker *et al.* (2018), which are 0.61 and 0.536, respectively. However, these two proportions[36] are statistically significantly greater than zero as shown by Tests 1 and 2. As such, they indicate that there is seemingly some reason or reasons for some individuals choosing to play the payoff inferior Game 2.

As in Condition 2 the probabilities of success are made explicit to the participants in a clear manner it seems that choosing to play Game 2 is not, at least not entirely, driven by an incorrect understanding of which game offers a greater expected payoff or chance of winning. Furthermore, Test 4 indicates that there is no statistically significant difference in the proportion of Game 2 selections between Conditions 1 and 2. Consequently, this experiment fails to find that informing participants of the payoff superiority

[36]Referring to 0.272 in Condition 1 and 0.226 in Condition 2 found in this thesis.

of Game 1 statistically significantly decreases the likelihood of participants choosing to play Game 2, even though it is in their financial interest to do so. Thus, while it is possible that informing participants about the probabilities of success has no effect on or decreases the proportion of Game 2 selections, any possible effect seems to be too small for this thesis to statistically significantly observe it.

This thesis expected that SDA in the presence of clear information of which option is payoff superior was due to a lack of monetary incentives in the experiment conducted by Walker *et al.* (2018). However, this is seemingly not the case as SDA is still exhibited in Condition 2. On the one hand, these results mirror the results found by Walker *et al.* (2018) in that SDA is found in both Conditions 1 and 2 in this thesis and SDA is found in their analogues of Conditions 1 and 2. On the other hand, the proportions found in this thesis are substantially lower than the proportions found by Walker *et al.* (2018). Thus, Conditions 1 and 2 reproduce the findings of Walker *et al.* (2018) in terms of statistical significance, but not in terms of magnitude[37].

The point estimate of the proportion of Game 2 selections in Condition 3 is 0.149, which is lower than in either of Conditions 1 or 2. This point estimate is statistically significantly greater than zero as indicated by Test 3. Thus, SDA is also exhibited in Condition 3. Furthermore, Test 5 indicates that the proportion of Game 2 selections in Condition 3 is statistically significantly different from the proportion of Game 2 selections in Condition 1. Thus, this thesis finds that informing participants of the proposed bias has seemingly statistically significantly decreased the proportion of Game 2 selections. Consequently, this nudge seems to be, at the very least, effective at making people make expected payoff superior decisions.

However, Test 6 shows that the proportion of Game 2 selections in Conditions 2 and 3 are not statistically significantly distinguishable from each other. Thus, all in terms of statistical significance, while this thesis does not find that using the nudge of informing participants of the probabilities of ultimate success is effective and it does find that informing participants of the proposed bias is effective, it does not find that the latter is not more effective than the former.

Relating these results back to the reasons why a participant would choose to play Game 2, the results are not consistent with the only reason being an incorrect understanding of which option is payoff superior. Furthermore, of the two nudges considered, only the nudge of informing participants of the proposed bias has substantial evidence in favour of it being effective at decreasing the likelihood of participants choosing to play Game 2. Thus, the results are more consistent with SDA being driven by a shortsighted focus on the probability of losing immediately than a misunderstanding of which option is payoff superior.

From the five reasons for SDA highlighted in Section II, only the myopic loss aversion explanation and the differing utility from playing the games clearly apply to Condition 2. In spite of attempts to remove the other reasons from influencing decisions, a substantial proportion of participants still chose to play Game 2. In Condition 3, incorrect knowledge of the probabilities can clearly affect decision making, while an attempt to mitigate the effect of a myopic loss aversion-esque reason has been made. The data found here indicates that informing participants of SDA has managed to increase the likelihood of choosing to play Game 1. This is in spite of the fact that informing participants of a proposed bias is often ineffective at mitigating the effects of said bias (Barberis & Thaler, 2003). All in all, this thesis has not found that SDA, in this setting, is simply due to an incorrect understanding of the payoff natures of

[37] Although, strictly speaking, only Condition 1 is a reproduction of an experiment conducted by Walker *et al.* (2018).

the games. This goes against the ex-ante expectation of this thesis, which was that SDA was an artefact of the lack of real-world monetary incentives to choose the payoff superior game, at least when there was no doubt about which option if payoff superior.

As Tests 4 and 5 indicate that the nudge which improves decision making is the nudge informing participants of the proposed bias, it is of interest to explain why this could be the case. If people receive differing utilities from playing the games and this leads to people choosing Game 2, it seems likely that this motivation would be the same in all of the conditions in this thesis. As such, the most intuitive explanation for why the second nudge was found to be effective and the first was not found to be effective is that the myopic loss aversion explanation has explanatory power and its effects can be mitigated by informing participants about the proposed bias.

To be explicit, based on statistical significance, on the one hand the expectations from Hypotheses 1 and 5 are consistent with the evidence found in this experiment. On the other hand, based on statistical significance, the expectations from Hypotheses 2, 4, and 6 are inconsistent with the evidence found in this experiment. Solely considering point estimates, the expectations from Hypothesis 4 is also consistent with the evidence found in this experiment. However, this could easily be due to random chance and as such Condition 2 having a lower proportion of Game 2 selections than Condition 1 need not contain useful information.

The main findings of this thesis are as laid out next. Firstly, there is evidence that people exhibit SDA, even when it is against their financial incentives to do so. Secondly, in a condition with information about the probabilities of success people still exhibit SDA. Thirdly, in a condition that informs participants about a proposed bias people still exhibit SDA. Finally, the nudge informing participants of the proposed bias seems to be effective at mitigating SDA, while the nudge informing participants of the probabilities of ultimate success has not been found to be effective. All of these findings come from a setting where participants have a financial incentive to not exhibit SDA. As such, this thesis has been able to provide more evidence in favour of the existence of a bias identified by Walker *et al.* (2018) by adding an element to the experiment which increases the credibility of the findings. However, SDA is exhibited to a lesser extent in this study than in the study by Walker *et al.* (2018). Additionally, it has been able to provide evidence in favour of the effectiveness of a nudge that could help improve the decision making of individuals.

These results have implications for more real-world decisions. If even in this simple choice, people exhibit SDA and this causes them to make payoff inferior choices, it seems likely that SDA is also exhibited in more real-world settings where there are many more complexities at play. There are a large number of potential complicating factors. These include, e.g., the options or their details not being as clearly laid out, the decision maker not necessarily making a single choice at a time, there being no guidance to make a better choice, social norms influencing decisions, etc. Thus, if even in this case, where it should be easy to make the payoff superior choice, people make the payoff inferior choice, it seems likely that in harder cases people will also make the payoff inferior choice. As such these results suggest that it is likely the case that people do make payoff inferior choices due to SDA in the real-world.

Furthermore, as the evidence found here indicates that informing individuals of the proposed bias is effective at mitigating SDA, a potentially useful nudge has been identified. This is particularly useful as the nudge does not involve knowing the probabilities of ultimate success. As such, the informational requirement for the nudge which has been found to be effective is lower than the informational require-

ment for the nudge which has not been found to be effective. Thus, the nudge which is arguably easier to implement has been found to be seemingly effective. While it is not necessarily true that the nudges would have the same effects in more real-world scenarios, this is a potentially beneficial finding. Furthermore, since the nudge does not have any clear emotionally taxing element to it, it is arguably likely that the nudge being effective indicates that it is also increasing the value of some appropriate objective function.

In short, this thesis identifies that people likely do make payoff inferior choices due to SDA, but also finds evidence indicating that it may be relatively easy to somewhat deal with this problem. However, to be clear, it seems to reduce SDA rather than fully eliminating it.

VI Limitations and Areas for Further Research

There are several limitations to this thesis. A substantial number of them are highlighted and discussed in this section. Some of these limitations are reasonably specific to the experiment conducted in this thesis, while others are more generally applicable to experimental economics. The limitations that are discussed in this section are: (1) small size of the the payoff inferiority, (2) potential harmful effects of the second nudge, (3) wording of the second nudge, (4) lack of trust in the probabilities of the first nudge, (5) trial sample issues, (6) differences in behaviour in less artificial settings, (7) issues with online experiments, (8) loss of effort, (9) non-compliance with the nudges, (10) generalizing to cases with multiple decision makers, and (11) lack of value available to be realized from improving decision making.

One may object to the idea of SDA being interesting to study in the way it has been in this thesis because the lost payoff to the participants is too small. Whilst this is a justifiable objection, it is worth noting two points. Firstly, even if the lost payoff to the participants is small, it is not clear why one would want to lose this payoff. In other words, even if the lost payoff is small, so long as it is non-trivial, SDA can be argued to cause poor decision making. As improving the decision making of individuals is arguably important, biases that cause small losses in expected payoffs may be interesting. Furthermore, if over a period of time an individual faces many decisions analogous to the experiment conducted here, a small loss in expected payoff in each situation can add up to be a large loss in expected payoff over the course of the period.

Secondly, the loss in payoff is arguably quite large and almost certainly non-trivial. This can be illustrated using simple expected value calculations[38]. The expected value of the payoff from choosing to play Game 1 and 2 are:

$$EV(\text{Game 1}) = Pr(\text{Win Game 1}) \cdot Pr(\text{Win Lottery}) \cdot \text{Monetary Payoff}$$
$$EV(\text{Game 2}) = Pr(\text{Win Game 2}) \cdot Pr(\text{Win Lottery}) \cdot \text{Monetary Payoff}$$

As the probability of winning the lottery and the payoff from doing so are the same for both Games

[38] Expected value is used here so as to not needlessly complicate the illustration with assumptions about the utility functions of decision makers.

1 and 2, the following holds true:

$$\frac{EV(\text{Game 1})}{EV(\text{Game 2})} = \frac{Pr(\text{Win Game 1})}{Pr(\text{Win Game 2})} = \frac{52}{49} \approx 1.061$$

Thus, the payoff associated with Game 1 has a just over 6% greater expected value than than the payoff associated with playing Game 2. Consequently, by choosing to play Game 2 an individual is willingly choosing to leave 6% of her expected payoff on the table. If one were to apply an expected foregone gain of 6% to most choices one faces, it is not unjustifiable to say that this loss in expected value is substantial in any given choice. As such, this thesis argues that the negative payoff effects that come from choosing to play Game 2 are substantial, let alone non-trivial. This is true in relative terms, however the absolute negative payoff effects are small. As the absolute payoff difference is small, it can be argued that the financial incentive is not strong enough. This concern could be investigated by increasing the stakes of the experiment.

However, as participants did choose to play Game 2 in this experiment and in the experiment by Walker *et al.* (2018), it could be of interest to investigate at what point does the loss in expected payoff remove the payoff inferior selections. This could be investigated in a further study by modifying the payoff structure of the choice in the experiment to make choosing the "slow" option result in giving up a larger amount of expected payoff relative to choosing the "fast" option.

On a related note, this thesis expected that the SDA observed by Walker *et al.* (2018) in experimental settings was due to both a lack of financial incentives to choose the payoff superior option and a lack of knowledge of which option was the payoff superior option. The approach taken in this thesis was to introduce financial incentives to choose the payoff superior option, investigate if SDA is exhibited in this setting, and if two nudges can mitigate SDA. Alternatively, one could have used conditions with and without financial incentives to choose the payoff superior option and test the difference in proportions of Game 2 selections between these conditions. This would be a more direct test of the effect of financial incentives to choose the payoff superior option on the amount of SDA exhibited. The alternative approach was not taken as it would involve more conditions, lowering the power of the study. However, it could be an interesting avenue for further research.

There are some potential issues with the nudges used in this experiment. This thesis found that the second nudge statistically significantly decreased the proportion of Game 2 selections relative to no nudge. However, there exist cases where the second nudge could cause individuals to make worse choices. These cases would be cases where the "slow" option is payoff superior relative to the "fast" option. There is nothing that ensures that the "slow" option is always the payoff inferior option. Consequently, the second nudge nudging individuals to choose the "fast" option instead of the "slow" option rather than nudging individuals to go for the payoff superior option can be problematic. To attempt to address this issue in practice, one could add a sentence suggesting individuals to calculate or estimate the probability of success prior to making a decision and advocating them to select the option with the higher calculated or estimated probability of success. Admittedly, this is close to a combination of the two nudges considered in this thesis.

Another potential issue with the second nudge is that it may not refer to the myopic loss aversion explicitly enough to isolate its effect on the myopic loss aversion explanation for SDA. As such it may mitigate SDA in general or for several reasons, not necessarily just SDA stemming from myopic loss

aversion. This is not a problem per se, but if the reasons for SDA are of interest, this does not pin them down.

The first nudge informs participants of the probabilities of winning Games 1 and 2. For this nudge to work in the intended manner to change decision making, it is important that the participants believe that the probabilities they have been told are correct. While the calculations could have been laid out in their entirety to the participants, this thesis also relies upon the convention that deception is not used in experimental economics and as such participants should have little reason to distrust the information. The stronger this norm becomes, the less of a concern this issue becomes. A sentence saying that deception is not accepted in experimental economics could have been included, but someone who distrusts the probabilities could just as easily distrust this statement. Furthermore, this would have brought attention to the fact that deception could be occurring. For the context of this thesis, it is arguable that the kind of individual to distrust the given likelihoods is also the kind of individual to calculate the likelihoods themselves. As such, the initial disbelief is likely not a substantial problem.

The population from which participants were drawn for the experiment conducted in this thesis are bachelor's and master's students at the Stockholm School of Economics. This differs from the general population and from the MTurk population from which Walker *et al.* (2018) draw their participants. As such, the results may not generalize to other populations of interest. The population from which participants are drawn in this setting differs in characteristics from the general population by, e.g., being younger and from a higher socio-economic background on average. Importantly, it is arguably the case that the population from which participants are drawn from for this thesis predominantly contains individuals who are more numerically-minded and familiar with choices of the nature presented in this experiment. Thus, one might expect that SDA would be less prevalent in this population[39]. Simply comparing the proportion of Game 2 selections with those found in Walker *et al.* (2018), shows that Game 2 selections are substantially less common in this thesis. As such, this expectation is at least not obviously incorrect. It is harder to have expectations about how individuals from these populations would differentially respond to the nudges used in the experiment conducted in this thesis. The characteristics of the population from which participants for this experiment are drawn lead to external validity concerns, but not internal validity concerns.

Another potential issue with the trial sample regards the reasons for non-response to the survey. The survey was sent out to over 1600 potential participants and 501 usable observations were obtained. If those who did not respond to the survey did so for some systematic reason, the results from this survey are not necessarily even applicable to all bachelor's and master's students at the Stockholm School of Economics. Thus, if the non-responses are systematic, the results may not even apply to the entire population from which participants are drawn from. Essentially, this limitation stems from the fact that the results only necessarily apply to the trial population and that a voluntary response survey was used.

[39]This is similar to expecting that professional chess players would be on average more skilled at using backward induction than the general population. Palacios-Huerta & Volij (2009) find that the higher the ranking of a chess player, the more likely they are to terminate the centipede game at the first node. However, Levitt *et al.* (2011) point out that there are many reasons other than not being able to use backward induction for why one might not terminate the centipede game at the first node, as such the centipede game is not a clean test of backward induction ability. These other reasons include, e.g., certain kinds of social preferences or beliefs about the proportion of the players in the relevant population having certain kinds of social preferences. Furthermore, Levitt *et al.* (2011) find that, in their experiment, professional chess players behave in essentially the same manner as standard experimental populations do in the centipede game, with very few terminating the game immediately. Thus, Levitt *et al.* (2011) fail to replicate the findings of Palacios-Huerta & Volij (2009). This is to highlight that population specific expectations regarding behaviour have been had before and that the intuitive expectation may or may not be correct.

It is well known that the voluntary response nature of voluntary response surveys can lead to biased findings (Bryson, 1976). One famous example is the Literary Digest presidential poll for the 1936 U.S. presidential election. The poll mailed out 10 million surveys, but only received roughly 2.3 million responses. The poll predicted a landslide victory for the candidate who lost in arguably the most one-sided U.S. presidential election in history. The bias introduced by voluntary response was, at least in large part, to blame (Bryson, 1976; Squire, 1988). Bryson (1976) attributes the biased results to certain respondents feeling more passionately about the election than others.

Non-response bias is larger if the non-response rate is larger and if the difference in means for the outcome variable between respondents and non-respondents is larger (Smith, 1983; Pearl & Fairley, 1985). Returning to the context of this thesis, the non-response rate is high. Additionally, it is plausible that the non-responding students would have systematically made different choices from responding students. However, what could have driven this difference is not clear[40]. Unfortunately, it would not have been feasible to conduct this thesis without a voluntary response experiment. Smith (1983) essentially concludes that no method, at least of the ones he considers, works well in estimating non-response bias. Furthermore, several of the methods would not have been feasible to use in this thesis, even if they did work. Thus, it would have been exceedingly difficult to address this limitation. Consequently, one has to be cautious when trying to extrapolate the results of this thesis to a population other than the trial sample.

Also regarding the trial sample, it is possible that some participants responded several times to the survey using different email addresses. Responses from identical email addresses were removed, as specified in the preregistration, but if a respondent used a slightly varied email address or both a student email address and a personal email address, both are included in the analysis. One could have used only responses from student email addresses to entirely resolve this issue. However, as this was not specified in the preregistration, this was not done. If this occurred, it is not clear that it biases the results. However, it would make the standard errors of the estimates too small as it would essentially be a case of overestimating the number of participants.

The experiment conducted here was an online experiment with a somewhat contrived choice. It is conceivable that individuals may behave differently in more common or field-like settings. Thus, the results from the kinds of experiments conducted in this thesis, Walker *et al.* (2018), and generally in more lab-like settings may not generalize to more field-like settings. Thus, the results may not generalize to different settings of interest where SDA is present even if no issues resulting from generalizing from the trial sample to a given population are relevant. Further research could investigate if the results hold in more field-like settings. Particularly, the second nudge may cause participants to exhibit experimenter effects in that they may act in such a way that they think the experimenter wants them to. Similar effects may or may not occur in more field-like settings.

As the experiment conducted in this thesis is an online experiment, some issues specific to online experiments apply to this thesis. One example of such an issue is that participants can easily talk to other people when partaking in the experiment. Another issue is that there may be more noise in the data when it comes from an online experiment rather than an in-person experiment. This is because participants may be more likely to try to quickly fill out the survey without paying attention to what is being asked.

[40] In contrast, it is easier to think of reasons, at least in hindsight, for why respondents and non-respondents would answer differently in the U.S. presidential election poll.

However, as the experiments conducted by Walker *et al.* (2018) were also conducted online, these issues also apply to the previous results to which the results from this thesis are compared. This said, it may be of interest to see if the results hold in non-online settings.

The structure of the experiment requires that participants read, pay attention to the given instructions, and give answers to questions about how the game that is played in the experiment works. It is possible that some participants find these tasks too taxing and then proceed to fill the survey without regard for their choices. This would require some level of trial-and-error to get past the understanding questions. This kind of random selection of responses would bias the proportion of Game 2 selections toward 0.5[41]. As the proportions found in this thesis are substantially below 0.5, random selection of responses by participants biases the results to show more SDA than they should. In contrast, the random responses also bias the results to show less of an effect for the nudges than they actually have if they reach the participant. On the one hand, it is believed here that random responses are likely not a major concern as the amount of text presented is reasonably low and the choice is simple in that it requires little math, only involves one individual, and involves a context (a deck of playing cards) which is likely familiar to participants. On the other hand, it can't be shown that this is not a concern.

Generally, experiments can struggle with compliance problems as one can randomize the offer of treatment, but not if those to whom treatment was assigned to are treated. In the context of this thesis, there may be participants who are in a treatment group, but are not affected by the treatment as they did not read the information provided. However, it seems likely that in the experiment conducted in this thesis non-compliance issues are non-substantial for two reasons. Firstly, the treatment is just reading a short paragraph. Thus, there is likely little to be gained from avoiding the treatment. Secondly, the participant does not necessarily know that this is the treatment, particularly prior to reading the paragraph. Thus, it is likely quite hard to complete the survey without reading the possible nudge as the survey requires correct answers to some questions to proceed. As such, non-compliance concerns, whilst plausible, are likely non-substantial. Furthermore, even if it were a problem, this would mean that the nudges actually affect fewer people than anticipated and as such this thesis would underestimate the efficacy of the nudges when they reach individuals. However, they may also not reach individuals in the real-world, so it need not underestimate the efficacy of nudges if one were to implement them.

The experiment conducted in this thesis involves an individual choosing an option, but some choices are better characterized as groups deciding an option to proceed with. It is not clear that the bias exhibited in this experiment generalizes to group settings or that the nudges would have the same effects in group settings. Thus, this thesis can give some insight into individual decision making, but the results are not necessarily generalizable to group settings.

In certain settings, such as the sports examples that initially motivated research into SDA, if every agent improves their decision making, the net effect may be that outcomes do not improve for anyone. This is because if everyone improves, one may be more likely to win when facing these situations, but also more likely to lose when one's opponents face these situations. However, there are two points worth highlighting in relation to this. Firstly, even if outcomes do not improve for anyone if everyone improves their decision making, it is still in the interest of any given decision maker to improve their decision making. Thus, learning about how to improve one's decision making is of interest. Secondly,

[41] Assuming that the random selections choose both options with equal likelihood. Given the motivation of not caring about the choices one makes, this seems like a sensible assumption to make in this context.

this only applies in settings which are zero-sum in their nature and improving the decision making of all decision makers in non-zero-sum contexts can yield substantial gains.

Finally, as this experiment is a very simplified way of investigating SDA, the results found here need not generalize to more complicated real-world scenarios. This is particularly where reasons other than SDA to avoid or favour a "fast" or "slow" option exist[42]. This combined with the fact that this thesis does not conduct a formal welfare analysis of the two nudges, but rather investigates if they are effective, a real-world policy suggestion of implementation of the nudges was never going to be suggested in this thesis. However, this thesis does highlight that in a certain setting people seem to exhibit SDA and as such this thesis could provide motivation for investigating SDA in more real-world settings and seeing if either of the nudges can improve decision making in these more, in practical terms, interesting cases. Thus, this thesis can provide a guiding step for further research justifying to real-world actions, but on its own it cannot be credibly used to justify real-world actions.

VII Conclusion

The first aim of this thesis was to investigate if SDA is exhibited in a simple card game. Further, if SDA was exhibited, the second aim was to investigate if SDA could be mitigated by either informing participants of the probabilities of success or the proposed bias.

It was expected, at least when there was no doubt about the payoff superiority of the payoff superior option, that SDA would not be exhibited. The findings contradict this expectation as it seems to be the case that SDA is exhibited in the simple card game used in this thesis, even though there are real-world monetary incentives to not do so.

Secondly, this thesis finds statistically significant evidence in favour of the efficacy of helping participants improve their decision making for the nudge informing participants of the proposed bias. No statistically significant evidence in favour of the efficacy of the nudge informing participants of the probabilities of ultimate success was found.

Thus, this thesis has provided more convincing evidence to the literature that SDA is exhibited. Additionally, on a positive note, it has found evidence in favour of a way to somewhat deal with the problems caused by SDA. Intuitively, the experiment conducted here should be a situation where it should be as easy as it gets to overcome SDA. Thus, one has to be careful to not put too much faith in the evidence in favour of the effectiveness of the solution found here. It may well be the case that the nudge is ineffective in more real-world settings. However, the evidence found here is evidence in favour of a simple informational change which potentially has the ability to substantially increase the value of some appropriate objective function.

To conclude, it seems that SDA is an example of a bias that individuals exhibit, but also one which may have an obvious and practical way to partially deal with the problems it causes.

[42] These include the aforementioned uncertainty regarding probabilities of success or perhaps being able to influence the probabilities of success in real life (e.g. being able to exert effort better than the opposing team in overtime in basketball) combined with uncertainty about how exactly this mechanism to influence the probabilities works.

VIII Appendix

Italicized text is used to distinguish descriptions of the upcoming exhibits from their contents.

Exhibit 1

The following is everything a participant in Condition 1 would be presented with up until the decision of which game they want to play.

Page 1

Welcome to this experiment! I am Patrik, a master's student at the Stockholm School of Economics (SSE) and I need your help for my thesis!

You will be asked to answer a few questions in this survey and play a short game. The responses will be used in an anonymous manner. The time it takes to complete this survey is roughly 3-5 minutes. 3 participants will be paid 1000 SEK for participating in this survey.

Once you've completed this experiment, please do not discuss it with other students.

Page 2

For the payment of 1000 SEK, I need to be able to contact you later. Please indicate the email address you would want to be contacted at for this purpose (preferably your SSE email). This information will not be used for anything else and will not be stored afterwards.

Participants are then presented with a textbox for filling out their email address. This textbox must be filled in with text to proceed.

Page 3

You will be presented with a description of two games. Then you will be asked to make your choice of which game you want to play. Then you will play your chosen game.

Participation in this experiment is entirely voluntary and you may quit this experiment whenever you want.

By proceeding with this survey, you are indicating your willingness to participate in this research project.

Page 4

You will choose between playing one of the two possible games. Your options are Game 1 and Game 2.

They are as follows:

Game 1:

You will draw a card from a randomly shuffled regular 52-card deck of playing cards.

If the card you draw is a Jack, Queen, King, or Ace, you win.

If it is a 2, 3, 4, 5, 6, 7, 8, 9, or 10, you lose.

If you win, you will be entered into a lottery to win 1000 SEK.

If you lose, you will receive no payment.

Game 2:

You will draw a card from a randomly shuffled regular 52-card deck of playing cards.

If the card you draw is an 8, 9, 10, Jack, Queen, King, or Ace, you proceed to the next stage.

If it is a 2, 3, 4, 5, 6, or 7, you lose immediately.

If you proceed to the next stage, you will draw a card from a new randomly shuffled regular 52-card deck of playing cards.

If the card you draw is an 8, 9, 10, Jack, Queen, King, or Ace, you win.

If it is a 2, 3, 4, 5, 6, or 7, you lose.

If you win, you will be entered into a lottery to win 1000 SEK.

If you lose, you will receive no payment.

If you are entered into the lottery to receive 1000 SEK, your chance of winning will be the same regardless of whether you played Game 1 or Game 2.

You will now be presented with some questions to make the above information more clear:

If you played Game 1 and drew an 8, what would happen?

Participants must now choose between "You would win" and "You would lose". To proceed, the participant must choose "You would lose".

If you played Game 2 and drew an 8 in the first draw, what would happen?

Participants must now choose between "You would win", "You would proceed to the next round of Game 2", and "You would lose". To proceed, the participant must choose "You would proceed to the next round of Game 2".

If you played Game 2 and drew an 8 in the second draw, what would happen?

Participants must now choose between "You would win", "You would proceed to the next round of Game 2", and "You would lose". To proceed, the participant must choose "You would win".

If in Conditions 2 or 3, the appropriate additional paragraph is presented here.

Please choose which game you will play (you will play the game on the next page):

Participants must now choose between "Play Game 1" and "Play Game 2". On the following page(s), the chosen game is played.

Exhibit 2

The following paragraph is the paragraph presented to participants in Condition 2 to inform them about the probabilities of winning Games 1 and 2, respectively.

Note: The chance of winning in Game 1 is 31%, while the chance of winning in Game 2 is 29%. Statistically speaking, going for the single draw game gives you a better chance of winning (though, of course, the odds of losing right away are higher if you go for the single draw game).

Exhibit 3

The following paragraph is the paragraph presented to participants in Condition 3 to inform them about the proposed explanation for SDA.

Note: There is a tendency for people to avoid "fast" options that have a higher chance of ultimate success, but also have a higher chance of immediate defeat in favour of "slow" options that have a lower chance of ultimate success, but also have a lower chance of immediate defeat. This tendency makes people avoid superior options (i.e. make worse decisions) because they feel riskier. A concrete example is a basketball team that is losing by 2 points with very little time remaining. This team must decide to either take the "fast" option of shooting a 3 point shot in an attempt to essentially win or lose right away or the "slow" option of shooting a safer 2 point shot and attempting to win in overtime later. There is a tendency for the team to choose the (worse) option of shooting the 2 point shot.

Exhibit 4

The following test is the text from the email sent out to potential participants to invite them to participate in the experiment.

Hey,

I am running an experiment for my masters thesis and I would really appreciate it if you would participate! **The experiment is short (roughly 3-5 minutes) and 3 participants will win 1,000 SEK.** Participation is anonymous to other participants.

You can participate in the experiment on your computer or phone using the link below:

`https://hhs.qualtrics.com/jfe/form/SV_cZVnrCWBsjLO55b`

Thank you so much for your time!

Best wishes,

Patrik Räty (41445@student.hhs.se)

Exhibit 5

The following text is the content of the preregistration available from `https://osf.io/h56tq`.

Purpose of this Registration:

This registration is a preregistration for my MSc thesis at the Stockholm School of Economics. It is being made before data collection has begun.

Purpose of the Project:

To make the rest of this preregistration clear, a short outline of the purpose of this project is given here. The purpose of this project is to investigate Sudden Death Aversion (SDA). SDA can be defined as "the tendency to avoid (so-called) fast strategies that provide a greater chance of success, but include the possible of immediate defeat, in favour of (so-called) slow strategies that reduce the possibility of losing quickly, but have lower odds of ultimate success". This is essentially the definition used in Walker et al. (2018). This paper also motivates a large part of this project. Walker et al. (2018) argue that SDA is driven by myopic loss aversion (focusing on the immediate loss probability rather than that of ultimate loss and being loss averse so weighting this immediate loss heavily).

This in mind, the purpose of this project can be divided into two sections. Firstly, the purpose is to investigate if people exhibit SDA in a card game experiment. The card game used is identical to that from Walker et al. (2018). It involves choosing between playing one of two card games, labelled Game

1 and Game 2. Game 1 is more of a "fast" option with a higher probability of ultimate success and immediate failure, while Game 2 is more of a "slow" option with a lower probability of ultimate success and immediate failure. The exact details of these games are:

Game 1: You will draw a card from a randomly shuffled regular 52-card deck of playing cards. If the card you draw is a Jack, Queen, King, or Ace, you win. If it is a 2, 3, 4, 5, 6, 7, 8, 9, or 10, you lose. If you win, you will be entered into a lottery to win 1000 SEK. If you lose, you will receive no payment.

Game 2: You will draw a card from a randomly shuffled regular 52-card deck of playing cards. If the care you draw is an 8, 9, 10, Jack, Queen, King, or Ace, you proceed to the next stage. If it is a 2, 3, 4, 5, 6, or 7, you lose immediately. If you win and proceed to the next stage, you will draw a card from a new randomly shuffled regular 52-card deck of playing cards. If the card you draw is an 8, 9, 10, Jack, Queen, King, or Ace, you win. If it is a 2, 3, 4, 5, 6, or 7, you lose. If you win, you will be entered into a lottery to win 1000 SEK. If you lose, you will receive no payment.

If you are entered into the lottery to receive 1000 SEK, your chance of winning will be the same regardless of whether you played Game 1 or Game 2.

Walker et al. (2018) find that a large amount of participants choose Game 2 over Game 1.

Secondly, if SDA is present, it is interesting to see how it could be removed or reduced. Consequently, this project has two different interventions to attempt to remove or reduce SDA.

The first intervention is to be explicit about the ultimate probabilities of success in Game 1 (roughly 31%) and Game 2 (roughly 29%). This is with the belief that being explicit may deal with participants being unable to calculate or otherwise not calculating the ultimate probabilities of success and choose the "wrong" game (Game 2) because of this. If this intervention removes all Game 2 choice, that would be evidence that it is not SDA driving Game 2 choices. The exact wording used is the following:

"Note: The chance of winning in Game 1 is 31%, while the chance of winning in Game 2 is 29%. Statistically speaking, going for the single draw game gives you a better chance of winning (though, of course, the odds of losing right away are higher if you go for the single draw game)."

This intervention is similar to one used in Walker et al. (2018) as they were also explicit about probabilities in one of their experiment. However, this intervention differs in two ways. Firstly, the experiment they were explicit about the ultimate probabilities of success was a hypothetical basketball example, not the card game they also used in another experiment. Secondly, and more importantly, when they were explicit about the ultimate probability of success, the participants got paid a "modest compensation" which seems to have been unaffected by their decision, thus there was no monetary incentive (nor other clear incentive) to choose the "better" option. This is to say that the participants payoff was seemingly entirely invariant to the decisions made in the experiment and as such it may well be that participants just did not care. The payoff structure used in this experiment introduces an incentive to choose the "better" option.

The second intervention is to explicitly explain the tendency that people have to avoid "fast" options in favour of "slow" options (in a more detailed manner than just saying that). It is conceivable that being explicit about the bias may reduce or remove it. Walker et al. (2018) did not have an intervention similar to this. The exact wording used is the following:

"Note: There is a tendency for people to avoid "fast" options that have a higher chance of ultimate

success, but also have a higher chance of immediate defeat in favour of "slow" options that have a lower chance of ultimate success, but also have a lower chance of immediate defeat. This tendency makes people avoid superior options (i.e. make worse decisions) because they feel riskier. A concrete example is a basketball team that is losing by 2 points with very little time remaining. This team must decide to either take the "fast" option of shooting a 3 point shot in an attempt to essentially win or lose right away or the "slow" option of shooting a safer 2 point shot and attempting to win in overtime later. There is a tendency for the team to choose the (worse) option of shooting the 2 point shot."

Statistical Significance Threshold Used in the Project:

The statistical significance level used in this experiment will be 0.005. This is to avoid issues with false positive results from multiple testing for example. With a power of 80% and a Cohen's d of 0.5, this would imply a target sample size of roughly 109 participants in each of the treatment groups.

Results with a p-value greater than 0.005 and less than 0.05 will be treated as suggestive evidence.

Collection of Observations:

Observations in this experiment will be collected using a Qualtrics survey. This survey will initially be emailed out to students at the Stockholm School of Economics. If this gives enough observations, the collection of observations will be cut-off here. If this does not give enough observation, the collection will have to be extended (perhaps to neighbouring universities). The choice to stop or continue gathering observations will be solely dependent on the number of (usable) observations gathered, not the results they lead to.

The data will not be inspected prior to stopping data collection in any way other than observing the number of usable observations (unique emails and completed the experiment).

It is possible that the aim of roughly 100-110 participants in each of the treatment groups is achieved and surpassed solely with students from the Stockholm School of Economics. I will not ignore later responses to the survey once this number of observations is reached, but further emails in the hopes of more observations will not be sent out beyond this aim for the number of participants. Once data analysis begins, no further observations will be considered (i.e. I will not stop recording later responses when the target sample size is hit, but I will stop recording later responses when I will begin analyzing the data).

Sample sizes in each condition will be roughly equal to each other (ideally they would be equal, but that cannot be guaranteed due to the software randomly allocating treatment conditions and non-responses to started surveys).

Using the available software, it is not possible for me to prevent the same person from doing the experiment several times. However, as emails are collected in the process of collecting observations, for cases where the same email appears more than once, I will only use the first observation.

Data collection will begin on the 28th of November 2019. Data collection will go on at least until the 15th of January 2020. If enough observations are achieved by this date, data collection will be stopped. If not enough observations are achieved, I will resend the email to the SSE students who did not complete the survey. If by the 1st of February 2020 I have not received enough observations, I

will seek observations from neighbouring universities such as Stockholm University (SU) or Kungliga Tekniska Hgskola (KTH).

Respondents are not told that other treatment conditions exist.

Hypotheses:

There are three conditions in this project, which are no intervention (Condition 1), probabilities intervention (Condition 2), and explicit bias information (Condition 3).

In Condition 1, I hypothesize that there will be a substantial number/proportion of participants choosing Game 2 instead of the superior Game 1. This is based on what was found by Walker et al. (2018), where their analog of Condition 1 in this project showed 61% of participants choosing Game 2.

In Condition 2, I hypothesize that there will not be a substantial proportion of participants choosing Game 2 instead of the superior Game 1. This is somewhat in conflict with what was found by Walker et al. (2018), where their nearest experiment to Condition 2 found roughly 53.6% choosing the equivalent of Game 2. I believe that this may largely be due to there being no substantial incentive to choose the "better" option on the part of the participants. Thus, I believe when being explicit in this project about the probabilities of ultimate success and having financial incentives to choose the "better" option, the vast majority will choose the better option. Thus, I believe that Condition 2 will have a lot fewer participants choosing Game 2 than in Condition 1 (statistically significantly so) and the number of participants choosing Game 2 in Condition 2 not being statistically significantly different from 0. To be clear, letting p_i denote the proportion of respondents choosing Game 2 in condition i, I believe that p_2 will be statistically significantly lower than p_1 and that p_2 will not be statistically significantly different from 0.

In Condition 3, I hypothesize that the number/proportion of participants choosing Game 2 instead of the superior Game 1 will be less than that in Condition 1. Thus, I expect being explicit about the bias will reduce the selections of Game 2.

If the selection of Game 2 is primarily due to calculating ability or effort, Condition 3 should be less effective in reducing Game 2 selections than Condition 2 is. If the selection of Game 2 is primarily due to SDA, Condition 3 may well be more effective in reducing Game 2 selections than Condition 2 is. I weakly expect that Game 2 selections are due to calculating ability or effort and as such I weakly hypothesize that Condition 2 will reduce Game 2 selections by more. To be clear, this means I have a weak expectation that p_2 will be less than p_3.

Finally, I am agnostic with regard to whether the proportion of Game 2 selections in Condition 3 is statistically significantly different from 0.

Tests Conducted in the Project:

As the hypotheses above hint at, there will be six main tests conducted in the project.

The first three are comparisons of the proportions of Game 2 selections in the three conditions to 0.

The next three are comparisons of the proportions of Game 2 selections between conditions. Condition 1 compared to Condition 2 allows us to see if calculating ability or effort is the driver of Game 2 selections. Condition 1 to Condition 3 allows us to see if being explicit about SDA helps in reducing

Game 2 selections. Condition 2 to Condition 3 allows us to see if being explicit about the probabilities is more or less effective at lowering Game 2 selections than being explicit about SDA is.

The main statistical test used will be a z-test between two conditions (testing differences in proportion of Game 2 selections) or z-tests of the proportion of Game 2 selections differing from 0 within a condition. The tests between conditions will be two-sided tests, even if there is an expectation of the direction of the effect. The comparison to zero within a condition will be one-sided tests as the proportion cannot go below 0.

Placement into treatments is randomly allocated by the software.

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