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Socioeconomic Status and Individual Decision Making

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Abstract

Socioeconomic disparities in life outcomes is a widely observed occurrence. In particular, low socioeconomic status (SES) has been related to a variety of behaviours that tend to perpetuate or even exacerbate the conditions that individuals of such social standing are already facing. The last two decades have been marked by a growing number of studies seeking to disentangle behavioural differences associated with one's socioeconomic status that fuel the persistence of these differences. This Doctoral thesis joins the discussion by investigating a subset of behaviours associated with low SES. Three empirical chapters and one literature review seeks to answer the following questions: first, what are the consumer behaviour patterns exhibited by low SES individuals? Second, how do poverty-related thoughts influence consumption of (un)taxed temptation goods? Third, what is the effect of perceived individual control on intertemporal preferences? And fourth, how does salient socioeconomic status affect intertemporal choices regarding effort?

Chapter 1 overviews literature on socioeconomic status and consumer happiness. We discuss channels through which low SES can impact consumption decisions and present main behavioural patterns of low SES consumers. Low SES has been found to have a significant impact on dietary patterns that include consumption of fruits and vegetables, foods with high calorie content, as well as consumption of temptation goods like alcohol, tobacco, and sugar-sweetened beverages. Moreover, low SES individuals also tend to engage in status-signalling behaviours, despite the scarcity of financial resources. We discuss how SES can delineate consumer happiness, in particular, regarding experiential and material goods. Finally, we review evidence on socioeconomic disparities in satisfaction with product-specific characteristics and health care as well as consumer loyalty.

Chapter 2 presents an empirical investigation of the intersection between socioeconomic status and consumer behavior. In particular, we study demand for temptation goods such as alcohol, tobacco or high-calorie foods. One of the most conventional ways that governments control the consumption of these products is through taxes; however, a growing body of research shows the presence of numerous behavioral biases that might prove such fiscal policies less effective. One of these biases is related to financial worries - a concept familiar to deprived individuals. Previous studies have shown that increasing worries shift attention towards pressing needs, potentially at the cost of forward-looking decisions. We run an online experiment in which we manipulate financial worries and ask participants to choose between necessities and temptation goods in the experimental market. We also randomly impose taxes on temptation goods for a subset of

participants. Results suggest that under financial worries and no taxes participants demand less temptation goods and this effect is stronger for lower income individuals. However, when taxes are introduced and financial concerns are salient, lower income participants do not react to taxes. This suggests that, on the one hand, financial worries can protect against over-consumption of temptation goods when there are no tax changes; however, low income consumers can be hurt the most when additional taxes are implemented.

Chapter 3 investigates another psychological occurrence - a feeling of control - and its impact on intertemporal preferences. Generally, low SES individuals have less chances to exert control in their lives compared with high SES people. If perceived control has a substantial impact on the intertemporal choice, these disparities may have a long lasting impact that might make it harder to move up in terms of social status. In an online experiment we manipulate the feeling of control by asking participants to remember a certain situation. We vary control in terms of level - not having control vs being in full control – and type – being in a situation involving other people or a non-social situation. Afterwards, we ask participants to make intertemporal allocation decisions - either regarding a monetary experimental budget or a number of real effort tasks. We find no evidence of present bias in monetary discounting for either of the control treatments. Results are different for effort discounting: on aggregate level, participants in this condition reverse their preferences more often as they choose to perform more tasks sooner when the decision involves only future points in time, but less when the decision involves also present. Moreover, we find evidence of significant present bias in the low control condition. Allocation decisions are mediated by emotional states activated in the feeling of control manipulation: for money condition, the strongest mediator is the feeling of fear, while for effort discounting it is sadness. Overall, the results suggest that although recalling a situation of (no) control does not influence intertemporal allocation decisions regarding windfall money, it can impact decisions about the allocation of effort.

Chapter 4 continues the discussion on intertemporal preferences and socioeconomic status. It is a well established that low SES is related to impatient behaviours. While many works have analyzed psychological channels which mediate this effect, such as cognitive load, stress, emotional affects, and self-control issues, this work seeks to test whether the mere salience of one's subjective SES has an impact on intertemporal preferences regarding effort. In an online experiment, I prime participants on their SES and ask to make effort allocation decisions. I find that priming affects only low status participants: this group made more present-biased choices by postponing effort to the future even if it mean higher workload. No effect was found for non-primed low SES participants or higher status individuals in both treatment and control groups. I conclude that even a mere act of making SES salient in your mind can have an adverse effect

to low SES individuals by pushing them to postpone work to the future.

Contents

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List of Figures

List of Tables

Chapter 1

Socioeconomic Status and Consumer Happiness[1]

with Lucia Savadori

When it comes to happiness and satisfaction, consumer socioeconomic status (SES) has an important moderating role. In this chapter, we outline in which way SES intervenes in shaping consumer preferences and consumer happiness. When considering consumer preferences, low socioeconomic status has been shown to impact dietary patterns, such as consumption of fruits and vegetables, high caloric food, sugar-sweetened beverages, as well as consumption of alcohol and tobacco. Studies also show that low SES consumers tend to engage in purchases of various status-signaling goods. Socioeconomic status has also been shown to intervene in delineating happiness for experiential and material goods, consumer loyalty behavior, and consumer happiness with food consumption. We discuss the factors responsible for these relationships.

Keywords: consumer happiness, consumer satisfaction, low socioeconomic status

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1.1 Socioeconomic Status and Consumer Happiness

Consumer happiness depends on several individual and contextual factors. Among these, consumer's socioeconomic status (SES) has an important moderating role. Socioeconomic status defines a relative position of an individual or a household within a society and is usually related to their income, education, and occupation. Low SES has been associated with diverse outcomes in different domains, such as health (Williams, 1990) or educational attainment (Paterson, 1991), as well as differences in cognitive performance (Mani et al., 2013), impulsivity (Haisley et al., 2008a), and self-control (Mullainathan, 2012), among others. Socioeconomic differences also translate into consumer behavior and consumer happiness. It is important to address consumer happiness of both high- and low-SES consumers, especially because the factors that increase happiness among one type of consumer might not have the same effect on the other type of consumer, or they might even have a counterproductive effect.

There are different channels through which low SES can influence consumer decision making. Hamilton et al. (2019) discuss financial deprivation and four perspectives through which it can affect consumer behavior. First, low SES consumers face financial constraints (scarce resources) which limits the possibility to acquire products and services they need or want. Second, consumers may have fewer options to choose from (choice restriction), such as when shopping in a small supermarket offering limited number per category of product or lower number of categories on the total as compared with bigger supermarkets. Third, consumers tend to engage in social comparison, usually, an upward one, which means that being in a relatively deprived setting may result in fewer opportunities to make a favorable social comparison. Fourth, consumers living in financial deprivation usually are uncertain about their future, especially, future income, therefore this results in a less predictable future. All these perspectives influence the way consumers make decisions and can alter patterns, distinguishing them from what is observed among medium and high SES consumers.

1.2 Status Goods

One type of consumption decisions widely observed among people living in poverty is status consumption (Banerjee and Duflo, 2011; Guillen-Royo, 2011; Jin et al., 2011; Van Kempen, 2004). It characterizes purchases through which consumers seek to improve their relative position within a society, provided that status signal of such goods coincides for both the individual and the society. The term is sometimes used interchangeably with conspicuous consumption, which has been defined as consumption with a goal to display a high social status or prestige (Page, 1992; Veblen, 1899).

Such display of status among low SES consumers can begin at a young age. Adolescent need to fit in among their peers is a widely observed behavior that is not constrained to particular social groups. Such a young age is generally marked by the presence of self-doubt, self-consciousness, and personal insecurities (Jiang et al., 2015; Kara and Chan, 2013). However, children from less affluent families see possession of well-known brands as a gateway to social circles that include their more well-off peers. Parents experience strong pressure from their children to obtain such branded products; otherwise, they refuse to wear cheaper, non-branded clothing items or shoes (Hamilton, 2012). Research shows that adolescent self-esteem is strongly influenced by material

possessions and money which are the main signals for social inclusion among this age group (Isaksen and Roper, 2012). A child or teenager tends to first be judged by appearance: brands work as a material norm symbol (Whelan and Hingston, 2018) and can determine a decision of peers to initiate a conversation with the owner of branded goods (Elliott, 2004). Children from poorer families put more importance on these status goods as compared with children from more affluent families (Isaksen and Roper, 2012). This does not come as a surprise as belonging to a higher social circle can seem like a way to escape the dire everyday environment. Failure to comply with these norms can result not only in no communication with the peers but also teasing or bullying (Elliott, 2004). To avoid their children experiencing social stigma, parents can get driven into taking loans. In this way, a state of low welfare might persist even further. Moreover, such experiences of status signaling in the young age can leave residual behaviors in the adulthood: as shown by studies, the adolescent experience of deprivation and attempts to fit in among the peers by acquiring and using branded goods can make the adult consider simple and cheaper everyday brands threatening to their self-esteem, this way fostering the further consumption of branded products (Whelan and Hingston, 2018).

Status signaling among adults of low SES is a particularly common behavior. Certain countries even share public government-backed messages discouraging conspicuous consumption seeing it as an activity that propagates deprivation further (Chipp and Manzi, 2011; Danzer et al., 2014). This is a paradox of status signaling: consumers are (subjectively or objectively) deprived, they spend a part of their already scarce income on status goods to signal that they belong to a higher social circle, which push them into more deprivation. Moreover, these are usually not a one-time purchase; once you publicly signal your status, you seek to maintain it, which asks for continuous investment in status goods (Chipp and Manzi, 2011). For a financially deprived consumer, their marginal utility of income is much higher than for their more affluent peer. If you are struggling with basic needs in your everyday life, to spend additional resources on goods that are not commodities of basic necessity is too costly or even impossible. Nevertheless, in some cases, consumers choose to exchange part of their essential need budget to obtain goods that signal status. In a study by Colson-Sihra and Bellet (2018) consumers, living in conditions close to or below the poverty line in India, indicated that they sacrifice around 13% of their daily caloric food intake to get what they call aspirational goods, that is, goods whose demand grows with an increase in deprivation. Even under threat of malnourishment, consumers spent a considerable amount of money on goods that do not serve a (high-) nutritional value. Examples would be cold drinks, branded clothing, packaged products, dairy and meat (which are an expensive source of caloric intake and could be more affordably replaced with cereals, fruits, and vegetables).

Although upward comparisons within a society are highly common, the biggest impact of them is concentrated for social circles close to ours. Status signaling tends to be directed not toward those at the top of the income pyramid, but rather the ones closer to you, whether it is people who are slightly more affluent or consumers physically close to you, such as neighbors (Colson-Sihra and Bellet, 2018; Hamilton, 2012). For this reason, status signaling plays a big role when an individual moves into a new environment. If you migrate to a new area, status signal can be a way for you to establish your position within the social network of more affluent residents, this way getting access to social circles and more resources (Danzer et al., 2014). Status signaling also can have a different end-goal: a person might seek to be distinct from others around them

or, vice versa, seek to integrate to a certain social group. Integration can also happen to fictional social groups. For example, poorer household members might wish to identify with middle-class characters from popular TV shows, thus engaging in the consumption of branded goods seen on the screen as a way to escape the daily routine (Tufte, 2000).

However, status consumption can increase consumer happiness, especially among the most deprived ones (Jaikumar et al., 2018). Purchases of seemingly unnecessary goods for households that are already deprived of financial resources might not seem like an optimal decision, but it can carry the benefits associated with a subjective understanding of one's well-being, one of them being the positioning of oneself in a higher social circle. A purchase of counterfeit branded goods is a widely observed practice among low SES consumers (Van Kempen, 2003). Nevertheless, although consumers are willing to pay a premium to acquire them, in the end, those goods serve the same functionality and have an added benefit of signaling higher status, provided that consumer disappointment with the strength of the signal is not significant (Van Kempen, 2004). Therefore, such purchases can have a welfare-enhancing effect as long as well serves its practical and status signaling purpose.

There are also other examples of signaling behaviors that serve a different purpose from an improvement in subjective social standing. As shown in a study by Hill et al. (2012b), examples from periods of economic difficulty have shown an increase — or at least no change — in consumption of female beauty products, which do not belong to the category of basic need commodities. However, the authors show that there is a practical reason for these observations. Results of the study suggest that females are willing to sacrifice a part of their already scarcer income on appearance enhancing products to increase their attractiveness and, in turn, boost their chances of attracting a more affluent mate who would give them more security and contribute to their living with additional resources. Netchaeva and Rees (2016) expand this argument by including another reason: increase in consumption of beauty products during difficult economic periods can also help in securing better job propositions which are in greater need during recession times. In their study, job-seeking dominates mate-seeking as the main reason behind the use of appearance enhancing products.

1.3 Nutritional Choices and Unhealthy Commodities

Socioeconomic disparities also reflect in dietary choices. For example, it is a commonly observed trend that low SES populations have higher rates of obesity both in developed and developing countries (James, 2004), their average diet is less diverse (**?**) and of lower quality (Erber et al., 2010). These patterns are also a big problem for children and adolescent (Kim, 2001; Olivares et al., 2007). Moreover, the experience of deprivation in childhood forms dietary behaviors for adulthood: if consumers grew up in low SES environment, they tend to consume unnecessary calories (surpassing their actual energy needs) in the adulthood as well (Netchaeva and Rees, 2016). The cause of poor nutritional state is usually a low intake of fruits and vegetables (Höglund et al., 1998; Lindström et al., 2001). Low SES consumers or households tend to prioritize price versus health in terms of food products (Pechey and Monsivais, 2016). Products that are rich in fat and sugar and more energy-dense are usually cheaper than the healthier alternatives (Pechey and Monsivais, 2016). Price levels are an important — yet, not a single — reason behind the choice of food products that have a lower nutritional value. Other potential

reasons include, among others, compensatory eating, which is a tendency to compensate for a potential scarcity of energy in the future with extra caloric intake in the presence (Sterling, 2015) as well as stress and anxiety, which are the states when consumers tend to prefer to consume foods with a higher caloric value that usually induce more pleasure and comfort (Bratanova et al., 2016; Cheon and Hong, 2017a; Langer et al., 2018). Food can also work as a status signaling good: in addition to previously mentioned example by Colson-Sihra and Bellet (2018), it has also been observed that consumers with low subjective SES tend to consider meat as a type of food that signals status, which can interfere with medical and environmental advice on consuming less meat Chan and Zlatevska (2019). Overall, food is an important constituent of general well-being, nutritionally and psychologically, and can affect happiness via different channels. Dietary choices can serve the purpose of counteracting negative experiences in daily life and satisfying emotional needs. On the other hand, external reasons might push consumers to make such decisions (such as income level and price of products), but this leads to poorer dietary habits that directly impact well-being.

In addition to processed foods high in fat and sugar, other unhealthy commodities — that are tobacco, alcohol, and sugar-sweetened beverages—show an increasing trend of consumption, especially for low SES populations (Stuckler et al., 2012).Sugar-sweetened beverages are immensely popular among teenagers and young adults (Singh et al., 2015), but the average consumption falls with increase in household income (Bolt-Evensen et al., 2018; Fontes and Fisberg, 2020; Han and Powell, 2013). For example, in the USA, the sugar-sweetened beverages constitute around 117 cal per day for the consumer in a household with income higher than $75,000 per year, while the daily amount of calories from such beverages almost doubles (200 cal) for consumers in the lower part of the income distribution, that is households with income less than $25,000 per year (average data for a period of 2009–2016) (Allcott et al., 2019b). The reasons for this vary. The poorer nutritional knowledge and cost are among the most cited (Bolt-Evensen et al., 2018; Fontes and Fisberg, 2020). In addition to sugar-sweetened beverages, alcohol and tobacco are two commodities that are widely consumed in low SES populations (Bhan et al., 2012). As noted in previous examples, relative deprivation is sufficient to increase the consumption of certain unhealthy commodities. Mulia and Karriker-Jaffe (2012) find that low SES consumers are at higher risk of having alcohol-related problems if they live in more affluent neighborhoods. However, some studies suggest that alcohol consumption is distributed more equally among the whole income distribution (Kell et al., 2015). In terms of smoking, the rates of tobacco use are highest in the most disadvantaged neighborhoods, especially among people experiencing long-term unemployment, single parents, homeless, people with mental illnesses, prisoners, certain groups of new immigrants and ethnic minorities (Hiscock et al., 2012).

1.4 Consumer Happiness in Poor and Affluent Societies

The history of humanity is studded with examples of deprivation and poverty that have faded from the earliest times to the present day. While our ancestors suffered from hunger and scarcity, in recent times, the wealth of nations has undergone a substantial increase that seems to want to grow more and more. But, does being wealthier consumers mean being also happier consumers? Does living in a bigger home, owning a more luxurious car, having a more varied diet, experiencing a more luxurious holiday, also translate into being a happier consumer?

The difference between rich and poor consumers can come in two forms: at the macrolevel and at the microlevel. At the macrolevel, we distinguish between poorer and richer countries: in lower-income countries, consumers are generally poorer; in wealthier countries, consumers are generally richer. At the microlevel, the difference is between richer and poorer consumers within the same society. The two-level distinction is important because the social comparison process is especially active at the microlevel, while at the macrolevel the comparison process occurs to a lesser extent. For example, looking at a neighbor who can afford a luxury car while we cannot, certainly generates a feeling of uneasiness and dissatisfaction with our utilitarian car. If instead, a person who lives in a particularly rich state has a luxury car, this does not generate in us the same extent of dissatisfaction with our utilitarian car. Comparison processes are relevant when we deal with the distinction between absolute happiness (i.e., independent of other people's happiness or on our previous happiness) and relative happiness (i.e., which depends on other people's happiness or our previous happiness).

The topic linking socioeconomic status to happiness at the macrolevel has received considerable attention from the literature, but the conclusions are not always straightforward. At the macrolevel, researchers try to answer the question of whether consumers in richer countries are happier than consumers in poorer countries. Stated in other terms, this is the age-old problem of whether money buys happiness. According to some research, wealthier people are also happier (Argyle, 2001; Hagerty, 2003) but other data indicate an absence of a relationship between income, wealth, and happiness (Clark et al., 2008; Diener and Biswas-Diener, 2002; Easterlin, 1995; Headey et al., 2008). Some authors have also argued that the inconsistent results are determined by an imprecise measurement of well-being. According to the psychologist, Daniel Kahneman, Nobel Laureate for economics in 2002, there are at least two different ways of measuring happiness. The first, defined as emotional well-being, refers to the emotional quality of experiences, for example, the frequency with which we feel joy or sadness during the day. The second, called life evaluation, refers to the evaluation that people make of their lives when they look back and must say, in general, how happy they are with the life they have lived up to that moment. The two forms of happiness are influenced by different factors. Income and education influence life evaluation, while health, care for others, loneliness, and smoking affect the emotional well-being. Having more money and being better educated (higher SES), therefore, increases happiness for life in general, measured by questions such as "In general, how satisfied are you with your life overall?". But when we turn to emotional well-being, having more money and more education increases happiness only up to an annual income of around $75,000 (Kahneman and Deaton, 2010). A higher SES therefore buys satisfaction for life but not happiness; on the contrary, not having money makes consumers both less satisfied with life and less happy (Kahneman and Deaton, 2010).

A related issue is a diverse capacity by rich and poor societies of extracting satisfaction from climbing the happiness ladder. Consumers living in poorer societies are still climbing the happiness ladder. For this reason, every step forward on this hypothetical scale will produce greater happiness for consumers in developing countries, compared to consumers in already developed countries,who are already in the regions of high value and every step forward will be neutralized from the lack of meaningful social comparison. Social comparison is especially important at the microlevel when consumers can make comparisons with their neighbors and

it is especially strong for those goods that are termed learned preferences as opposed to innate preferences (Tu and Hsee, 2016). While innate preferences are those that satisfy the needs of survival, such as, for example, the preference for hot rather than freezing water, learned preferences, instead, are the result of years of interactions between human beings with each other and have the value of signaling the distinction between belonging to one social status or another. For example, a Gucci watch is an asset that makes consumers happy not for its intrinsic value but for its status-symbol value. According to some authors, goods like these also have a shorter duration in the degree to which they can make a person happy: happiness lasts less because it adapts more quickly (Tu and Hsee, 2016). Indeed, happiness derived from status-symbol goods needs a social comparison process to survive, whereas happiness derived from essential goods does not. Furthermore, goods that have a status-symbol value do not have a stable comparison scale from which to derive happiness, because they are based on comparison with others, which is a changing element of the context. For example, we could derive a lot of happiness from buying a luxury watch, if the others do not own it but if the others become like us, our advantage to have a luxury item disappears. In other words, the value of a good that derives its' value from a social comparison process undergoes a form of hedonic adaptation (Tu and Hsee, 2016).

The problem, therefore, becomes that of understanding how consumers in the richest and most developed countries can still increase their happiness, given the limited room for maneuver. One recommendation is to identify needs that have not yet been met. For example, even in the most advanced societies, many people suffer from boredom, depression, or lack of free time. Not to mention more serious health problems such as real mental illness. One suggestion is to try to satisfy this type of needs. By doing so a substantial increase in happiness will be obtained. Being able to have free time (Hsee et al., 2010), find the true meaning of life and have a satisfactory network of social relationships (Baumeister and Leary, 1995), or being able to help others (Aknin et al., 2013; Dunn et al., 2008), are all types of goods that can increase happiness in richer countries where social comparison and hedonic adaptation reduce the possibility of a further increase in happiness.

1.5 Socioeconomic Status and Happiness With Material and Experiential Purchases

Buying an experiential good offers a greater feeling of happiness than buying a material good (Millar and Thomas, 2009; Thomas and Millar, 2013; Van Boven and Gilovich, 2003). The difference between the two types of goods, and the happiness they produce, has been widely examined by several researchers. The first type of goods indicates those products that make us feel an emotional experience deriving from the senses, such as a holiday, a film, a day at the spa, a cooking seminar, a perfume, a dinner at a restaurant, a trip to the museum, a horseback ride, an experience of river rafting or a relaxing massage, to name a few. Tangible goods, on the other hand, are concrete purchases, which offer an experience of pleasure linked to the fact of owning them, such as a car, a watch, a smartphone, a T-shirt, a necklace, a house, a boat, and so on.

The reasons why experiential goods make us happier than material ones are still being evaluated, but it seems that two factors contribute to this difference. The first is the centrality

of the type of experience for the definition of our identity: an experiential good contributes to a more positive definition of our identity and achieves the need for self-fulfillment and personal growth. In other words, after having lived an experience, a consumer will feel internally richer, while after purchasing a material good a consumer will feel externally richer. The two would have a different weight on happiness: the first would contribute more to make consumers happy than the second one (Kasser and Ahuvia, 2002; Kasser and Ryan, 1996; VanBoven, 2005). The second factor is social interaction. Experiential goods usually go hand in hand with greater social interaction, compared to material goods (VanBoven, 2005). Going to dinner in a restaurant determines a greater possibility of weaving relationships with others than simply owning a new dress. And social relationships are known to make us happy (Diener, 2009).

Socioeconomic status determines a different happiness with the consumption of the two types of goods. People with lower SES status are happier after consuming material goods, while people with higher SES status seems to be happier with experiential goods (Kasser and Ahuvia, 2002). For example, Thomas and Millar (2013) found confirmation for the fact that consumers are happier for experiential purchases than for material ones; and they found also confirmation for the explanation that the reason is that experiential goods enrich the person and the identity more in terms of personal growth. However, not all consumers behaved in this way. Consumers with low SES (a watch, a piece of jewelry, a car, a smartphone) than they were when buying a life experience. On the contrary, high SES consumers were equally happy with both types of purchases (Thomas and Millar, 2013). Similarly, it was found that the utilitarian value of a purchase (i.e., finding it useful) was a predictor of social confidence (i.e., how much I think I impress people with the purchase I made) for low-income consumers, but not for high-income consumers; on the contrary, the hedonic orientation (i.e., enjoy the shopping trip for itself) was a predictor of social confidence for both high- and low-income consumers (Paridon et al., 2006).

According to Kasser and Ahuvia (2002), the difference between people of low and high SES is also determined by the fact that material goods relieve the poorest from their constant concern with the scarcity of resources that they constantly face and this would increase happiness. And in fact, the poor buy material goods to feel "less poor", that is, to feel that they belong to a higher social class (Sangkhawasi and Johri, 2007).

1.6 Socioeconomic Status and Happiness with Specific Product Characteristics

Socioeconomic status not only affects the different happiness resulting from the consumption of material goods rather than experiential ones. But within each of these types of goods, socioeconomic status also determines a different appreciation for some aspects rather than others. For example, researchers involved in studying what are the aspects that have the greatest impact in determining a satisfying experience with a meat steak have identified some fundamental characteristics: tenderness, juiciness, and flavor. Among these, the flavor is the one that contributes most to the overall satisfaction with the product with 49.4%, followed by tenderness (43.4%) and juiciness (7.4%) (Felderhoff et al., 2020; **?**). But these results cannot be generalized to the various socioeconomic strata of society. It has been discovered that high-SES consumers appreciate tenderness more, while low-SES consumers appreciate juiciness and flavor more (Felderhoff

et al., 2020).

Where beef food safety is concerned, certification strategies (traceability or quality labels) are used to increase perceived safety. High-income consumers are more likely to pay a premium for certified beef than low-income consumers, showing that they give to safety a priority role (Angulo and Gil, 2007). Higher-income consumers are also different in their food preferences. For example, they were found to be more likely to choose fish and other seafood products frequently (Myrland and Lund, 2000; Thong and Solgaard, 2017) probably because low-income consumers are highly sensitive to price (Steptoe and Pollard, 1995). However, income does not predict a different quality perception and a different taste perception for Pringles and Coca Cola, when these products were offered in English packaging or adapted in Urdu packaging showing no income effects on satisfaction with food as a function of the type of packaging (Khan and Lee, 2020). On the same vein, higher-income consumers pay more attention to informational aspects of food consumption (label use, information use, healthy eating, food safety, consumer rights, and consumer responsibility) (Nam, 2019). Therefore, lower-income consumers do indeed show different preferences and satisfaction with some food characteristics, but only up to a certain point.

1.7 Socioeconomic Status and Satisfaction With Health Care

Socioeconomic status also influences how satisfied we are with medical care. Lower income consumers are generally more satisfied with the received medical care than higher-income consumers (Fox and Storms, 1981). This is probably because people are different concerning their preferences for certain types of care and what they expect to receive. The less wealthy consumers, having lower expectations and less detailed preference models, are probably happier with the care received. For example, if a person who has a sore throat thinks she/he has tonsillitis and goes to the doctor for a cure, she will be satisfied if she receives an antibiotic and a certificate of disease to stay home a week from work. In other words, receiving the diagnosis that is expected makes you satisfied (Ong et al., 2007). The lower-income consumers may not have a clear idea of the diagnosis, might not have a clear expectation regarding the cure and therefore would experience less dissatisfaction with the lack of congruence between expectation and results.

This ties in with paradoxical data that shows that patients who receive worse medical care because they live in poorer countries have greater satisfaction with the care received (Kruk et al., 2018). Just to quote some data, out of eight low-income countries surveyed, 79% of the consumers said they were very satisfied with the care received, even though they received less than half of the essential care (Kruk et al., 2018). The explanation seems linked to the factors that contribute to determining satisfaction with the medical treatments. Satisfaction with the treatment depends both on the treatment received but also on the accessibility, the costs, the state of health, the expectations, the immediate results and the gratitude. Among these factors, low expectation of medical care seems to explain the anomalous data found among the poorest consumers of medical care. In other words, poorer people would have less knowledge of what should be expected and therefore less chance of properly evaluating a medical treatment. For example, a vignette in which a doctor changed the medication of a hypertensive patient without measuring his pressure, was rated as good to excellent by 53% of the 17,966 interviewed, all living in low-income and middle-income countries (LMICs).

On a different but parallel vein, in a meta-analysis of the literature, it was found that the oldest, the least educated, those with a higher social status (occupational status) and the married were the most satisfied with the medical care (Hall and Dornan, 1990). This result is noteworthy because it is very strange to observe that social status goes in the opposite direction of education. According to the authors, apart from the need for further studies that can shed light and possibly disconfirm this relationship, a possible explanation is that being very educated but having a low-status job creates a great frustration and dissatisfaction with the doctor who has a high-status occupation. This resentment would result in dissatisfaction with the service. The relationship between high social status and greater satisfaction, however, seems easier to understand. Those who have a good job usually turn to the best doctors and also get better treatment.

The link between socioeconomic status and satisfaction with health care is still a matter of debate. More recent studies have tried to determine in which way the relationship stands, but without any success. In some studies, higher socioeconomic consumers are happier with health care, in other studies, the opposite was found with no clear explanation for the incongruent findings (Batbaatar et al., 2017).

1.8 Socioeconomic Status, Customer Satisfaction, and Loyalty

The greater the competitive forces in a market, the greater costumers' expectation and the greater the need for the firms to hold on to existing customers (i.e., loyalty). One of the factors considered as most important in inducing a customer to repeat the purchase of the same product is the happiness of the customer with that product. Happier costumers turn into more loyal customers (Anderson and Sullivan, 1993). However, this relationship has been questioned: there are loyal but not happy costumers and there are happy costumers who, however, do not remain faithful (Jones et al., 1995). Here, we will examine the role that socioeconomic status differences play in this relationship.

Indeed, while lower-income costumers tend to satisfy this relationship, that is, those who are more satisfied are also more likely to remain faithful to the product, this is not always true for medium–high-income costumers. For the latter, being more satisfied with the product does not always imply that they will also be more loyal costumers and therefore will buy the same product again in the future (Homburg and Giering, 2001). According to the researchers, this could be explained by the fact that, while for a low-income customer making a purchase involves a very high financial risk, for a customer with more financial availability, a mistake would not be catastrophic (Kaplan et al., 1974; Murray and Schlacter, 1990). Therefore, a richer customer could repeat the same purchase even if he was not satisfied with it the previous time, giving, in fact, a second chance to the product: a risk that he can afford. The research also highlighted that only the wealthiest customers are influenced by the purchasing process (e.g., how kind the salespeople are, etc.) when they have to decide whether to buy the same product again. Probably, consumers with higher income also have a higher education which makes the relationship and the exchange of information with the seller an important factor for them. However, other data show that consumers with higher SES are also more satisfied with the three primary needs: autonomy (the need to be in control of their actions), competence (feeling capable of managing important tasks), and relational (the need to feel close and supported by significant others) (Deci and Ryan,

2000). And this greater satisfaction translates into greater loyalty, at least for performing arts type of product (White and Tong, 2019).

Chapter 2

Blinded by worries: sin taxes and demand for temptation under financial worries[1]

with Sergiu Burlacu, Piero Ronzani, and Lucia Savadori

Imposing "sin" taxes has been the preferred way governments tried to discourage the over-consumption of temptation goods for decades. However numerous evidence shows that consumers exhibit behavioral biases which can affect their reaction to taxes. This paper investigates a potential bias and how it affects demand for temptation: financial worries associated with poverty have been shown to shift attention towards pressing needs, often at the expense of forward looking decisions. In an online experiment with UK participants, we randomly induce financial worries and ask participants to allocate a budget between basic necessities and temptation goods in an experimental market. We randomly impose "taxes" on temptation by increasing its price. We find that, in the absence of any tax, inducing financial worries lowers demand for temptation, effect stronger for lower income participants. However, when financial concerns are salient, increasing the tax does not lower demand among lower-income participants. While financial worries might protect against over-consumption of temptation in the absence of tax changes, they also might hurt the poor the most when additional taxes are introduced.

Keywords: psychology of poverty, temptation, addiction, sin taxes, bandwidth, scarcity

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2.1 Introduction

The shift in behavior concerning the over-consumption of *temptation* (addictive) goods, such as alcohol and tobacco, has been a goal of policy makers for decades. One of the main methods employed by governments is the so called *sin taxes* - a mechanism levied on goods that produce negative health effects and tend to be over-consumed. However, evidence shows that consumption of such goods as junk food, sugary drinks, or tobacco, has a strong disproportionate socioeconomic concentration (Gruber, 2001; Allcott et al., 2019b; Colman and Remler, 2008; Maclean et al., 2014), which implies that low-income consumers might carry a bigger tax burden. Using US cigarette consumption data, Gruber and Kőszegi (2004) show that overall benefits from averted internalities offset the incurred costs and overturn the regressivity[3]. However, the overall welfare gains from sin taxes can end up being lower than expected solely because of behavioral aspects that are not considered in the classical economic models of addiction.

The last few decades have seen numerous studies documenting differences in behaviors by low socioeconomic status consumers. Financial deprivation has been found to have a negative emotional impact on consumers (Zhou and Fishbach, 2016; Kristofferson et al., 2017; Botti et al., 2008) and through time alter their self-beliefs in an unfavourable way (Botti et al., 2008; Sharma and Alter, 2012). Such feelings of financial inferiority cause willingness to counteract it by engaging in e.g. selfish acts (Roux et al., 2015), status- (Griskevicius and Kenrick, 2013; Chaplin et al., 2014; Hill et al., 2012c) or exclusivity- (Sharma and Alter, 2012) seeking behaviors. In addition to this, a separate stream of works on *the psychology of poverty* has found financial deprivation to affect cognitive functioning, attention (Mullainathan and Shafir, 2013; Mani et al., 2013), memory (Tomm and Zhao, 2016), investment in human capital (Lichand et al., 2018; Burlacu et al., 2019), productivity (Kaur et al., 2019), risk and time preferences (Haushofer and Fehr, 2014). When contemplating such evidence, one is certain to ask whether these behaviors would alter the normative response to taxes. A handful of works on tax salience have found consumers to under-react to not-fully-salient taxes (Morrison and Taubinsky, 2019; Feldman et al., 2015; Chetty et al., 2009; Taubinsky and Rees-Jones, 2017). However, in line with studies showing that consumers who face resource scarcity also tend to make more attentive decisions (Shah et al., 2012, 2015) and be more efficient with the use of those resources (Mullainathan and Shafir, 2013; Mehta and Zhu, 2016; Rosa et al., 2012), Goldin and Homonoff (2013) suggest that low income consumers might be more attentive to non-salient taxes levied on tobacco.

This paper explores a behavioral bias emerging from the psychology of poverty literature which has not been studied in the context of consumption of temptation and sin taxes[4]. Poverty, scarcity of financial resources, has been shown to affect cognitive performance (Mani et al., 2013) and investment decisions in human capital (Lichand et al., 2018; Burlacu et al., 2019). The proposed channel is scarcity shifting focus towards the scarce resource (Shah et al., 2018), often at the expense of forward looking decisions. One feature of poverty not explored until

[3]This is an empirical exercise and cannot be generalized, the exact outcome depends on various behavioral biases. Furthermore, they only consider time inconsistency as bias in their models, while there is evidence that other biases may also affect disproportionately the poor.

[4]For theoretical work on relationship between poverty and temptation, see Banerjee and Mullainathan (2010) and Bernheim et al. (2015). Banerjee and Mullainathan (2008) explores theoretically the link between poverty and limited attention.

now is that it also implies scarcity of immediate gratification. A person experiencing poverty has much stricter constraints when it comes to the choices she or he can make. Shopping for groceries means carefully selecting necessary items, not allowing much slack for goods that offer immediate pleasure. Temptation goods may serve this need and are much harder to substitute given a limited budget (low income individuals cannot afford going to cultural events, dining out or going on holidays as easily as the higher income persons can). If low income people perceive temptation goods as scarce, attention may be redirected towards them which could be a channel explaining why they over-consume them. In this paper we investigate which of the two forms of scarcity is more likely to drive the behavior of low income people when financial worries are salient. In addition, we investigate if such psychological mechanisms may change how people respond to sin taxes.

In an online experiment in the UK, 808 participants are first assigned to a psychological manipulation which aims to mimic the mental burden of poverty by making thoughts associated with economic vulnerability and lack of financial resources salient. For brevity, we will refer to this as increasing financial worries (FW henceforth)[5]. Next, participants are endowed with a £30 budget to be spent on basic necessities (food, household products) or on temptation (alcohol, tobacco, sugary drinks, sweets, unhealthy snacks and personal luxury goods) in an experimental market. At this stage a second treatment is introduced by exogenously varying the price of temptation: a random share of participants face the market price while the rest face higher prices by 10% or 20%. We will refer to this treatment group as the *Tax group* throughout the paper.

We find that increasing FW decreases the demand for temptation when no additional tax is applied. The magnitude of the effect is the equivalent of a 10%, suggesting that the shift in focus towards necessities may actually protect against the over-consumption of temptation. However, in line with Shah et al. (2012), Zhu and Ratner (2015) and Burlacu et al. (2019), we find that the shift in focus comes at a price: participants become far less sensitive to other relevant information, in our design this being the increase in prices. For the Hard group, the effect of the tax is statistically insignificant at all tax levels. In contrast, demand for temptation is highly elastic in the Easy group.

We check if results vary by tertiles of income. As expected, both effects are stronger for lower income participants. Increasing FW has a large effect on their demand in the absence of the tax. However it also makes them unresponsive to taxes. When FW are not made salient, lower income participants decrease demand only in response to the 20% tax. Averaging the tax effects in the Easy and Hard conditions results in a flat demand curve for the lower income group. In contrast, higher income participants respond strongly to the tax, especially in the Easy condition. However, increasing FW also appears to decrease the demand and to dampen their response to the tax. Among the middle tertile, increasing FW does not have any significant impact on demand at any tax level.

We investigate several mechanisms suggested by the literature for the observed effects (lower

[5]Adapted from Mani et al. (2013), the treatment consists of asking participants to ponder how their household could cope if they had to face various economic shocks. The treatment varies the severity of the shock, from mild for control group participants (Easy Scenarios - *Easy group/condition*) to severe for the treatment group (Hard Scenarios - *Hard group/condition*).

demand for temptation and non-response to increases in prices when FW are salient): the hypothesized *shift-in-focus* channel (proxied through through two survey questions), cognitive reflection, risk and time preferences, and affective states. Only the first channel explains a substantial share of both effects.

Welfare implications are not straightforward and should be treated only as suggestive given the absence of a normative counterfactual. Independently of the Easy-Hard condition, we find the elasticity of demand with respect to price to be increasing in income which would suggest such taxes to be potentially regressive. Looking at the dynamics by Easy-Hard condition, we again find that the elasticity increases with income when FW are not salient. Increasing FW reduces demand by the highest amount for lower income participants when no additional tax is added, but at the expense of making them unresponsive to taxes. From a policy perspective, the results suggest that low income individuals may not respond optimally to sin tax increases in periods of economic instability.

This paper speaks to several literature. To our knowledge, this is the first paper to study addictive goods in the psychology of poverty framework and to provide causal evidence that perceived financial worries associated with poverty can lower the demand for addictive goods, while distorting how people respond to sin taxes. In these lines, the paper contributes to the vast behavioral economics literature documenting the role played by psychological factors on (i) addiction (Gruber, 2001; Gruber and Kőszegi, 2004; Allcott et al., 2019a) and (ii) public policy in general (Amir et al., 2005; Bernheim and Rangel, 2007; Chetty et al., 2009; Congdon et al., 2011; Chetty, 2015; Bernheim and Taubinsky, 2018). Furthermore, the paper contributes to the growing literature on the psychology of poverty (Mani et al., 2013; Mullainathan and Shafir, 2013) and in particular strengthening the finding that poverty shifts focus to pressing needs at the cost of under-weighting other relevant information (Shah et al., 2012, 2018; Tomm and Zhao, 2016; Lichand et al., 2018; Burlacu et al., 2019). Finally, the paper contributes to the growing experimental literature using laboratory or field experiments to study public policies (Alm, 2010; Rees-Jones and Taubinsky, 2016; Mullainathan and Shafir, 2013; Taubinsky and Rees-Jones, 2017; Lunn and Choisdealbha, 2018).

The paper is organized as follows. Section 4.2 reviews the recent literature on sin taxes and psychology of poverty. Section 2.3 presents the hypotheses, experimental design, data details, descriptive statistics and balance checks, while Section 3.3 discusses the manipulation check and main results. Finally, Section 2.5 provides a discussion and concludes.

2.2 Literature

Deviations from behavioral norms due to financial deprivation have gained momentum in various literatures of a recent decade. Studies on resource scarcity and consumer behavior have underlined an array of such examples. In general, consumers tend to continuously make upward social comparisons (Corcoran et al., 2011; Hill et al., 2012a), which can result in feelings of inferiority (Sharma and Alter, 2012) when they are relatively deprived in finances. To counteract it people may develop a wish to establish status (Griskevicius and Kenrick, 2013), usually manifested through higher level of materialism (Chaplin et al., 2014). Status-seeking has also been observed through higher consumption of beauty products by women facing financial constraints (Hill et al., 2012c; Netchaeva and Rees, 2016). Feeling relatively deprived of finances in com-

parison to others also pushes consumers to mitigate this by directing their attention to scarce goods, especially if they make your ownership more exclusive (Sharma and Alter, 2012). Conversely, Karlsson et al. (2005) find that households which feel comparatively worse-off in financial terms report less acquisitions of durable goods and tend to plan their purchases in greater details (Karlsson et al., 2005).

A restricted choice set, i.e. a smaller number of available products and services, due to lower financial resources, can also have an emotional impact on decision making (Zhu and Ratner, 2015) causing feelings of aggression (Kristofferson et al., 2017), anger, depression or stress (Botti et al., 2008). Limited availability of products can trigger the need to consume more of them; moreover, the amount consumed can go unnoticed while trying to meet this new higher level of satiation (Sevilla and Redden, 2014). Chronic choice restriction can decrease self-esteem, efficacy and autonomy (Bone et al., 2014). When reminded of scarce resources, consumers tend to engage in selfish acts, directing resources towards their own needs; even generosity is exhibited only in cases where it is also possible to achieve personal gains (Roux et al., 2015). A study on scarcity, consumer choice, and neuroimaging by Huijsmans et al. (2019) has suggested that the increased focus on scarce resources decreases the activity in the brain centre associated with goal-directed decision making and the effect is strongest when scarcity is preceded by the period of abundance, which would suggest that focus on financial deprivation interferes with the ability to follow goals in decision making.

A stream of empirical studies on *psychology of poverty* and its impact on economic decision-making ties into the previously mentioned examples: by shifting individual attention to that which is lacking, scarcity of resources is found to affect cognitive functioning (Mullainathan and Shafir, 2013). This leads people to overlook certain information when making a decision (Mullainathan and Shafir, 2013) or keep monetary concerns on top of their mind even when it is not explicitly linked to the situation one is supposed to think of (Shah et al., 2018). For example, in an experiment by Tomm and Zhao (2016) participants with smaller endowment spent more time looking at the prices when asked to choose from a menu in a restaurant and were able to remember them more accurately as compared to "richer" participants; however this came at a cost of unnoticed discount announcements that could have helped them to save money. Financial concerns were also linked to cognitive functioning in a work by Mani et al. (2013). In a first study carried out in a shopping mall in the US, participants were asked to reflect how they would cope with hard financial situations as compared with easy ones. People of lower income performed much worse in unrelated cognitive tasks when prior exposed to hard financial scenarios, while people of higher income were found to exhibit no difference in their scores. In a natural experiment carried out in sugarcane farmers' villages in India, farmers performed worse in cognitive tasks before harvests (period of higher financial scarcity) than after harvest. In a similar design to the sugarcane farmers' experiment, Carvalho et al. (2016a) administered before and after payday surveys to US households. Results indicate stronger present bias in decisions involving monetary rewards in the before-payday survey, although no differences are found in regard to risk preferences, cognitive functions, and quality of other decisions. Burlacu et al. (2019) suggest that, when faced with financial worries, parents tend to overlook the opportunity to invest in the human capital of their child by choosing necessities, such as groceries, instead of highly subsidized educational materials for children.

Although some of the purchasing decisions might look sub-optimal when made under a financial constraint, consumers facing resource scarcity are also found to be more focused on the greatest needs when evaluating the trade-offs in their consumption decisions, react less to framing (Shah et al., 2015), and be more attentive and engaged (Shah et al., 2012). People with low income show higher efficiency in the use of resources (Mullainathan and Shafir, 2013) and choose necessities more as compared with discretionary goods (which are chosen more by people that are relatively financially unconstrained) (Cole et al., 2008). Restricted choice of available purchases can also foster more creative problem solving (Botti et al., 2008), such as thinking of more uses for the product as compared to its intended function (Mehta and Zhu, 2016; Rosa et al., 2012).

This plethora of consumer behaviors comes into focus when discussing such matters as effect of price increases. Certain policies can have the intention to tackle the over-consumption of particular products, however, as highlighted above, lack of financial resources or monetary concerns can interact with purchasing decisions. Evidence tends to point out that the consumption of tobacco, sugar-sweetened beverages, or junk food is prevalent among lower socioeconomic status individuals (Gruber, 2001; Colman and Remler, 2008; Maclean et al., 2014; Allcott et al., 2019b; Dubois et al., 2017; Wang, 2015; Allcott et al., 2019a). These products are - in many cases - also subject to so called *sin taxes*, i.e. taxes put on goods that are associated with over-consumption and negative effects to both consumer and society. However, if low income consumers tend to consume more temptation goods, they might carry a bigger tax burden on their shoulders. As Allcott et al. (2019b) point out, we need a clear distinction between the weight of tax burden and the overall harms and benefits. First, consumption decision depends on price elasticity of demand, meaning that although poorer individuals might consume more temptation goods *per se*, they may be more price elastic. Moreover, if poorer households decrease their consumption of temptation goods such as tobacco or sugary drinks, this results in better health outcomes and, in turn, lower medical expenditures, increase productivity and life expectancy, thus overturning the regressivity argument (Gruber and Kőszegi, 2004; Allcott et al., 2019b). The problem arises when we discuss behavioral biases: the choice of consuming temptation goods can stem from such issues as misinformation or self-control and it can bias the estimated positive impact of sin taxes downward. Allcott et al. (2019a) estimate that, for example, sugar-sweetened beverage tax designed without addressing behavioral issues can result in $1 billion a year less of welfare gains in the United States.

Poverty itself may amplify such biases. Recent theoretical work building on the framework of time inconsistent preferences (Thaler and Shefrin, 1981; Laibson, 1997; O'Donoghue and Rabin, 1999) suggests a causal relationship between poverty and temptation. Banerjee and Mullainathan (2010) allow for good specific discount factors by defining temptation as goods providing utility only in the present. The present-self does not want future-selves to consume and as a result prefers consuming more today, leading to apparently higher observed discount factors. Assuming that temptation has a lower share of marginal expenditure as income increases (temptation does not rise proportionally with income), the structure of temptation described above is much more consequential for the poor by causing more severe self-control issues. This can help explain a wide range of puzzling behaviors, from savings, credit and investment behaviors to the emergence of poverty traps conditional on initial wealth. In a different theoretical setting concerning

savings and credit constraints, Bernheim et al. (2015) demonstrate a similar perpetuating causal relationship between poverty and self-control. According to their model, below a certain asset level self-control is even impossible to exert.

To our knowledge there are only a handful of experimental studies looking at the behavioral response to taxes. A stronger focus was dedicated to the issue of tax salience. Feldman and Ruffle (2015) run a series of experiments to look at how different tax schemes - tax-inclusive, -exclusive, and -rebate - impact final demand of products in the experimental market. The results imply that consumers tend to overweight posted prices and exhibit higher demand when taxes are calculated at the checkout. Feldman et al. (2015) elaborate further on this matter by asking how different tax levels affect this failure to account for taxes which are not included in the posted price; they do not observe a decline in good purchases when tax levels increase. Taubinsky and Rees-Jones (2017) look at this matter from a perspective of consumer mistakes. According to the results of their experiment, although the reaction to not-fully-salient taxes is very heterogeneous among the sample, on average consumers under-react to taxes, i.e. they respond to sales tax as if its size was just 25% of the original tax level. Moreover, as underlined by Morrison and Taubinsky (2019), consumers use rules of thumb for reacting to taxes (which are very heterogeneous among the experimental population), but once taxes increase they pay for increased attention with higher mental cost. To put tax salience to test in a field setting, Chetty et al. (2009) run an experiment where they compared demands for products with normal and tax-inclusive price tags. They find that tax salience reduces consumption by 8 percent. Moreover, in their observational study on alcohol consumption, increase in taxes which are posted on price tags is found to have a higher effect on lowering alcohol consumption when compared to taxes applied in the register. Similarly to Chetty et al. (2009), Goldin and Homonoff (2013) checked for the effect of tobacco tax salience by income levels. All consumers reacted to taxes in the posted prices, but lower income individuals were more attentive and reactive to taxes applied in the register.

The gap this study is trying to fill is investigating temptation consumption decisions under financial worries. Previous studies have focused on time inconsistency as a main bias, affecting dis-proportionally the poor, and leading to over-consumption of temptation (Gruber, 2001; Gruber and Kőszegi, 2004; Allcott et al., 2019a,b). The attention reallocation caused by focusing on monetary concerns may be an important additional factor affecting to a larger magnitude the consumption decisions of the poor.

The setting of this study is The United Kingdom, which has a long history of tobacco and alcohol duties. Among European Union countries, UK has one of the highest rates of beverage taxes among all categories (Angus et al., 2019). Duty rates differ based on a the type of beverage (beer, cider, wine, or spirit) and the strength of it, where drinks with higher strength are taxed more. On the demand side, Sousa (2014) finds that in the UK a great majority of alcohol products have an inelastic demand and the elasticity estimates do not change significantly between low, medium, and high income households (data from 2007-2012). Moreover, national data suggests the presence of the so-called "alcohol harm paradox": although low socioeconomic status individuals consume lower quantities of alcohol compared to other groups, they experience significantly more health problems related to alcohol use, which are possibly aggravated due to worse health choices in other domains (smoking, unhealthy diet, lack of exercise, etc.) (Bellis

et al., 2016). For tobacco products the UK government uses a so-called "tobacco tax escalator" which means that tax rises automatically by 2% above the inflation level every year since 2010 (Fuchs et al., 2019). Although overall smoking rates and cigarette consumption have been gradually decreasing in the last decades, not all tobacco products are price elastic; in particular, rolling tobacco has an estimated price elasticity of -0.57 (Whitaker, 2019). Among OECD countries, in 2016 UK exhibited one of the highest average cigarette prices (Whitaker, 2019), yet the prevalence of smoking and, more importantly, inequality in smoking habits, remains high: the difference between smoking rates of individuals with low education levels compared to high reaches 15-20 percentage points and is expected to keep growing for the next decade (Song et al., 2020). To combat child obesity problem, in 2018 the United Kingdom has implemented a new type of *sin tax* - sugar-sweetened beverage tax (SSB). A drink containing 8g or more of sugar per 100ml is taxed by 24p per liter, and 18p per liter if sugar content is between 5-8g of sugar, with the exception of fruit juices with natural sugars and drinks high in calcium. In 2017-18, obesity problem affected around 10% of children aged 4-5 and 20% children aged 11-12; moreover, obesity prevalence in most deprived areas was twice as high as in least deprived areas (NHS, 2019). This problem affects also the adult population: in 2017, 64% of adults in England were considered overweight or obese (NHS, 2019). Previous attempts to tackle SSB consumption in UK with price increase on individual restaurant level have shown positive results - drop in SSB purchases - in short and medium term (Cornelsen et al., 2017); however, a nation-wide models on SSB tax effects suggest that a regressive impact on low income consumers can be particularly strong due to prevalence of sugary drink purchases among this socioeconomic group (Tiffin et al., 2015). Other sugar sweetened products such as cakes, confectionery, and sweet snacks, are not yet subject to taxation. According to the study on price sensitivity for these product groups in the UK by Smith et al. (2018), price increase due to fiscal measures is likely to reduce purchases, especially for low income consumers.

2.3 Empirical Strategy

This section presents the empirical strategy starting with the hypotheses and experimental design in Subsection 2.3.1, followed by the description of the data collection in Subsection 2.3.2. Subsection 2.3.3 presents descriptive statistics, and checks for balance and selective attrition across treatment arms.

2.3.1 Hypotheses and Experimental Design

The study has been run in May 2019, on Prolific, a crowdworking platform which has been noted for its better representativeness at national level (UK) compared to other widely used platforms (Peer et al., 2017; Palan and Schitter, 2017). Participants are paid on hourly wage basis with potential bonuses conditional on their performance in given experimental tasks. The study was pre-registered on the Open Science Framework (OSF) platform[6]. Sections of the analysis that deviate from the pre-registration are highlighted throughout the paper.

The main motivation of this study is to understand how financial worries (FW) affect: (i) the trade-off between addressing pressing needs (purchasing necessities for the household)

[6]To view the pre-registrations access `https://osf.io/fpkjw`

and falling into temptation (purchasing temptation goods) and (ii) the response to potential policies which aim to discourage the consumption of temptation, in particular to sin taxes. Treatment effects are expected to be much stronger among low income individuals, however the ex-ante hypotheses are not straightforward for several reasons. The mental bandwidth/scarcity framework lacks a testable theoretical model[7]. Empirically, financial worries are expected to lower mental bandwidth (induce cognitive load) while also redirecting it towards what is perceived as scarce (the so called tunneling effect) (Lichand and Mani, 2020). The former effect may lead to a higher likelihood of falling to temptation due to reduced cognitive control of impulses (see Mani et al., 2013). The latter effect is expected to shift focus towards necessities if necessities are perceived as relatively more scarce than temptation, thus reducing demand for temptation. As a result, the net effect is thus an empirical question. How financial worries may interact with sin taxes is even a more complex issue. This is because increasing the price of temptation may increase the perception of its scarcity. Temptation goods provide immediate gratification which may be perceived as scarce by low income people, since they cannot afford the same substitutes as the high income individuals (e.g. restaurants, cultural events, holidays). As a result, financial worries and sin taxes may interact in complex ways. This paper is limited in its testable predictions and is to an extent exploratory.

In practice, we manipulate the perceived FW through exposure to hypothetical financial scenarios and observe purchasing decisions in an experimental market where participants can choose to spend a fixed budget on necessities and temptation. A random subset of participant face higher prices of temptation goods than the retail prices. This treatment aims to mimic taxes on temptation - or "sin taxes" - how they are commonly referred to. We will refer to this treatment as the Tax condition throughout the paper. These two treatments are also interacted to observe if higher FW may change how individuals respond to sin taxes. The rate of the price increase was also assigned randomly to either 10% or 20% level relative to the baseline prices[8]. Throughout the analysis, we will also explore this heterogeneity. To summarise, the experiment used between-subject design where each participant was first randomly assigned to one of the two conditions within Financial scenarios; afterwards, they were randomly assigned to Tax or No Tax condition in Experimental market (participants in Tax treatment were also randomized between two tax levels - 10% and 20%). All participants then completed the Survey section. The experiment structure was as follows: Financial scenarios treatment ⇒ Manipulation check ⇒ Experimental market and Tax treatment ⇒ Survey section[9]. Figure 2.1 presents experimental design graphically.

In what follows we describe the treatments, the manipulation check and the experimental market.

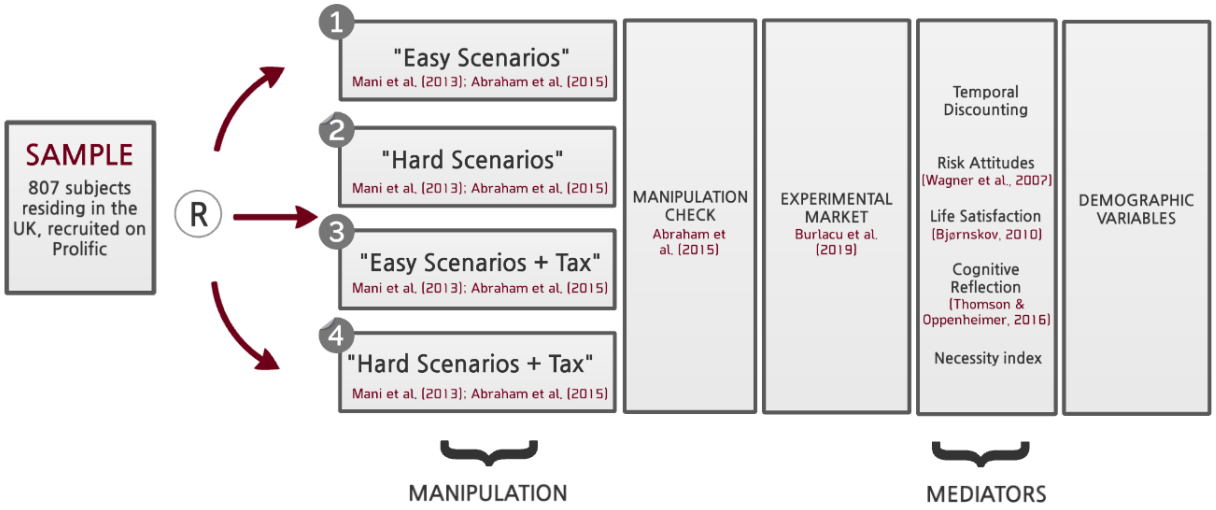
Financial Scenarios: Participants were asked to reflect how their household would cope

[7]Banerjee and Mullainathan (2008) build a simple attention model which provides a good reference point but is not easily testable empirically.

[8]We randomly assigned 40% of participants to the no Tax group and 60% to the Tax group (half to 10% tax and half to 20% tax). This was to have more power in detecting differences between the no-tax and the Tax group, than to detect differences between the two tax levels

[9]This section included the measurement of the proposed mediators (further discussed in Section 2.4.3 and socio-economic information (income, employment status and household size). Other socio-economic variables were measured by the experimental platform when participants registered.

Figure 2.1: Flowchart describing the procedure of the experiment



with two income shocks: (i) a large one time shock and (ii) a deterioration in economic conditions at national level leading to higher costs of living. Adapted from Mani et al. (2013)[10], the scenarios aim to trigger mental thoughts of economic vulnerability which participants from low income households are likely to experience often in their daily lives[11]. Participants were asked to answer to both open questions and questions with Likert scales. What varies between conditions is the severity of the situations presented[12]. Participants in the control group were presented with easy scenarios (henceforth Easy group/condition). For the treatment group scenarios were much more severe (henceforth Hard group/condition). The order of the two scenarios was randomized at individual level.

Manipulation Check: After completing the two scenarios, all participants were asked to state, on a Likert scale, how worried they are about (i) their financial situation and (ii) about not being able to find money in case of need (adapted from Abraham and Haushofer, 2015). The aim of these questions is to test if the hard financial scenarios successfully triggered the response we described above.

Experimental market: Next, participants proceeded to the main task. Each participant received an endowment of £30 which they could spend in the experimental market. They could choose from 66 items sold by one of the largest low cost retailers in the United Kingdom. The products were chosen based on their popularity on the online store platform of the retailer. Half of the items were basic necessities (e.g. bread, eggs, milk, fruits, vegetables etc.), or household items, such as washing liquid or cleaner[13]. The prices ranged between £0.59 to £6. The other half were

[10]Differently from Mani et al. (2013) we reduced the number of scenarios to 2 (from 3), and increased the severity of the scenarios for the treated group while decreasing it for the control group, based on qualitative evidence from a previous pilot study suggesting that the control group scenarios were too difficult, triggering high financial worries.

[11]Following the design of the manipulation by Mani et al. (2013), we have not included a pure control group that was not asked financial worries inducing questions.

[12]See Appendix 3.B

[13]See Table A-1 in Appendix 3.B for the full list of products

temptation goods, such as alcohol, tobacco, unhealthy foods (sweets, sugar sweetened beverages, chips etc.) and personal luxury products, with prices ranging from £1 to £20. Each product had a picture, name, price and a link to the retailer's online shop web-page with additional information on the product[14]. The interface looked very similar to a typical online shop. Participants could add goods to their shopping cart, increase quantities and revise their selection at any time. They had to spend at least £28 to advance to the next stage; the remainder from £30 were sent as bonus payment[15]. The order of the goods was randomised. The task is weakly incentivized: 1 out of every 100 participants was randomly selected to receive the goods they selected on a date of their choice[16]. In spite of the weak incentives, 87.5% of participants reported that they chose what they would normally choose when they do the groceries[17].

Tax treatment: Participants assigned to the Tax condition were informed that some of the goods have higher prices than the retail price (not by how much)[18]. In the experiment we did not frame this price increase as a tax because we aimed to focus on the price channel driving changes in behavior. Standard economic theory predicts that taxes change behavior only by increasing prices. Rees-Jones and Rozema (2019) show that, in practice, tax changes are accompanied by other non-price interventions (information provision, attempts at persuasion etc.). While we expect the absence of the frame to reduce the influence of such non-price channels, we note that the price increase itself may signal the desired behavior and could lead to experimental demand effects[19]. In the task, participants could see the old price crossed out next to the new price. While this lacks realism as price and tax increases are rarely made salient in the posted price[20], we did not want visual salience effects (not noticing the higher prices) (as in Chetty et al., 2009) or effort (to discover which products have higher prices than the retail prices) to interact with

[14]See Figures A-1 and A-2 in Appendix 3.B for screenshots of the task

[15]The £2 margin was chosen so that participant spent most of the endowment in the task while also not being too restrictive and cognitively demanding

[16]Although other experimental studies on consumer behavior and taxation also administered monetary incentives only to a subset of participants (see Taubinsky and Rees-Jones, 2017; Morrison and Taubinsky, 2019), there is a lack of evidence on differences in hypothetical vs incentivized consumer choice in similar experimental markets. A review by Charness et al. (2016) suggests that paying a subset of participants might not decrease their motivations in the tasks substantially and could work as efficiently as paying all, although this depends on the theoretical framework behind the experimental task. Some works on choice experiments with real market goods have found that the introduction of monetary incentives does not significantly alter product preferences as compared to purely hypothetical choices (Mørkbak et al., 2014; Yue and Tong, 2009).

[17]At the end of the study participants were asked to describe their motivations during the shopping task. 8 out of 808 participants described making choices in the experimental market randomly and 2 participants mentioned that they perceived shopping budget as windfall or bonus money. The majority of experimental subjects (87.5%) described choosing goods based on their routine product choices, current household needs or personal preferences. The remaining 11.3% did not provide informative enough answers. This question was administered after the shopping task and participants were not told in advance about it.

[18]Which goods had a price increase was not made explicit to reduce the risk of experimenter demand effects. We did not want explicit labeling of price increase as *tax* make participants more self-conscious about choosing *unhealthy* goods due to the idea of corrective taxes. We acknowledge that the way the price changes were presented might be perceived as noisy. To minimize such possibility, as described previously, we aimed to design shopping task to closely resemble a real online shop, and offered several opportunities for participants to observe the prices of selected goods: (i) both old and new prices were presented, while (ii) the decision process required minimum two steps: goods were first added to the basket followed by a review of the shopping cart (modify quantities, remove goods, the option of going back to the goods selection step.)

[19]Given their reduced social acceptance, for temptation goods it is very challenging to eliminate the influence of non-price channels.

[20]In the UK, sin taxes are already included in the posted price and are not made explicit.

the psychological treatment.

After completing the task, participants proceeded to the survey section of the experiment. The variables measured are presented in subsections 2.3.2 and 2.4.3.

Limitations: While the experimental market featured products familiar to participants and had an easy to use interface that made it feel like an ordinary online purchasing platform, there are several concerns regarding the extent to which the task can capture its real world counterpart. The first concern is that participants can substitute "extra-taxed" products in the experimental market with identical products at lower prices in the real world market. Thus participants in the Tax condition could avoid the tax by simply re-optimizing their household consumption plans (e.g. buy more groceries in the experiment and more temptation outside the experiment). With this in mind, it is possible that the elasticities of demand estimated are upward biased but we do not have strong reasons to expect this to vary by treatment status. Second, participants could choose goods which have a higher reselling value to exchange them for cash outside the experiment. In our setting, such goods would likely be the temptation goods. However, we would expect a higher demand for temptation goods if this would be the case which does not match our data. Third, being *forced* to spend at least £28 out of the experimental budget might have created unnatural circumstances where participants might have constructed their good basket and then rounded up the sum with some temptation goods. However, due to experimental market design, we were not able to check the order of product selection. Finally, whether the endowment was earned or not can matter in some settings (Harrison, 2007; Cherry et al., 2005; Luccasen and Grossman, 2017; Ackert et al., 2006). It is not clear however, if in our setting this would lead to a higher or lower demand for temptation or how it would interact with the psychological treatment.

2.3.2 Data and Power

Participants could not take part in the experiment if they were (i) below the age of 24, (ii) heavy drinkers (more than 14 units per week) or (iii) have undergone therapy for alcohol abuse. These variables are included in the Prolific's pre-screening database which means participants could not lie to be able to participate in our study. We decided to impose an age limit to screen out participants which may not be financially independent. Eligibility criteria (ii) and (iii) were added for ethical concerns. At the end of the study, participants received a debriefing. The sample size (808 participants) was chosen, motivated by estimates from a previous study, to detect effects above £3 by income subgroup at 5% significance level with 80% power. For the whole sample, the estimated minimum detectable effect is around £1.5.

2.3.3 Descriptive Statistics, Balance Checks and Selective Attrition

Random assignment into treatment groups leads to causal inference if attrition was not influenced by treatment assignment and if the randomization was successful in terms of observable (and unobservable - not testable) characteristics. In this subsection, we evaluate both concerns and also present descriptive statistics.

Attrition. Online experiments often suffer from high rates of attrition, which when left unattended, can lead to flawed causal inferences (Zhou and Fishbach, 2016; Horton et al., 2011). Taking part in online experiments has lower fixed costs than laboratory experiments which usually

require registering ahead of time and going in person to the lab. Furthermore, participants can exit at any time without fearing any social punishment, from other participants or the experimenters. In our setting, both treatments could induce participants to exit the experiment before completion. Reflecting on ones financial vulnerability and facing price increases could trigger negative emotions which may increase the likelihood of dropping out of the study. If this were to happen, causal inference would be challenged since treated participants leaving the survey may be systematically different from those who opt to stay. We evaluate this by regressing the decision to drop out on treatment status. We consider only cases where participants left the survey when assigned or after being assigned to one of the treatments. Some participant left the survey prior to this and are not considered in the analysis. Only 39 participants dropped out, which represent less than 5% attrition rate, remarkable in an online experiment. Table A-1 in Appendix 2.A shows the results. Participants are slightly more likely to drop out when exposed to hard scenarios and taxed but the differences are small and statistically insignificant. Overall, the results show attrition is not a major concern for causal inference.

Balance Checks. Given the 2x2 experimental design, we need to evaluate whether randomization was successful for both treatments, accounting also for the interaction between the treatments. Table 2.1 shows means for the Easy group in Column (1), Hard group in Column (2), Tax group in Column (3) and Hard condition and Tax group in Column (4). The last Column displays the p-value associated with the F-test of joint significance of the differences between the treatment arms. Out of 14 comparisons, we find 2 variables to be significantly different across treatment groups. The Hard Tax group, in particular, has a lower share of overweight participants whereas the Hard group has fewer participants which are parents. Neither variable is a strong predictor of behavior in the task. Nonetheless, to alleviate concerns, we include them as covariates in all models. Notably, yearly income per adult equivalent[21], which is our explored source of heterogeneity in treatment effects, is very well balanced across treatment arms. We also evaluate whether randomization was successful within the Tax group, across the two levels. Table A-2 in Appendix 2.A shows the means for the three tax level groups and the p-value of the differences. Only the share of parents is statistically significant at 10% level and other 3 differences have low p-values. Again, to mitigate concerns, we also control for them throughout the analysis.

Descriptive Statistics. Females are over-represented in our sample with 68% of participants. The age of participants range from 26 to 86 with a mean of roughly 43 years. 56% attended university and about 72% are employed either full or part-time. Less than 7% are immigrants, 57% self-report being overweight and 17% are smokers. On average, participants report consuming around 3.3 units of alcohol a week. The average household size is 2.8, 60% of the sample are parents and the average total yearly household income is £36,800 with a median of £32,500, both higher than the national levels in the UK in 2019. The large share of females is a major concern regarding the representativeness of our sample[22]. We also acknowledge that the profile

[21]We followed Mani et al. (2013) and used the OECD square root equivalence scale, dividing total yearly household income by the square root of household size (Rights and Unit, 2008; OECD, 2011). We had two measures of income: (i) one measured after the main task in the survey section of the experiment, and (ii) one reported by participants when they registered on Prolific. Given that the latter is possibly outdated, we used the former in the analysis. Nonetheless, they are strongly correlated (Pearson correlation coefficient = 0.79).

[22]The demand for temptation good is roughly twice as large for males than for female. Treatment effects

Table 2.1: Descriptive statistics and Balance Checks

	(1) Easy	(2) Hard	(3) Tax	(4) Hard Tax	(5) p-value
Female	0.72	0.64	0.70	0.68	0.41
Age	42.88	42.77	43.26	42.08	0.76
High education	0.58	0.53	0.57	0.57	0.85
Student	0.10	0.05	0.04	0.06	0.12
Employed	0.71	0.76	0.73	0.77	0.57
Nationality UK	0.94	0.93	0.96	0.91	0.23
Overweight	0.61	0.56	0.62	0.50	**0.04**
Alcohol consumption	3.43	3.40	3.22	3.15	0.80
Smoker	0.20	0.18	0.15	0.18	0.53
Household size	2.75	2.68	2.86	2.82	0.52
Parent	0.61	0.50	0.65	0.61	**0.03**
Subjective SES	5.20	5.18	5.16	5.35	0.60
Income	22.47	22.23	23.51	23.36	0.77
Observations	179	163	244	222	

Note: Columns (1) - (4) show the means across treatment arms. Column (5) displays the p-value associated with the F test of joint orthogonality across treatment arms. Easy defines the control group exposed to easy scenarios and no Tax. Hard defines the treatment group exposed to the hard financial scenarios and no Tax. Tax defines the treatment group exposed to the 10% or 20% increase in prices. Subjective SES is measured on a scale (ladder) from 1 to 10, with 10 being represented by the the people who are better off (in terms of education, money and jobs) in the UK. Alcohol consumption is measured in units of alcohol (1 unit of alcohol = 1 small glass of wine; half pint of beer; pub measure of spirits). Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds. The higher number of observations in the Tax conditions is due to our sampling strategy (40% No Tax, 60% Tax) which allows more power to detect difference between the two tax levels.

of the participant in online experiment might exclude certain relevant categories of people. Even though our experiment was mobile friendly, registering on the platform requires some level of proficiency with mobile and internet use, and having a bank account.

The main outcome variable we will use throughout the paper is total expenditure on temptation using baseline (no Tax) prices. The distribution of the variable is strongly censored at 0, with about 37% of participants purchasing no temptation. Only 3%, spent all the budget on temptation[23]. Pooling together all conditions, participants spent 73% of their budget on necessities but there is substantial variation. Across the subcategories of temptation goods, unhealthy food products and alcohol had the higher demands with mean expenditures at baseline price of £3.4 and £2.2 respectively. Tobacco and luxury items were demanded only by 2.35% and 3.74% of participants. We check to what extent income is associated with higher consumption of temptation using (i) self-reported behaviors and (ii) behavior in the experimental market. Panel A in Table A-5 in Appendix 2.A presents the correlation between income and self-reported consumption of temptation. Since we do not have information on consumption of unhealthy foods, we use weight as a proxy. We find that income predicts a lower probability of being overweight or a smoker, but higher weekly alcohol consumption. Panel B in Table A-5 presents the correlation between income and expenditure in the task by subcategories of temptation goods, controlling for treatment assignment. Despite the fact that income predicts a lower probability of being overweight, it does not predict higher demand for unhealthy foods in the task. On the other

are also stronger for males (results available upon request). This is possibly due to stronger floor effects for females: 35.6% do not demand any temptation in the baseline condition. If our samples of women and men are representative for their Prolific sub-populations, we could expect even larger treatment effects in a more representative sample with respect to gender. However, outside the lab, the size of treatment effects will depend on the degree of income pooling and relative decision power of women and men in the household.

[23]See Figure A-1 in Appendix 2.A

had, income does predict higher demand for alcohol and lower demand for tobacco, consistent with the correlations with the self-reported behaviors presented above.

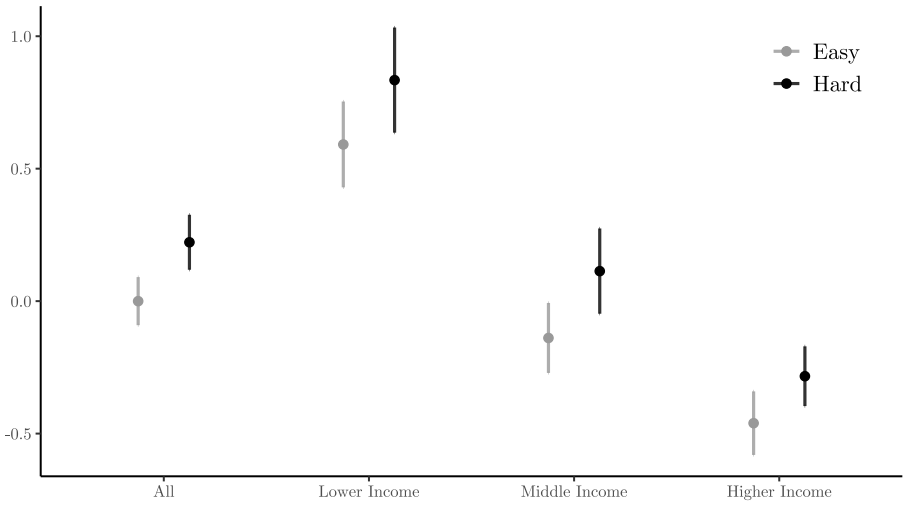
2.4 Results

This section begins with manipulation checks in Subsection 2.4.1. Subsection 2.4.2 presents the main results while Subsection 2.4.3 investigates potential mechanisms explaining the main effects.

2.4.1 Manipulation Check

We begin by examining if being asked to reflect on difficult financial scenarios increases the salience of FW. In the baseline condition, 34% of participants report not being worried at all about their financial situation. 14.5% are very or desperately worried, the rest being somewhat worried. Similar proportions are observed for worries about not being able to find money in case of need. Among treated participants, the distribution shifts to the right. 21% report being worried or desperately worried and only 26% not being worried at all. Figure 2.2 summarizes these finding. We compute a standardized index of the two variables and plot means with 95% confidence intervals. The differences between treated participants and control participants are

Figure 2.2: Manipulation checks: treatment effects on financial worries index



Notes: The outcome variables is an index of worries computed using the inverse covariance weighting method in Anderson (2008), standardized using the control group mean and standard deviation. The variable used to compute the index are: (i) worries about financial situation and (ii) worries about not being able to find money in case of need. Both variables are coded as: 0 "not worried as all", 1 "somewhat worried", 2 "very worried" and 3 "desperately worried". The labels on the horizontal axis indicate the samples used: the entire sample (left) followed by the sample divided by income tertiles. The dots indicate the means while the vertical lines indicate 95% confidence intervals. Easy indicates participants assigned to the easy scenarios, while Hard indicates participants assigned to the hard scenarios.

large and highly statistically significant. Table **??** in Appendix 2.A shows regression results of the two variables and the standardized index of them on treatment assignment, including covariates. The treatment leads to 0.25 standard deviations higher index of FW. Such an increase in FW is equivalent to having a lower total yearly household incomes by £17,000.

Several of the covariates included have high explanatory power. Females, younger participants, immigrants, smokers, lower SES and lower income participants report significantly higher

FW. Other variables associated with higher FW which are only marginally insignificant are being overweight and being a parent. These results suggest a potential relationship between FW and variables indicating higher consumption of temptation goods. Drinking, however, is not associated with higher FW.

Next, we check if treatment effects vary by income. Previous research has shown that inducing FW impacts behavior only among lower income people (Mani et al., 2013; Burlacu et al., 2019). Given the large sample size for a laboratory experiment and the fact that our sample has a higher income than the UK mean, we split our sample into three income groups - low, medium and high[24]. The average yearly household income is £16,600 for the lower income group (close to the UK relative and absolute poverty line), £33,000 for the medium income group (close to the UK mean) and £60,000 for the higher income group.

Figure 2.2 also plots treatment means and 95% confidence intervals for each income group. Looking at the reference group (Easy), there are stark differences, larger than 0.5 standard deviations, between the lower and other income groups [25]. While statistically significant, the differences between the medium and the higher income group are smaller in magnitude, despite the fact that gap in average incomes between these group is much higher. This suggests that FW are particularly salient at lower income levels and reduce at increasing rates at higher income levels. Even though the effect of the treatment is largest among the lower income group, it is not statistically significant from the other two groups as evidenced in Table A-4 in Appendix 2.A. The scenarios adapted from Mani et al. (2013) were augmented in severity for the treatment group. This may have contributed to increased financial worries also at higher levels of income.

It is worth mentioning that even though we experimentally manipulate only transitory worries, our paper explores permanent worries as a relevant source of treatment heterogeneity. Given the large differences in baseline levels of worries by income group (a proxy of permanent worries at group level), when analysing treatment effects by income group[26] in the following subsection, we are comparing differences between groups with large differences in average permanent worries[27]. Even though the literature is scarce on the impact of permanent financial worries given that it is difficult to manipulate experimentally, other more permanent features of life in poverty (such as stress, depression, happiness, life satisfaction) which are likely correlated with financial worries (possibly caused by them), were studied to a greater extent (see Haushofer and Fehr, 2014, for a review).

[24]This deviates from the median split strategy specified in the pre-analysis plan. Based on previous studies performed on Prolific, we expected participants to have lower incomes than what we obtained in the sample. Since the focus of this research is households living in poor condition or at risk of falling into poverty, a median split for our sample would include a large number of households falling outside these categories.

[25]It should be noted however than comparisons between income groups within the Easy scenario group may not reflect differences in financial worries which we would observe if no scenarios were administered. Qualitative and quantitative evidence from the answers of participants to scenarios' items suggest that the easy scenarios may have induced FW to lower income participants. Lower income participants are significantly more likely to report that even these hypothetical scenarios may significantly impact their lives. As a result, all comparisons between income groups within the easy scenario group may suffer from this issue.

[26]We can not use the index of worries as a source of heterogeneity in treatment effects because the variable is endogenous to the treatment.

[27]Notably, other characteristics which are different by income group could be potential confounders.

2.4.2 Main Results

We begin the analysis by estimating the change in demand as a result of price increases independently of the Easy-Hard condition using the following specification:

$$Y_i = \alpha + Tax_i'\beta + X_i'\gamma + \epsilon_i \tag{2.1}$$

where Y_i is expenditure on temptation at baseline prices (or demand for temptation)[28] and Tax_i is a vector of tax levels (0%, 10% and 20%). X_i is a vector of individual and household characteristics[29].

Next, we introduce the Easy-Hard condition in the model and allow it to interact it with the tax using the following specification:

$$Y_i = \alpha + \gamma Hard_i + Tax_i'\beta + Hard \times Tax_i'\delta + X_i'\gamma + \epsilon_i \tag{2.2}$$

where $Hard = 1$ if assigned to the Hard condition and 0 if assigned to the Easy condition. $\widehat{\gamma}$ indicates the estimated effect of increasing FW in the no Tax condition, the vector $\widehat{\beta}$ gives the effect of the taxes in the Easy condition while $\widehat{\delta}$ indicates if the effect of the tax varies by Easy-Hard condition. We will interpret the estimates both in levels but also in percentage changes relative to the baseline no Tax condition in order to compute elasticities. All models are estimated through OLS and use robust standard errors.

Table 2.2 reports the results across Easy-Hard condition (Equation 2.1), in Column (1) without covariates and Column (2) including covariates[30]. We note that including covariates does not alter the estimates, expected since randomization was successful. Turning to the results, we observe that being assigned to any of the Tax conditions lowers demand for temptation. The 10% tax level decreases demand by £1.1 (14.4%), while the drop is roughly twice as large with the 20% tax (£2.4 or 32.7%). Thus, across the entire sample and Easy-Hard conditions, participants display an elastic demand as response to the taxes. Note also that elasticities are roughly constant at the two tax levels.

In Column (3) we estimate Equation 2.2 allowing tax responses to vary by the Easy-Hard condition. The first two estimates (Tax 10% and Tax 20%) are interpreted as the effect of the tax in the baseline (Easy scenarios) condition, that is the effect of the taxes when FW are less salient. We observe larger estimates than the ones in Columns (1) and (2) (£1.88 or 22.7% at 10% Tax and £3.85 or 46.7% at 20% Tax) indicating demand elasticities of close to 2.

The estimate on the Hard condition indicates the difference in demand for temptation relative to the Easy condition, when experimental taxes are absent, and responds to our first research

[28] In Appendix 2.A, Table A-6 and Table A-7 we report also the results by subcategories of temptation goods, by income tertile. We do not discuss the findings since we are under-powered to detect differences by subgroups of products.

[29] Covariates include: gender, age, education (1 if attended university and 0 otherwise), student status (1 if currently studying and 0 otherwise), employment status (1 if employed full or part time and 0 otherwise), whether the participant is overweight, medium-heavy drinker and smoker (all self-reported), parental status, household size, subjective socio-economic status (1 to 10 scale) and income per adult equivalent (income divided by the square root of household size).

[30] See Figure A-2 in Appendix 2.A for a graphical representation of results in Table 2.2. The lines report the means while the bars indicate the 95% confidence intervals.

Table 2.2: Treatment effect on demand for temptation

	Demand for Temptation: total expenditure at baseline prices					
	(1)	(2)	(3)	(4) Lower	(5) Middle	(6) Higher
Tax 10% (10%)	-1.21*	-1.14*	-1.88**	0.17	-0.86	-3.88**
	(0.66)	(0.65)	(0.93)	(1.76)	(1.61)	(1.70)
Tax 20% (20%)	-2.40***	-2.40***	-3.85***	-3.23*	-2.38	-6.39***
	(0.67)	(0.68)	(0.93)	(1.68)	(1.79)	(1.59)
Hard (H)			-2.12**	-3.13**	-0.44	-1.98
			(0.94)	(1.55)	(1.64)	(1.84)
Hard × Tax 10% ($H \times 10\%$)			1.57	0.39	0.63	1.66
			(1.29)	(2.33)	(2.35)	(2.44)
Hard × Tax 20% ($H \times 20\%$)			3.08**	5.36**	-0.94	5.05**
			(1.37)	(2.50)	(2.35)	(2.45)
Control Mean	7.33	7.33	8.23	7.06	7.74	10.06
Controls	No	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.01	0.04	0.05	0.04	0.03	0.07
Observations	808	808	808	271	268	269
p-values - Tests:						
(i) 10% = 20%	0.08	0.06	0.04	0.06	0.39	0.08
(ii) 10% + $H \times 10\%$ = 20% + $H \times 20\%$			0.64	0.41	0.06	0.63
(iii) 10% + $H \times 10\%$ = 0			0.73	0.69	0.89	0.21
(iv) 20% + $H \times 20\%$ = 0			0.44	0.24	0.04	0.47
(v) $H + H \times 10\% = 0$			0.54	0.11	0.91	0.85
(vi) $H + H \times 20\% = 0$			0.34	0.25	0.42	0.04

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. The sample used is the entire sample in Columns (1)-(3) and the first, second and third teriles of income in Columns (4)-(6). Hard indicates participants assigned to the hard scenarios condition. Tax 10% and 20% indicate participants assigned to the treatment groups where the prices of temptation goods were increased by 10% and 20% respectively. The control means in Columns (1)-(2)/(3)-(6) are the means of the outcome variable in the no Tax groups/Easy scenario no Tax group. Tests (iii)-(iv) test for the effect of taxes in the Hard condition. Tests (v)-(vi) test for difference between the Easy and Hard condition at each Tax level.

question. Increasing FW leads to a large and significant drop of £2.1 (26%) in the demand for temptation when no tax is added. The effect is roughly equivalent to increasing prices by 10% in the Easy condition and suggests that FW may potentially limit the over-consumption of temptation[31].

Finally, we move to the estimates on the interaction terms which respond to our second research question. The estimates are interpreted as differences in tax responsiveness in the Hard condition relative to the Easy condition. For instance, a positive estimate indicates a lower response to the tax in the Hard condition. This is what we observe. While the previously discussed finding suggested a potentially protective role of FW, the results on the interaction with taxes point to a more nuanced picture. Increasing FW greatly attenuated the elasticities of demand with respect to price. The estimates on the interaction terms offset almost completely the effect of the tax, at each level. Specifically, in the Hard condition, a 10% Tax lowers demand by only £0.31 (5%)[32], while the effect of the 20% Tax reduces demand by only £0.77 (12.6%), both inelastic responses. In fact, at 20% Tax the participants in the Hard condition actually demand more temptation than the Easy group, albeit the difference is not statistically significant ($p - value = 0.34$).

[31] We stray from making normative statements given than we do not observe the normative counterfactual of each participant.

[32] This value is obtained by summing the estimates on $Tax10\%$ and Hard × Tax 10%, $-1.88 + 1.57 = 0.31$

We now turn to the question of whether treatment effects vary by income. In Subsection 2.4.1, we have shown that the psychological manipulation increase FW for all income groups by roughly the same level. However, it is unlikely that the effect of FW on behavior is linear. A FW "shock" for someone already experiencing a lot of FW will probably have a different impact that an equivalent shock for someone with little FW. Previous research by Mani et al. (2013) and Burlacu et al. (2019)[33] find effects only on the behaviors of low income participants. In these lines, we test if the effects described previously vary by income tertile[34].

First, looking at the means of the control group (Easy and not Tax) we note that demand for temptation increases with the income group. The response to taxes in the Easy condition also varies substantially by income group. At 10% Tax, the lower income group does not respond to the tax. The strongest demand drop comes from the higher income group (£3.88 or 38.5%). At 20% tax, the lower income reduce demand by £3.23 (45.8%), more than the middle income group (£2.38 or 39.7%), but again lower than the higher income group (£6.39 or 63.5%).

Next, looking at the estimate for the Hard condition, we observe that increasing FW lowers demand by the highest amount for the lower income group (£3.13 or 44.3%), roughly the equivalent of the 20% Tax. No effect is found for the middle income group, while the estimate for higher income group is negative and close to £2 though imprecisely estimated.

Turning to the estimates on the interaction terms, among the lower income group we observe that increasing FW leads to a non-download sloping demand curve. Demand remains roughly constant at 10% Tax ($p - value = 0.69$) and actually increases at 20% Tax ($p - value = 0.24$). In line with the results in Burlacu et al. (2019), this finding suggests that policies aiming to (dis)incentivize consumption of certain types of goods may not have the intended results when FW are top of mind. For the middle income group, the differences between the demand curves across the two Easy-Hard conditions are small and statistically insignificant. Finally, turning to the high income group, we observe that being assigned to the Hard condition, attenuates the response to the taxes. The demand curves for the Easy-Hard condition cross each other. At 20% tax demand becomes statistically higher in the Hard condition than in the Easy condition ($p - value = 0.04$.

Averaging across the Easy-Hard conditions, the elasticities of demand with respect to price are increasing with income. In addition, we observe the same pattern focusing on baseline (Easy) condition, suggesting that for this sample "sin taxes" show signs of being regressive. Increased FW leads to the largest drop in demand for the lower income group, but only when no additional tax is introduced. When coupled with an increase in tax, they appear to harm lower income participants the most. Note than one of the main limitation of the task, discussed in a previous section, is that participants could just substitute taxed temptation goods in the experimental market with the same goods at lower price outside the experiment. This observation makes even

[33]The study did not sample high income participants on Prolific. Their comparison by income group is roughly equivalent to our comparison of the lower and middle income groups.

[34]We note that the study is under-powered to measure difference among income groups with statistical precision. We are also under-powered to detect heterogeneous effects by the continuous measure of income. In addition: (i) income is likely to be measured with error, (ii) other factors besides income (such as assets, credit access, social capital) are likely to be very important in determining ones self perceived economic vulnerability and financial worries and, (ii) including income linearly would not suffice to capture the observed pattern by income group.

more striking the fact that averaging across both Easy-Hard conditions, the lower income group is insensitive to price increases.

Robustness checks: We perform several robustness checks and report the results in Table A-9 in Appendix 2.A. First, given the censored distribution of the outcome variable at 0, we report also Tobit models estimates in Columns (1) to (4). Results are consistent across models.

Second, we investigate if results are robust to the amount left by participants as bonus payment in the task (of the £30 endowment). We remind that participants were not constrained to spend the entire endowment in the task and were allowed a £2 margin in order to advance to the next experimental section (see Subsection 2.3.1 for further details). Under some conditions, this feature could be problematic. For instance, in response to the price increase, participants may decrease demand for temptation but in the same time leave a higher amount as bonus payment. Assuming the amount would be spent on temptation outside the experiment[35], this would imply that the impact of the tax is over-estimated. We perform two analyses to investigate if this is indeed a concern, running the specification in Equation 2.2 using as outcome variables: (i) the amount left as bonus payment and (ii) a sum of demand for temptation and the amount left as bonus payment. The former is used to investigate treatment effects on the amount left, while the latter corrects for any distortions caused by this design feature when assessing treatment effects on demand for temptation. Results are reported in Columns (5) to (12) and indicate that our results are highly robust to this design feature.

Third, we test if the inclusion of two moisturizing creams in the temptation basket, goods which do not comply we our definition of temptation, affected our main results. These goods fit more in the category of personal luxury goods (especially for low income participates) but are not goods with negative externalities. Their demand in the task is low (about 2% of participants purchased any of the two and only 4 participants purchased two items). Nonetheless, as a robustness check we re-did the main analysis excluding these goods from the basket of temptation and report the results in Columns (13) to (16). Again, we see that results are robust, generally becoming larger in absolute terms.

Finally, one relevant design concern is that lower income participants exposed to the Hard scenarios, due to higher mental preoccupations may have perceived the crossed out prices as discounts, ignoring actual prices. This could explain why they exhibit a non-negative demand curve. We can not rule out that it did not affect at least some participants. But if that would be a major concern, we should not observe differences by tax level, given that the two groups received exactly the same information. We would expect an upper trend from the baseline Hard condition to the Hard and 10% Tax condition, followed by a relatively flat curve at 20% Tax. Instead, we observe a roughly flat curve from the baseline to the 10% followed by a positive (though non significant, p-value = 0.4) increase in the 20% condition. In addition to this result, it is worth noting that the sequential design of the task allowed participants several instances to observe prices. Moreover, since the goods were selected from a low-cost retailer, low-income participants are expected to be more familiar with the prices of the products and observe with

[35]This is a strong and conservative assumption. In response to the price increases or the hard scenarios, participants may also lower demand for temptation and increase the amount left on the table in an attempt to save money or spend it outside the experiment on non-temptation goods.

higher easy if certain products are priced higher[36].

We conclude this section by briefly summarizing the main results. First, across the entire sample and Easy-Hard condition, we find that an elastic demand for temptation, which hides substantial heterogeneity by the psychological manipulation. Increasing FW significantly lowers the responses to the tax leading to an inelastic demand curve. Furthermore, in the baseline no Tax condition, increasing FW lowers demand for temptation by the equivalent of a 10% price increase. Results vary by income group. In the Easy condition, the higher income group shows the largest elasticities while the lower income group reduces demand only in response to the 20% Tax. With no additional taxes, increasing FW decreases demand more for the lower income group, the equivalent of a 20% price increase. Among the middle income, increasing FW does not affect behavior. A puzzling U shape pattern is observed - the behavior of the higher income group is similar to the lower income group when FW are made salient, though slightly lower in magnitude.

2.4.3 Mechanisms

Up to this point, we interpreted the results in light of the mental bandwidth (scarcity) theory. We assumed that reflecting on the hard financial scenarios lead to mental preoccupations which shift attention towards necessities[37] at the cost of failing to respond to the increase in prices. In this subsection, we explore the validity of our hypothesis, considering also several alternative channels in light of main results from the literature. We try to answer which channels appear to be better at explaining our main findings: (i) increased FW lowers demand for temptation and (ii) increased FW reduces elasticities of demand with respect to price.

Much of the work in the field of mental bandwidth/scarcity has payed little attention to alternative channels which may explain how similar psychological manipulations may affect behavior. For instance, reflecting on potential future economic shocks may change risk attitudes, affective states, how individuals discount the future, or the cognitive systems employed when making decisions. All these channels may be particularly relevant when deciding how much temptation to consume and are potential confounders for our proposed channel. For this reason, after the task we measured several potential mediators: (i) an index proxying the shift of focus towards necessities, (ii) cognitive reflection, (iii) life satisfaction, (iv) risk attitudes, and (v) temporal discounting. We proceed by first motivating the choice of each variable individually, explaining how they were measured, followed by the mediation analysis.

Focus on pressing needs. First, we compute a proxy for our proposed mediator - shift in attention towards pressing needs - by asking participants to state on a 4 item Likert scale if, in the experimental market, they chose goods which gives them pleasure or if instead they chose goods which are necessary for the household. We reverse code the first item and compute a standardized index of the two.

Cognitive Reflection. In Mani et al. (2013), asking participants to go through hard financial scenarios reduced both fluid intelligence and inhibitory control. We measure cognitive reflection,

[36]In addition, the qualitative evidence from the survey question on their choice motivation suggest a high attentiveness to price.

[37]This effects is often referred to as tunneling.

which relates to both fluid intelligence and inhibitory control, and is generally used as an indicator of System 1 - System 2 thinking. Schilbach et al. (2016) argue that when mentally taxed, people are less likely to use the reflective, System 2 thinking (Kahneman, 2011). We measure cognitive reflection using 3 items from the CRT-2 in Thomson and Oppenheimer (2016) which has the advantage requiring only minimal numeracy skills[38]. A higher score is considered to indicate higher use of System 2 reflective thinking.

Life satisfaction. In a review, Haushofer and Fehr (2014) propose affective states as one causal channel through which poverty can impact decision making among the poor. Given that our financial scenarios may have induced negative affect, we measure participant's life satisfaction by asking how satisfied they are with their lives on a 1-10 scale (Bjørnskov, 2010).

Temporal discounting. Consumption of temptation is generally modelled in a dynamic framework (Becker and Murphy, 1988; Gruber, 2001; Gruber and Kőszegi, 2004; O'Donoghue and Rabin, 2003, 2006). With time consistent agents, ones discount rate will influence consumption decisions today. With time inconsistent agents, besides the discount rate, ones degree of present bias and sophistication will also weight in. As a measure of time preferences, we ask participants what would be the minimum amount of money they would prefer to receive today instead of receiving £200 in 2 months. The task has its limitations since it was not incentivized and does not allow to distinguish between discount rates and present bias.

Risk attitudes. Risk preferences are not usually included in models of addiction. However they are likely to play a role since the discounted negative effects vary by individual and are uncertain. Indeed, several studies, including this one, find a strong association between risk attitudes and consumption of temptation even though causality can not be established (Anderson and Mellor, 2008; Dave and Saffer, 2008). We measure self-reported risk attitudes using an item from SOEP (Wagner et al., 2007). Participants are asked to reflect, on a scale from 1 to 10, in general, how willing they are to take risks, with 10 indicating the highest willingness.

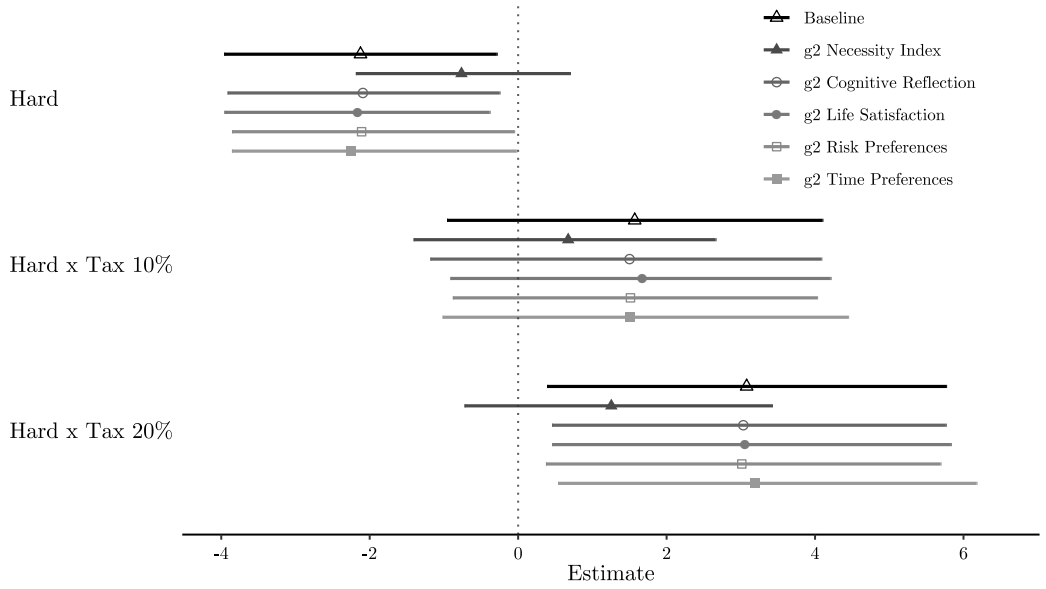
We estimate the Average Controlled Direct Effect (ACDE) for each proposed mediator following the sequential two stage g-estimation procedure in Acharya et al. (2016)[39]. In the first stage, treatment effects are estimated conditioning on the mediator, covariates and potentially confounding mediators. Using the estimates from the first stage, the outcome is demediated by partialling out the mediator. Then, in the second stage the demediated outcome is regressed on the treatment indicators and covariates. If treatment estimates in the 2nd stage change significantly, then the variable is a relevant mediator. We report full-sample treatment assignment estimates and 95% confidence intervals for the baseline model for reference and the sequential estimation models for each proposed mediator in Figure 2.3. Confidence intervals for the ACDE

[38]The questions and the proportions of participants solving them correctly are the following: (i) "If you're running a race and you pass the person in second place, what place are you in? Please write the place as a number." (57.43%), (ii) "A farmer had 15 sheep and all but 8 died. How many are left?" (73.27%), and (iii) "Emily's father has three daughters. The first two are named April and May. What is the third daughter's name?" (71.04%)

[39]In the setting of an experiment, the procedure rests on the assumption of sequential unconfoundedness, that is, conditional on covariates and potentially confounding mediators, there are no omitted variables for the effect of the mediator on outcomes. This assumption is credible in our setting given that we include a relevant set of potentially confounding channels indicated by the literature, conditioning also on individual and household characteristics.

are constructed using bootstrapped standard errors with 1000 repetitions.

Figure 2.3: Mediation analysis: baseline effects and ACDE for each proposed mediator



Notes: The outcome variable is total expenditure on temptation at baseline prices. Symbols indicate treatment indicators' estimates, while the lines indicate 95% confidence intervals (bootstrapped for all the sequential g estimates). Hard indicates participants assigned to the hard scenarios. Tax 10% and 20% indicate participants assigned to the treatment groups where the prices of temptation goods were increased by 10% and 20% respectively.

Results are straightforward. The only mediator having a meaningful impact on the estimated treatment effects is the *change in focus towards necessities* channel. The estimates on both the main treatment effect and the two interactions with the tax levels effects are driven towards zero after demediating the effect that operates through the shift in focus. In other words, financial worries shift focus towards pressing needs reducing demand for temptation but at the same time reducing the responsiveness to the increase in price levels. For all the other mediators considered, the estimates remain largely unchanged relative to the baseline model. Table A-8 in Appendix 2.A reports the results estimated separately by income tertile. Generally, the results are consistent with what was observed for the full sample, especially for the lower and the higher income group; the shift of focus towards necessities being the only mediator having a strong influence on treatment effects[40].

The results are only suggestive and should be interpreted with caution. We acknowledge that at least some of the proposed mediators are likely to be measured with error. None of the tasks were incentivized and some rely on simple measures. In addition, they were measured after the main task which means that we need to assume treatment effects on the mediators lasted throughout the experiment and were not affected by the task itself. There is the possibility that this is not true, at least for some of the participants. In spite of these limitations, this is one of the first studies backing up with suggestive evidence the *shift in focus* mechanism behind the effect of financial worries on behavior.

[40]The only exception is the $Hard \times Tax10\%$ estimate for the lower income group. The necessity index ACDE is actually larger that the baseline effect. However, both estimates have very wide and overlapping confidence intervals.

2.5 Conclusion

In an online experiment with UK participants, we investigate if inducing financial worries impacts purchasing decisions across two categories of goods: necessities and temptation. Additionally, we randomly increase the price of temptation to try to capture if financial worries might affect how people respond to "sin" taxes. In the absence of any price increase, financial worries appear to protect against the over-consumption of temptation, reducing its demand by the equivalent of a 10% price increase. In contrast, when the price of temptation increases, financial worries reduce the elasticity of demand with respect to price, suggesting that the protective effect comes at the cost of not fully processing or responding to other relevant information. Consistent with our hypotheses, we explore several potential mechanisms. We find that increasing financial worries appears to shift focus towards necessities, the mediator capturing a significant share of both effects.

Estimating the two effects by income tertile, we find both to be stronger among lower income participants. Among them, increasing financial worries significantly reduces the demand for temptation in the absence of any tax, while making them completely unresponsive to taxes. No effect is found for the middle income group. In contrast, among higher income participants increasing financial worries appears to lower demand for temptation while also decreasing their elasticity of demand with respect to price. The U shaped relationship by income group is puzzling. The manipulation check shows a similar increase in worries regardless of income group, while the mediation analysis does not point to significant differences in the underlying channels. We can only speculate that such mental preoccupations, as the ones produced by the manipulation, may not occupy the minds of the high income individuals as often as for low income people. As a result, since possibly it is something the higher income individuals are not commonly used to doing, it may have triggered a stronger cognitive or emotional response not fully captured by the manipulation check. This result should also be viewed in relationship to the external validity of the study. In this experiment worries were manipulated only once. However, in real life, the number of times one experiences such "shocks" is likely to vary with income. If for low income individuals this takes place on a regular basis while for those with high income it is rather an unlikely event, then overall the impact on behavior will be stronger for the former group, in spite of the fact that a one time shock has a similar effect. Since we do not have the data to support these claims, we suggest this as a relevant topic for further research in the field.

Since we did not measure the normative counterfactual, we advice caution in drawing strong policy implications. Our results suggest that increasing sin taxes may hurt low income individuals the most if they are experiencing high financial worries. Absent of any tax increase, financial worries reduce demand for temptation, thus possibly protecting low income people from over-consuming such goods. But if additional sin taxes are introduced, they may not decrease their demand further. While we can only speculate, it might be that high financial worries bring increased feelings of economic vulnerability and stress which are likely to produce disutility. Consumption of temptation goods may be a way to compensate such disutility, especially since lower income individuals can afford fewer substitutes (holidays, social and cultural events, etc.). Introducing higher taxes on temptation makes them even harder to afford which may increase their desirability since they are even scarcer than before.

The study has other limitations. First, we do not know if the finding that low income individuals fail to respond to taxes when worried is short-lived or not. If the effect is only temporary and they end up adjusting their demand, then financial worries may end up protecting them from over-consuming temptation. This is a relevant question for future research. Secondly, as mentioned above, we are unable to explain the similar pattern for the higher income group. Thirdly, common to most of the existing literature in the subfield, the degree of external validity is a concern. We are unable to inform on the dynamics of financial worries: how frequent they manifest in the daily lives of low income individuals, how sophisticated they are to anticipate them, or how they can lead to more permanent shifts in worries (possibly manifesting in chronic stress, anxiety, depression or other mental issues). Lastly, in our experimental setting we taxed all temptation goods which is rarely the case in reality. Thus, we can not generalize our results to situations when only some types of temptation goods are taxed, allowing people to substitute them with other un-taxed goods.

Appendix

2.A Appendix A

Table A-1: Attrition by Treatment Status

	(1)	(2)	(3)	(4)
Hard	0.015			-0.012
	(0.015)			(0.021)
Tax		0.0070		
		(0.014)		
Tax 10%			0.0070	-0.0047
			(0.017)	(0.023)
Tax 20%			0.0069	-0.030
			(0.018)	(0.020
Hard Tax 10%				0.0077
				(0.026)
Hard Tax 20%				0.032
				(0.030)
Control mean	0.039	0.042	0.042	0.048
Adj. R^2	0.00	-0.00	-0.00	0.00
Observations	847	847	847	847

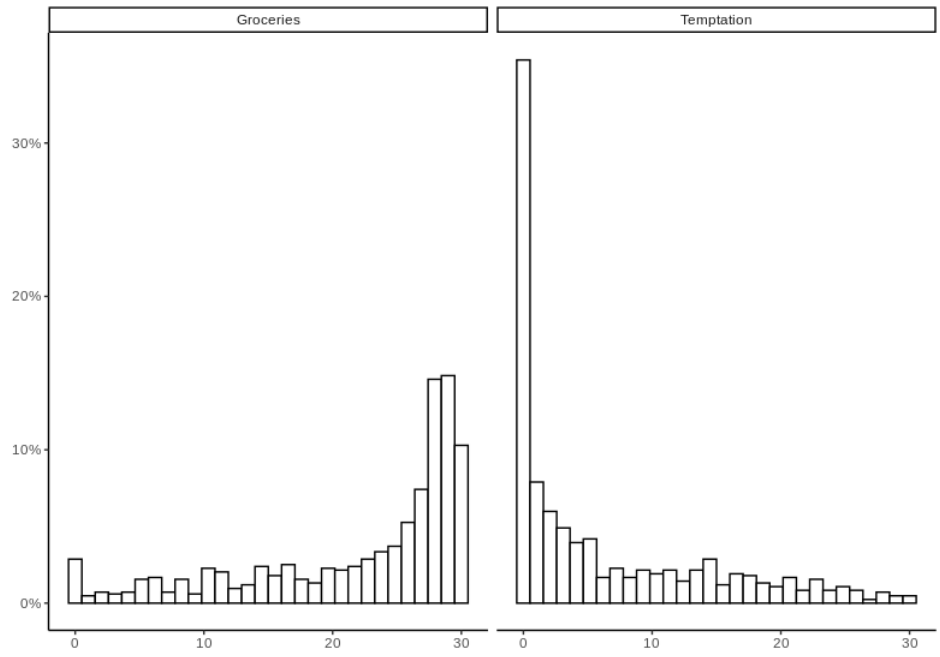
Note: Results obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The outcome variable equals 1 if the participant left the study from the first scenario onwards and 0 if the participant completed the survey. Hard indicate participants assigned to the hard scenarios. Tax indicates participates assigned to any of the two Tax levels. Hard Tax 10%/20% indicate treatment groups assigned to both hard scenarios and Tax, and are not defined as interaction terms in this analysis.

Table A-2: Balance checks across treatment groups assigned to different Tax levels

	(1) No Tax	(2) Tax 10%	(3) Tax 20%	(4) p-value
Female	0.68	0.70	0.68	0.88
Age	42.83	42.17	43.31	0.58
High education	0.56	0.55	0.59	0.55
Student	0.07	0.06	0.03	0.15
Employed	0.73	0.78	0.71	0.27
Nationality UK	0.94	0.92	0.95	0.45
Overweight	0.58	0.57	0.55	0.77
Alcohol consumption	3.42	3.13	3.25	0.58
Drinks moderate/high	0.28	0.24	0.24	0.38
Smoker	0.19	0.16	0.17	0.58
Household size	2.72	2.76	2.95	0.11
Parent	0.56	0.61	0.66	0.08
Subjective SES	5.19	5.25	5.25	0.88
Income	22.36	23.86	22.94	0.46
Observations	342	252	214	

Note: Columns (1) - (3) show the means across treatment arms. Column (4) displays the p-value associated with the the F test of joint orthogonality across treatment arms. Subjective SES is measured on a scale (ladder) from 1 to 10, with 10 being represented by the the people who are better off (in terms of education, money and jobs) in the UK. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds. Alcohol consumption is measured in units of alcohol (1 unit of alcohol = 1 small glass of wine; half pint of beer; pub measure of spirits). The higher number of observations in the Tax conditions is due to our sampling strategy (40% No Tax, 60%) which allows more power to detect difference between the two tax levels.

Figure A-1: Demand for temptation and necessities - total expenditure at baseline prices.



Notes: The outcome variable is total expenditure on groceries and temptation goods, at baseline prices - no Tax - prices.

Table A-3: Manipulation check - treatment effects on financial worries - by income group

	(1) Financial situation	(2) Finding money	(3) Index
Hard	0.22**	0.27**	0.31**
	(0.096)	(0.11)	(0.12)
Middle income	-0.42***	-0.47***	-0.57***
	(0.086)	(0.099)	(0.11)
Higher income	-0.50***	-0.56***	-0.68***
	(0.11)	(0.12)	(0.13)
Hard × Middle income	0.00073	-0.055	-0.033
	(0.12)	(0.14)	(0.16)
Hard × Higher income	-0.091	-0.17	-0.16
	(0.12)	(0.13)	(0.15)
Control Mean	0.83	0.87	-0.02
Controls	Yes	Yes	Yes
Adj. R^2	0.24	0.29	0.29
Observations	808	808	808

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The dependent variables are worries about the financial situations: *How worried do you feel about your financial situation?* and worries about finding money in case of need: *How worried do you feel about not being able to find money in case you really need it?*. Both variables are coded as: 0 not worried as all, 1 somewhat worried, 2 very worried and 3 desperately worried. The index variable in the last column is computed through the inverse covariance weigthing procedure in Anderson (2008) and standardized using the control group mean and standard deviation. All models include individual and household characteristics. Hard indicates being assigned to the hard scenarios. Reference category is the lower income group. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds.

Table A-4: Manipulation check - treatment effects on financial worries - by income group

	(1) Financial situation	(2) Finding money	(3) Index
Hard	0.22**	0.27**	0.31**
	(0.096)	(0.11)	(0.12)
Middle income	-0.42***	-0.47***	-0.57***
	(0.086)	(0.099)	(0.11)
Higher income	-0.50***	-0.56***	-0.68***
	(0.11)	(0.12)	(0.13)
Hard × Middle income	0.00073	-0.055	-0.033
	(0.12)	(0.14)	(0.16)
Hard × Higher income	-0.091	-0.17	-0.16
	(0.12)	(0.13)	(0.15)
Control Mean	0.83	0.87	-0.02
Controls	Yes	Yes	Yes
Adj. R^2	0.24	0.29	0.29
Observations	808	808	808

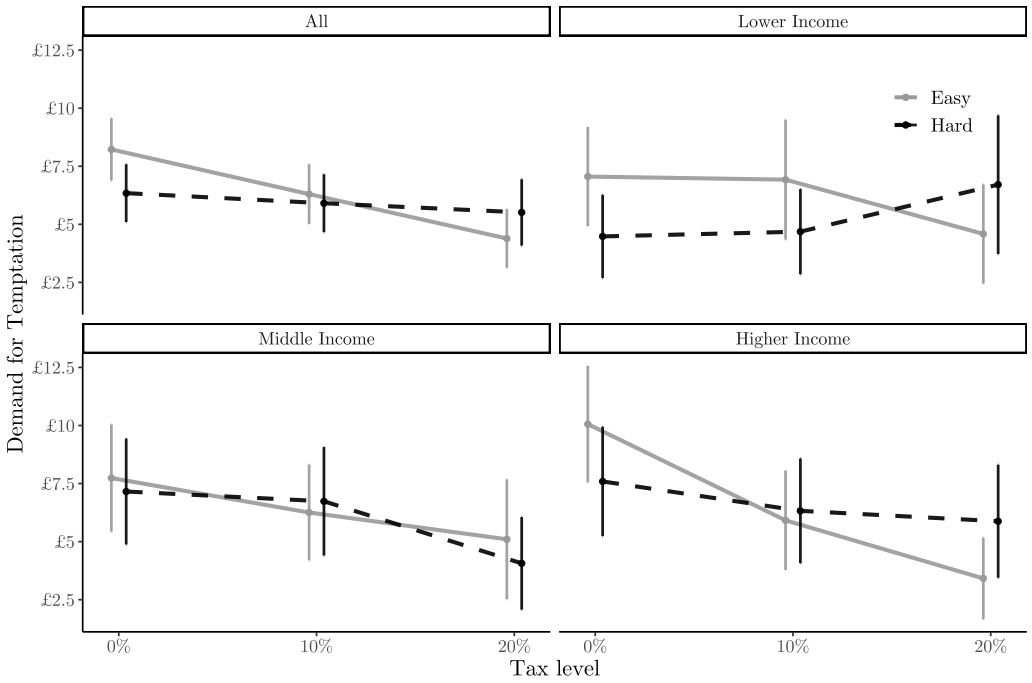
Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. The dependent variables are worries about the financial situations: *How worried do you feel about your financial situation?* and worries about finding money in case of need: *How worried do you feel about not being able to find money in case you really need it?*. Both variables are coded as: 0 not worried as all, 1 somewhat worried, 2 very worried and 3 desperately worried. The index variable in the last column is computed through the inverse covariance weigthing procedure in Anderson (2008) and standardized using the control group mean and standard deviation. All models include individual and household characteristics. Hard indicates being assigned to the hard scenarios. Reference category is the lower income group. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds.

Table A-5: Correlation between income, self-reported "sin" behaviors and demand for subcategories of temptation goods in the experimental market

Panel A: Self-reported behaviors	(1) Overweight	(2) Alcohol consumption	(3) Smoker
Income	-0.0021*	0.024***	-0.0037***
	(0.0012)	(0.0085)	(0.00082)
Panel B: Expenditure in the task	**(1) Unhealthy foods**	**(2) Alcohol**	**(3) Tobacco**
Income	0.0012	0.029**	-0.0085***
	(0.012)	(0.013)	(0.0031)

Note: Estimates are obtained via OLS regressions. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Outcome variables are listed in column headers. Outcome in Panel A are computed using survey items in Prolific's database. Alcohol consumption is expressed in units of alcohol per week. Outcomes in Panel B are total expenditures in the experimental market for each category of goods expressed at baseline prices. Panel B regressions include variables indicating treatment assignment as covariates. Income is computed by dividing total yearly household income by the square root of the household size and is expressed in thousand pounds

Figure A-2: Demand for temptation goods at different Tax levels by financial scenarios condition and income group



Notes: The outcome variable is expenditure on temptation at baseline - no Tax - prices. Dots indicate means by financial scenarios condition at each level of the tax, while the vertical lines indicate 95% confidence intervals. The labels on each plot indicate the samples used: the entire sample (upper left) and samples divided by income tertiles. Easy indicate participants assigned to the easy scenarios, while Hard indicate participants assigned to the hard scenarios

Table A-6: Treatment effects of prime and tax by income tertile on demand for temptation by subgroups of temptation goods

	Alcohol			Tobacco			Junk Food			Sugary Drinks			Sweets		
	(1) Lower	(2) Middle	(3) Higher	(4) Lower	(5) Middle	(6) Higher	(7) Lower	(8) Middle	(9) Higher	(10) Lower	(11) Middle	(12) Higher	(13) Lower	(14) Middle	(15) Higher
Tax 10% (10%)	-0.10	-0.10	-1.07	-0.34	-0.014	-0.43	0.099	-0.39	-0.12	-0.13	-0.041	-0.27	0.23	-1.85**	-2.18**
	(1.00)	(1.06)	(1.22)	(0.37)	(0.26)	(0.27)	(0.40)	(0.31)	(0.25)	(0.42)	(0.32)	(0.38)	(0.99)	(0.73)	(0.92)
Tax 20% (20%)	-0.23	-0.64	-2.84**	-0.51*	-0.0065	-0.40	-0.43	-0.59*	-0.089	-0.67*	-0.18	-0.016	-1.16	-0.73	-2.72***
	(1.16)	(1.11)	(1.10)	(0.27)	(0.29)	(0.29)	(0.31)	(0.31)	(0.31)	(0.38)	(0.34)	(0.48)	(1.01)	(1.04)	(0.86)
Hard (H)	-1.53*	0.82	-0.24	-0.17	0.46	-0.29	-0.28	-0.50	-0.26	-0.81**	0.021	-0.074	-0.80	-1.74**	-1.81*
	(0.90)	(1.18)	(1.36)	(0.37)	(0.39)	(0.25)	(0.31)	(0.31)	(0.24)	(0.32)	(0.34)	(0.37)	(0.86)	(0.73)	(0.95)
Hard × Tax 10% ($H \times 10\%$)	1.29	-0.60	0.89	-0.050	-0.29	0.39	-0.062	0.63	0.31	0.74	0.44	-0.051	-0.37	2.65**	1.08
	(1.31)	(1.75)	(1.81)	(0.47)	(0.40)	(0.30)	(0.50)	(0.42)	(0.39)	(0.53)	(0.50)	(0.51)	(1.25)	(1.04)	(1.21)
Hard × Tax 20% ($H \times 20\%$)	1.98	-1.24	2.45	0.68	-0.021	0.33	0.38	0.25	0.48	1.16*	-0.34	-0.57	1.20	1.06	2.90**
	(1.56)	(1.70)	(1.84)	(0.51)	(0.54)	(0.32)	(0.47)	(0.37)	(0.44)	(0.61)	(0.41)	(0.55)	(1.38)	(1.18)	(1.20)
Control Mean	1.64	2.46	4.07	0.47	0.20	0.39	1.07	1.21	0.79	1.00	0.78	0.94	2.52	2.69	3.48
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	-0.01	0.04	0.06	0.13	0.02	0.12	-0.00	0.18	0.03	0.02	0.05	-0.01	0.02	-0.01	0.07
Observations	271	268	269	271	268	269	271	268	269	271	268	269	271	268	269
p-values - Tests:															
(i) 10% = 20%	0.91	0.64	0.09	0.51	0.95	0.83	0.18	0.46	0.93	0.17	0.68	0.59	0.19	0.20	0.34
(ii) $10\% + H \times 10\% = 20\% + H \times 20\%$	0.64	0.32	0.89	0.13	0.51	0.76	0.81	0.03	0.61	0.83	0.01	0.35	0.84	0.53	0.08
(iii) $10\% + H \times 10\% = 0$	0.13	0.60	0.90	0.15	0.32	0.70	0.90	0.40	0.49	0.05	0.33	0.32	0.84	0.31	0.18
(iv) $20\% + H \times 20\% = 0$	0.08	0.14	0.79	0.71	0.95	0.57	0.87	0.11	0.16	0.29	0.05	0.05	0.96	0.62	0.81
(v) $H + H \times 10\% = 0$	0.81	0.85	0.63	0.44	0.34	0.28	0.38	0.66	0.86	0.86	0.25	0.70	0.21	0.20	0.28
(vi) $H + H \times 20\% = 0$	0.72	0.72	0.06	0.14	0.25	0.78	0.78	0.31	0.51	0.52	0.26	0.12	0.71	0.47	0.09

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. The outcome variables are expenditure levels at baseline - no Tax prices - on the subcategories of temptation goods in the column headers. The sample used are divided by income tertiles. Hard indicates participants assigned to the hard scenarios condition. Tax 10% and 20% indicate participants assigned to the treatment groups where the prices of temptation goods were increased by 10% and 20% respectively. The control means are the means of the outcome variables in the Easy scenario no Tax group. Tests (iii)-(iv) test for the effect of taxes in the Hard condition. Tests (v)-(vi) test for difference between the Easy and Hard condition at each Tax level.

Table A-7: Treatment effects of prime and tax by income tertile on demand for temptation by subgroups of temptation good aggregating all food products into one category

	Alcohol				Tobacco				Sugary and junk foods			
	(1) All	(2) Lower	(3) Middle	(4) Higher	(5) All	(6) Lower	(7) Middle	(8) Higher	(9) All	(10) Lower	(11) Middle	(12) Higher
Tax 10% (10%)	-0.53	-0.10	-0.10	-1.07	-0.26	-0.34	-0.014	-0.43	-1.82***	0.20	-2.28**	-2.57**
	(0.62)	(1.00)	(1.06)	(1.22)	(0.16)	(0.37)	(0.26)	(0.27)	(0.59)	(1.27)	(0.96)	(1.07)
Tax 20% (20%)	-1.10*	-0.23	-0.64	-2.84**	-0.30**	-0.51*	-0.0065	-0.40	-2.20***	-2.26*	-1.50	-2.83***
	(0.64)	(1.16)	(1.11)	(1.10)	(0.15)	(0.27)	(0.29)	(0.29)	(0.65)	(1.23)	(1.26)	(1.05)
Hard (H)	-0.50	-1.53*	0.82	-0.24	0.028	-0.17	0.46	-0.29	-2.15***	-1.89*	-2.21**	-2.14*
	(0.64)	(0.90)	(1.18)	(1.36)	(0.20)	(0.37)	(0.39)	(0.25)	(0.60)	(1.09)	(0.93)	(1.09)
Hard × Tax 10% ($H \times 10\%$)	0.65	1.29	-0.60	0.89	-0.023	-0.050	-0.29	0.39	2.35***	0.30	3.72***	1.34
	(0.90)	(1.31)	(1.75)	(1.81)	(0.23)	(0.47)	(0.40)	(0.30)	(0.82)	(1.63)	(1.32)	(1.47)
Hard × Tax 20% ($H \times 20\%$)	0.95	1.98	-1.24	2.45	0.32	0.68	-0.021	0.33	2.24***	2.74	0.97	2.81*
	(0.94)	(1.56)	(1.70)	(1.84)	(0.26)	(0.51)	(0.54)	(0.32)	(0.86)	(1.73)	(1.44)	(1.44)
Control Mean	2.67	1.64	2.46	4.07	0.36	0.47	0.20	0.39	4.81	4.58	4.68	5.20
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.04	-0.01	0.04	0.06	0.06	0.13	0.02	0.12	0.05	0.04	0.06	0.09
Observations	808	271	268	269	808	271	268	269	808	271	268	269
p-values - Tests:												
(i) 10% = 20%	0.37	0.91	0.64	0.09	0.63	0.51	0.95	0.83	0.51	0.06	0.48	0.76
(ii) 10% + $H \times 10\%$ = 20% + $H \times 20\%$	0.70	0.64	0.32	0.89	0.11	0.13	0.51	0.76	0.42	0.98	0.05	0.20
(iii) 10% + $H \times 10\%$ = 0	0.86	0.13	0.60	0.90	0.07	0.15	0.32	0.70	0.34	0.59	0.15	0.22
(iv) 20% + $H \times 20\%$ = 0	0.83	0.08	0.14	0.79	0.93	0.71	0.95	0.57	0.94	0.69	0.52	0.98
(v) $H + H \times 10\%$ = 0	0.82	0.81	0.85	0.63	0.96	0.44	0.34	0.28	0.72	0.19	0.12	0.39
(vi) $H + H \times 20\%$ = 0	0.52	0.72	0.72	0.06	0.04	0.14	0.25	0.78	0.89	0.52	0.28	0.45

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. The outcome variables are expenditure levels at baseline - no Tax prices - on the subcategories of temptation goods in the column headers. The sample used are divided by income tertiles. Hard indicates participants assigned to the hard scenarios condition. Tax 10% and 20% indicate participants assigned to the treatment groups where the prices of temptation goods were increased by 10% and 20% respectively. The control means are the means of the outcome variables in the Easy scenario no Tax group. Tests (iii)-(iv) test for the effect of taxes in the Hard condition. Tests (v)-(vi) test for difference between the Easy and Hard condition at each Tax level.

Table A-8: Sequential g-estimates by income group for all the selected candidate mediators

	(1) Baseline	(2) Necessity Index	(3) Cognitive Reflection	(4) Life Satisfaction (1-10)	(5) Time Preferences	(6) Risk (1-10)
Hard						
All	-2.12	-0.76	-2.09	-2.16	-2.26	-2.11
	(-3.96, -0.29)	(-2.19, 0.7)	(-3.91, -0.25)	(-3.96, -0.38)	(-3.86, -0.02)	(-3.86, -0.05)
Low income	-3.13	-1.83	-2.99	-3.07	-3.17	-3.15
	(-6.18, -0.09)	(-4.48, 0.74)	(-5.95, 0.51)	(-6.11, -0.21)	(-6.17, -0.03)	(-6.41, -0.09)
Middle income	-0.44	1.06	-0.48	-0.48	-0.92	-0.3
	(-3.66, 2.78)	(-1.6, 3.7)	(-3.63, 2.77)	(-3.43, 3.2)	(-3.95, 2.43)	(-3.4, 3.15)
High income	-1.98	-0.86	-1.95	-1.95	-1.9	-1.81
	(-5.61, 1.65)	(-3.68, 1.26)	(-5.68, 1.3)	(-5.73, 1.64)	(-5.84, 1.54)	(-6.24, 1.47)
Hard x Tax 10%						
All	1.57	0.67	1.5	1.67	1.51	1.51
	(-0.96, 4.1)	(-1.41, 2.66)	(-1.18, 4.09)	(-0.91, 4.21)	(-1.02, 4.45)	(-0.88, 4.03)
Low income	0.39	1.52	0.32	0.48	0.21	0.3
	(-4.21, 4.99)	(-2.51, 5.3)	(-4.36, 5.11)	(-4.49, 5.23)	(-4.52, 4.66)	(-3.73, 4.86)
Middle income	0.63	-0.94	0.69	0.79	1.03	0.28
	(-3.99, 5.25)	(-4.4, 2.76)	(-3.64, 5.75)	(-3.88, 5.51)	(-4.18, 5.41)	(-4.8, 4.61)
High income	1.66	0.16	1.52	1.76	1.65	1.64
	(-3.15, 6.46)	(-3.32, 4.35)	(-3.04, 6.32)	(-2.83, 6.26)	(-3.36, 6.62)	(-3.35, 6.3)
Hard x Tax 20%						
All	3.08	1.26	3.03	3.05	3.19	3.01
	(0.39, 5.77)	(-0.72, 3.42)	(0.46, 5.76)	(0.46, 5.83)	(0.54, 6.18)	(0.37, 5.69)
Low income	5.36	3.23	5.24	5.41	5.08	5.53
	(0.44, 10.28)	(-0.14, 7.39)	(0.48, 10.18)	(0.45, 10.46)	(0.33, 10.14)	(0.72, 10.09)
Middle income	-0.94	-1.71	-0.76	-0.79	-0.43	-0.89
	(-5.57, 3.7)	(-5.83, 1.7)	(-6.19, 3.44)	(-5.94, 3.48)	(-5.6, 4)	(-6.27, 3.16)
High income	5.05	2.19	4.72	4.87	4.81	4.77
	(0.22, 9.88)	(-0.69, 5.82)	(-0.36, 10.29)	(-0.13, 9.64)	(0.6, 11.19)	(0.43, 10.32)

Note: The outcome variable in all models is expenditure on temptation, expressed at baseline - no Tax - prices. Reported are estimates for the selected variables - being assigned to hard scenarios (Hard), Hard interacted with the 10% Tax and Hard interacted with the 20% Tax, for the baseline OLS model (Column (1)) and sequential g-estimation procedures for the candidate mediators (Columns (2) - (6)). 95% confidence intervals in parentheses (bootstrapped (1000 repetitions) BCA confidence intervals in columns (2) - (6)). All models control for individual and household characteristics and the levels of the Tax.

Table A-9: Robustness checks

	Temptation (Tobit)				Left as bonus payment (0-2£)				Temptation + Left as bonus				Temptation excluding moisturizers			
	(1) All	(2) Lower	(3) Middle	(4) Higher	(5) All	(6) Lower	(7) Middle	(8) Higher	(9) All	(10) Lower	(11) Middle	(12) Higher	(13) All	(14) Lower	(15) Middle	(16) Higher
Tax 10% (10%)	-2.10	1.12	-0.45	-4.90**	0.18**	0.35***	0.16	0.12	-1.70*	0.52	-0.70	-3.76**	-2.60***	-0.24	-2.40*	-4.07***
	(1.29)	(2.35)	(2.25)	(2.24)	(0.075)	(0.14)	(0.14)	(0.13)	(0.93)	(1.74)	(1.60)	(1.71)	(0.85)	(1.66)	(1.45)	(1.56)
Tax 20% (20%)	-6.01***	-5.20**	-3.74	-9.41***	-0.026	0.071	0.050	-0.091	-3.88***	-3.16*	-2.33	-6.48***	-3.59***	-3.00*	-2.14	-6.07***
	(1.46)	(2.52)	(2.70)	(2.35)	(0.082)	(0.14)	(0.16)	(0.15)	(0.94)	(1.65)	(1.78)	(1.61)	(0.92)	(1.66)	(1.74)	(1.51)
Hard (H)	-2.86**	-4.93**	-0.13	-2.68	0.068	0.11	-0.032	0.21	-2.06**	-3.02**	-0.47	-1.77	-2.63***	-3.58**	-0.94	-2.68
	(1.29)	(2.25)	(2.15)	(2.38)	(0.073)	(0.12)	(0.13)	(0.13)	(0.94)	(1.54)	(1.64)	(1.88)	(0.88)	(1.44)	(1.54)	(1.73)
Hard × Tax 10% ($H \times 10\%$)	1.50	0.50	-0.75	1.64	-0.27**	-0.30	-0.13	-0.50**	1.30	0.093	0.50	1.16	2.98**	1.55	2.82	2.62
	(1.88)	(3.37)	(3.24)	(3.40)	(0.11)	(0.19)	(0.19)	(0.19)	(1.30)	(2.33)	(2.35)	(2.47)	(1.21)	(2.22)	(2.20)	(2.27)
Hard × Tax 20% ($H \times 20\%$)	4.73**	8.53**	-1.64	7.54**	0.058	0.046	0.090	-0.038	3.14**	5.40**	-0.85	5.01**	3.50***	5.39**	-0.29	5.59**
	(2.09)	(3.69)	(3.55)	(3.47)	(0.12)	(0.21)	(0.22)	(0.21)	(1.38)	(2.47)	(2.38)	(2.49)	(1.32)	(2.42)	(2.28)	(2.36)
Control Mean	8.23	7.06	7.74	10.06	1.03	0.95	1.02	1.13	9.26	8.01	8.76	11.19	7.84	6.69	7.34	9.66
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo % Adj. R^2					0.02	0.03	0.02	0.05	0.05	0.05	0.04	0.08	0.06	0.07	0.05	0.09
Observations	808	271	268	269	808	271	268	269	808	271	268	269	808	271	268	269
p-values - Tests:																
(i) 10% = 20%	0.01	0.02	0.23	0.06	0.01	0.06	0.49	0.15	0.02	0.04	0.36	0.05	0.25	0.11	0.87	0.14
(ii) 10% + $H \times 10\%$ = 20% + $H \times 20\%$	0.66	0.54	0.11	0.60	0.20	0.74	0.48	0.12	0.73	0.39	0.07	0.55	0.63	0.56	0.08	0.59
(iii) 10% + $H \times 10\%$ = 0	0.66	0.47	0.61	0.20	0.26	0.68	0.84	0.01	0.66	0.66	0.91	0.15	0.66	0.33	0.80	0.38
(iv) 20% + $H \times 20\%$ = 0	0.40	0.22	0.03	0.47	0.73	0.48	0.38	0.38	0.46	0.21	0.05	0.44	0.92	0.17	0.12	0.79
(v) $H + H \times 10\%$ = 0	0.33	0.07	0.72	0.68	0.01	0.21	0.23	0.05	0.40	0.09	0.99	0.71	0.68	0.22	0.22	0.97
(vi) $H + H \times 20\%$ = 0	0.25	0.22	0.54	0.05	0.19	0.39	0.75	0.30	0.28	0.22	0.45	0.04	0.37	0.35	0.47	0.05

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. All models control for individual and household characteristics. Pseudo R^2 reported is in Columns (1) to (4), while Adjusted Pseudo R^2 in Columns (5) to (16). The outcome variables is expenditure on temptation at baseline - no Tax - prices, the amount left as bonus payment in the experimental market (out of £30), the sum of the previous two, expenditure on temptation at baseline - no Tax - prices excluding two moisturizing creams. The sample used is the entire sample or the sample divided by income tertile as listed in the column headers . Hard indicates participants assigned to the hard scenarios condition. Tax 10% and 20% indicate participants assigned to the treatment groups where the prices of temptation goods were increased by 10% and 20% respectively. The control means in Columns (1)-(2)/(3)-(6) are the means of the outcome variable in the no Tax groups/Easy scenario no Tax group. The control means in Column (5) reports the average expenditure in the group exposed to easy scenarios and no price increase. Tests (iii)-(iv) test for the effect of taxes in the Hard condition. Tests (v)-(vi) test for difference between the Easy and Hard condition at each Tax level.

2.B Appendix B

2.B.1 Experimental Task

Financial Scenarios

Instructions - In the following section you will be presented 2 scenarios and asked to answer how you would go about dealing with the situations if they were to happen to you. Please take your time answering the questions. Try to have at least 3 sentences in your open question answers.

1. Imagine that an unforeseen event requires of you an immediate (£2000/£100) expense. You need to raise the money in less than a week.
    - Are there ways in which you may be able to come up with that amount of money on a very short notice? (yes/no)
    - How would you go about getting (£2000/£100) on a very short notice? Three sentences should be enough. (open)
    - To what extent do you agree with the following statements? (4 item Likert: strongly disagree - strongly agree)
        (a) "Coming up with (£2000/£100) on a very short notice would cause me longlasting financial hardship."
        (b) "Coming up with (£2000/£100) on a very short notice would require me to make sacrifices that have long-term consequences."
2. Imagine that the economy is going through difficult times. Your household's monthly expenses increase by (£300/£15) due to higher energy and housing prices.
    - Please indicate to what extent do you agree with the following statement: "Given my situation, I would be able to maintain roughly the same lifestyle under those new circumstances." (4 item Likert: strongly disagree - strongly agree)
    - In what ways would the (£300/£15) increase in your monthly expenses would impact your leisure, housing or travel plans? What changes would you need to make? Three sentences should be enough. (open)
    - To what extent do you agree with the following statement: "The (£300/£15) increase in our monthly expenses would strongly impact our leisure, housing, or travel plans." (4 item Likert: strongly disagree - strongly agree)

Purchasing Task

Instructions: In the following task you have to choose what goods to purchase with a budget of £30 .

You will see a list of available goods, with a picture, title and the price displayed for each of them. The price of the goods is the retail price including the discounts offered by the retailer. If you need additional information on the goods, by clicking on the picture a new window will open with further details from the website of the retailer.

Some of the goods have a higher price than that of the retailer.

By clicking on the **ADD button**, the goods will be added to the shopping cart. You can edit the shopping cart content at any time by clicking on the **Shopping Cart** section in the top-right side of your screen.

A new window will open with the goods already selected. You can modify the quantities of each good or remove them from the shopping cart. You can return to the main window at anytime by clicking on close, or anywhere outside the shopping cart window.

When you are satisfied with your selection, click on **Checkout** in the shopping cart window to proceed to the next page. Try to spend as close to the £30 budget as possible. To proceed to the next page you need to spend a minimum of £28. Any remainder will be added as bonus payment on Prolific.

You can access these instructions at any time by clicking on the **Instructions** section in the top-left side of the page.

1 out of every 100 participants will be selected for payment. If you are selected, the goods will be delivered to a collection location of your choice at a date and time that is convenient for you. You can pick up your goods with the code we will send you.

Figure A-1: Main screen of the purchasing task

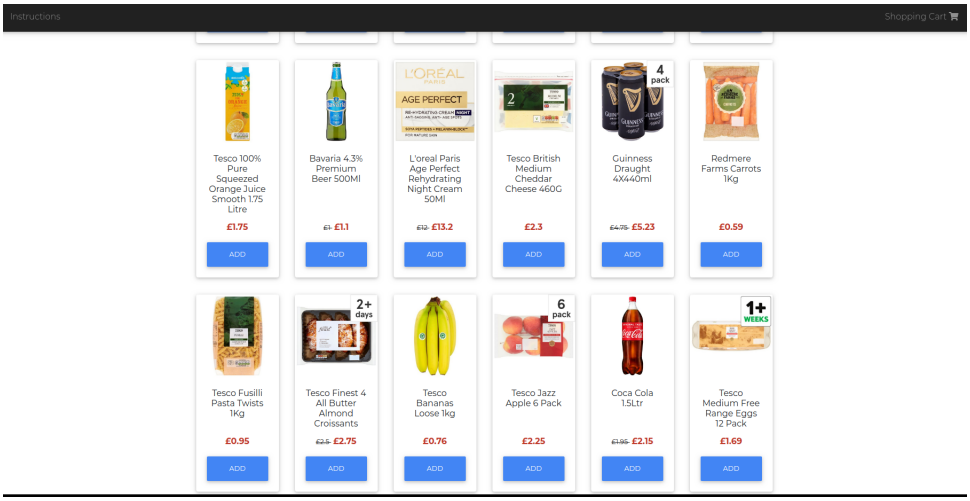


Figure A-2: Checkout screen of the purchasing task

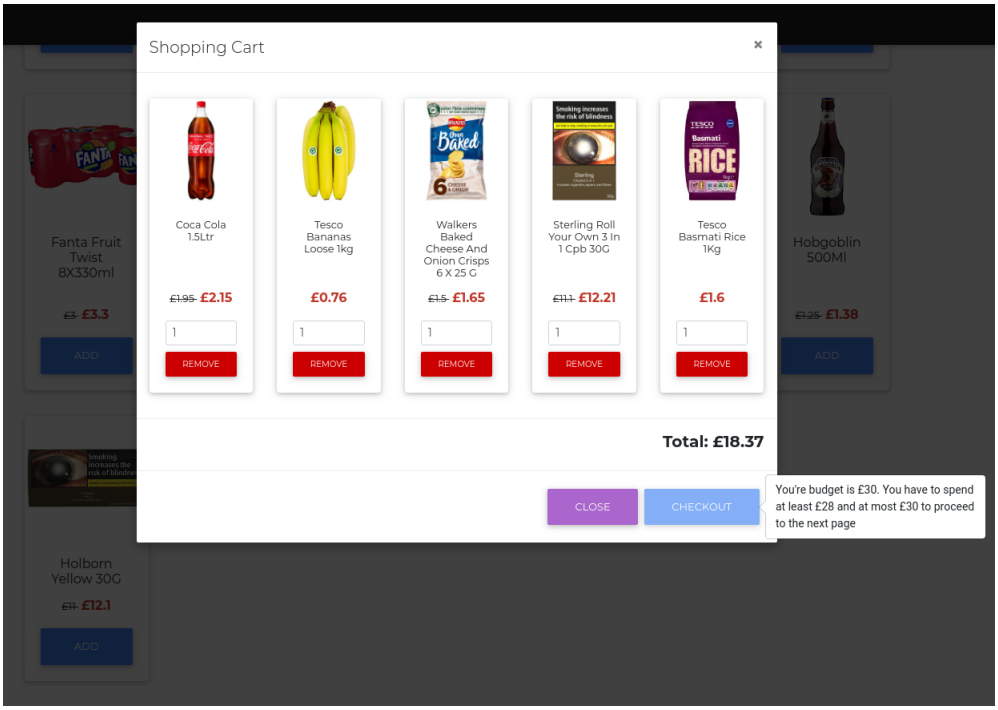


Table A-1: List of products and their prices from the experimental market task

	Temptation		Groceries	
	Product	Price £	Product	Price £
1	Moisturiser Cream 50Ml	10.50	Semi Skimmed Milk 2.272L	1.09
2	Night Cream 50Ml	12.00	Eggs 12 Pack	1.69
3	Dry Gin 70Cl	18.00	Medium Bread 800G	1.10
4	Pale Ale Pack 12X330ml	13.00	Yogurt 500G	0.90
5	Rolling Tobacco 30G	11.00	Peas 1Kg	1.60
6	Rolling Tobacco 30G	11.10	Mixed Vegetables1Kg	1.50
7	Cigarettes 20	8.60	Bananas 1kg	0.76
8	Cigarettes 20 Pack	8.70	Beef Mince 500G 5% Fat	3.39
9	Lager 4 X 440Ml	3.35	Chicken Breast Portions 650G	3.80
10	Beer 15 X 440Ml	13.00	Toilet Tissue 2 Packs	3.35
11	Chocolate Treats 12 Pack 170G	2.79	Cleaner Spray 500Ml	0.70
12	Boxed Chocolate 305G	13.00	Body Wash 250Ml	1.80
13	Boxed Chocolates 172G	5.00	Washing Liquid 1995Ml	6.00
14	Doughnuts 12 Pack	2.50	100% Orange Juice 1.75 Litre	1.75
15	Croissants	2.50	Cheddar Cheese 460G	2.30
16	Chocolate Selection 200G	5.00	Carrots 1Kg	0.59
17	Whisky 35Cl	9.00	Pasta 1Kg	0.95
18	Cheese Pizza 555G	3.00	Spaghetti 1Kg	0.95
19	Beer 4X440ml	4.75	Basmati Rice 1Kg	1.60
20	Beer 4X568ml	5.25	Baked Beans 4 X415g	2.00
21	Apple Cider 12 X 440Ml Can	7.00	Tuna Chunks 4 X 160G	3.25
22	Cola Soft Drink 12 X 330Ml	4.50	Mackerel Fillets 125G	1.40
23	Cola Soft Drink 1.5Ltr	1.95	Oats 1Kg	1.10
24	Soft Drink 8X 330Ml	3.00	Sunflower Oil 1L	1.10
25	Soft Drink 8X330ml	3.00	Olive Oil 1L	3.60
26	Crisps 200G	2.25	Spinach 500G	2.00
27	Crisps 6 X 25 G	1.50	Potatoes 2.5Kg	2.00
28	Microwave Popcorn 3X60g	1.50	Washing Liquid 1.33L 38 Washes	6.00
29	Chocolate Bars 7 Pack 291.9G	2.50	Chicken 1Kg	4.00
30	Chocolate Bars 7 X28.5G	2.50	Mushrooms 300G	0.95
31	Beer 500Ml	1.00	Apple 6 Pack	2.25
32	Beer 500Ml	1.25	Salmon Fillets 330G	3.70
33	Beer 12X330ml	16.50	Brown Rice 1Kg	1.50

2.B.2 Additional Variables

- **Life Satisfaction**: All things considered, how satisfied are you with your life as a whole these days? On a scale from 1 to 10, where 1 means you are "completely dissatisfied" and 10 means you are "completely satisfied" where would you put your satisfaction with life? (Bjørnskov, 2010)
- **Risk**: In general, how willing or unwilling you are to take risks? Please use a scale from 0 to 10, where 0 means completely unwilling to take risks and a 10 means you are very willing to take risks (Wagner et al., 2007)
- **Time Preferences**: What is the smallest amount of money to be received today that you would prefer to receiving £200 in 2 months?
- **Cognitive reflection**: 3 questions from Thomson and Oppenheimer (2016)

Chapter 3

Weak in Control, Strong in Procrastination? A Study on Perception of Control and Intertemporal Preferences

with Lucia Savadori

A feeling of control is a fundamental component of psychological well-being and a loss of it can trigger certain behavioural and cognitive patterns. In this study, we aim to investigate the relationship between the feeling of control and time preferences, in particular, money and effort discounting. In an online experiment, we ask participants to recall a situation of control. We vary treatments by changing the level of perceived control – not having control vs being in full control – and the type of control – recalling a situation involving other people or recalling a non-social situation. Then participants are asked to make intertemporal allocation decisions in a convex time budget environment. This task has two between-subject conditions - allocation of a monetary experimental budget or real effort tasks.

We do not find evidence of present bias in monetary discounting condition. We estimate the beta parameter to be higher than 1, indicating a future focus. Results do not change when allocations are divided by treatment status. The effort discounting condition presents a different picture: we find evidence of static preference reversal as, on average, participants choose to perform more effort tasks sooner when the decision involves only future points in time. When dividing by treatments, we find present bias in low control (beta=0.793): participants in this group chose to postpone work by doing less on a sooner date, even if it means doing more work in the future due to interest rates. We find that intertemporal allocation decisions are impacted solely by decision time frames and interest rates. We also suggest that emotional states from a recall task mediate the treatment effect: for money condition, the strongest mediator is the feeling of fear, while for effort discounting it is sadness. This study suggests that intertemporal preferences should not be generalized over different domains: remembering a situation of (no) control does not influence intertemporal choice regarding (windfall) money; on the other hand, low (recalled) control increases preference for postponing work.

Keywords: feeling of control, intertemporal preferences, effort discounting, monetary discounting, agency, power

3.1 Introduction

A feeling of control is an essential element of psychological health and well-being (Shapiro Jr et al., 1996). A deficiency of control can be detrimental to the healthy state of an individual as it is closely related to various psychological inflictions such as depression. Thus, for a normally functioning psychological state, there exists a need to possess a particular collection of cognitive and social skills which lead a person to perceive and reinstate at least a certain level of control in their life (Declerck et al., 2006). While the lack of control ignites a process of psychological balance deterioration, this also affects cognition and behavior. Therefore the two states - in control and lacking it - can produce different behavioral responses to the same situation. This is particularly true for low socioeconomic status individuals: as this group experiences reduced personal control due to numerous contextual factors, it thus faces a higher likelihood of biased decision making and the accumulation of such behaviors can make a long term impact on the general well-being of an individual (Pepper and Nettle, 2017).

Literature on perceived control has in numerous ways analysed two concepts, closely related to perceived control - *power* and *agency*. *Power* can be defined as (a feeling of) control over resources, either personal or those belonging to others (Galinsky et al., 2003; Keltner et al., 2003; Galinsky et al., 2015). In some works, this defines the power to decide for other individuals, which in other words can be called *social power*. Numerous works have highlighted a variety of behaviors that are affected by the presence or absence of power. For example, power priming has been shown to result in proneness to engage in action: those who feel more in power, are also more likely to pursue goal-oriented action, even when domains of power and action differ (Galinsky et al., 2003). Power has also been shown to impact the perception of risk as those high in power were found to be more optimistic about it and more likely to engage in risky behaviors (Anderson and Galinsky, 2006). Among other findings, high power makes an individual be more loss averse (Inesi, 2010), want more choice or, when having few choices, want more power (Inesi et al., 2011), save more to remain in the same state of a relative abundance (Garbinsky et al., 2014), show better results at the executive-function tasks such as planning, inhibiting or updating (Smith et al., 2008), be more capable to multitask compared to those low in power (Cai and Guinote, 2017), and improve working memory (Hadar et al., 2020). Further works on *agency* - the capacity to exert power over your own outcomes or those of other people - expand the previous list. Choshen-Hillel and Yaniv (2011) analyze agency in light of social preferences and find that those low in agency tend to be more concerned with inequality, while those high in the agency are more concerned with the welfare of others. Gneezy et al. (2020) suggest that lack of agency results in a lower willingness to take risks.

In this study, we seek to analyze the relationship between perceived control and a type of decision that is an integral part of our everyday lives - intertemporal choice. Intertemporal discount rates have been shown to have a significant impact on life outcomes, from educational attainment to health and income (Golsteyn et al., 2014; Frederick et al., 2002). Therefore numerous research works investigated potential determinants of temporal discounting stemming from individual differences and situational factors. A stream of literature attributes these disparities in intertemporal choice to emotions. One of the more widely quoted models of temporal discounting states that preference for sooner (immediate) rewards is attributed to the "hot" system,

which is influenced by emotions, while more deliberate and future-looking decisions are made using a "cold" system (Laibson, 1997; Metcalfe and Mischel, 1999). A general finding is that positive emotions decrease delay discounting (Liu et al., 2013; Benoit et al., 2011; Ifcher and Zarghamee, 2011; Handa et al., 2020). In particular, happiness was found to reduce preference for *sooner* rewards (Ifcher and Zarghamee, 2011; Handa et al., 2020). On the contrary, Suo et al. (2021) find that also anger increases preference for delayed rewards, while sadness does not affect intertemporal choice. As suggested by Hirsh et al. (2010), there might exist an interaction effect between affect and other individual characteristics; in particular, they find that extroversion interacts with positive affect which, in turn, increases the likelihood to choose immediate monetary rewards. Zhao et al. (2015) check for an interaction effect between anger and Behavioral Activation System (BAS). Since positive and negative affect has also been associated with activation of Behavioral Activation System (BAS) and Behavioral Inhibition System (BIS)[1], respectively (Merchán-Clavellino et al., 2019), Zhao et al. (2017) check whether individual differences in BAS scores and anger have an impact on intertemporal choices. Results suggest that people with higher BAS score tend to choose *smaller-sooner* rewards when put in an angry mood. In another study on BIS, Zhao et al. (2015) find that more sensitive BIS pushes individuals to choose more future-oriented rewards. Sohn et al. (2015), on the other hand, argue that valence of emotional affect does not matter, since it is the state of high arousal that impact intertemporal choice. Apart from emotions, research has also examined the relationship between time discounting and stress. Riis-Vestergaard et al. (2018) find that administration of hydrocortisone (a hormone related to stress) results in an increased preference for *smaller-sooner* rewards. A study by Haushofer et al. (2021) complements this finding by showing that such preference remains consistent across domains (monetary gains, losses, or effort allocation). Other works raised the hypothesis that time preferences might be impacted by intelligence: a meta-analysis by Shamosh and Gray (2008) suggest that in general delay discounting and intelligence have a negative relationship. While their explanation of such results related to working memory, Shamosh et al. (2008) suggest that working memory does not have any impact on delay discounting. In this work, we seek to analyze the potential effect of perception of control manipulation on intertemporal choice. To our knowledge, two works have investigated the intersection between these two domains. Through a series of experiments where participants were asked to recall a situation when they were or were not in control, Duan et al. (2017) find that feeling of power reduces future discounting. Gneezy et al. (2020) complement this result with their experimental study in which they administer a different manipulation of agency and find that those participants who had the power to achieve agency were more patient compared to those who did not have agency or the opportunity to gain it. Moreover, they find that this effect is moderated by risk preferences: higher agency increases risk tolerance, therefore making participants more willing to wait for a larger award in the future instead of demanding a small reward at present.

Our study contributes to this discussion by providing a further investigation of the perceived control impact on intertemporal choice. We follow the control manipulation used by Duan et al. (2017) - recalling a situation of high or low control (Galinsky et al., 2003). As highlighted by Bargh et al. (1995) and Galinsky et al. (2003), remembering the situation of power activates

[1]BAS is usually related to person's pursuit and achievement of goals, while BIS tends to get activated as a response to negative events and stimuli in the environment and seeks to avoid these circumstances in the future.

the construct of control in one's mind which in turn triggers cognitive and behavioral patterns that would have otherwise been triggered during the actual - *on the spot* - experience of control. Unlike Duan et al. (2017) and Gneezy et al. (2020), we investigate the impact of the feeling of control not only on classic monetary discounting decisions but also on intertemporal choices regarding effort. The seminal paper of Augenblick et al. (2015) demonstrated that individual discounting differs depending on the domain of decision (for example, deciding about receiving money or performing something that involves effort). As suggested by Gneezy et al. (2020); Duan et al. (2017), we predict that a high sense of control is positively related to the future-oriented intertemporal preferences for both money and effort discounting. However, we also raise a hypothesis that intertemporal choices in the effort condition will show more present bias than in the money condition. Moreover, we also consider that emotional response to control manipulation might differ based on the type of control. In numerous studies concept of control (power) is approached through the view of social psychology, meaning, that it analyzes how control by or over other people influences the behavior of individuals. Another stream of research on the effect of perceived control focuses on a general aspect of being in (out of) control over something[2]. For this reason, we intend to explore whether recalling a situation that involves other people raises a different response compared to recalling a situation that does not. We call these *social* and *task* control conditions respectively and we raise a hypothesis that the low social control condition produces more negative emotions than the low task control condition.

To check for the validity of our hypotheses, we run an online experiment. A sample of 622 participants was recruited via Prolific platform[3]. Participants are first randomly assigned to one of four treatments of the *recall task* - a control manipulation where participants are asked to remember and describe a situation of (no) control. Treatments correspond to the level of control (*low vs high*) and type of control (*task vs social*). Afterwards, participants are asked to do an intertemporal choice task. Unlike Duan et al. (2017) and Gneezy et al. (2020), we use Convex Time Budget (CTB) environment (Andreoni and Sprenger, 2012) to elicit time preferences. We believe it to give more precision in estimating intertemporal parameters of discounting and present bias (as compared with dichotomous choice questions used in the aforementioned studies). Participants are then randomly assigned to one of the two intertemporal task conditions: discounting of money or effort. In the CTB environment, they make 15 intertemporal allocation decisions that cover different time frames and interest rates. For monetary discounting, experiment subjects have to allocate an experimental budget of £10. For effort treatment, allocation is done for 100 real effort tasks (following the example of Gill and Prowse (2012)).

We find that participants in the monetary discounting condition exhibit no present bias in either of control treatments or with any payment delay length. On the contrary, they show a clear preference for *later-larger* payments. If the interest rate is 0%, the share of the experimental budget allocated to an earlier date within each of the three decision frames varies between 70% and 75%, depending on the type and level of control, but falls sharply to around 6-8% as interest rate reaches 100%. One indicator of present bias is a static preference reversal: making a myopic

[2]An example of this is a research on *learned helplessness* (Maier and Seligman, 1976) where control is experienced over a task instead of other people

[3]It should be noted that despite random selection, over 95% of the recruited participants were students

allocation decision when front-end delay is $t = 0$, but reversing this decision to be more future-oriented when $t \neq 0$ (keeping delay length k constant across the two decisions). When comparing choices made for the 2 week delay lengths, we find no evidence of static preference reversal for money discounting. We then estimate marginal effects from ordered logistic regression and find that only employment status and low social control condition have an impact on preference reversal, where both variables decrease the probability of making zero present biased choices. When we talk about the impact on the budget allocation decisions in general, the main variable decreasing the share of budget allocated to an earlier date consistently across all three decision frameworks is the interest rate. We then perform parametric estimations to check whether they are consistent with our non-parametric results. Tobit regressions uncover that, on average, participants' preferences for monetary payments are close to linear, while β ranges between 1.016 to 1.071 across treatments, indicating a relative future focus. Other parameters show that participants' preferences over money are close to linear with $\delta = 0.98$ and $\alpha = 0.94$. Robustness checks confirm our findings. First, we restrict our sample to include only those participants whose allocation choices did not violate the law of demand and then replicate our analysis using the full sample. Results largely confirm the trends observed in the original analysis. To check whether emotional states participants reported in the *recall task* mediated the treatment effect on intertemporal choice, we estimate Average Causal Mediation Effects: among all the states, we find fear to be the strongest mediator, followed by joviality.

The situation is different for effort discounting. Allocations of effort tasks are distributed more equally across different time frames and interest rates and shares range between 50% and 70%. We find clear evidence of static preference reversal: participants in low control condition choose to do more tasks on a sooner date when that date was in the future rather than in the present (39.42% of choices in low control condition show preference reversal as compared to 22.5% in the high control condition). Being in low control groups increases the probability of reversed decisions. In particular, the probability of no present biased decision decreases by 25% if the participant is in either of the low control groups. Low control conditions also decrease the budget share allocated to an earlier date by $10 - 12\%$, but this effect is observed only in decision time frames where the front-end delay is present. Apart from low control conditions, a higher interest rate significantly increases budget allocations to sooner work date, as observed for money discounting. Parametric estimations confirm present bias for the low control group ($\beta = 0.793$), while for the high control group β does not differ from 1. Participants also show a preference to complete fewer tasks in the future as the time delay length increases ($\delta > 1$) and smooth work through time ($\gamma > 1$). Robustness checks confirm $\beta < 1$ only for a low level of control. Across all emotional states, sadness is found to have the strongest mediation effect between low level of control and intertemporal allocations. Other emotional states having a significant mediation effect were self-assurance and joviality.

This paper contributes to several areas of research. First, it joins the discussion on the effect of control on intertemporal preferences (Gneezy et al., 2020; Duan et al., 2017), while providing, to our knowledge, a first experimental test on the effect of control on the discounting of effort. Second, this work contributes to a vast literature on intertemporal choice and use of convex time budget (Augenblick et al., 2015; Andreoni and Sprenger, 2012; Balakrishnan et al., 2020) by employing this methodology in an online experiment instead of a traditional lab setting.

Thirdly, we contribute to a stream of research on the use of recall task to manipulate the sense of control and its respective effects on individual behavior and decision making (Galinsky et al., 2003, 2015; Anderson and Galinsky, 2006; Hadar et al., 2020; Cai and Guinote, 2017; Inesi, 2010; Inesi et al., 2011). We also explore differences in the type of control - involving social situations and not. Finally, we also provide complementary evidence on the mediating effect of emotions on intertemporal choice (Ifcher and Zarghamee, 2011; Lerner et al., 2013).

The rest of the paper is structured as follows: Section 3.2 presents experimental design, details on data and descriptive statistics. Section 3.3 discusses main results and Section 3.4 provides conclusions.

3.2 Empirical Strategy

This section presents experimental design (subsection 3.2.1), data collection process (subsection 3.2.2), and discussion on descriptive statistics, manipulation checks, and attrition (subsection 3.2.3).

3.2.1 Experimental Design and Hypotheses

The main motivation of this study is to investigate how a sense of control relates to intertemporal preferences, mainly, (1) whether the high sense of control is positively related to future-oriented time preferences both over money and over effort. Following the results by Augenblick et al. (2015), we expect that (2) intertemporal choices in the effort condition will show more present bias than in the money condition. We intended not only to investigate the effect of a different level of control (low vs high) but also to check whether a type of control (task vs social) produces any tangible differences. For this reason, we raise a hypothesis that (3) being in a low social situation evokes different emotions compared to a non-social situation and these emotions can moderate the effect of perceived control (in particular, we state that low social control produces more negative emotions than low task control).

To test these hypotheses, we first manipulated the feeling of control using a *recall task* and then asked participants to state their preferences over allocation of money or effort over time. This translates into 2 (type of control) x 2 (level of control) x 2 (type of intertemporal choice task) between-subject experimental design, summarized in Table 3.2.1.

Table 3.2.1: Experimental arms

		Control			
		Social control		Task control	
		Low social control	High social control	Low task control	High task control
Intertemporal choice	Money	SL (money)	SH (money)	TL (money)	TH (money)
	Effort	SL (effort)	SL (effort)	SL (effort)	SH (effort)

Manipulation of control. To manipulate the feeling of control, participants were randomly assigned to one of the four treatments in a *recall task*. In this task, adapted from Galinsky et al. (2003); Whitson and Galinsky (2008), experimental subjects are asked to remember a situation when they were or were not in control. The original work is concerned with the effects of power, i.e. having control over another individual or being controlled by another individual. In this study we manipulate the *level of control* (low vs high), that is, how much in control a person

feels in that situation. To investigate whether control in social or non-social situations produces a different response, we also introduce a second dimension - *type of control* (task vs social). In simple terms one can think of these two conditions as either involving people (*social*) or not (*task*)[4].

Overall, within a *recall task* we have four conditions. Participants were asked to describe a situation in which (a) they were in control over another person(s) (*high social control* or *SH*), (b) other person(s) were in control over them (*low social control* or *SL*), (c) they were in control over the recalled situation (*high task control* or *TH*), or (d) they were not in control over the recalled situation (*low task control* or *TL*). The exact wording of the task can be found in Appendix 3.B. It must be noted that instructions for *task control* conditions do not rule out the possibility that the participants will recall a situation involving control over or by other people. We chose not to ask them explicitly to recall a *social* situation as this hint might have altered their natural thought process. For this reason we over-sampled *task control* treatment by assigning 60% of participants to it and remaining 40% to *social control* treatment. Open answers from this task were then coded by two blind independent coders to check for fit between the assigned treatment and the actual answer; we describe this process in detail in the subsection 3.2.2.

After completing the recall task, all participants were given (a) a manipulation check on control, and (b) a Positive And Negative Affect Schedule (PANAS-X). The order of the questions was randomized. A manipulation check asked participants to mark on a scale from 0 (no control) to 10 (full control) how much control they had or did not have in the recalled situation. The exact wording of the question was adapted to fit each treatment. In a second question, participants were given a list of emotional states from the PANAS-X scale (order of the items was randomized) and asked to select which of these positive or negative emotions they felt during the event they have just described. The exact wording of these questions can be found in Appendix 3.B. An additional manipulation check was also administered at the very end of the study. Participants were asked whether they felt mainly negative or mainly positive feelings when asked to recall the situation of control.

Intertemporal choice task. After completing the recall task and subsequent questions, participants were then asked to do an intertemporal choice task. We used convex time budget (CTB) method by Andreoni and Sprenger (2012) as an allocation environment. Subjects were randomly assigned to either of two conditions - money or effort discounting. In both conditions participants had to choose how to allocate a certain amount of units (money or effort) between two dates - one sooner and the other later - under different interest rates. For both tasks, we measured the share of the budget allocated to the earlier date.

In money condition, each participant was given an experimental budget of £10 and asked to decide how to split this sum in 3 budget sets. Each budget set concerned a different time frame. These time frames were: now vs 2 weeks, now vs after 4 weeks, after 2 weeks vs after 4 weeks. For each of these time frames, subjects were asked to make an allocation decision under different interest rates: 0%, 11%, 25%, 43%, 100%. These interest rates were chosen for comparison

[4]In the pre-registration form we have used terms *power* and *control* to refer to *social* and *task* control respectively. In this paper, we use the latter terms as we believe them to be less confusing since *power* also implies control and that might puzzle the reader as to which treatment we might be talking about

purposes with prior works on intertemporal choice under CTB (Andreoni and Sprenger, 2012; Augenblick et al., 2015). The main question behind this task is whether you prefer to receive less money now or more money in the future, therefore the interest rate adds up to the sum to be received at the later date. For example, if the participant chooses to split £10 equally when choosing in time frame *now vs 2 weeks* under an interest rate of 100%, it means they would prefer to receive £5 today (on the day of the experiment) and £10 (remaining £5 with 100% interest rate) exactly two weeks from today. For every decision, participants had to make their choice using a slider bar[5] that automatically calculated the interest rate for later payment, therefore they did not have to make any calculations themselves. Each budget set (time frame) was presented on a different screen, but participants were able to move back and forth among them.

For participants in an effort condition, the task followed the same CTB environment with the matching time frames, but instead of money, they were asked to allocate units of effort task. Following the work of Augenblick et al. (2015) which found that individuals discount effort differently from money, participants were given 100 units of real effort *slider* tasks, following an example of Gill and Prowse (2012). To complete one unit of this task, the participant was shown a number from 1 to 100 and a slider; they then had to slide the pointer on the slider (ranging from 1 to 100) to a number matching the one given to them. Each participant got one example of such a real effort task to try. Afterwards, they were presented with three budget sets, equivalent to the ones of money condition apart from interest rates. They were: 1, 1.11, 1.25, 1.43, 1.75. The rest of the task features were equivalent to the ones presented for the monetary discounting group.

3.2.2 Data Collection

This experimental study was run online in February 2020 using crowdworking platform *Prolific*, noted for its transparency, continuously evolving functionality, and representativeness (Palan and Schitter, 2018). The experiment was coded using oTree software (Chen et al., 2016). Participants were paid an average hourly wage rate of £5. Those who were randomly selected to receive a bonus payment from the intertemporal choice task were sent the money or invitation to a separate study in which they had to complete the effort tasks exactly on the selected date (same day, two weeks later, or four weeks later). The study was preregistered on the Open Science Framework (OSF) platform[6]. Any deviation from the pre-registration plan is noted across the paper.

1 in 300 participants was randomly chosen to receive the payment or perform the effort tasks[7]. For those selected randomly, one of 15 choices they have made in the task was drawn at random and implemented. If a participant was in a money condition, they were paid the

[5]On the initial screen participants saw sliders with zero values on two dates; to move forward with the task they had to click on each slider and split the budget so that at least one of the values will be higher than zero. In this way we tried to avoid passive responses.

[6]Pre-registration is accessible at https://osf.io/nt9ey

[7]Although weakly incentivized tasks might suffer from lower engagement, Brañas-Garza et al. (2020) find that such payment scheme used in intertemporal choice tasks does not have a significant impact on discounting parameters in short term allocation decisions (up to one month delay) and generally delivers results equivalent to using real or hypothetical incentives. Nevertheless, it should be acknowledged that probabilistic incentive could have impacted the level of attrition and engagement with the task.

respective amounts on the given dates. If a participant was in the effort condition, they were asked to perform a chosen number of effort tasks on respective dates. If they did it with 95% accuracy, they received a bonus of £20. Those, randomly chosen from the effort condition, have received a link that redirected them to a separate experimental software where they had to do their effort task. Links were sent on exact dates indicated in their choice[8]. In addition to recalled situation, intertemporal allocations, emotional response, and manipulation checks, other measured variables were: age, gender, education, socioeconomic status, income. The study was restricted to include only participants with fluent English language skills to minimize misunderstanding of study questions, especially the recall task.

Since we could not guarantee the quality or compliance with the treatment for the open answers in the recall task, we have used two blind independent coders to code the verbatims. Each coder was presented with a list of answers from the recall task and asked to assign them to one of the four conditions (SH, SL, TH, or TL). The overall inter-rater agreement measured by Cohen's Kappa was 82.63%. After resolving inter-rater disagreements together with the experimenter, they prepared the final list of recoded verbatims which was used for the analysis[9].

3.2.3 Descriptive Statistics, Manipulation Checks, Attrition

Overall, 688 participants have taken part in the experiment. After dropping unsatisfactory and duplicate answers, the final sample reached 622 participants. There were 54 rater disagreements and the total of 92 changes in the treatment status which resulted in the following treatment group sizes: 156 in *high social control*, 124 in *low social control*, 150 in *high task control* and 192 in *low task control* groups. One of the concerns of the online experiments is a high level of attrition which can lead to false causal interpretations (Zhou and Fishbach, 2016). Participants in the online experiments might find it easier to withdraw from it (without receiving payment) at any stage as compared to the lab experiments. If our control manipulation has evoked strong negative feelings in a certain group of participants, they might have decided to drop out of the study, meaning that those who stayed could differ from those who left (for example, those who stayed recalled moderate instead of intense low control situations). For this reason, we look at participants' decision to drop out from the study and regress it on the treatment status (see Table A-1 in Appendix 3.A). We have used original treatment status in a full sample of 778 participants, but we do not consider participants who left the study before the *recall task* in this analysis. Overall 121 participants have dropped out of the study, representing a 15.55% attrition rate. None of the estimates is significant except for the interaction between low control, social

[8]The difference between receiving a monetary award with no effort and having to work for a monetary reward across two types of intertemporal conditions can make it difficult to compare the results. It is possible that participants in effort condition were unable or unwilling to complete the work tasks on future dates due to uncertainty. In such a case we would expect to observe a high share of choices in effort condition being corner (100% of budget allocated to $t = 0$). We find that 28% of participants choose to allocate all experimental budget to an earlier date at least once and 15% of them consistently choose only sooner date. For comparison, in money condition, 57.45% of participants allocate all the budget to an earlier date at least once. We believe that evidence of consumption smoothing across intertemporal decisions in effort condition serve as an encouraging sign that bonus payment condition did not impact choices significantly).

[9]In the pre-registration plan we have stated that each recall task answer will also be separately scored on "powerfulness" and "powerlessness" scales. However, this idea was not implemented since not all answers had enough indication of how powerful/powerless the individual felt

control, and money conditions which shows only a weak significance at 10% level[10] We conclude that overall attrition by treatment status should not be a critical issue in this study.

Table 4.3.1 presents key data on participants' characteristics across the treatments. To ensure that randomization was successful, we perform the F-test of joint equality of the averages across the treatment arms and present the respective p-values in column (6). None of the variables differs significantly across the treatments. The age of participants ranges from 18 to 75 with a mean of 24 years. On average, 46% of respondents are women, and 45% have completed at least an undergraduate degree; 96% are currently studying and 51% are currently employed. In terms of subjective socioeconomic status, the observations range from 1 (low SES) to 9 (high SES), with an average of 5.5.

Table 3.2.2: Descriptive statistics

	(1) Full sample	(2) High social control	(3) Low social control	(4) High task control	(5) Low task control	(6) p-value
Age	23.92	24.27	24.22	23.37	23.88	0.54
	(5.966)	(5.484)	(6.851)	(6.201)	(5.542)	
Female	0.46	0.41	0.52	0.46	0.46	0.31
	(0.499)	(0.493)	(0.501)	(0.500)	(0.500)	
Education	0.45	0.46	0.47	0.41	0.46	0.68
	(0.498)	(0.500)	(0.501)	(0.493)	(0.500)	
Student	0.96	0.96	0.97	0.95	0.96	0.94
	(0.193)	(0.193)	(0.177)	(0.212)	(0.188)	
Employed	0.51	0.54	0.48	0.46	0.55	0.31
	(0.500)	(0.500)	(0.502)	(0.500)	(0.499)	
Subjective SES	5.46	5.43	5.49	5.55	5.40	0.75
	(1.404)	(1.340)	(1.394)	(1.417)	(1.458)	
N	622	156	124	150	192	

Note: Mean coefficients; sd in parentheses. Column (1) shows overall sample mean; columns (2)-(5) show means across treatment arms. Col (6) shows p-values associated with one-way ANOVA. Subjective SES (socioeconomic status) is measured using a scale from 1 to 10, where 10 represents people with highest social standing, education, income in the country. Differences in observations across the treatments is due to re-coding of participants' responses.

In order to confirm that feeling of control manipulation was successful, we look at the manipulation checks. Participants in *high control* condition have reported substantially higher perceived level of control in the recalled situation as compared to those in *low control* condition: on a scale from 0 (no control) to 10 (complete control), average level of perceived control for *high control* groups was 7.5, while for *low control* it was around 2.4 (see Figure A-1 in Appendix 3.A). To check whether recalling a particular incident has evoked an emotional response during the study, we asked participants to indicate how the mere act of *remembering* the situation has made them feel. The *high control* groups reported to have experienced more *mainly positive* feelings compared to the *low control* groups (Figure A-2 in Appendix 3.A).

We also regress (both via OLS and ordered logistic regression) manipulation checks on

[10]Marginal effects estimations shows that being in the low social control group and having to make intertemporal decisions regarding money increases the probability of dropping out by 19.99%.

treatment assignment and covariates (see Table A-2 in Appendix 3.A). Subjective socioeconomic status has a positive impact on scores of both manipulation checks (significant at 1% level for perceived control and 10% level for emotional response). This could indicate a potential moderation effect; we control for this and other covariates in the further analysis. In terms of treatment, the only level of control but not the type is significant.

3.3 Results

This section begins with the non-parametric analysis of intertemporal preferences (subsection 3.3.1) and proceeds with the parametric estimations (subsection 3.3.2). We then present robustness checks (subsection 3.3.3) and look over potential mediators (subsection 3.3.4.

3.3.1 Non-parametric analysis

We hypothesize that feeling of control has a positive impact on time preferences, meaning, it is related to a more future-looking behaviour. Considering the experimental design, this implies that participants in high control conditions would prefer to wait and receive more money *later* or would prefer to perform more effort tasks *now* instead of *later*. Our results suggest that the hypothesis holds for the intertemporal allocation of effort, but not the money. *Money allocation.* Before starting the non-parametric analysis, we look at the consistency - or adherence to the law of demand - of the participants' decisions. Following Giné et al. (2018), the consistent intertemporal choice is such that allocation to a sooner date c stays constant or decreases as interest rate r increases but the time frame remains fixed. That is, if we keep t and k constant, then $c \leq c'$ where c is made under interest rate r, c' under r', and $r < r'$. Out of 15 allocation decisions in this study, participants had 12 chances to violate consistency assumption. At an individual level, 131 participants (40.68%) have at least one inconsistent allocation choice and 49 participants (15.22%) have more than 5 such choices. This level is much higher compared to the one observed in similar studies such as Augenblick et al. (2015); Andreoni and Sprenger (2012); Balakrishnan et al. (2020). One potential cause behind it could be a low engagement with the task due to an online - as opposed to the lab - environment. However, as suggested by Becker (1962), even random choice will follow the law of demand on average, therefore this indicator should not be taken for granted as a strict measure of rationality. Moreover, our intertemporal choice task design does not prevent the participant from unintentionally making an inconsistent allocation choice. To split the experimental budget between two dates, participants had to click on a slider or drag a pointer along it to choose the splitting point; clicking around the same area on the slider might visually look as if keeping budget shares constant across all decisions, however, the actual allocation values might have minor variations (see Figure A-1 in Appendix 3.B). For example, participants might have intended to split the experimental budget equally across all the decisions by clicking on a midpoint on the slider, but due to inattention, inaccuracy or technical difficulties in using a slider they might have made decisions that vary by a few units in either direction. For this reason, we consider as violating the law of demand only those allocations that are inconsistent by more than 5% of the budget, i.e. more than 0.5 unit difference for monetary discounting and more than 5 unit difference for effort discounting. After allowing variations of up to 5%, in monetary discounting condition 212 participants (65.84%) show no inconsistencies, while 19 participants (5.9%) have more than 5. We constrict our sample to include only participants with consistent choices. In subsection 3.3.3 we replicate our analysis

with a full sample[11].

We next look at the aggregate allocation behaviour. When decision frame involved present ($t = 0$, *now vs 2 weeks* and *now vs 4 weeks*), participants allocated 72% (£7.2) of their experimental budget to the sooner period under 0% interest rate (aggregated across all treatments). This share decreases with the interest rate: when interest is 100%, the average allocation to the sooner date is £6.5 lower (7% of the budget). One indicator of present bias is a static preference reversal: allocating bigger budget share to an earlier date when money is to be received in the present, but being more patient and allocating more to the future period when money is to be received in the future at earliest. Looking at the decisions made for the time frame that did not include the present ($t \neq 0$, *2 weeks vs 4 weeks*), we do not find consistently significant differences compared to $t = 0$ choices: on average, participants allocate 73% (£7.3) to an earlier date when the interest rate is 0% and this share falls as the interest grows (when it is 100%, participants allocate 8% of their budget, that is, £6.5 less). Results can be found in Table A-3 in Appendix 3.A.

Breaking down results by level and type of control we verify the absence of significant differences between treatments. Figures 3.3.1 and 3.3.2 plot the average allocation of money over the three time frames across the treatment arms. Each panel graphs mean allocation of £10 to an earlier date under different interest rate (ranging from 0% to 100%) and different dates: an earlier date set to *today* or *in two weeks* and a later date set to *in two weeks* or *in four weeks*. Figure 3.3.1 shows differences in allocations between low control and high control treatments, and Figure 3.3.2 displays allocations for task and social control conditions. We see no clear differences between different conditions (a graphical representation of combined treatment status can be found in Appendix 3.A). On average, participants in the low control group allocated a negligible amount of budget, £0.17, ($s.e. = 0.296$, $p = 0.56$), more to the earlier date as compared to the high control group. For social control treatment, the difference was even lower, only £0.10 more compared to task control ($s.e. = 0.296$, $p = 0.74$).

To check for static preference reversal, we follow Giné et al. (2018) and compare decisions with the same delay length of 2 weeks (*now vs 2 weeks* and *2 weeks vs 4 weeks*). We calculate the share of present biased allocations: participants had a chance to reverse their preferences over 5 pairs of decisions, therefore we calculate the percentage of decisions where allocation to the sooner period is bigger when $t = 0$ compared to $t \neq 0$. Static preference reversal constitutes around 22.17% of all money allocation choices: in the low control group we find $5pp$ more present biased decisions compared to high control group ($mean(low) = 0.123$, $mean(high) = 0.070$, $t = 1.73$, $p = 0.084$). A difference between task control and social control groups is $1pp$ ($mean(task) = 0.090$, $mean(social) = 0.101$, $t = 0.35$, $p = 0.724$). To check whether static preference reversal is influenced by the treatment status or covariates, we run ordered logistic regression and estimate marginal effects (see Table A-6 in Appendix 3.A). According to the results, the only variables that influence the probability of making (no) reversed choices are employment status and low social control condition. Being employed increases the probability of making 1 or 2 present biased decisions by approximately 4%, but decrease the probability of making no present biased decisions by 11%. Being in social control condition decreases the probability of making no

[11]We did not specify the checks on violations of the law of demand in the pre-registration plan.

Figure 3.3.1: Mean allocation of money (low control vs high control)

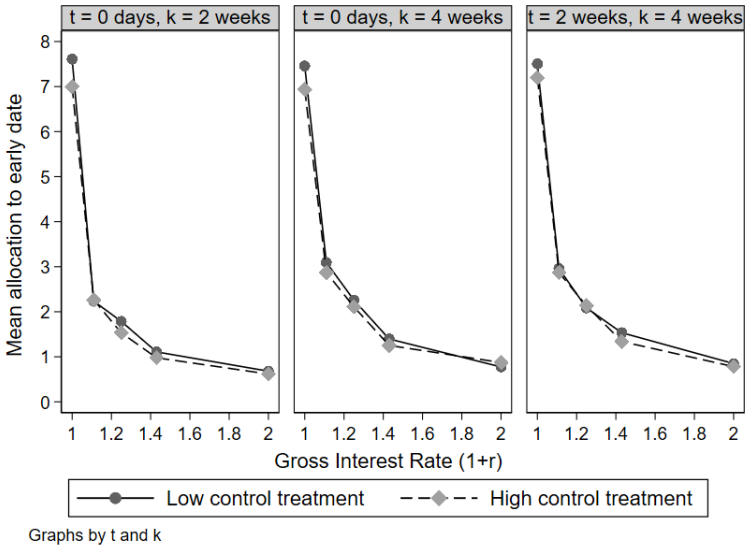
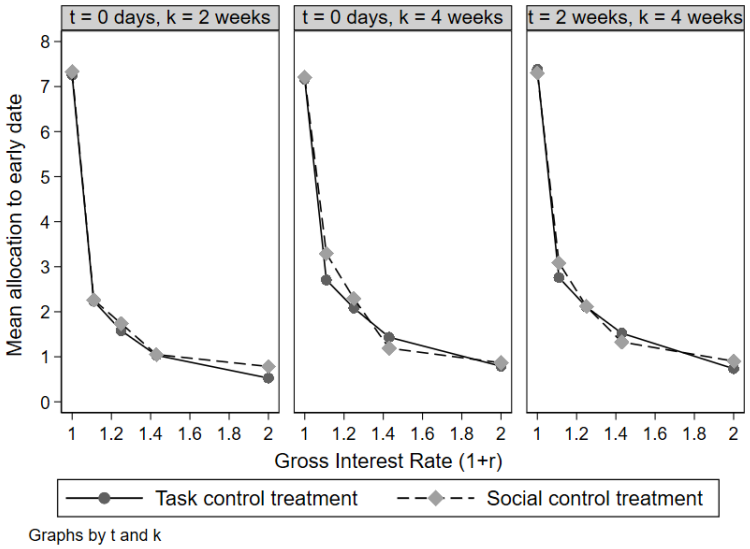


Figure 3.3.2: Mean allocation of money (task control vs social control)



reversed choices by 17%, but increases the probability of making 1 such choice by 6% (although results are significant only at 10% level).

Next, we regress allocation decisions (expressed as a share of experimental budget allocated to an earlier date within a decision time frame) on control conditions, interest rates and covariates[12]. Table 3.3.1 shows results from OLS regression with the robust standard errors clustered at the individual level[13]. We provide estimates for each of the three time frames. For money

[12]In pre-registration plan we have specified that we will regress powerfulness score and high/low power/control treatment on both beta parameter and share of allocations. As mentioned previously, due to the type of answers given in the recall task, coders were unable to construct consistent powerfulness scores throughout the sample.

[13]Since this experiment does not have a control group, for estimations we construct 4 treatment condition variables and use *high social control* as a baseline, as non-parametric and parametric analyses hint that observed differences are pushed forward by the low control conditions rather than high control groups).

condition, results confirm previous observations: neither the level nor the type of control has a significant impact on a share of budget allocated to an earlier date within a decision time frame. However, a higher interest rate results in a lower dependent variable, although estimated coefficients do not vary much between decision time frames. We also check whether personal characteristics impact the allocation decisions: a majority of covariates across all decision time frames are insignificant, with an exception of SES and age, which decrease allocation share when the decision is made between *today* and *4 weeks* (although the effect is marginal).

Table 3.3.1: Regression analysis

	Monetary Discounting			Effort Discounting		
	(1)	(2)	(3)	(4)	(5)	(6)
	Now vs 2 weeks	Now vs 4 weeks	2 weeks vs 4 weeks	Now vs 2 weeks	Now vs 4 weeks	2 weeks vs 4 weeks
Low task control	0.006	0.004	0.011	-0.081	-0.101*	0.034
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Low social control	0.029	0.041	0.037	-0.104*	-0.118**	-0.022
	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)
High task control	-0.011	0.005	0.018	-0.011	-0.028	0.043
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
Interest rate	-0.462***	-0.472***	-0.477***	0.161***	0.160***	0.146***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Age	-0.004	-0.006**	-0.001	0.004*	0.004*	0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Education	0.010	0.040	0.051	0.040	0.020	0.049
	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Student	-0.070	-0.096	-0.053	0.177**	0.166**	0.241***
	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.08)
Female	-0.002	0.010	-0.013	-0.027	-0.032	-0.025
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Employed	0.004	0.037	0.012	-0.061	-0.044	-0.035
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)
SES	-0.008	-0.018*	-0.016	0.023	0.017	0.023
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Constant	1.077***	1.200***	1.074***	0.068	0.139	-0.016
	(0.11)	(0.12)	(0.14)	(0.14)	(0.14)	(0.14)
N	1060	1060	1060	965	965	965
Adj.R^2	0.187	0.191	0.183	0.083	0.077	0.076
Cluster	212	212	212	193	193	193

Note: Table reports coefficients from OLS regression. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Dependent variable is a *share of budget* allocated to an earlier date. Columns (1) - (3) present OLS regression results for money allocation condition, while columns (4) - (6) show OLS regression results for effort allocation condition. Regressions are run separately for each of the three decision time frames, indicated in the names of the columns. We use *high social control* condition as a baseline.

Effort allocation. In the effort condition, 156 participants (52%) have at least 1 inconsistent choice and 44 (14.67%) have more than 5 such choices. When we adjust our consistency requirements to allow for slight variation in allocated amounts, choices of 193 participants (64.33%) follow the law of demand and only 11 participants (3.67%) have more than 5 inconsistent choices. Just like in the case of money condition, this level is much higher compared to similar studies like Augenblick et al. (2015); Augenblick and Rabin (2019) and we attribute a high likelihood of this to an online environment. For further analysis, we use a sample of 193 participants with no consistency violations and replicate estimations using the full sample in Section 3.3.3.

On average, participants chose to perform 61% of 100 effort tasks at $t = 0$ date (see Table A-3). The variation of interest rate does not bring about big differences in budget shares: at 0%,

participants allocate 54% of their task budget to the current period while at the maximum rate of 75% the number increases by around 13 tasks (to 67%). When asked to make their decisions in the time frame involving only future (*2 weeks* vs *4 weeks*), participants allocate 4 tasks more to the sooner period ($p = 0.010$). For all interest rates, allocations to an earlier date are significantly higher when the decision is made in the future ($p < 0.1$). This hints at a presence of a static preference reversal: when asked to make a decision in the present, participants choose to do fewer tasks on that exact day rather than when making an equivalent decision with the same time delay length but involving only the future.

Figures 3.3.3 and 3.3.4 show allocation of 100 real effort tasks over 5 different interest rates. Participants in the *effort* condition faced the same three time periods as in *money* condition. With an increase in the interest rate, the number of tasks in the future also increases, therefore, following the law of demand, allocation of tasks to the later period should decrease. Static preference reversal is visible in the case of level of control: in Figure 3.3.3 participants in high control group choose to do more tasks on the sooner date when $t = 0$, however, the difference diminishes when $t \neq 0$. Those in high control condition, on average, allocated 7.1 tasks more to the earlier date ($s.e. = 3.95$, $p = 0.07$). Participants in social control group chose to do, on average, 1.15 tasks less on an earlier date ($s.e. = 3.93$, $p = 0.77$). Moreover, Figure 3.3.4 shows that these two treatments diverge slightly when the front-end delay of the intertemporal decision is $t \neq 0$. When decision time involves present, there are no visible differences between the type of control conditions. Graphical representation of aggregate allocation decisions by combined control treatment status can be found in Appendix 3.A.

Next, we compare decisions with the same delay length to check for static preference reversals. For the effort allocation condition, 45.08% of all decisions can be categorized as present biased. When breaking down by treatment, in low control group 39.42% of all choices exhibit static preference reversal as compared to 22.5% in high control group ($p = 0.003$), while the difference between task and social control groups is only $5.4pp$ ($mean(task) = 0.341$, $mean(social) = 0.287$, $p = 0.354$).

The results from the ordered logit regression and marginal effects estimations suggest that only low control conditions affect probabilities of reversed decisions (see Table A-6). In particular, being either in a low task or in low social control groups decreases the probability of 0 present biased decisions by around 25%, but consistently increase probabilities to make two or more such decisions (estimates range from 1% for 2 biased decisions to over 15% for 5 such choices). Regression results in Table 3.3.1 show the effect of control treatments, interest rates and personal characteristics on allocation decisions. We find that interest rate increases a share of effort tasks allocated to an earlier date, although less so when $t \neq 0$. When decision is made regarding *now* and *2 weeks*, being in low social control decreases budget share by $10.4pp$ ($p < 0.5$). When delay length increases to $4weeks$, allocations in low task control condition fall by $10.1pp$ ($p < 0.1$) and by $11.8pp$ ($p < 0.1$) in low social control condition. Control treatments do not have a significant impact when $t \neq 0$. Among the covariates, we find a strong effect of being a student: across all time frames, students allocated a higher share of effort task budget by $17 - 24pp$. However, this can be partly determined by a very high share of students in the sample. Age is also significant, though only at a 10% level.

Figure 3.3.3: Mean allocation of effort (low control vs high control)

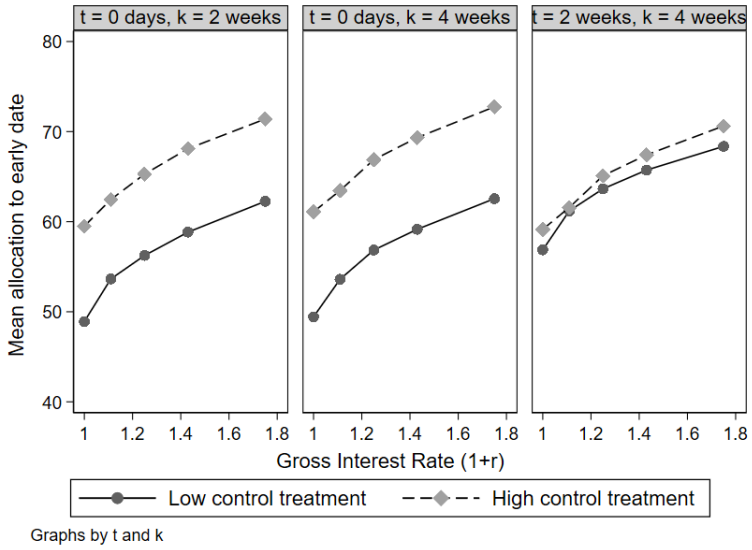
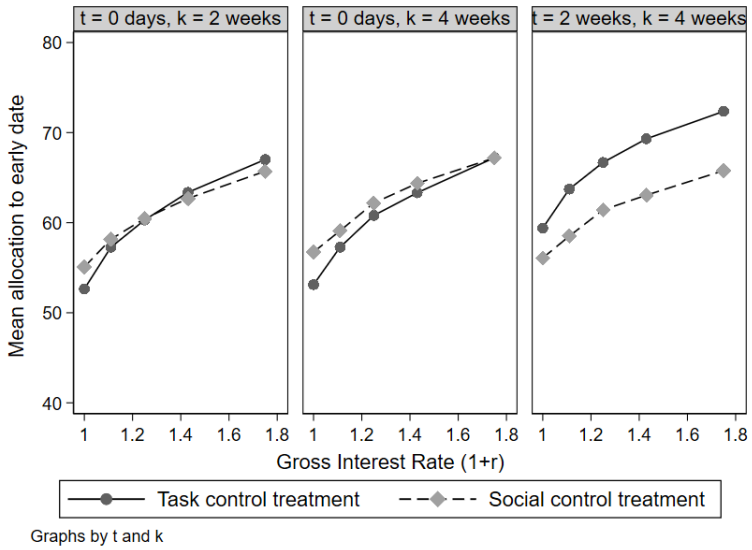


Figure 3.3.4: Mean allocation of effort (task control vs social control)



3.3.2 Parametric analysis

Following Andreoni and Sprenger (2012) and Augenblick et al. (2015), we also perform parametric analysis to investigate the existence of present bias in both intertemporal choice conditions further. We assume quasi-hyperbolic $\beta - \delta$ discounting function (Laibson, 1997; O'Donoghue and Rabin, 2001) with Stone-Geary background consumption parameters. The quasi-hyperbolic discounted utility function for money discounting with experimental payments $c_t$at sooner date t, and c_{t+k} and later date $t + k$ is:

$$U(c_t, c_{t+k}) = (c_t + \omega)^\alpha + \beta^{1_{t=0}} \delta^k (c_{t+k} + \omega)^\alpha \tag{3.1}$$

where β is the present bias parameter, *delta* is the discount parameter. $1_{t=0}$ is an indicator variable whether the sooner payment is done in the present. If $\beta = 1$, it implies no present bias

and the function takes the standard temporal discounting form. We assume $\alpha < 1$, meaning that the utility function is concave. The Stone-Geary background consumption ω is set to the minimum experimental payment of £0.77.

For effort discounting, function takes a similar form. Taking into account effort to be performed $e_t$at sooner date t, and e_{t+k} and later date $t + k$, quasi-hyperbolic discounting with Stone-Geary background parameters produce the following discounted effort function:

$$C(e_t, e_{t+k}) = (e_t + \omega)^\gamma + \beta^{1_{t=0}}\delta^k(e_{t+k} + \omega)^\gamma \tag{3.2}$$

Here, Stone-Geary background consumption ω represents a minimum amount of effort (work) to be performed, which we set to 1 (every participant had to do a trial of one real effort task before making their intertemporal allocations). Moreover, $\gamma > 1$ represents a parameter of convex instantaneous cost of effort function. To estimate intertemporal parameters we use two-limit Tobit[14] regressions with standard errors clustered at the individual level.

To check whether there are any substantial differences in discounting over different domains, we first look at the intertemporal parameters aggregated over the treatments. Table 3.3.2 presents results for both monetary and effort discounting. For intertemporal choice involving money, we find a weekly discount factor of 0.979 and a present bias parameter β higher than 1 ($\beta = 1.042$), which falls in line with the results by Augenblick et al. (2015); Andreoni and Sprenger (2012). Testing hypothesis that $\beta = 1$, we actually find an indication of future bias ($p = 0.003$). Utility function curvature parameter $\alpha = 0.939$ suggests that preferences over money are close to linear. This would indicate that participants treat the experimental budget as a fungible good, having no desire to smooth their consumption over time. The situation is different for effort discounting. Here we estimate the present bias parameter β to be lower than 1 ($\beta = 0.886$) and we reject the hypothesis of no present bias ($p = 0.046$). Discount parameter δ is higher and significantly different from 1 ($\delta = 1.174$, $p = 0.000$); this implies that participants want to complete less work in the future as the delay increases. A cost of effort parameter γ is also higher than 1, indicating a desire to smooth the work through time. This can be also seen in the number of corner allocations: in a full sample, only 18% of effort allocation choices were corner compared to 35.4% in money condition.

One of our hypotheses concerns the level of control, therefore, next, we look at the parametric estimations for low and high control conditions. In monetary discounting condition, we find that low control group exhibits no present bias ($\beta = 1.023$, $p = 0.265$) and has a standard discounting parameter δ equal to 0.980 ($p = 0.036$). On the other hand, for high control group we see a preference for future payments as $\beta = 1.061$ ($p = 0.001$), but there is just a slight difference in terms of discounting factor $\delta = 0.979$ ($p = 0.043$) compared with the low control group. The curvature parameter remains close to 0.94 in both groups. We also check parameter estimations for the type of control (see Table A-7 in Appendix 3.A) and the interaction between type and

[14]In pre-registration form we have specified the use of non-linear least squares estimation. Andreoni and Sprenger (2012) suggested using both non-linear least squares and two-limit Tobit regressions. Augenblick et al. (2015) discuss the issue of the potential presence of corner solutions in the CTB environment, which should be accounted for through the use of two-limit Tobit regression. We follow the latter suggestion.

Table 3.3.2: Parameter estimates

	(1) Monetary Discounting	(2) Effort Discounting
Present bias parameter β	1.042	0.886
	(0.014)	(0.057)
Discount factor (weekly) δ	0.979	1.174
	(0.007)	(0.041)
Curvature parameter (monetary) α	0.939	
	(0.008)	
Cost of effort parameter γ		1.391
		(0.049)
Observations	3180	2895
Clusters	212	193
p-values - Tests:		
$H_0 : \beta = 1$	0.003	0.046
$H_0 : \delta = 1$	0.004	0.000

Notes: Parameters identified from two-limit Tobit regressions (3.1) and (4.1). SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

level of control[15] (Table A-8 in Appendix 3.A). We do not observe stark differences between task and social control groups ($\beta = 1.041$, $p = 0.036$ for task control condition and $\beta = 1.043$, $p = 0.032$ for social control condition). Discounting parameter δ varies from 0.980 to 0.978 with $p < 0.1$. This result suggests that a type of a recalled situation does not produce substantial differences in the money allocation choices. We also look at the parametric estimations across all experimental arms[16]. In neither of the treatments, the present bias parameter β is lower than 1. Moreover, in low control conditions, we cannot reject the hypothesis that $\beta = 1$ ($p > 0.1$). In high control conditions β parameter is significantly higher than 1 ($p < 0.05$). It could be an indicator of a treatment effect: participants who recalled a situation of high control also chose more *larger-later* rewards.

Effort discounting presents a different picture. Low control group has $\beta = 0.793$ ($p = 0.015$), while for high control $\beta = 1.004$ ($p = 0.952$). These results hint that the aggregate present bias parameter ($\beta = 0.886$) is pushed below 1 mainly by low control group allocation decisions. High control condition has $\delta = 1.222$: different from 1 ($p = 0.001$) and higher than δ of low control group ($\delta = 1.138$, $p = 0.006$). Cost of effort parameter γ varies between 1.425 for low control and 1.352 for high control conditions. When looking at the results aggregated over the type of control (see Table A-7 in Appendix 3.A), we find task control group to have $\beta = 0.827$ ($p = 0.019$) as compared to $\beta = 1.004$ ($p = 0.967$) in social control group. Discount factor ranges from 1.153 for task and 1.211 for social control condition with $p < 0.05$ for both groups. Cost of effort parameter γ is higher for the social control ($\gamma = 1.505$) compared to the task control group ($\gamma = 1.327$). To check whether level of control is driving these differences in type of control estimations, we report estimates for each control condition interaction (see Table A-9 in Appendix 3.A). The results are less straightforward here: hypothesis of no present bias is rejected only for low task control group ($\beta = 0.776$, $p = 0.037$). This hints that participants in low control group - in particular, those who

[15]In pre-registration plan we raised the hypothesis of impact on intertemporal preferences regarding only a level of control. These estimations serve as an exploratory exercise.

[16]It should be noted that because of a sample adjustment to include only consistent answers, sample sizes in these estimations decrease substantially, meaning, that the results can suffer from the power issue, therefore, they should be interpreted with caution

Table 3.3.3: Parametric estimations by treatment: level of control

	Monetary Discounting		Effort Discounting	
	(1)	(2)	(3)	(4)
	Low	High	Low	High
Present bias parameter β	1.023	1.061	0.793	1.004
	(0.020)	(0.019)	(0.085)	(0.070)
Discount factor (weekly) δ	0.980	0.979	1.138	1.222
	(0.010)	(0.010)	(0.050)	(0.069)
Curvature parameter (monetary) α	0.941	0.937		
	(0.011)	(0.011)		
Cost of effort parameter γ			1.425	1.352
			(0.073)	(0.065)
N	1515	1665	1575	1320
Clusters	101	111	105	88
p-values - Tests:				
$H_0 : \beta = 1$	0.265	0.001	0.015	0.952
$H_0 : \delta = 1$	0.036	0.043	0.006	0.001

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

had to recall a situation that did not involve other people - have chosen to postpone more work for future, even if the total amount of work tasks was increasing with the delay and interest rate. Standard discounting is non-linear for both low control groups ($\delta = 1.126$, $p = 0.034$ for low task and $\delta = 1.170$, $p = 0.045$ for low social control condition) and for high social control ($\delta = 1.241$, $p = 0.047$). However, as mentioned previously, due to low sample size these estimations should be interpreted with caution.

3.3.3 Robustness checks

In the section 3.3.1 we discussed inconsistency of the intertemporal allocations. For the main analysis, we have chosen to restrict the sample by including only those participants whose choices follow the law of demand as the inconsistent allocations can be an indicator of a low engagement with the task. However, it is possible that low attention was caused by the experimental treatment (for example, participants recalling low control situations were less attentive in the subsequent allocation task). For this reason, we check the impact of treatment status and personal characteristics on inconsistent decisions. Results are reported in the Table A-10 in Appendix 3.A. We regress treatment status and covariates on a binary variable with a value of 1 indicating that the participant has at least 1 inconsistent decision. We consider only strict inconsistency and do not allow variations of allocation decisions by up to 5%. Results show that treatment status does not influence inconsistency in either of the intertemporal conditions, although being in an effort condition increases the probability of making an inconsistent choice by 12%. We also run a regression with the interaction effect between the type of intertemporal choice task and treatments and covariates. Results show that age decreases the probability of inconsistent choice by 1.1% while being female increases it by 10.9%. The only significant interaction term includes socioeconomic status: participants in effort allocation had a lower probability of making an inconsistent decision with higher SES (-5.1%).

Next, we replicate our analysis with the full sample and present results in Appendix 3.A. The results largely confirm our previous findings. Looking at the non-parametric tests, we find that experimental budget allocations follow the same pattern as in a restricted sample: for effort

allocation condition, we find evidence of static preference reversal ($p < 0.05$ across all interest rates), while in money condition results do not show consistently different means in allocations when $t = 0$ and $t \neq 0$ (Table A-12). The same situation persists for low control conditions in effort allocations as differences between budget share allocated to an earlier date are consistently lower when $t = 0$ as compared to $t \neq 0$; these results do not hold either for high control treatment nor either of control levels in money allocations decisions (Table A-13). In terms of the type of control, only effort allocations made in task control condition exhibit significant differences under varying front-end delays (Table A-14).

In money condition, 43.17% of choices show static preference reversal, however, when dividing by treatments, no clear differences arise. Difference between present biased choices in low and high control conditions is 0.2% ($t = 0.08, p = 0.929$); it reaches 2.3% between task and social control groups ($t = 0.73, p = 0.468$). In the effort condition, 60.67% of observations can be identified as present biased: in low control condition 45.1% of choices can be categorized such way as compared to 31.7% in high control condition ($t = 3.00, p = 0.003$), while task and social control conditions show only a negligible difference ($mean(task) = 41.81\%$, $mean(social) = 35.70\%$, $t = 1.35$, $p = 0.176$). When static preference reversals are regressed on treatment status and covariates, results remain robust: for monetary discounting only being employed increases the probability of making biased decisions, while for effort discounting it is the low control conditions that have such effect (Table A-11). These results are mostly confirmed also for regression of allocation share on treatment status and covariates: as with the restricted sample, we find that money allocations are influenced mainly by interest rate (Table A-20). We also find age to decrease allocation shares, although just marginally. For effort discounting, apart from the interest rate, being in low social control condition decreases effort task budget share allocated to an earlier date by 11.9*pp* ($p < 0.05$) while for low task control it falls by 7*pp* ($p < 0.1$); however, this applies only to a decision made regarding the trade-off between *now and 4 weeks*. When decision concerned *now and 2 weeks*, only low social control significantly reduces budget allocations by 8.9*pp* ($p < 0.05$). No effect is found for decisions involving only the future. Across all the covariates, only age had a consistent, though minor, effect on the dependant variable. On average higher age increased allocation to an earlier date by 0.6*pp* across all three decision time frames.

Intertemporal parameter estimations for full sample confirm no present bias in monetary discounting condition (Table A-15), neither when observations are divided by level of control (Table A-16) nor by type of control (Table A-17) or across all control conditions (Table A-18). For effort allocation, we confirm presence of $\beta < 1$ only for low control condition ($\beta = 0.772$, $p = 0.006$): when divided by type of control, we find $\beta \neq 1$ for both task and social control conditions ($\beta = 0.724$, $p = 0.032$ for low task and $\beta = 0.719$, $p = 0.055$ for low social control). Looking over these results, we conclude that analysis with the full sample including participants answers that violate the assumption of rationality still largely confirm our previous results.

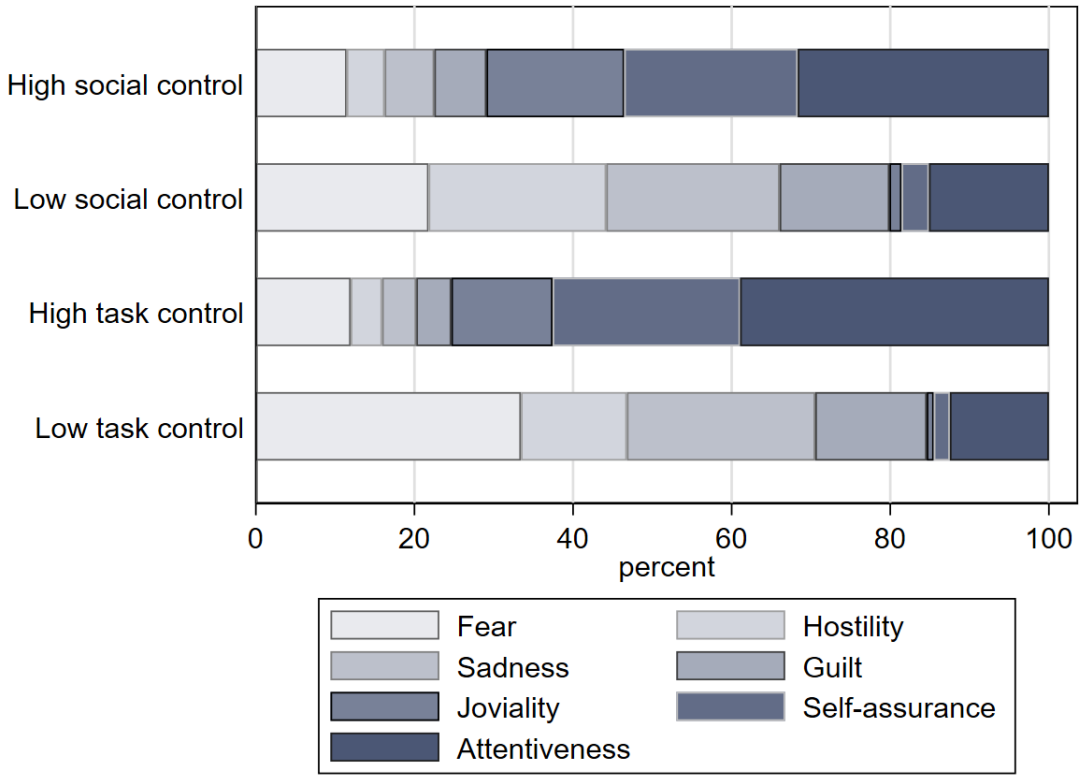
3.3.4 Mechanisms

In this study, we raise hypotheses regarding the effect of control level on intertemporal choice and the difference in emotions between social and non-social situations. It is possible that emotions mediated the control effect on allocation decisions. For this reason, we look at the

trends in terms of emotional states that participants have reported in the *recall task*, differences across treatments, and the mediation effects[17].

After recalling a situation, participants were given a list of emotional states, grouped by type, and asked to choose which of these items could describe best how they felt during the recalled situation. This scale is comprised of two types of emotions - positive and negative - which are grouped in distinct categories (states) composed of a varying number of items (the entire scale can be found in Appendix 3.B). Figure 3.3.5 represents the average proportions of each emotional state across the treatments. A low level of control shows a higher percentage - around 80% - of negative emotional states (*fear, hostility, sadness, guilt*) selected. In particular, the predominant emotion selected by the low *task* control group was fear (33% of all responses), followed by sadness (24%), while the low *social* control group was determined more equally by fear, hostility and sadness (around 20% each). High control treatments exhibit the opposite: among the positive emotions, which are predominant in both groups, attentiveness was the main emotional factor (39% for task and 32% for social control groups), followed by self-assurance (around 20% in both types of control). Overall, the variation in dominating emotional states is relatively clear in terms of the level of control, but less so for the type of control.

Figure 3.3.5: Mean proportions of emotional state in each treatment



Notes: Average percentage frequencies of emotion states in the recalled event across treatments. Exact list of items within each group can be found in Appendix 3.A

We next check for differences among treatments (Table A-21 in Appendix 3.A). Results confirm the observations in Figure 3.3.5: level of control produces significant differences for each

[17]Mediation analysis was not included in the pre-registration plan. We include it as an exploratory exercise.

emotional state, while the results associated with a type of control are less straightforward. Being in high control situation evokes a higher degree of positive emotions, mostly emotional states related to attentiveness (26% difference in mean frequency). For this group participants also report negative emotions less frequently. For the type of control, the significant differences appear mainly in terms of joviality, fear, and hostility: participants in the task control group report on average 5.5% less jovial and 5.7% less hostile emotions, but 9% more fear. Taking a more in-depth look we see that these results are driven mainly by the combination of low task vs low social type of control. Participants recalling low control situations involving other people reported significantly more feelings of hostility (13%) and fewer feelings of fear (12.7%).

Finally, we estimate Average Causal Mediation Effects (ACME) bootstrapped from 100 repetitions for each emotional state under low compared to a high control condition. This is repeated separately for monetary and effort discounting. We find that for effort discounting condition 68.71% of total effect (0.0457) is mediated by sadness ($ACME$ = 0.031), followed by self-assurance which mediates 62.14% ($ACME$ = −0.028) and joviality which mediates 36.10% ($ACME$ = 0.0165). For monetary discounting, 96.64% of the total effect (-0.009) was mediated by fear ($ACME$ = −0.008) and 70.62% by joviality ($ACME$=-0.014). ACME estimates can be found in Table A-22 in Appendix 3.A.

3.4 Conclusions

In an online experiment, we investigate the effect of perceived control in a recalled situation on intertemporal preferences, in particular, involving money and effort discounting. We vary control conditions by changing the level of perceived control (low and high) and the type of control (task and social). Overall, we do not find evidence of present biased behaviour in monetary discounting conditions. On average, participants in this group allocated more than 70% of their experimental budget to an earlier date when an interest rate was r = 0% and this share fell to approximately 7% (£6.5 less) as the amount of money could be doubled when allocated to the future period (r = 100%). We estimate the present bias parameter β to be higher than 1, indicating a future focus in participants' decisions. Results do not change when divided by treatment status. Interest rate is the only predictor of the change in experimental budget allocations.

Results for the effort discounting group present a different picture. There is less variation in allocation shares across different interest rates: depending on the rate, the share of effort task budget allocated to be done on an earlier date varies between 50% and 70%. However, when comparing time frames where an earlier date is *today* with a time frame where it is *2 weeks* from the date of experiment, participants, on average, chose to work more earlier when a decision was made about a point of time in the future. These differences stand out when we look at the results divided by treatments. We find that high control treatment exhibits no present bias (β = 1.004), while for low control group β = 0.793. Considering the type of control, the task control group has an aggregate present bias parameter lower than the social control group (β = 0.827 and β = 1.004, respectively). This indicates that those participants who had to remember and describe a situation where they were not in control in a non-social situation, also chose to do less work now, even if it meant doing more work in the future. Regression results suggest that effort discounting is affected by interest rate and low control conditions, although

the latter is significant only when $t = 0$.

Analysis of the emotional states reported in the *recall task* suggests that participants in low control conditions reported more negative emotions (in particular, fear, sadness, and hostility) as opposed to positive emotions in high control conditions (in particular, attentiveness and self-assurance). Treatments differ mainly in level rather than the type of control. Mediation analysis shows that the treatment effect of low control on allocation decisions was mediated mainly by sadness, followed by self-assurance and joviality for effort discounting and fear, followed by joviality, for money discounting.

We draw several words of caution from our results. Since we observe a relatively high level of inconsistent choices, we cannot dismiss a concern that this indicates low engagement with the experimental task. Previous works measuring intertemporal preferences such as Gneezy et al. (2020); Augenblick et al. (2015); Andreoni and Sprenger (2012) ran studies in the lab setting; our work, on the other hand, is run online and can potentially suffer from low attention. For this reason, we run an analysis with both a restricted sample that includes only consistent allocation decisions and the full sample. Although results prove to be robust, in particular, parametric estimation would benefit from a bigger sample of consistent intertemporal choices. Low power could be one of the reasons why our results for money discounting do not follow previous findings by Duan et al. (2017); Gneezy et al. (2020). Another reason for such differences could lie in different intertemporal preference elicitation methods using different incentive schemes, discounting periods Secondly, the only requirement for the *recall task* was to provide at least a few sentence descriptions of the recalled situation; we did not establish a time limit or other factor controlling for commitment to the task; moreover, we also did not ask participants to rank identify the degree of personal importance to the recalled situation. For these reasons we cannot control whether recollection of (no) control situation caused a proper emotional engagement that one could have when *experiencing* - instead of remembering - a control situation and this might have resulted in weaker treatment effects. Having participants assign a level of personal importance to the (no) control situation would have also allowed us to check for moderation effect in terms of manipulation. Third, we should be careful in drawing implications from these results. Although they hint towards the potential impact of perceived control on allocation of effort (work) in time and no present bias in money discounting, experimental design does not mimic real-life closely. For example, individuals rarely have to make decisions about allocating windfall money between two points in time; moreover, there might be differences between choosing to exert effort in activities that are essential in your life (such as one's job) and effort in experimental tasks which might look more like a game. For this reason, future works could address these issues by investigating the relationship between the feeling of control and intertemporal preferences in a field setting. Finally, due to the intertemporal nature of analyzed choices, participants might have also been affected by their degree of optimism and risk perception as well as age and occupation (in our sample, over 95% of participants were students). Future studies should control better for such individual characteristics.

Appendix

3.A Appendix A

Table A-1: Attrition by treatment status

	(1)	(2)	(3)	(4)
Low control	0.026			0.038
	(0.03)			(0.04)
Social control		-0.112		
		(0.20)		
Money condition			0.040	
			(0.20)	
Low social control				-0.519
				(0.42)
Low control x Money condition			-0.457	
				(0.38)
Social control x Money condition				-0.264
				(0.35)
Low control x Social control x Money condition				1.546*
				(0.67)
Constant	-1.789***	-1.643***	-1.713***	-1.728***
	(0.14)	(0.13)	(0.14)	(0.16)
N	778	778	778	778

Note: Estimates obtained via logistic regressions. Robust standard errors in parentheses. *, **, *** denote level of significance (10%, 5%, and 1% respectively). Outcome variable is a dummy on decision to drop out (0 means participant completed the study, 1 means participant dropped out at any point after being assigned to the control treatment).

Table A-2: Manipulation check: treatment effects on perceived control and emotional response

	(1) Manipulation	(2) Emotions	(3) Manipulation	(4) Emotions
High control	5.131***	3.556***	3.447***	2.395***
	(0.19)	(0.20)	(0.22)	(0.17)
Social control	-0.084	-0.153	0.030	-0.095
	(0.19)	(0.21)	(0.14)	(0.14)
Age	-0.007	-0.001	-0.007	-0.003
	(0.02)	(0.02)	(0.01)	(0.01)
Female	-0.155	-0.395	-0.106	-0.238
	(0.19)	(0.20)	(0.14)	(0.14)
Education	0.150	-0.123	0.193	-0.122
	(0.22)	(0.23)	(0.15)	(0.16)
Student	0.242	0.390	0.203	0.053
	(0.52)	(0.51)	(0.39)	(0.30)
Employed	0.179	0.264	0.070	0.152
	(0.20)	(0.21)	(0.14)	(0.15)
Subjective SES	0.244***	0.153*	0.185***	0.097*
	(0.07)	(0.07)	(0.05)	(0.05)
N	622	622	622	622
R^2 (adj./pseudo)	0.541	0.336	0.000	0.000

Note: Estimates obtained via OLS regressions in columns (1) and (2); columns (3) and (4) present results from ordered logit regressions. Robust standard errors in parentheses. *, **, *** denote level of significance (10%, 5%, and 1% respectively). Outcome variables are (1) manipulation check on control (*How much in control you felt during the recalled situation? Scale from 0 - no control to 10 - complete control*, and (2) manipulation check on emotions (*What type of feelings you felt when recalling the situation? Scale from 0 - mainly negative to 10 - mainly positive*. Exact wording of questions and scales can be found at Appendix 3.A. Columns (1)-(2) report adjusted R^2; columns (3)-(4) report pseudo R^2.

Table A-3: Nonparametric analysis. Aggregate behavior by interest rate

	(1) $t = 0$ Budget share	(2) $t \neq 0$ Budget share	(3) t-test (p value)
Panel A: Money allocation			
r = 0%	0.724	0.734	0.88
	(0.027)	(0.027)	(0.381)
r = 11%	0.261	0.291	2.00
	(0.024)	(0.027)	(0.046)
r = 25%	0.192	0.211	1.35
	(0.020)	(0.023)	(0.177)
r = 43%	0.118	0.143	2.25
	(0.015)	(0.019)	(0.025)
r = 100%	0.074	0.081	1.19
	(0.013)	(0.014)	(0.235)
Overall	0.274	0.292	2.16
	(0.015)	(0.016)	(0.031)
Panel B: Effort allocation			
r = 0%	0.542	0.579	2.67
	(0.022)	(0.022)	(0.008)
r = 11%	0.579	0.614	2.67
	(0.022)	(0.021)	(0.010)
r = 25%	0.609	0.643	2.72
	(0.021)	(0.021)	(0.007)
r = 43%	0.634	0.665	2.44
	(0.021)	(0.020)	(0.016)
r = 75%	0.668	0.694	1.87
	(0.021)	(0.020)	(0.063)
Overall	0.606	0.639	2.58
	(0.020)	(0.020)	(0.010)

Note: Panel A tabulates shares of money allocated to an earlier date when it $t = 0$ (1) and $t \neq 0$ (2). Each row presents results calculated from 10 $t = 0$ and 5 $t \neq 0$ allocations with standard errors in the parentheses. Column (3) presents paired t-test results with 211 degrees of freedom (p-values in parentheses). Panel B tabulates shares of effort task allocated to an earlier date. Column (3) presents paired t-test results with 192 degrees of freedom (p-values in parentheses).

Table A-4: Nonparametric analysis. Aggregate behavior by interest rate and level of control

	(1) t = 0 Budget share Low control	(2) t ≠ 0 Budget share Low control	(3) t-test (p value) Low control	(4) t = 0 Budget share High control	(5) t ≠ 0 Budget share High control	(6) t-test (p value) High control
Panel A: Money allocation						
r = 0%	0.753	0.750	0.14	0.697	0.720	1.95
	(0.037)	(0.037)	(0.888)	(0.039)	(0.038)	(0.053)
r = 11%	0.267	0.296	1.11	0.257	0.287	1.96
	(0.034)	(0.039)	(0.268)	(0.033)	(0.037)	(0.053)
r = 25%	0.202	0.208	0.25	0.182	0.213	1.98
	(0.029)	(0.033)	(0.802)	(0.027)	(0.032)	(0.050)
r = 43%	0.125	0.153	1.52	0.111	0.134	1.71
	(0.021)	(0.028)	(0.132)	(0.021)	(0.025)	(0.090)
r = 100%	0.073	0.085	1.34	0.074	0.078	0.40
	(0.019)	(0.022)	(0.183)	(0.019)	(0.020)	(0.690)
Overall	0.284	0.299	0.99	0.264	0.286	2.31
	(0.020)	(0.024)	(0.322)	(0.021)	(0.023)	(0.023)
Panel B: Effort allocation						
r = 0%	0.492	0.569	3.72	0.603	0.591	0.74
	(0.027)	(0.026)	(0.000)	(0.035)	(0.037)	(0.461)
r = 11%	0.536	0.612	3.72	0.630	0.616	0.92
	(0.07)	(0.026)	(0.000)	(0.034)	(0.035)	(0.361)
r = 25%	0.565	0.636	3.82	0.661	0.651	0.65
	(0.027)	(0.025)	(0.000)	(0.032)	(0.033)	(0.516)
r = 43%	0.590	0.657	3.71	0.687	0.674	0.82
	(0.026)	(0.025)	(0.000)	(0.032)	(0.033)	(0.416)
r = 75%	0.624	0.684	2.79	0.721	0.706	0.94
	(0.027)	(0.026)	(0.006)	(0.031)	(0.032)	(0.350)
Overall	0.561	0.632	3.74	0.660	0.648	0.85
	(0.026)	(0.025)	(0.000)	(0.032)	(0.033)	(0.399)

Note: Columns (1) - (3) present results for low control treatment groups. Columns (4) - (6) present results for high control treatment groups. Panel A tabulates share of money allocated to an earlier date when $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Each row presents results calculated from 10 $t = 0$ and 5 $t \neq 0$ allocations. Column (3) presents paired t-test results with 100 degrees of freedom. Column (6) presents paired t-test results with 110 degrees of freedom. Panel B tabulates share of effort tasks allocated to an earlier date $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Column (3) presents paired t-test results with 104 degrees of freedom. Column (6) presents paired t-test results with 87 degrees of freedom.

Table A-5: Nonparametric analysis. Aggregate behavior by interest rate and type of control

	(1) $t = 0$ Budget share Task control	(2) $t \neq 0$ Budget share Task control	(3) t-test (p value) Task control	(4) $t = 0$ Budget share Social control	(5) $t \neq 0$ Budget share Social control	(6) t-test (p value) Social control
Panel A: Money allocation						
r = 0%	0.721	0.738	1.25	0.727	0.730	0.16
	(0.038)	(0.038)	(0.213)	(0.038)	(0.038)	(0.874)
r = 11%	0.247	0.276	1.85	0.278	0.309	1.17
	(0.033)	(0.038)	(0.067)	(0.033)	(0.039)	(0.245)
r = 25%	0.183	0.211	1.47	0.202	0.212	0.46
	(0.027)	(0.032)	(0.143)	(0.029)	(0.032)	(0.648)
r = 43%	0.123	0.152	1.90	0.112	0.133	1.26
	(0.021)	(0.027)	(0.060)	(0.022)	(0.026)	(0.209)
r = 100%	0.066	0.074	0.72	0.082	0.090	1.17
	(0.017)	(0.022)	(0.473)	(0.021)	(0.022)	(0.246)
Overall	0.268	0.290	1.93	0.280	0.295	1.12
	(0.021)	(0.024)	(0.055)	(0.021)	(0.023)	(0.267)
Panel B: Effort allocation						
r = 0%	0.529	0.594	3.29	0.559	0.561	0.10
	(0.030)	(0.028)	(0.001)	(0.033)	(0.035)	(0.922)
r = 11%	0.573	0.637	3.28	0.586	0.585	0.07
	(0.030)	(0.027)	(0.001)	(0.032)	(0.033)	(0.940)
r = 25%	0.605	0.667	3.42	0.613	0.614	0.05
	(0.029)	(0.026)	(0.000)	(0.030)	(0.032)	(0.956)
r = 43%	0.633	0.6693	3.32	0.635	0.630	0.28
	(0.029)	(0.026)	(0.001)	(0.029)	(0.032)	(0.777)
r = 75%	0.671	0.723	2.60	0.664	0.658	0.38
	(0.029)	(0.026)	(0.011)	(0.029)	(0.032)	(0.706)
Overall	0.602	0.663	3.37	0.612	0.610	0.12
	(0.028)	(0.026)	(0.001)	(0.030)	(0.032)	(0.903)

Note: Columns (1) - (3) present results for task control treatment groups. Columns (4) - (6) present results for social control treatment groups. Panel A tabulates shares of money allocated to an earlier date when $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Each row presents results calculated from 10 $t = 0$ and 5 $t \neq 0$ allocations. Column (3) presents paired t-test results with 112 degrees of freedom. Column (6) presents paired t-test results with 98 degrees of freedom. Panel B tabulates shares of effort task allocated to an earlier date $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Column (3) presents paired t-test results with 105 degrees of freedom. Column (6) presents paired t-test results with 86 degrees of freedom.

Table A-6: Static preference reversal

		Marginal effect					
Variables	Odds ratio	Bias = 0	Bias = 1	Bias = 2	Bias = 3	Bias = 4	Bias = 5
Panel A: money allocations							
Low task control	0.436	-0.076	0.028	0.025	0.005	0.006	0.012
	(0.504)	(0.092)	(0.032)	(0.032)	(0.008)	(0.008)	(0.016)
Low social control	0.907*	-0.172*	0.058*	0.058	0.013	0.014	0.029
	(0.496)	(0.103)	(0.034)	(0.035)	(0.012)	(0.012)	(0.023)
High task control	0.173	-0.029	0.011	0.010	0.002	0.002	0.004
	(0.526)	(0.091)	(0.034)	(0.030)	(0.007)	(0.007)	(0.014)
Age	-0.010	0.002	-0.001	-0.001	-0.000	-0.000	-0.000
	(0.032)	(0.005)	(0.002)	(0.002)	(0.004)	(0.004)	(0.001)
Education	-0.481	0.079	-0.030	-0.026	-0.006	-0.006	-0.012
	(0.345)	(0.057)	(0.023)	(0.019)	(0.005)	(0.005)	(0.009)
Student	0.074	-0.012	0.005	0.004	0.001	0.001	0.002
	(0.826)	(0.136)	(0.051)	(0.045)	(0.010)	(0.010)	(0.020)
Female	0.226	-0.037	0.014	0.012	0.003	0.003	0.005
	(0.343)	(0.056)	(0.022)	(0.019)	(0.004)	(0.004)	(0.009)
Employed	0.691**	-0.114**	0.043**	0.038*	0.008	0.008	0.017
	(0.333)	(0.055)	(0.022)	(0.028)	(0.005)	(0.006)	(0.011)
Socioeconomic status	0.035	-0.006	0.002	0.002	0.000	0.000	0.001
	(0.121)	(0.020)	(0.008)	(0.007)	(0.002)	(0.001)	(0.003)
N	212	212	212	212	212	212	212
Pseudo R^2	0.0307						
Panel B: effort allocations							
Low task control	0.978**	-0.240***	0.006	0.015**	0.028**	0.040**	0.151**
	(0.382)	(0.090)	(0.005)	(0.007)	(0.012)	(0.018)	(0.065)
Low social control	1.025**	-0.250**	0.002	0.012**	0.026**	0.041**	0.170*
	(0.449)	(0.104)	(0.006)	(0.006)	(0.011)	(0.019)	(0.088)
High task control	0.326	-0.081	0.003	0.006	0.010	0.014	0.048
	(0.45)	(0.112)	(0.003)	(0.007)	(0.014)	(0.02)	(0.070)
Age	-0.012	0.003	-0.000	-0.000	-0.000	-0.0005	-0.002
	(0.019)	(0.005)	(0.000)	(0.000)	(0.000)	(0.001)	(0.003)
Education	0.269	-0.067	0.003	0.006	0.009	0.012	0.037
	(0.327)	(0.081)	(0.004)	(0.007)	(0.011)	(0.014)	(0.045)
Student	0.334	-0.083	0.004	0.007	0.011	0.014	0.046
	(0.486)	(0.12)	(0.005)	(0.010)	(0.016)	(0.022)	(0.069)
Female	0.331	-0.082	0.004	0.007	0.011	0.014	0.046
	(0.299)	(0.074)	(0.004)	(0.007)	(0.011)	(0.013)	(0.041)
Employed	-0.098	0.024	-0.001	-0.002	-0.003	-0.004	-0.014
	(0.306)	(0.076)	(0.0035)	(0.007)	(0.010)	(0.013)	(0.042)
Socioeconomic status	0.026	-0.006	0.000	0.001	0.001	0.001	0.003
	(0.122)	(0.030)	(0.0014)	(0.003)	(0.004)	(0.005)	(0.017)
N	193	193	193	193	193	193	193
Pseudo R^2	0.0238						

Note: Odds ratios obtained via ordered logistic regressions. Table also reports predicted marginal effects for each level of present bias variable (keeping other variables at their means). Standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. We use number of present bias decisions (ranging from 0 to 5) as a dependent variable. Panel A presents results for money allocation condition, while Panel B shows results for effort allocation condition. Baseline is *high social control* condition.

Table A-7: Parametric estimations by treatment: type of control

	Monetary Discounting		Effort Discounting	
	(1)	(2)	(3)	(4)
	Task	Social	Task	Social
Present bias parameter β	1.041	1.043	0.827	1.004
	(0.020)	(0.020)	(0.074)	(0.094)
Discount factor (weekly) δ	0.980	0.978	1.153	1.211
	(0.010)	(0.010)	(0.047)	(0.076)
Curvature parameter (monetary) α	0.947	0.930		
	(0.009)	(0.013)		
Cost of effort parameter γ			1.327	1.505
			(0.053)	(0.102)
N	1695	1485	1590	1305
Clusters	113	99	106	87
p-values - Tests:				
$H_0 : \beta = 1$	0.036	0.032	0.019	0.967
$H_0 : \delta = 1$	0.055	0.023	0.001	0.005

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-8: Parametric estimations by treatments: level of control x type of control (monetary discounting)

	(1) Low task	(2) Low social	(3) High task	(4) High social
Present bias parameter β	1.016	1.032	1.071	1.051
	(0.022)	(0.040)	(0.034)	(0.018)
Discount factor (weekly) δ	0.982	0.975	0.978	0.980
	(0.012)	(0.015)	(0.017)	(0.013)
Curvature parameter (monetary) α	0.951	0.925	0.941	0.933
	(0.012)	(0.022)	(0.015)	(0.015)
N	885	630	810	855
Clusters	59	42	54	57
p-values - Tests:				
$H_0 : \beta = 1$	($p = 0.460$)	($p = 0.416$)	($p = 0.035$)	($p = 0.005$)
$H_0 : \delta = 1$	($p = 0.154$)	($p = 0.102$)	($p = 0.196$)	($p = 0.106$)

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-9: Parametric estimations by treatments: level of control x type of control (effort discounting)

	(1) Low task	(2) Low social	(3) High task	(4) High social
Present bias parameter β	0.776	0.846	0.916	1.112
	(0.108)	(0.114)	(0.065)	(0.142)
Discount factor (weekly) δ	1.126	1.170	1.204	1.241
	(0.059)	(0.085)	(0.075)	(0.122)
Cost of effort parameter γ	1.340	1.662	1.308	1.402
	(0.075)	(0.156)	(0.068)	(0.122)
N	960	615	630	690
Clusters	64	41	42	46
p-values - Tests:				
$H_0 : \beta = 1$	0.037	0.178	0.200	0.431
$H_0 : \delta = 1$	0.034	0.045	0.196	0.047

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-10: Treatment effects on inconsistent choices

	(1) Logit	(2) LPM	(3) LPM
Effort condition	0.498***	0.120***	0.506
	(0.17)	(0.04)	(0.32)
Low task control	-0.059	-0.014	-0.030
	(0.22)	(0.05)	(0.07)
High task control	-0.068	-0.015	0.013
	(0.24)	(0.06)	(0.08)
Low social control	0.206	0.050	0.019
	(0.25)	(0.06)	(0.09)
Age	-0.037**	-0.008**	-0.011**
	(0.02)	(0.00)	(0.00)
Female	0.439***	0.105***	0.109**
	(0.17)	(0.04)	(0.06)
Education	-0.051	-0.013	0.002
	(0.18)	(0.04)	(0.06)
Student	-0.006	-0.002	0.072
	(0.42)	(0.10)	(0.12)
Employed	0.095	0.021	0.052
	(0.17)	(0.04)	(0.06)
SES	-0.058	-0.014	0.009
	(0.06)	(0.01)	(0.02)
Effort x Low task control			0.032
			(0.11)
Effort x High task control			-0.063
			(0.11)
Effort x Low social control			0.046
			(0.12)
Effort x age			0.006
			(0.01)
Effort x female			-0.014
			(0.08)
Effort x education			-0.034
			(0.09)
Effort x student			-0.203
			(0.20)
Effort x employed			-0.069
			(0.08)
Effort x SES			-0.051*
			(0.03)
Constant	0.568	0.624***	0.462**
	(0.71)	(0.16)	(0.22)
N	622	622	622
LL	-416.988	-437.346	-433.956
R^2		0.039	0.049

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Column (1) present estimates from logistic regression, columns (2) and (3) - from linear probability model. Dependent variable takes value 0 if participant's choices followed law of demand, and 1 if participant has at least 1 violation of monotonicity assumption. Baseline is *high social control* condition.

Table A-11: Static preference reversal (full sample)

		Marginal effect					
Variables	Odds ratio	Bias = 0	Bias = 1	Bias = 2	Bias = 3	Bias = 4	Bias = 5
Panel A: money allocations							
Low task control	0.149	-0.037	0.007	0.010	0.008	0.006	0.005
	(0.297)	(0.073)	(0.013)	(0.020)	(0.017)	(0.012)	(0.011)
Low social control	0.265	-0.066	0.011	0.018	0.015	0.011	0.010
	(0.332)	(0.083)	(0.012)	(0.022)	(0.020)	(0.015)	(0.014)
High task control	0.407	-0.100	0.017	0.027	0.024	0.017	0.015
	(0.302)	(0.075)	(0.011)	(0.021)	(0.019)	(0.014)	(0.013)
Age	-0.029	0.007	-0.001	-0.002	-0.001	-0.001	-0.001
	(0.0234)	(0.006)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Education	-0.137	0.034	-0.007	-0.009	-0.008	-0.005	-0.005
	(0.247)	(0.060)	(0.012)	(0.017)	(0.014)	(0.010)	(0.009)
Student	0.246	-0.060	0.012	0.017	0.014	0.010	0.009
	(0.549)	(0.134)	(0.026)	(0.037)	(0.031)	(0.022)	(0.019)
Female	0.193	-0.047	0.009	0.013	0.011	0.008	0.007
	(0.224)	(0.055)	(0.011)	(0.015)	(0.012)	(0.009)	(0.008)
Employed	0.543**	-0.133**	0.026**	0.037**	0.030**	0.022**	0.018*
	(0.231)	(0.057)	(0.013)	(0.017)	(0.014)	(0.010)	(0.010)
Socioeconomic status	0.102	-0.025	0.005	0.007	0.006	0.004	0.003
	(0.080)	(0.020)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)
N	322	322	322	322	322	322	322
Pseudo R^2	0.0142						
Panel B: effort allocations							
Low task control	0.763***	-0.173***	-0.014*	0.004	0.023***	0.045***	0.115***
	(0.263)	(0.057)	(0.008)	(0.005)	(0.008)	(0.017)	(0.044)
Low social control	0.824**	-0.181***	-0.019	-0.001	0.021***	0.047**	0.133**
	(0.333)	(0.065)	(0.012)	(0.008)	(0.007)	(0.018)	(0.063)
High task control	0.46	-0.105	-0.009	0.002	0.014*	0.028	0.070
	(0.306)	(0.066)	(0.008)	(0.003)	(0.008)	(0.019)	(0.050)
Age	-0.013	0.003	0.000	-0.000	-0.000	-0.001	-0.002
	(0.018)	(0.004)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)
Education	0.2	-0.047	-0.003	0.006	0.007	0.012	0.028
	(0.247)	(0.058)	(0.004)	(0.003)	(0.009)	(0.015)	(0.035)
Student	0.53	-0.126	-0.007	0.007	0.019	0.033	0.074
	(0.354)	(0.083)	(0.006)	(0.005)	(0.013)	(0.023)	(0.051)
Female	0.357	-0.085	-0.005	0.005	0.013	0.022	0.050*
	(0.217)	(0.052)	(0.004)	(0.004)	(0.009)	(0.014)	(0.030)
Employed	-0.008	0.002	0.000	-0.000	-0.000	-0.001	-0.001
	(0.224)	(0.053)	(0.003)	(0.003)	(0.008)	(0.014)	(0.031)
Socioeconomic status	-0.043	0.010	0.001	-0.001	-0.002	-0.003	-0.006
	(0.088)	(0.021)	(0.001)	(0.001)	(0.003)	(0.005)	(0.012)
N	300	300	300	300	300	300	300
Pseudo R^2	0.0158						

Note: Odds ratios obtained via ordered logistic regressions. Table also reports predicted marginal effects for each level of present bias variable (keeping other variables at their means). Standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. We use number of present bias decisions (ranging from 0 to 5) as a dependent variable. Panel A presents results for money allocation condition, while Panel B shows results for effort allocation condition. Baseline is *high social control* condition.

Table A-12: Nonparametric analysis. Aggregate behavior by interest rate (full sample)

	(1) t = 0 Budget share	(2) t ≠ 0 Budget share	(3) t-test (p value)
Panel A: Money allocation			
r = 0%	0.630	0.637	0.63
	(0.021)	(0.022)	(0.529)
r = 11%	0.309	0.333	1.90
	(0.018)	(0.020)	(0.058)
r = 25%	0.253	0.274	1.67
	(0.015)	(0.018)	(0.095)
r = 43%	0.210	0.218	0.76
	(0.014)	(0.016)	(0.447)
r = 100%	0.179	0.181	0.12
	(0.016)	(0.016)	(0.901)
Overall	0.316	0.329	1.68
	(0.012)	(0.013)	(0.093)
Panel B: Effort allocation			
r = 0%	0.505	0.546	3.31
	(0.017)	(0.016)	(0.001)
r = 11%	0.534	0.568	2.74
	(0.016)	(0.016)	(0.006)
r = 25%	0.548	0.577	2.40
	(0.016)	(0.016)	(0.017)
r = 43%	0.567	0.613	3.99
	(0.016)	(0.016)	(0.000)
r = 75%	0.591	0.618	2.19
	(0.016)	(0.017)	(0.029)
Overall	0.549	0.585	3.45
	(0.015)	(0.015)	(0.000)

Note: Panel A tabulates shares of money allocated to an earlier date when it $t = 0$ *(1) and* $t \neq 0$ *(2). Each row presents results calculated from 10* $t = 0$ *and 5* $t \neq 0$ *allocations with standard errors in the parentheses. Column (3) presents paired t-test results with 321 degrees of freedom (p-values in parentheses). Panel B tabulates shares of effort task allocated to an earlier date. Column (3) presents paired t-test results with 299 degrees of freedom (p-values in parentheses).*

Table A-13: Nonparametric analysis. Aggregate behavior by interest rate and level of control (full sample)

	(1) t = 0 Budget share Low control	(2) t ≠ 0 Budget share Low control	(3) t-test (p value) Low control	(4) t = 0 Budget share High control	(5) t ≠ 0 Budget share High control	(6) t-test (p value) High control
Panel A: Money allocation						
r = 0%	0.644	0.648	0.22	0.619	0.628	0.80
	(0.031)	(0.032)	(0.827)	(0.030)	(0.030)	(0.423)
r = 11%	0.308	0.329	1.00	0.310	0.337	1.74
	(0.026)	(0.030)	(0.315)	(0.024)	(0.028)	(0.083)
r = 25%	0.256	0.278	1.11	0.251	0.270	1.28
	(0.023)	(0.027)	(0.271)	(0.021)	(0.025)	(0.202)
r = 43%	0.211	0.241	1.83	0.209	0.198	0.89
	(0.021)	(0.026)	(0.069)	(0.020)	(0.021)	(0.376)
r = 100%	0.178	0.186	0.55	0180	0.175	0.32
	(0.023)	(0.024)	(0.585)	(0.021)	(0.022)	(0.747)
Overall	0.319	0.336	1.38	0.314	0.322	0.97
	(0.017)	(0.019)	(0.170)	(0.016)	(0.017)	(0.334)
Panel B: Effort allocation						
r = 0%	0.471	0.550	4.311	0.545	0.543	0.15
	(0.021)	(0.020)	(0.000)	(0.026)	(0.027)	(0.881)
r = 11%	0.495	0.576	4.79	0.580	0.559	1.20
	(0.020)	(0.020)	(0.000)	(0.026)	(0.025)	(0.233)
r = 25%	0.516	0.576	3.35	0.586	0.578	0.54
	(0.021)	(0.021)	(0.001)	(0.025)	(0.025)	(0.588)
r = 43%	0.534	0.623	5.46	0.606	0.601	0.32
	(0.021)	(0.020)	(0.000)	(0.025)	(0.026)	(0.747)
r = 75%	0.562	0.617	2.99	0.626	0.618	0.54
	(0.021)	(0.021)	(0.003)	(0.026)	(0.027)	(0.590)
Overall	0.516	0.588	4.91	0.589	0.580	0.67
	(0.019)	(0.019)	(0.000)	(0.024)	(0.025)	(0.501)

Note: Columns (1) - (3) present results for low control treatment groups. Columns (4) - (6) present results for high control treatment groups. Panel A tabulates share of money allocated to an earlier date when $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Each row presents results calculated from 10 $t = 0$ and 5 $t \neq 0$ allocations. Column (3) presents paired t-test results with 150 degrees of freedom. Column (6) presents paired t-test results with 171 degrees of freedom. Panel B tabulates share of effort tasks allocated to an earlier date $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Column (3) presents paired t-test results with 164 degrees of freedom. Column (6) presents paired t-test results with 135 degrees of freedom.

Table A-14: Nonparametric analysis. Aggregate behavior by interest rate and type of control (full sample)

	(1) $t = 0$ Budget share Task control	(2) $t \neq 0$ Budget share Task control	(3) t-test (p value) Task control	(4) $t = 0$ Budget share Social control	(5) $t \neq 0$ Budget share Social control	(6) t-test (p value) Social control
Panel A: Money allocation						
r = 0%	0.617	0.626	0.66	0.647	0.652	0.25
	(0.029)	(0.030)	(0.512)	(0.031)	(0.032)	(0.797)
r = 11%	0.295	0.325	2.02	0.327	0.343	0.76
	(0.024)	(0.027)	(0.045)	(0.027)	(0.031)	(0.450)
r = 25%	0.247	0.288	2.62	0.261	0.256	0.27
	(0.020)	(0.025)	(0.010)	(0.024)	(0.027)	(0.784)
r = 43%	0.222	0.242	1.45	0.195	0.188	0.43
	(0.019)	(0.023)	(0.150)	(0.021)	(0.023)	(0.669)
r = 100%	0.183	0.180	0.18	0.175	0.181	0.45
	(0.021)	(0.022)	(0.858)	(0.024)	(0.024)	(0.657)
Overall	0.313	0.332	2.12	0.321	0.324	0.26
	(0.016)	(0.018)	(0.035)	(0.018)	(0.019)	(0.792)
Panel B: Effort allocation						
r = 0%	0.502	0.568	3.79	0.508	0.521	0.71
	(0.022)	(0.021)	(0.000)	(0.025)	(0.026)	(0.480)
r = 11%	0.529	0.587	3.50	0.539	0.546	0.35
	(0.022)	(0.021)	(0.001)	(0.024)	(0.025)	(0.728)
r = 25%	0.551	0.601	2.90	0.544	0.548	0.23
	(0.022)	(0.021)	(0.004)	(0.024)	(0.025)	(0.814)
r = 43%	0.567	0.646	5.22	0.567	0.574	0.39
	(0.022)	(0.021)	(0.000)	(0.024)	(0.025)	(0.696)
r = 75%	0.600	0.644	2.51	0.580	0.586	0.36
	(0.023)	(0.022)	(0.013)	(0.024)	(0.026)	(0.719)
Overall	0.550	0.609	4.33	0.548	0.555	0.47
	(0.021)	(0.019)	(0.000)	(0.023)	(0.024)	(0.641)

Note: Columns (1) - (3) present results for task control treatment groups. Columns (4) - (6) present results for social control treatment groups. Panel A tabulates shares of money allocated to an earlier date when $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Each row presents results calculated from 10 $t = 0$ and 5 $t \neq 0$ allocations. Column (3) presents paired t-test results with 176 degrees of freedom. Column (6) presents paired t-test results with 144 degrees of freedom. Panel B tabulates shares of effort task allocated to an earlier date $t = 0$ ((1) and (4)) and $t \neq 0$ ((2) and (5)). Column (3) presents paired t-test results with 164 degrees of freedom. Column (6) presents paired t-test results with 134 degrees of freedom.

Table A-15: Parameter estimates (full sample)

	(1) Monetary Discounting	(2) Effort Discounting
Present bias parameter β	1.051	0.834
	(0.016)	(0.071)
Discount factor (weekly) δ	0.986	1.172
	(0.008)	(0.048)
Curvature parameter (monetary) α	0.846	
	(0.014)	
Cost of effort parameter γ		1.721
		(0.111)
Observations	4830	4500
Clusters	322	300
$H_0 : \beta = 1$	0.001	0.019
$H_0 : \delta = 1$	0.096	0.000

Notes: Parameters identified from two-limit Tobit regressions (3.1) and (4.1). SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-16: Parametric estimations by treatment: level of control (full sample)

	Monetary Discounting		Effort Discounting	
	(1)	(2)	(3)	(4)
	Low	High	Low	High
Present bias parameter β	1.051	1.051	0.722	0.989
	(0.026)	(0.018)	(0.101)	(0.095)
Discount factor (weekly) δ	0.984	0.988	1.126	1.235
	(0.012)	(0.012)	(0.057)	(0.082)
Curvature parameter (monetary) α	0.852	0.841		
	(0.021)	(0.020)		
Cost of effort parameter γ			1.745	1.690
			(0.158)	(0.154)
N	2265	2565	2475	2025
Clusters	151	171	165	135
p-values - Tests:				
$H_0 : \beta = 1$	0.050	0.005	0.006	0.906
$H_0 : \delta = 1$	0.174	0.317	0.027	0.004

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-17: Parametric estimations by treatment: task vs social type of control (full sample)

	Monetary Discounting		Effort Discounting	
	(1)	(2)	(3)	(4)
	Task	Social	Task	Social
Present bias parameter β	1.067	1.035	0.788	0.934
	(0.023)	(0.022)	(0.085)	(0.137)
Discount factor (weekly) δ	0.989	0.983	1.149	1.218
	(0.012)	(0.011)	(0.053)	(0.098)
Curvature parameter (monetary) α	0.835	0.858		
	(0.021)	(0.019)		
Cost of effort parameter γ			1.592	1.979
			(0.112)	(0.258)
N	2655	2175	2475	2025
Clusters	177	145	165	135
$H_0 : \beta = 1$	0.004	0.103	0.013	0.627
$H_0 : \delta = 1$	0.353	0.140	0.005	0.026

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-18: Parametric estimations by treatments: level of control x type of control (monetary discounting, full sample)

	(1) Low task	(2) Low social	(3) High task	(4) High social
Present bias parameter β	1.070	1.029	1.063	1.041
	(0.034)	(0.042)	(0.032)	(0.020)
Discount factor (weekly) δ	0.988	0.979	0.989	0.987
	(0.017)	(0.017)	(0.019)	(0.015)
Curvature parameter (monetary) α	0.839	0.868	0.831	0.849
	(0.031)	(0.028)	(0.029)	(0.027)
N	1365	900	1290	1275
Clusters	91	60	86	85
p-values - Tests:				
$H_0 : \beta = 1$	0.043	0.493	0.048	0.042
$H_0 : \delta = 1$	0.478	0.214	0.557	0.398

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-19: Parametric estimations by treatments: level of control x type of control (effort discounting, full sample)

	(1) Low task	(2) Low social	(3) High task	(4) High social
Present bias parameter β	0.724	0.719	0.895	1.136
	(0.129)	(0.146)	(0.069)	(0.246)
Discount factor (weekly) δ	1.131	1.110	1.182	1.313
	(0.070)	(0.100)	(0.074)	(0.192)
Cost of effort parameter γ	1.611	2.101	1.562	1.870
	(0.166)	(0.309)	(0.133)	(0.365)
N	1515	960	960	1065
Clusters	101	64	64	71
p-values - Tests:				
$H_0 : \beta = 1$	0.032	0.055	0.128	0.580
$H_0 : \delta = 1$	0.062	0.027	0.104	0.047

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. Chi-squared tests in the last two rows.

Table A-20: Regression analysis (full sample)

	Monetary Discounting			Effort Discounting		
	(1)	(2)	(3)	(4)	(5)	(6)
	Now vs 2 weeks	Now vs 4 weeks	2 weeks vs 4 weeks	Now vs 2 weeks	Now vs 4 weeks	2 weeks vs 4 weeks
Low task control	-0.003	-0.001	0.024	-0.040	-0.070*	0.061
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Low social control	0.003	0.012	-0.004	-0.089**	-0.119**	-0.008
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
High task control	-0.014	-0.009	-0.005	-0.011	-0.035	0.041
	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Interest rate	-0.316***	-0.329***	-0.337***	0.098***	0.118***	0.096***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Age	-0.004*	-0.006***	-0.003	0.006***	0.006***	0.006***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Education	0.013	0.033	0.053*	0.011	-0.007	0.018
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Student	-0.077	-0.076	-0.040	0.064	0.086	0.160***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Female	-0.025	-0.005	-0.008	-0.041	-0.039	-0.023
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Employed	-0.002	0.032	0.005	-0.052	-0.051	-0.026
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
SES	0.001	-0.002	-0.005	0.014	0.012	0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.907***	0.979***	0.892***	0.214**	0.195*	0.090
	(0.10)	(0.11)	(0.12)	(0.11)	(0.11)	(0.11)
N	1610	1610	1610	1500	1500	1500
Adj.R^2	0.103	0.107	0.107	0.052	0.063	0.050
Cluster	322	322	322	300	300	300

Note: Table reports coefficients from OLS regression. Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Covariates include *age, gender, education, eployment, student status, socioeconomic status.* Dependent variable is a *share of budget* allocated to an earlier date. Columns (1) - (3) present OLS regression results for money allocation condition, while columns (4) - (6) show OLS regression results for effort allocation condition. Regressions are run separately for each of the three decision time frames, indicated in the names of the columns. We use *high social control* condition as a baseline.

Table A-21: Differences across treatments: PANAS-X scale

	Diff. (Low vs high)		Diff. (Task vs social)		Diff. (Low task vs social)		Diff. (High task vs social)	
Positive emotions	-0.217***	(-20.00)	-0.0402**	(-2.90)	-0.0231*	(-2.46)	-0.00642	(-0.32)
Negative emotions	0.178***	(16.66)	0.0126	(0.97)	-0.00153	(-0.08)	-0.0158	(-1.31)
Joviality	-0.173***	(-12.56)	-0.0572***	(-3.73)	-0.0110	(-1.32)	-0.0630*	(-2.38)
Self-assurance	-0.247***	(-17.46)	-0.0335	(-1.94)	-0.0231*	(-1.99)	0.0142	(0.54)
Attentiveness	-0.260***	(-11.85)	-0.0164	(-0.67)	-0.0473	(-1.80)	0.0758*	(2.12)
Sadness	0.227***	(13.40)	0.0169	(0.87)	0.00608	(0.20)	-0.0262	(-1.60)
Fear	0.221***	(11.19)	0.0892***	(4.15)	0.127***	(3.85)	0.00128	(0.06)
Guilt	0.110***	(6.61)	-0.00485	(-0.28)	-0.00832	(-0.28)	-0.0279	(-1.73)
Hostility	0.163***	(11.60)	-0.0503**	(-3.26)	-0.130***	(-5.48)	-0.0121	(-0.88)
N	622		622		316		306	

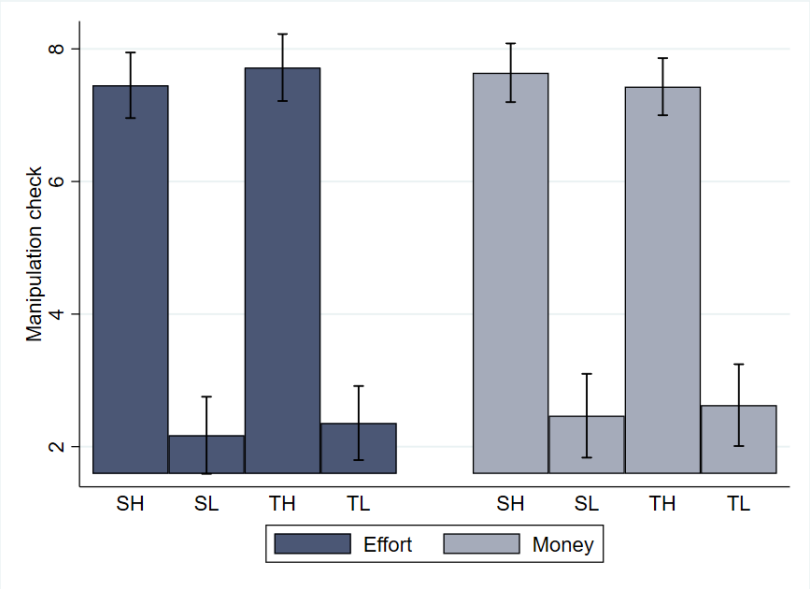
Note: T-test results with t statistics in parentheses. Significance level: * ($p<0.05$), ** ($p<0.01$), *** ($p<0.001$). Each item can take value between 0 (no emotional states selected under the category) to 1 (all emotional states selected under the category). Differences between mean values across treatments are presented in percentage points.

Table A-22: Average Causal Mediation Effects for selected mediators (emotional states)

	(1)	(2)
	Money discounting	Effort discounting
Mediator: Sadness		
Mediation effect	0.00163	0.0314***
Direct effect	-0.01026	0.0143
Total effect	-0.00863	0.04572***
Mediator: Guilt		
Mediation effect	-0.00291	0.00384
Direct effect	-0.00572	0.04189***
Total effect	-0.00863	0.04572***
Mediator: Fear		
Mediation effect	-0.00834**	0.00471
Direct effect	-0.00029	0.04101***
Total effect	-0.00863	0.04572***
Mediator: Hostility		
Mediation effect	-0.00229	0.00615
Direct effect	-0.00633	0.03958***
Total effect	-0.00863	0.04572***
Mediator: Joviality		
Mediation effect	-0.01476***	0.0165***
Direct effect	0.00614	0.0293**
Total effect	-0.00863	0.04572***
Mediator: Self-assurance		
Mediation effect	0.00071	-0.0284***
Direct effect	-0.00934	0.0741***
Total effect	-0.00863	0.04572***
Mediator: Attentiveness		
Mediation effect	0.00135	-0.00427
Direct effect	-0.00997	0.05000***
Total effect	-0.00863	0.04572***

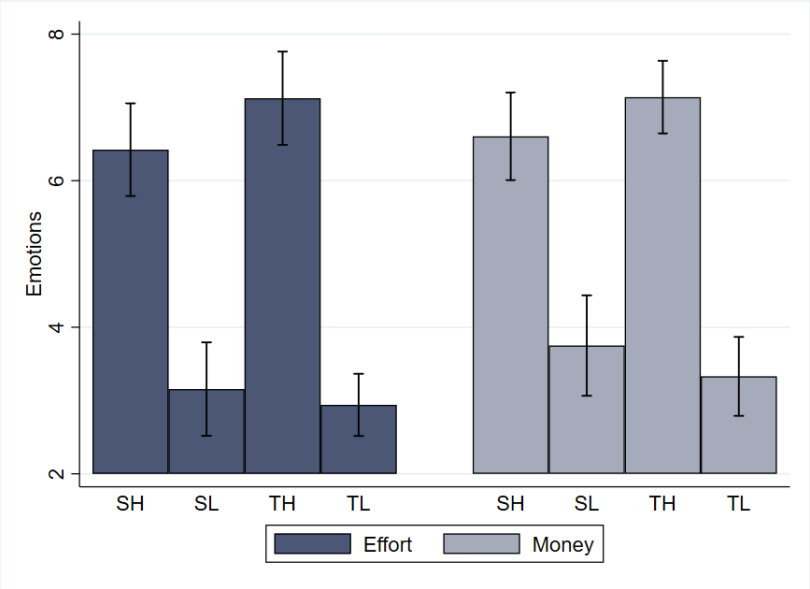
Note: The outcome variable is a share of intertemporal budget allocations to an earlier date; predictor is level of control (low vs high). Table presents mediation effect estimates (bootstrapped from 100 repetitions) for money discounting in column (1) and effort discounting in column (2). *, **, *** denote significance at the 10%, 5% and 1% levels respectively.

Figure A-1: Manipulation check: treatment effect on perceived control in a recalled situation



Notes: Dependant variable measures the feeling of (not) being in control in the situation described in the recall task. Scale goes from 0 "Not being in control" to 10 "Being in control". Exact version of wording in questions and scales by treatment can be found in Appendix 3.A. Bars represent mean for the treatment group, brackets indicate standard errors. Results are divided by the type of intertemporal task (money vs effort) and control conditions (*SH* stands for *Social High*, *SL - Social Low*, *TH - Task High*, *Task Low*).

Figure A-2: Manipulation check: treatment effect on emotional response after recalling a situation



Notes: Dependant variable measures the feelings after recalling a particular situation. Scale goes from 0 "Mainly negative feelings" to 10 "Mainly positive feelings". Bars represent mean for the treatment group, brackets indicate standard errors. Results are divided by the type of intertemporal task (money vs effort) and control conditions (*SH* stands for *Social High*, *SL - Social Low*, *TH - Task High*, *Task Low*).

Figure A-3: Mean allocation of money (low social control vs high social control)

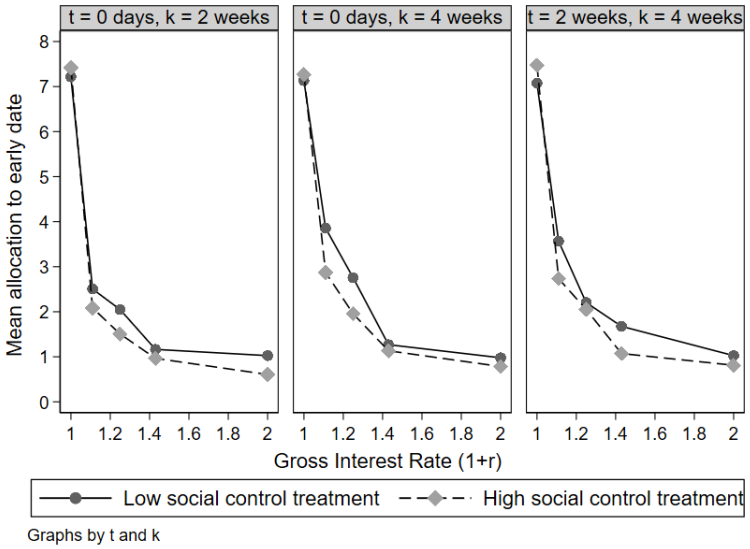


Figure A-4: Mean allocation of money (low task control vs high task control)

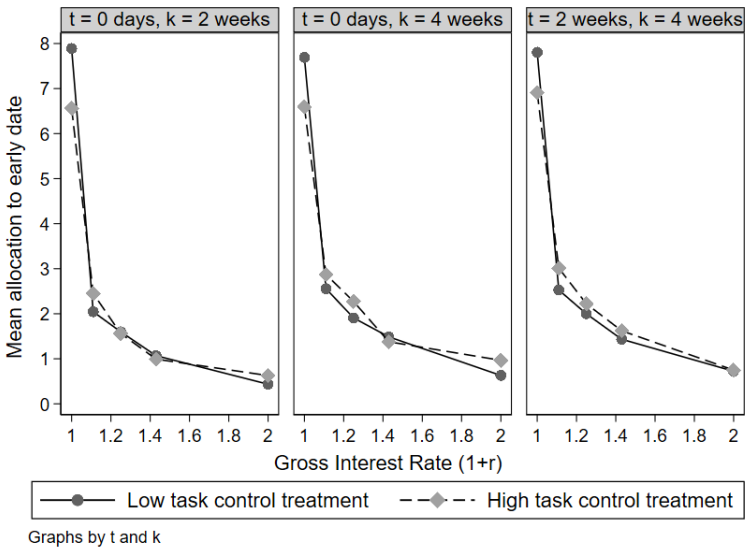


Figure A-5: Mean allocation of money (low task control vs low social control)

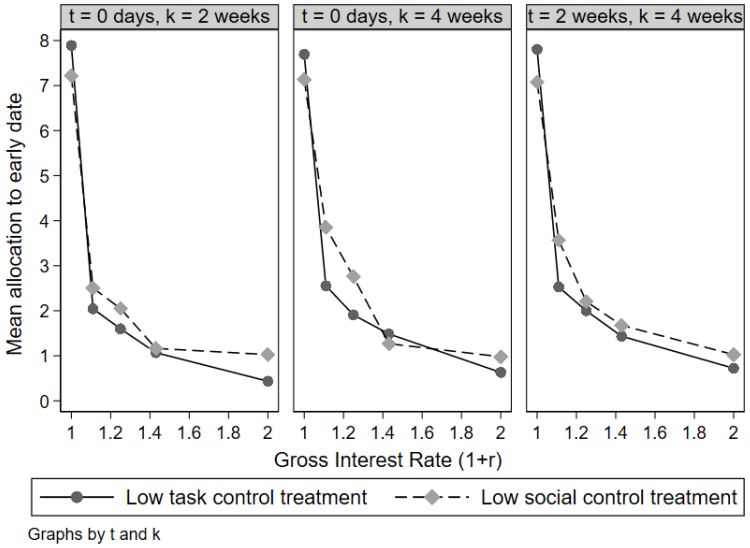


Figure A-6: Mean allocation of money (high task control vs high social control)

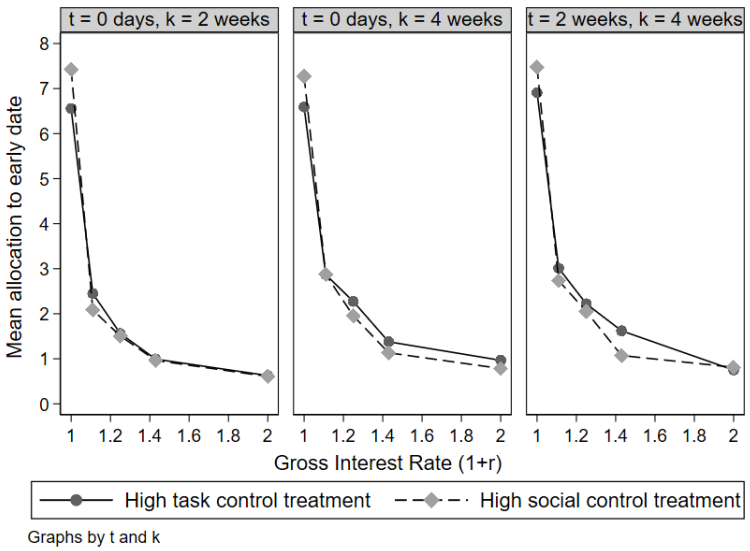


Figure A-7: Mean allocation of effort (low social control vs high social control)

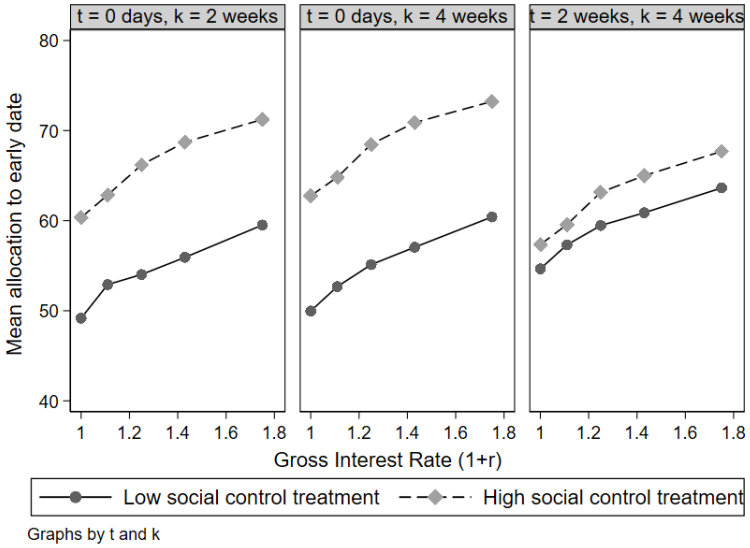


Figure A-8: Mean allocation of effort (low task control vs high task control)

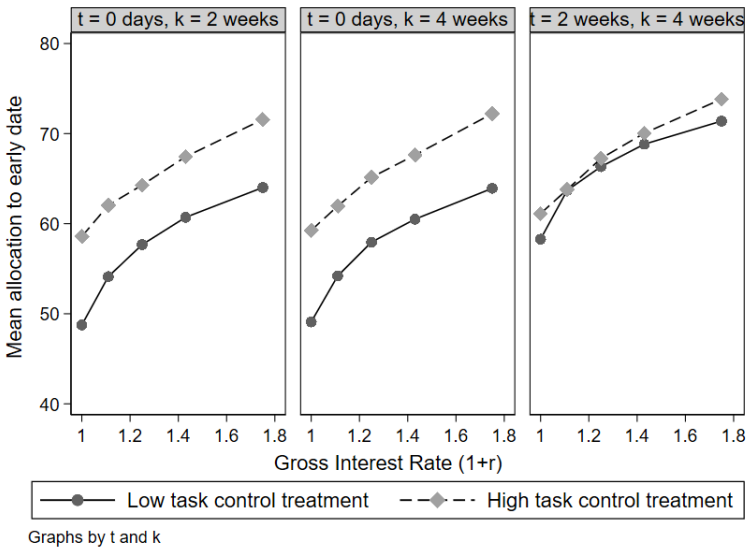


Figure A-9: Mean allocation of effort (low task control vs low social control)

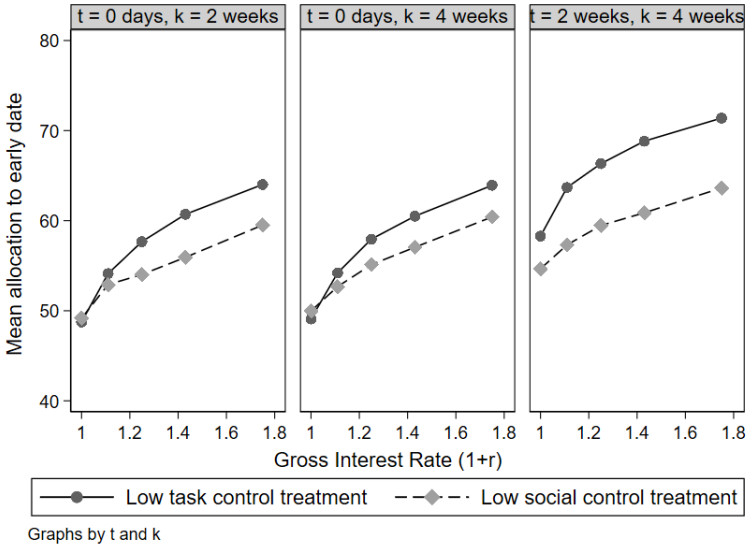
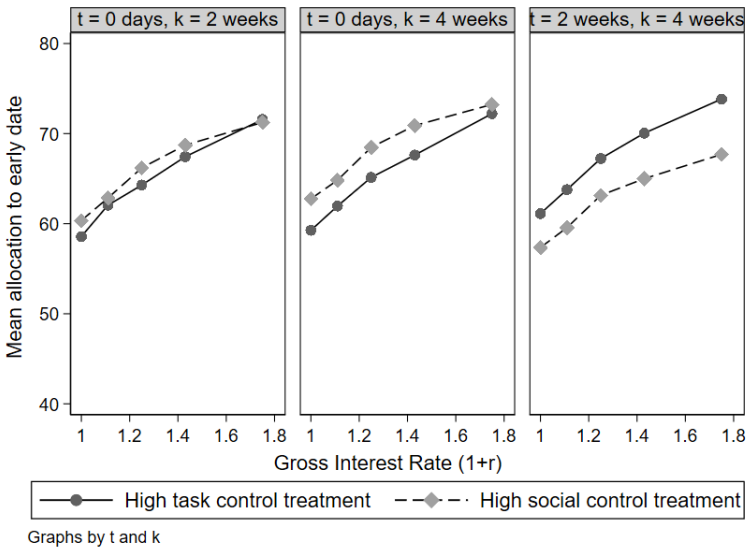


Figure A-10: Mean allocation of effort (high task control vs high social control)



3.B Appendix B

3.B.1 Experimental task

Figure A-1: Intertemporal choice task

3.B.2 Recall task

High social control condition. From Galinsky et al. (2003).

Please recall a particular incident in which you had power over another individual or individuals. By power, we mean a situation in which you controlled the ability of another person or persons to get something they wanted or were in a position to evaluate those individuals. Please describe how you felt, etc.

Low social control condition. From Galinsky et al. (2003).

Please recall a particular incident in which an individual or individuals had power over you. By power,we mean a situation in which another person or persons controlled your ability to get something you wanted or were in a position to evaluate you. Please describe this situation in which you did not have power — what happened, how you felt, etc.

High task control condition. From Whitson and Galinsky (2008)

Please recall a particular incident in which something happened and you were in complete control of the situation. Please describe the situation in which you felt in complete control– what happened, how you felt, etc.

Low control condition. From Whitson and Galinsky (2008)

Please recall a particular incident in which something happened and you did not have any control over the situation. Please describe the situation in which you felt a complete lack of control – what happened, how you felt, etc.

3.B.3 Secondary measures

Measure of emotions. PANAS-X scale (Watson and Clark, 1999)

Please think back at the incident you just described and describe how you felt during this incident (mark all that apply):

Basic Negative Emotion Scales

- **Fear** (6) afraid, scared, frightened, nervous, jittery, shaky
- **Hostility** (6) angry, hostile, irritable, scornful, disgusted, loathing
- **Guilt** (6) guilty, ashamed, blameworthy, angry at self, disgusted with self, dissatisfied with self
- **Sadness** (5) sad, blue, downhearted, alone, lonely

Basic Positive Emotion Scales

- **Joviality** (8) happy, joyful, delighted, cheerful, excited, enthusiastic, lively, energetic
- **Self-Assurance** (6) proud, strong, confident, bold, daring, fearless
- **Attentiveness** (4) alert, attentive, concentrating, determined

Manipulation check on sense of control

High social control condition In the event that you described on the previous page, to what extent did you feel that yourself other than someone else had the power to influence what was happening? *Scale from 0 = Someone else had the power to 10 = I had the power*

Low social control condition In the event that you described on the previous page, to what extent did you feel that someone else other than yourself had the power to influence what was happening? *Scale from 0 = Someone else had the power to 10 = I had the power*

High task control condition In the event that you described on the previous page, to what extent was the event under your control? *Scale from 0 = The events were beyond my control to 10 = The events were under my control*

Low task control condition In the event that you described on the previous page, to what extent was the event beyond your control? *Scale from 0 = The events were beyond my control to 10 =The events were under my control*

Adapted from Lerner and Keltner (2001).

Manipulation check on general feelings during the study

Think back at when we asked you to recall an incident, at the beginning of this study. We are interested in knowing how you felt after you finished recalling the incident. We are not interested in knowing how the incident made you feel, but how the mere act of remembering it made you feel. Would you say that the recall task made you feel:

Scale from 0 = Mainly negative feelings to 10 = Mainly positive feelings

Chapter 4

Socioeconomic Status and Intertemporal Preferences over Effort

Empirical evidence recognizes a relationship between low socioeconomic status (SES) and impatient behavior. Proposed psychological channels include high cognitive load, stress, self-control issues, emotional affects, among others. In this study, I test whether the mere salience of one's subjective SES has an impact on intertemporal preferences regarding effort. In an online experiment, participants are randomly assigned to either of two conditions: treatment which consists of questions related to one's SES and control, or a control group that has to answer questions unrelated to SES. Afterwards, participants are asked to make choices regarding the intertemporal allocation of real effort tasks. Results show that SES priming affects only low-status participants: in a control group, participants were making relatively equivalent effort allocation decisions regardless of their status, while in the treatment group low SES individuals were significantly more willing to postpone effort to the future. This result remains relatively robust when controlling for decision time frames or interest rates. Such a finding suggests that a pure act of having a subjective socioeconomic status salient in your mind can affect the distribution of effort across time for individuals with low SES.

Keywords: socioeconomic status, intertemporal choice, effort discounting

4.1 Introduction

Low socioeconomic status (SES) has been shown to have a considerable impact on a plethora of life domains, starting from childhood development (Guo and Harris, 2000; Evans and Rosenbaum, 2008) and education achievements (Silver et al., 2005; Ready, 2010) to cognitive skills in the adulthood (Singh-Manoux et al., 2005; Turrell et al., 2002) as well as health behaviours (Pampel et al., 2010), consumption choices (Cole et al., 2008; Allcott et al., 2019b; Dubois et al., 2015) or financial decision making (Haisley et al., 2008b; Rhine et al., 2006; Carvalho et al., 2016a), to name the few. Many of such decisions have intertemporal preferences at their base, however, as a large body of research has documented, low SES impacts various psychological channels - cognitive load, self-control, and working memory, among others - which, in turn, affect intertemporal choices (see Sheehy-Skeffington (2018) and Adamkovič and Martončik (2017) for review).

This study seeks to explore how the salience of subjective socioeconomic status affects intertemporal choices regarding effort. Previous research has analysed the impact of subjective SES in various domains: salient low SES has been shown to increase appetite (Cheon and Hong, 2017b) and affect the perception of energy density in foods (Cheon et al., 2018), as well as a degree of aggression (Greitemeyer and Sagioglou, 2016), selfishness (Dubois et al., 2015), and charitable giving (Piff et al., 2010). A work most closely related to this study is a field experiment by Bartoš et al. (2018) in which authors manipulated the financial concerns of low-income individuals and asked them to state their intertemporal preferences over leisure. Salient poverty-related thoughts have increased preferences for early entertainment and delayed work. Authors explain this result by *myopic-misery* theory - a choice of leisure is a way to counteract disutility that comes from increased financial worries. The current study follows in these footsteps by analyzing intertemporal preferences in the non-monetary domain. However, unlike in Bartoš et al. (2018), participants are not required to think about stressful scenarios involving financial losses but instead are asked just to provide information about their socioeconomic status. There is a slight difference distinguishing the two methods: following results from Bartoš et al. (2018) study, when financial worries preoccupy thoughts of the low-income individuals, they procrastinate more, while the goal of this work is to see whether the fact of remembering your social standing - and not a negative situation *per se* - affects your preferences regarding work timing.

In a pre-registered[1] online experiment 316 participants were first randomly assigned to treatment (socioeconomic status priming) or control group and then asked to state their decisions regarding work in an intertemporal choice task. Participants in the treatment group were asked to answer questions regarding their economic situation and then mark their position on the MacArthur scale of subjective socioeconomic status (Adler et al., 2000). On the contrary, the control group was given neutral demographic questions and asked to mark their position on a scale that was visually equivalent to MacArthur's scale but concerning the importance of breakfast. Afterwards, both experimental groups were asked to make intertemporal choices in a convex time budget environment (Andreoni and Sprenger, 2012; Augenblick et al., 2015). They were given an experimental "budget" of 150 real effort tasks and asked to split it between three pairs of

[1]The study was preregistered on the Open Science Framework (OSF). Preregistration link can be found at osf.io/f4xag

sooner vs *later* dates, where allocations to a later date were subject to an interest rate. Under each pair of dates (which were *now and 2 weeks*, *now and 4 weeks*, and *2 weeks and 4 weeks*), participants had to make 5 allocation choices, each corresponding to an increasing interest rate r. This parameter increased the number of effort tasks to be done on a *later* date within a decision time frame. Overall, participants had to make 15 such decisions. At the end of the study, I also measured general mood and procrastination tendencies. I hypothesize that high SES participants will not be affected by the SES priming, while the opposite will be true for low SES individuals. I also expect to see differences in allocation decisions between participants whose SES worsened or is expected to become worse and participants who had (will have) relatively stable or improving SES.

The main finding of this study is that the salient socioeconomic status makes low SES participants less future-focused. In general, the average share of experimental effort budget allocated to an earlier date within each decision time frame does not differ for control and treatment groups: across all time frames and interest rates participants chose to do approximately 115 out of 150 real effort tasks (77%) on an earlier date which was either the day of the experiment ($t = 0$) or two weeks after the experiment ($t \neq 0$). When results were divided by SES, low SES participants in the control group were slightly less likely to *procrastinate*: on average, they allocated 5.7 tasks (3.8%) more to an earlier date as compared with high SES participants, however, the differences were small and statistically insignificant. In the treatment group, low SES individuals consistently chose to postpone the effort to the later date by approximately 11 effort tasks (7.3%). This result remains generally robust when allocation decisions are divided by interest rate and time frame (although differences in allocation shares decrease when $t \neq 0$). In addition to this, low SES participants in the treatment group also made more static preference reversals - a switch from present biased allocation choices when $t = 0$ to more future-oriented decisions when $t \neq 0$ - than a control group. Present bias is confirmed also by parametric estimations of β parameter: for both SES levels in the control group and high SES individuals in the treatment β is close to 1, while for low SES group in treatment $\beta = 0.937$. Standard (weekly) discount factor δ indicates that low SES group discounts effort tasks less than high SES individuals in the treatment group ($\delta(low) = 1.250$ and $\delta(high) = 1.354$). Interestingly, δ parameter shows reversed preferences in the control group: here low SES participants prefer to complete less tasks in the future when delay length k increases ($\delta(low) = 1.393$ and $\delta(high) = 1.226$).

Participants in the treatment group were asked questions about the change of their SES both in the past (a year ago) and in the future (a year from now). In both cases the majority of answers indicate a stable status; only for 11.11% of participants SES has become worse since a year ago, while 7.60% of participants expect their SES to decrease in a year. Overall, I do not find significant evidence that change in SES, in particular, deteriorating conditions, have a meaningful impact on current effort allocation decisions. Regression analysis shows that interest rate, treatment status, and being in the low SES group increase budget allocation shares to a sooner date, while interaction between low SES status and treatment has an opposite - decreasing - effect. However, this applied to interior allocation decisions; a separate analysis of a probability that the dependant variable is 1 showed that neither treatment nor SES or their interaction is significant in explaining the results. The only independent variables having an effect are interest

rate and gender.

Although these results give a good indication of the effect of salient SES on intertemporal decisions regarding effort, the results should be interpreted with caution. One major drawback of this study is a high degree of inconsistent choices. Following the law of demand principle, participants should increase their allocations to an earlier date t as interest rate r grows and $t - k$ decision time frame remains the same. However, 33.79% of participants in the control group and 34.50% in the treatment group have one or more decisions that do not adhere to the law of demand. I replicate the main analysis without inconsistent choices: although reduced sample results in lower significance levels, coefficients generally follow the same trends as in the full sample. It should also be noted that although there are differences in intertemporal choices between low SES individuals in control and treatment groups, they are relatively minor and the overall preference for earlier work is high (77% of effort budget is allocated to earlier work date). Overall, participants of this experiment do not show a high degree of present-focused behavior.

This work joins the discussion of the low socioeconomic status effects on intertemporal preferences (Haushofer and Fehr, 2014; Carvalho et al., 2016a; Bartoš et al., 2018; Brown et al., 2015). Moreover, it also contributes to a growing body of research regarding the effect of SES priming on individual behavior (Greitemeyer and Sagioglou, 2016; Cheon and Hong, 2017b; Cheon et al., 2018). Results of this study suggest that under situational contexts that highlight one's relative (subjective) social standing, lower socioeconomic status individuals might internalize these cues and adjust their intertemporal choices by becoming more present-focused and postpone effort even if this increases the amount of work to be done in the future.

The paper is organized as follows: Section 4.2 reviews literature on socioeconomic status and intertemporal preferences, Section 4.3 discusses sampling strategy and study design, Section 4.4 present results and Section 4.5 provides conclusions.

4.2 Socioeconomic status and intertemporal choice

The last decade saw an increase in the works analyzing the intersection between low socioeconomic status and intertemporal preferences. It is a well-documented finding that financially deprived individuals tend to discount the future more and have a higher degree of present bias (Haushofer and Fehr, 2014). On the one hand, higher future discounting can be caused by liquidity constraints: a scarcity of monetary resources pushes financially deprived individuals to choose *smaller-sooner* monetary rewards in the experimental context to reestablish financial stability and smooth consumption throughout the time (Carvalho et al., 2016a; Brown et al., 2015). However, intertemporal decision-making can be influenced by other factors as well. As highlighted by Adamkovič and Martončik (2017), financial deprivation has an impact on four major areas: cognitive load, executive functions (such as attention, working memory, self-control), intuition or deliberation, and economic decision-making (such as time and risk preferences). Moreover, these areas can also influence each other on varying levels, meaning that three initial factors can have an impact on intertemporal preferences.

Cognitive load is one of the more widely discussed channels through which financial deprivation affects behavior (Sheehy-Skeffington, 2020). Individuals who experience deprivation tend to focus their attention on that which is scarce and thus overlook other issues, usually unrelated to

monetary concerns (Shah et al., 2012; Mullainathan and Shafir, 2013; Mani et al., 2013). Yaple and Yu (2020) suggest that continuous concerns about scarcity overstimulate brain regions related to rewards and decrease the activity of the executive region. This implies that individuals facing an intertemporal choice between *smaller-sooner* and *larger-later* rewards would lack inhibition to wait and would instead behave more impatiently by choosing *smaller-sooner* option. In a study by Carvalho et al. (2016a) low-income households in the US were administered a survey measuring cognitive functioning as well as risk and time preferences pre- and post-payday; authors find that on pre-payday survey participants behaved in a more present-bias manner, but only in the domain of the money and not effort, suggesting that liquidity constraints were a more likely explanation for myopic choices in the monetary intertemporal task. Other works, such as the seminal study by Mani et al. (2013), find that inducing scarcity related thoughts has a significant impact on cognitive performance for financially deprived individuals, which, consecutively, can affect choices in time. For example, Bartoš et al. (2018) manipulated financial concerns of low-income individuals and asked them to make intertemporal choices regarding the trade-off between work and leisure; they find that salient poverty-related thoughts increased preferences to consume leisure and postpone work. They explain this result with the *myopic-misery* hypothesis: to compensate for the disutility arising from the negative effect caused by the manipulation, the individual would choose to postpone work and engage in leisure behavior early. The negative effect can enter utility function also via a channel of emotions. In general, mood tends to have a significant influence on impulsiveness (Herman et al., 2018). Positive affect has been found to increase patience (Ifcher and Zarghamee, 2011), while the opposite applies to negative emotions: in particular, sadness was found to increase present bias though not an overall degree of impatience (Lerner et al., 2013).

Continuous management of limited resources can also deplete an already limited pool of self-control and thus result in self-regulation failures that can be detrimental for future planning (Vohs, 2013). Hoel et al. (2016) find heterogeneous effects in self-control regarding socioeconomic status: when experiencing self-control fatigue, induced via a Stroop task, lower-income participants were behaving less patiently in an intertemporal choice task, while differences in patience levels were insignificant for people with higher income. The models of self-control would suggest that decreasing the taxing nature of financial scarcity could also ease self-regulation demands, thus allowing an individual to engage in more future-oriented behaviors; however, Bernheim et al. (2015) show that there exists a minimum asset level below which it is even impossible to exert sufficient degree of self-control that would allow escaping the *poverty trap*.

Impulse control is also linked with working memory capacity (Nichols and Wilson, 2015). Working memory has been found to have an inverse relationship with the temporal discounting as higher memory load tends to decrease future focus Aranovich et al. (2016); Basile and Toplak (2015). However, working memory is also highly susceptible to the impact of stress (Luethi et al., 2009). In general, living in deprivation is associated with increased stress levels among all age groups (Santiago et al., 2011; Haushofer and Fehr, 2014). However, studies analyzing the impact of stress on intertemporal preferences present mixed evidence. For example, Haushofer et al. (2013); Robinson et al. (2015) find no effect of (lab-induced) stress on impatience or present bias. In a more recent study, Haushofer et al. (2021) argue that stress increases not the present bias or discounting, but rather a propensity to simply choose *sooner* options (in the domain of monetary

gains, monetary losses, or effort provision). Riis-Vestergaard et al. (2018) find that *acute* stress (induced by hydrocortisone injections) increase preference for *smaller-sooner* rewards. They do not find any significant effect of intertemporal preferences when the stress effect was delayed. Byrne et al. (2019) complement these findings by suggesting that although acute stress pushes individuals to forego *larger-later* rewards, preferences are reversed when individuals *learn* from their experience even under stressful conditions.

4.3 Method

4.3.1 Sampling strategy

This experiment was run in May 2021 on the Prolific platform. The experimental task was coded using oTree software (Chen et al., 2016). Participants were paid an average wage of £5 per hour. To be eligible to participate in the study, one had to be proficient in the English language (indicated via Prolific internal screening criteria) and be at least 24 years old. The latter requirement was included to minimize the likelihood that participants' dependence on, e.g. family, influences their subjective understanding of themselves in terms of the social hierarchy. Using estimates from a pilot study, I set a target sample size of 300 participants with an equal split among control and treatment groups to detect a minimum detectable effect above 9% in terms of an average effort task allocation to an earlier date by doing a median split for SES at 5% significance level and with 80% power (for treatment group)[2]. I have oversampled the number of participants by 10% for two main reasons: to avoid decreasing sample below 300 if answers were unsatisfactory or if participants have correctly identified the aim of the study (they had to identify the aim of the study at the end of the experiment)[3] After dropping unsatisfactory answers, the final sample consists of 316 participants (145 in control and 171 in the treatment group).

Table 4.3.1 presents descriptive statistics of the participants. The average age is around 32 years, with a minimum of 24 and a maximum of 72. Overall, 45% of the sample consists of females and 65% are employed (full-time or part-time). The average subjective socioeconomic status level indicated by the participants is 5. Unlike for previous items, the status of higher education differs significantly among control and treatment conditions ($p = 0.00$): in the control group, 90% of participants have completed higher education, while in treatment, this share falls to 77%. Differences in education status will be controlled for in the main analysis.

High attrition tends to be a big issue in online experiments (Zhou and Fishbach, 2016). There is a possibility that SES priming has caused strong negative feelings, in particular, for those with lower status levels. If these participants systematically dropped out of the study, this will bias the results as only those that have a moderate or null reaction to priming choose to continue the experiment. In total, 437 participants have registered for the study, but only 316 (72.31%) have finished it. Although this implies the attrition rate of 27.69%, 70 participants (16.02%) left the study before being assigned to the treatment. Next, I regress the treatment status on the decision to drop out in the sample that does not include participants who dropped out before

[2]In the current dataset, low SES individuals, on average, allocated 10.07% more tasks to an earlier date compared to high SES participants

[3]None of the participants has correctly guessed the actual aim of the experiment.

Table 4.3.1: Descriptive statistics

	Control	Treatment	p-value
Age	31.82	32.75	0.34
Female	0.47	0.44	0.59
Employed part- or full-time	0.68	0.64	0.48
Student	0.29	0.28	0.86
Obtained higher education	0.90	0.77	0.00
Socioeconomic status	5.47	5.29	0.37
N	145	171	

Note: Mean coefficients. Column (1) shows control group mean; column (2) shows treatment group mean. Col (3) shows p-values associated with two-way t-test with equal variance. *Higher education* indicates degree from technical college, undergraduate, graduate or doctorate studies. *Socioeconomic status* is measured on a scale from 1 (lowest) to 10 (highest).

the assignment to the treatment (see Table A-1 in Appendix 4.A). Although treatment status is significant at the 10% level, it decreases the dropout rate. Overall, 18.12% of the participants dropped out in the control group, while in the treatment group this share is twice smaller (9.18%). Since the control condition did not include potentially sensitive priming questions, I conclude that a high attrition rate should not pose a significant threat to the interpretation of the results.

4.3.2 Study design

This experiment uses a between-subject design with two randomly assigned conditions: control and treatment. The goal of this study is to assess how salient subjective socioeconomic status influences intertemporal choice regarding effort (work). In particular, I raise the hypotheses that (1) SES priming will not have a significant effect on the effort allocations for high SES participants, however (2) lower SES individuals will exhibit differences in their allocations when primed[4]. As the treatment group was also asked about their SES at three points in time, I raise an additional hypothesis that (3) there will exist a significant difference in effort allocations between participants that had a relatively stable SES compared with those whose status worsened or is expected to get worse in the future. To check the validity of hypotheses, participants were first randomly assigned to either control or treatment conditions and then asked to state their preferences in the intertemporal choice task. In what follows, I describe the structure of the experiment in more detail.

Instructions. Participants were first introduced to the study by explaining that they will have to make choices about the allocation of 150 real effort tasks. They were given both an example of the real effort task and the example of an allocation environment and were presented with instructions on how their choices will have to be done. To make sure participants understood the task, they were given three comprehension questions which had to be answered correctly to proceed with the study. The order of the questions and their answers was randomized for every attempt. Instructions can be found in the Appendix 4.B.

[4]This hypothesis does not have a clear directional prediction. Priming can activate negative emotions which, in turn, can affect the willingness to exert effort, most likely, by increasing the willingness to postpone it. However, priming can also push participants to exert more effort as a way of *compensating*, i.e. earning monetary reward to counteract the feeling of financial scarcity.

Priming. After correctly answering comprehension questions, participants were randomly assigned to either control or treatment conditions. The treatment group had to answer three questions regarding their (1) current employment status, (2) total household income per year, and (3) household size. They were then asked to complete the MacArthur scale for subjective socioeconomic status (Adler et al., 2000). In this scale, the individual is presented with a 10-rung ladder representing people of varying income, education, and employment levels and is then asked to position herself on it. The higher one puts herself on the ladder, the higher the subjective socioeconomic status of that person is. Participants were asked to mark their places on the ladder at three points in time - present (now), past (one year ago), and future (one year from now).

Participants assigned to the control group had to answer three questions regarding (1) their age, (2) their gender, and (3) their favourite colour. They also received questions including a 10-rung ladder (visually equivalent to MacArthur scale): they were asked about the importance they place on breakfast. These questions were also presented for three periods in time: present (now), past (one year ago), and future (one year from now). The Control group received the equivalent number of questions as the treatment group to avoid the issue of fatigue influencing intertemporal choices. The precise formulation of questions for treatment and control groups can be found in Appendix 4.B.

Manipulation check. Afterwards, participants were asked how they feel about their answer regarding the *current* position on the ladder. This served as a manipulation check for the treatment group. Before the intertemporal choice task, the control group did not receive any questions related to their socioeconomic status to avoid activating any ideas about their relative position in the society and thus biasing the results.

Intertemporal choice task. Both control and treatment groups were then asked to make their choices regarding the allocation of 150 real effort tasks in a convex time budget environment (Andreoni and Sprenger, 2012; Augenblick et al., 2015). Literature on intertemporal preferences and socioeconomic status has been mainly measuring choices regarding the allocation of monetary rewards (some exceptions include Haushofer et al. (2021); Bartoš et al. (2018); Carvalho et al. (2016b); however, following Augenblick et al. (2015) who were among the first to demonstrate the differences in monetary vs non-monetary currency discounting, this study used effort as an experimental currency to measure how SES priming affects willingness to exert effort instead of preferences over windfall monetary gains. The experimental budget consisted of 150 units of real effort tasks, following (Gill and Prowse, 2011). In these tasks, participants are presented with an unmarked slider, ranging in numbers from 1 to 100, and are asked to match a point on this slider to a given numerical value (an example of the slider can be found in Appendix 4.B). At the time of the study, participants were only given one trial of the real effort task but were not required to *work* before making their intertemporal choices.

In total, participants were asked to make 15 allocation decisions. Choices were made for three time frames (*now* vs *2 weeks*; *now* vs *4 weeks*; *2 weeks* vs *4 weeks*) and for each time frame participants faced allocation decisions under 5 interest rates (0%, 11%, 25%, 43%, 75%). Each choice was made using a slider: participant had to decide how to split 150 units of experimental effort budget between the two dates - *sooner* and *later* - and under a given interest rate, which increased the number of tasks postponed to the future. The adjustment of future work according

to the interest rate was calculated by the software. A screenshot of the task can be found in Appendix 4.B.

This task was weakly incentivized: 1% of participants were randomly drawn to be eligible for a bonus payment of £10. To receive the bonus, one choice was drawn randomly out of 15 choices they have made; they were then asked to complete the number of tasks on dates corresponding to the ones in the randomly selected choice. If participants have completed all the effort tasks on both dates correctly with a 5% margin of error, they received the bonus payment.

Finishing questionnaire. After making their allocation decisions, participants were asked questions on their emotional state and procrastination tendencies. As positive-negative affect was shown to have an impact on patience levels (Herman et al., 2018; Ifcher and Zarghamee, 2011; Lerner et al., 2013), a question on the general mood of that day was included as a potential indicator on whether mood has influenced the intertemporal choice in this study. There also exists a possibility that certain character traits impact allocation decisions: in particular, if a person is more likely to procrastinate in daily life, this might influence the choices in the experiment. To control for this I include 6 item version of the Irrational Procrastination Scale (Steel, 2010). Finally, participants were also asked to indicate what the goal of this study might have been. This was done to avoid experimenter demand effects for participants who correctly guessed that this experiment was measuring the relation between SES and intertemporal choices. Participants who correctly guessed the goal was to be removed from the analysis (however, none have provided such an answer). The precise formulation of the questions can be found in Appendix 4.A.

4.4 Results

4.4.1 Socioeconomic status

For the analysis of the results, I use socioeconomic status measured via Prolific platform[5]. It is possible that this variable contains outdated information on participants' SES since I cannot control when they have provided judgement of their status. For this reason, I measure the correlation between SES reported on Prolific and SES reported directly in the study by the treatment group[6]. The Pearson correlation coefficient is 0.8139, indicating a strong correlation between two sets of SES measurements.

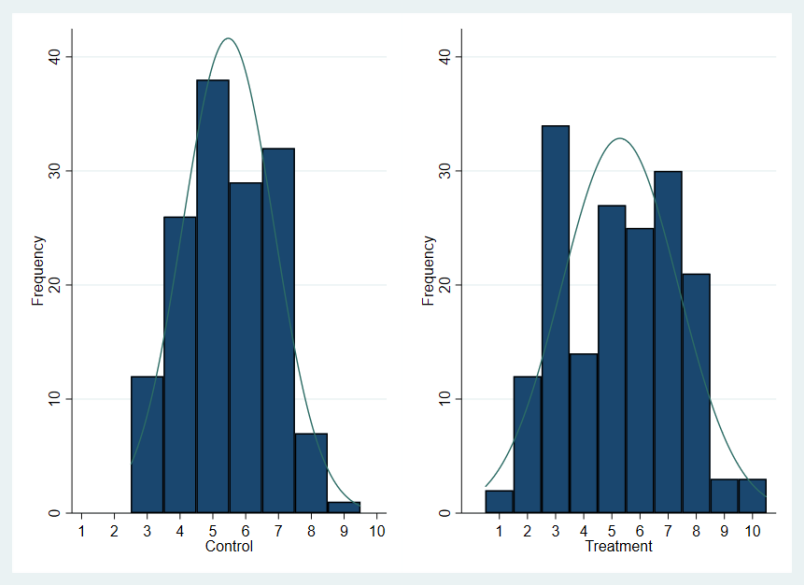
The aggregate mean of SES in the sample is 5.3, with a median value of 5. There are no significant differences between treatment and control group ($M(control) = 5.49$, $M(treatment) = 5.29$, $t(314) = 0.90$, $p = 0.369$). Figure 4.4.1 shows the frequency distribution of the variable for treatment and control groups. For the control group, SES values are more concentrated around the median value and there are no observations of the extreme ends of the scale. For further analysis, individuals with $SES \leq 5$ will be considered as *low SES* participants and with $SES > 5$ as *high SES* participants. In the control group, 52.41% of participants belong to low SES and 47.59% to the high SES group; for treatment, these shares are 52.05% and 47.95%, respectively.

In the study treatment group was asked to indicate the level of their SES a year ago and predict how it will change in a year from the day of the study. Figure 4.4.2 show the frequency

[5]Participants have to fill in an extensive questionnaire, including a question on socioeconomic status measured using MacArthur scale of subjective socioeconomic status, when registering on the platform

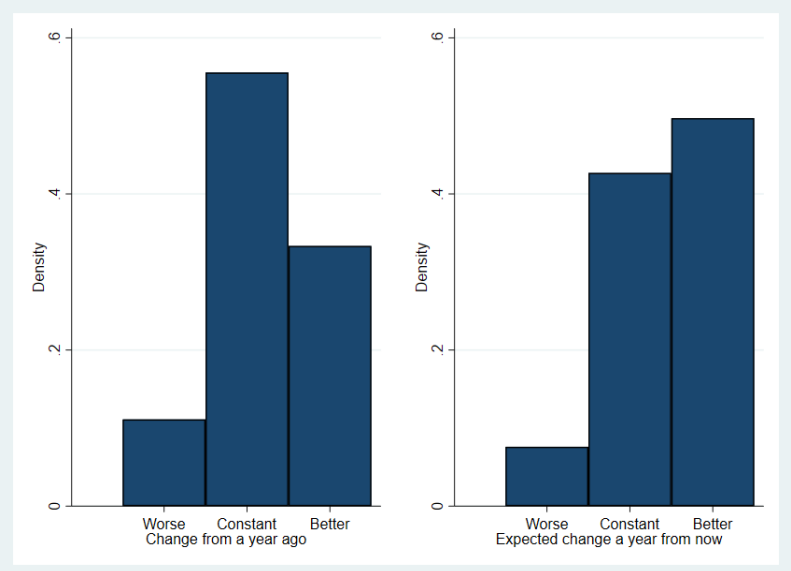
[6]Due to technical issues, the control group was not administered equivalent question at the end of the study.

Figure 4.4.1: Distribution of socioeconomic status in treatment and control groups



distribution of the answers: 55.56% of participants indicated that their SES has not changed since a year ago, followed by 33.33% who said it got better, and 11.11% for whom it got worse. When asked about their expectations for the future, 49.71% believe their SES will improve, 42.69% expect it to stay constant, and 7.60% expect it to get worse. When dividing by levels of SES, I find no significant differences between lower and higher SES individuals in terms of past SES change ($M(lower) = 2.18$, $M(higher) = 2.27$, $t(169) = -0.92$, $p = 0.360$); however, when thinking about future, lower SES group, on average, expects higher status in a year ($M(lower) = 2.56$, $M(higher) = 2.27$, $t(169) = 3.11$, $p = 0.002$). Despite the higher degree of optimism, manipulation check showed that lower SES individuals were systematically more dissatisfied about their current status ($M(lower) = 4.86$, $M(higher) = 6.89$, $t(169) = -6.00$, $p = 0.000$)[7].

Figure 4.4.2: Distribution of socioeconomic status changes

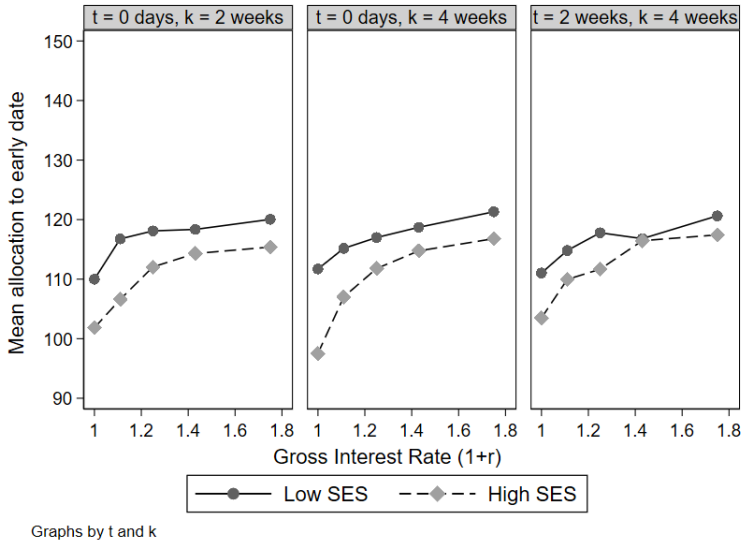


[7]It should be noted that manipulation check was administered only to the treatment group and it is impossible to draw conclusions on the average satisfaction with SES level for participants in the control group.

4.4.2 Effort allocations

Figures 4.4.3 and 4.4.4 plot the mean effort task allocations to an earlier date for control and treatment groups, respectively. Each panel represent decisions regarding the three pairs of *sooner* work date t and *later* work date k. Results are aggregated over each interest rate separately. As mentioned before, the number of effort tasks allocated to a later date increases with respect to the interest rate r. If individual allocation decisions follow the rationality assumption, mean allocation curves within each time frame should take a concave shape: as interest rate increases from 0% to 75%, rational participants should choose to do more tasks earlier rather than postponing them and doing more in the future. In control group, on average, participants allocate 75.77% of 150 effort tasks to an earlier date and this share varies minimally across the three time frames (75.67% when t = 0, k = 2 weeks; 75.57% when t = 0, k = 4 weeks; 75.77% when t = 2 weeks, k = 4 weeks). 40.69% of participants have only corner choices. Next, I divide allocations by socioeconomic status: on average, higher SES individuals allocated around 102 effort tasks (67.7%) to an earlier date when r = 0% and approximately 117 tasks (77.85%) when r reached 75%. Lower SES group chose to do around 111 tasks (74%) earlier when r = 0% and increased this share to 121 tasks (80.4%) when interest rate reached 75% (Table A-3 in Appendix 4.A). Division by SES does not indicate any heterogeneous effects: although in Figure 4.4.3 we can see slightly higher mean allocations - of around 5.7 effort task units - to an earlier date by low SES participants (in particular, when t = 0), two-sample t-test results do not allow me to reject the hypothesis that allocation means are equal among the two SES groups as $p > 0.1$ for decisions under each interest rate and earlier work date t (see Table A-3 in Appendix 4.A). It can be concluded that SES in the control group does not produce distinguishable differences in effort task allocations.

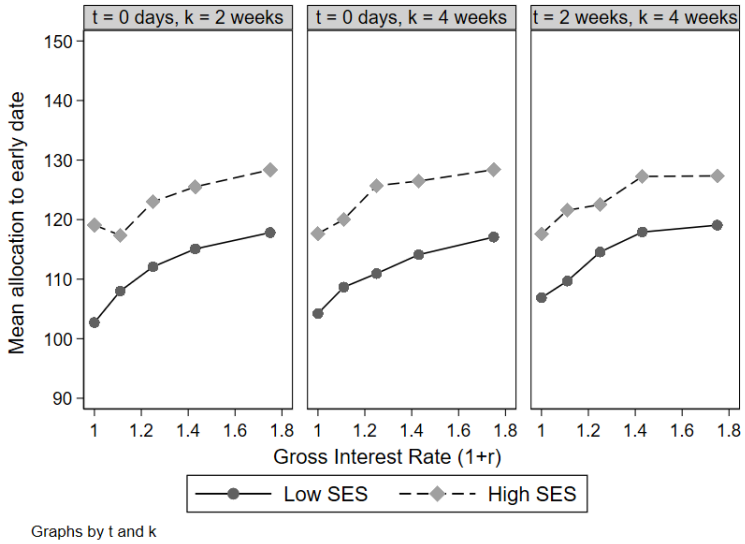
Figure 4.4.3: Mean allocation of effort by socioeconomic status (control group)



In the treatment group, the mean effort allocations to an earlier date constitute 78.21% of the experimental budget. It is 2.44% higher than in the control group, however, the difference is not significant ($t(314) = -0.92$, $p = 0.358$). 36.26% of participants made only corner choices in all 15 allocation decisions. The average share is similar across the time frames as well (77.78%

when $t = 0$, $k = 2$ weeks; 78.04% when $t = 0$, $k = 4$ weeks; 78.82% when $t = 2$ weeks, $k = 4$ weeks). However, as seen in Figure 4.4.4, there are stark differences in allocation decisions when dividing by socioeconomic status: across the three time frames low SES individuals consistently chose to do fewer effort tasks on an earlier date and this difference is particularly pronounced when decision time involves $t = 0$. Low SES group allocate around 12 tasks less (8.1%) across all decisions when $t = 0$ compared to high SES group: when interest rate $r = 0\%$, low SES group allocate 69% of their budget (103 tasks) to an earlier date - 15 tasks less than high SES individuals (78.9%, or 118 tasks), while under highest interest rate ($r = 75\%$), the difference is 11 tasks (78.3% for low SES and 85.6% for high SES). Moreover, the differences are in most cases significant across all interest rates with $p < 0.05$ (see Table A-3). When $t \neq 0$, the average difference between allocations falls to 10 tasks (6.5%). Under $r = 0\%$, the low SES group chose to do 71.3% of the effort tasks (107 units) on an earlier date while for high SES group share was 78.4% (118 tasks); when the interest rate was 75%, low SES individuals allocated 79.4% of their experimental budget (119 tasks) to t whilst high SES group chose to do 84.9% (127 tasks) earlier. However, both the overall difference among the two SES groups ($t(169) = -1.9$, $p = 0.06$) and the allocation disparities divided by r are not consistently significant at 5% level (see Table A-3).

Figure 4.4.4: Mean allocation of effort by socioeconomic status (treatment group)



The treatment group was also asked about the change in their SES - past and expected. I raised a hypothesis that effort task allocations will differ for people whose socioeconomic status got worse or is expected to get worse. A potential channel for such differences is a salient SES change activating negative (in case of worsening conditions) or positive (in case of improving conditions) emotions. I regress negative past and future change in SES on allocation decisions (see Table A-5 in Appendix 4.A). Negative past change in SES decreases the share of effort tasks allocated to the present by 3.8%; however, when interacted with the expected worsening of SES in the future, the net effect surprisingly becomes positive (allocation increases by around 13.2%). Overall, results do not have a high significance level, therefore I cannot conclude that change in SES has a meaningful impact on effort allocation.

4.4.3 Present bias

One of the present bias indicators in the intertemporal decisions is a *static preference reversal*[8]: when making a decision regarding two time frames with the same delay length k, the individual is considered to exhibit present bias (static preference reversal) if she, in case of effort allocation, chooses to work more in the future when $t = 0$, but reverses this decision - chooses to work less in the future - when the sooner work date is $t \neq 0$. In this study, on an aggregate level, neither control nor treatment groups exhibit a significant presence of static preference reversals (Table A-2 in Appendix 4.A). The differences between allocation shares are, on average, 0.5% and 0.9% for control and treatment groups respectively. Dividing responses by SES levels does not underline stark differences in preference reversal either. On average, high SES individuals in the control group chose to do 1.3% more tasks sooner when the earlier date was $t \neq 0$, while for low SES individuals there is more variation in the direction of differences, with an aggregate value being 0.3% more when $t = 0$ (Table A-3 in Appendix 4.A). However, the allocation differences, aggregate or divided by r, are minor and insignificant ($p > 0.05$). In the treatment group situation is the opposite: allocation shares fluctuate around the mean of 0.1% ($p > 0.05$ for all r) for high SES participants but are systematically lower when $t = 0$ for low SES participants (although not significant on an aggregate level at 5%). These results give a hint that low SES individuals might be more likely to make present biased choices when their SES is made salient; however, we should be careful in making strong conclusions due to a low significance level.

Figure 4.4.5: Static preference reversals: proportion by experimental condition and SES

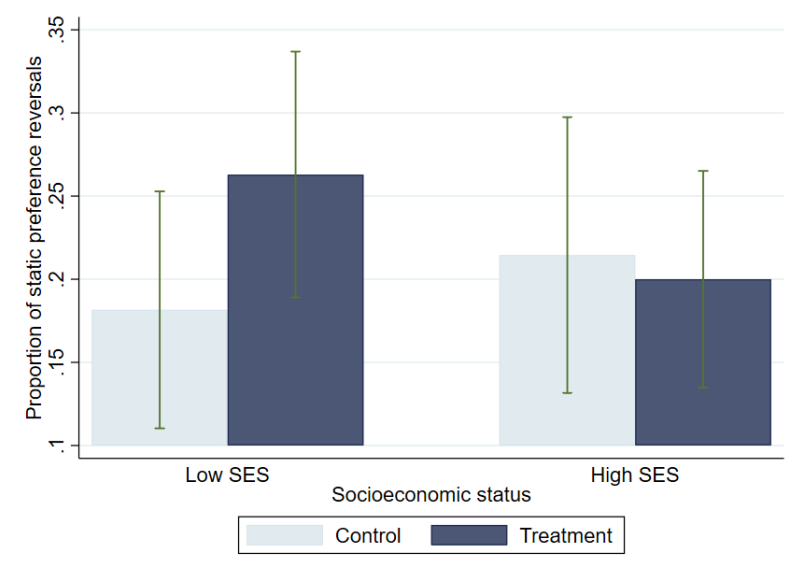


Figure 4.4.5 plots the proportion of static preference reversals for treatment and control groups (divided by SES). In control group 19.72% of all allocation decisions were reversed, while in treatment group it was 23.27% ($t(314) = -0.96$, $p = 0.337$). Low SES group reversed 8.13% more decisions in treatment as compared to the control ($M(control) = 0.1816$, $M(treatment) = 0.2629$), though I cannot reject the hypothesis that means in these two groups are equal ($t(163) = -1.56$, $p = 0.120$). The difference is much smaller for high SES individuals: 21.44% of decisions

[8]In general, another indicator of present bias is *dynamic preference reversal*: revising choices made in future once the date draws closer.

were biased towards present for participants in control condition, and 20% for participants in the treatment group ($t(149) = 0.28$, $p = 0.782$).

To check for robustness of these results, I then perform parametric estimations of present bias parameter β and discount factor δ using two-limit Tobit regressions[9] Table 4.4.1 reports aggregate β and δ parameters for low and high SES individuals in control and treatment groups. Results confirm previous observations: in terms of present bias, in the control group, for low SES $\beta > 1$, indicating potential future focus, while for high SES $\beta = 0.974$. However, a hypothesis $\beta = 1$ cannot be rejected for both SES groups ($p = 0.753$ for low and $p = 0.633$ for high SES individuals), meaning that the control group does not exhibit a significant degree of present bias. In the treatment group, high SES $\beta = 1.012$, but as $p = 0.798$, it is also not possible to reject the hypothesis of no present bias. The situation is different for low SES participants in the treatment group as there $\beta = 0.937$ (however, $p = 0.060$ which could be an indicator of low power). Overall, results from parametric estimations, albeit being weekly significant, hint towards lower present bias parameter only in treated low SES group. In terms of the discount factor, the chi-squared test confirms the non-linear discounting with δ significantly different from 1 across all experimental groups: in the control group, $\delta = 1.393$ for low SES and $\delta = 1.226$ for high SES individuals, while in treatment, it is 1.250 and 1.354, respectively. A discount factor higher than 1 indicates that participants choose to perform more tasks sooner when the delay length increases. What is unusual in these results is the reverse pattern: when in the control group, high SES individuals discount future effort less than low SES group, while in treatment the result is the opposite.

Table 4.4.1: Parametric estimations by treatment and socioeconomic status

	Control		Treatment	
	(1)	(2)	(3)	(4)
	Low SES	High SES	Low SES	High SES
Present bias parameter β	1.026	0.974	0.937	1.012
	(0.082)	(0.054)	(0.033)	(0.045)
Discount factor (weekly) δ	1.393	1.226	1.250	1.354
	(0.130)	(0.073)	(0.071)	(0.102)
N	1140	1035	1335	1230
Clusters	76	69	89	82
$H_0 : \beta = 1$	0.753	0.633	0.060	0.798
$H_0 : \delta = 1$	0.002	0.002	0.000	0.000

Note: Parameters identified from two-limit Tobit regressions. SE clustered at individual level and reported in parentheses. P-values from chi-squared tests in the last two rows.

[9]Following Augenblick et al. (2015) I assume quasi-hyperbolic $\beta - \delta$ discounting function with Stone-Geary background parameters:

$$C(e_t, e_{t+k}) = (e_t + \omega)^\gamma + \beta^{1_{t=0}} \delta^k (e_{t+k} + \omega)^\gamma \tag{4.1}$$

where β is the present bias parameter, *delta* is the discount parameter. $1_{t=0}$ is an indicator variable whether the effort is done in the present. If $\beta = 1$, it implies no present bias and the function takes the standard temporal discounting form. Stone-Geary background consumption ω represents a minimum amount of effort (work) to be performed, which was set to 1 (every participant had to do a trial of one real effort task before making their intertemporal allocations); $\gamma > 1$ represents a parameter of convex instantaneous cost of effort function.

4.4.4 Treatment effects

Next, I run regression analysis to estimate aggregate treatment effects. Shapiro-Wilk Test showed that dependant variable (share of budget allocated to an earlier date) is non-normally distributed ($W = 0.979$, $p = 0.000$); in fact, 48.35% of responses are concentrated on value 1 (see Figure A-1 in Appendix 4.A). As dependent variable describes a proportion in values ranging between - and including - 0 and 1, I use a one inflated beta model[10] with robust standard errors. Table 4.4.2 reports the estimates. Panel A presents estimates of the general beta model. I find that interest rate and treatment status is related to a higher experimental budget allocation shares on a sooner date; the same direction of effect is found for a low SES group, albeit significant only at the 10% level. However, the interaction between low SES and treatment status decreases the allocation proportion (as seen also in non-parametric analyses in Section 4.4.2). It is interesting to note that according to estimations of the probability that dependant variable is equal to 1 (i.e. all effort tasks are allocated to a sooner period across all decision time frames), neither treatment status nor socioeconomic status or their interaction are significant in explaining the results. Conversely, the only two significant predictors are interest rate and gender: higher interest rate increases, while being a woman decreases the probability of assigning all effort tasks to a sooner period. After the intertemporal task participants were also asked about their general mood of the day and procrastination tendencies: there is a possibility that allocation behaviour was influenced by negative emotions (Delis et al., 2021; Herman et al., 2018; Lerner et al., 2013) or a general characteristic to postpone tasks. Only in a general beta model, the mood seemed to increase the proportion of effort tasks allocated to an earlier date (significant only at 10% level), although participants who scored higher at procrastination scale[11], allocated a lower proportion of tasks to the earlier dates. Decisions do not seem to be influenced by the actual work date t or delay length either.

4.4.5 Consistency and robustness

When eliciting preferences it is important to check whether participants' responses adhere to the basic assumptions of rationality. In this study, I follow the example of Giné et al. (2018) and calculate the level of *basic consistency*. This measure checks how much individual choices adhere to the law of demand: as the interest rate r in the intertemporal choice task increases the number of real effort tasks to be done on the later date within a decision time frame, a monotonically non-increasing function of allocation decisions would be an appropriate utility-maximizing response. If participants allocate an increasing number of effort tasks to a later date within a t - k pair as r grows, such decisions will be considered inconsistent, i.e. violating the law of demand.

In the control group, 66.21% of participants made no inconsistent choices, 6.90% made one, and 26.90% made two or more. The situation is similar for the treatment group: 65.50% show no inconsistent behaviour, 4.09% made one choice and 30.41% made two or more choices violating the law of demand. The complexity of the intertemporal choice task was minimized, however,

[10] Since 0 answers represent only 2.11% of all decisions, model accounts only for one - and not zero - inflation.

[11] Although procrastination scale measures character trait, it is possible that answers were influenced by the treatment; however, there appears to be no significant difference between control and treatment groups in terms of procrastination score ($M(control) = 2.95$, $M(treatment) = 3.01$, $t(314) = -1.12$, $p = 0.261$).

Table 4.4.2: Regression analysis (one-inflated beta model)

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: proportion			Panel B: one-inflated		
Treatment	0.629***	0.613***	0.585***	0.023	0.025	0.025
	(0.17)	(0.17)	(0.17)	(0.30)	(0.31)	(0.31)
Low SES	0.284*	0.287*	0.299*	-0.092	-0.120	-0.117
	(0.16)	(0.16)	(0.15)	(0.31)	(0.31)	(0.31)
Low SES x treatment	-0.590***	-0.552***	-0.501**	-0.362	-0.323	-0.309
	(0.21)	(0.21)	(0.21)	(0.41)	(0.42)	(0.42)
Interest rate	0.388***	0.385***	0.391***	0.287***	0.288***	0.290***
	(0.07)	(0.07)	(0.07)	(0.11)	(0.11)	(0.11)
$t = 0$	0.001	0.002	0.002	-0.000	0.000	-0.000
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Delay length	0.012	0.012	0.011	-0.013	-0.013	-0.012
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Age		0.004	0.001		-0.008	-0.010
		(0.01)	(0.01)		(0.01)	(0.01)
Gender		0.052	0.063		-0.633***	-0.628***
		(0.10)	(0.10)		(0.21)	(0.21)
Employed		0.045	0.069		-0.078	-0.077
		(0.12)	(0.11)		(0.23)	(0.23)
Student		0.000	0.027		-0.094	-0.082
		(0.11)	(0.11)		(0.25)	(0.25)
High education		0.060	0.049		0.341	0.325
		(0.09)	(0.09)		(0.31)	(0.31)
General mood			0.046*			0.007
			(0.03)			(0.06)
Procrastination score			-0.076			-0.058
			(0.06)			(0.11)
Constant	-0.525***	-0.747***	-0.769*	-0.221	0.132	0.310
	(0.15)	(0.26)	(0.41)	(0.26)	(0.65)	(0.91)
N	4740	4740	4740	4740	4740	4740
Clusters	316	316	316	316	316	316

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Dependant variable is share of effort task budget allocated to an earlier date. Panel A present estimates of a general beta model. Panel B present separate estimates of the probabilities to have value 1. *General mood* measures participant's general mood, self-reported on a 1-10 scale. *Procrastination score* measures participant's procrastination tendencies.

it is possible that the convex budget environment was still confusing for some participants (for example, inconsistency could have been caused by differences in education levels, as seen in Table 4.3.1, or by treatment if it increased stress or negative affect levels which in turn influenced attention). However, percentage of inconsistent choices does not differ significantly across these two groups ($M(control) = 1.50$, $M(treatment) = 1.87$, $t(314) = -1.10$, $p = 0.273$) or when divided by SES ($M(low) = 1.703$, $M(high) = 1.70$, $t(314) = 0.00$, $p = 0.997$). There also exists a possibility that inconsistent choice was made purely accidentally. Allocation decisions were made using sliders: if, for example, participants tried clicking on a visually equivalent spot across all sliders without checking the actual effort task shares, their choices might differ only by a small number of effort tasks, yet be considered as violating the law of demand. For this reason, I also calculate the number of inconsistent choices but allow the variation of up to 5 effort tasks for each decision. This means that if, for example, the participant has chosen to do 100 tasks earlier under $r = 0\%$, but 102 tasks when $r = 11\%$, this will still be considered a consistent choice. In this case, for the control group, 74.48% of choices are consistent, 6.21% of participants have one inconsistent choice and 19.31% have two or more. In the treatment group, 73.68% of participants do not violate the law of demand, 2.92% have one inconsistent decision and 23.39% have two or more. Once again, there is no difference in the inconsistency

levels between treatments ($M(control) = 1.05$, $M(treatment) = 1.19$, $t(314) = -0.49$, $p = 0.621$) or when divided by SES ($M(low) = 1.230$, $M(high) = 1.01$, $t(314) = 0.82$, $p = 0.415$).

Table 4.4.3: Regression analysis (one-inflated without inconsistent choices)

	(1)	(2)	(3)	(4)
	Panel A: proportion		Panel B: one-inflated	
Treatment	0.636***	0.830***	-0.259	-0.061
	(0.25)	(0.22)	(0.44)	(0.38)
Low SES	0.357	0.237	-0.545	-0.275
	(0.26)	(0.20)	(0.43)	(0.38)
Low SES x treatment	-0.452	-0.572**	-0.100	-0.252
	(0.31)	(0.27)	(0.59)	(0.52)
Interest rate	0.801***	0.700***	0.916***	0.718***
	(0.12)	(0.09)	(0.15)	(0.13)
$t = 0$	0.032	0.012	-0.005	-0.010
	(0.03)	(0.03)	(0.03)	(0.03)
Delay length	-0.022	-0.024	-0.020	-0.005
	(0.02)	(0.02)	(0.02)	(0.02)
General mood	0.047	0.073**	0.062	0.084
	(0.03)	(0.03)	(0.09)	(0.08)
Procrastination score	-0.031	-0.092	-0.055	-0.086
	(0.09)	(0.08)	(0.15)	(0.13)
Age	-0.015	-0.003	0.006	-0.024
	(0.01)	(0.01)	(0.02)	(0.02)
Gender	-0.087	0.082	-0.548*	-0.606**
	(0.16)	(0.14)	(0.29)	(0.26)
Employed	-0.024	0.030	-0.101	-0.104
	(0.17)	(0.15)	(0.33)	(0.29)
Student	-0.231	-0.077	0.008	-0.216
	(0.16)	(0.15)	(0.35)	(0.31)
High education	0.022	-0.002	-0.225	0.040
	(0.16)	(0.13)	(0.46)	(0.41)
Constant	-0.744	-1.029**	0.139	0.630
	(0.52)	(0.45)	(1.31)	(1.12)
N	3120	3510	3120	3510
Cluster	208	234	208	234

Note: Robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Dependant variable is share of effort task budget allocated to an earlier date. Panel A present estimates of a general beta model. Panel B present separate estimates of the probabilities to have value 1. In columns (1) and (3) I use sample without strictly inconsistent choices and in columns (2) and (4) I allow variation of inconsistent answers for up to 5 task units. *General mood* measures participant's general mood, self-reported on a 1-10 scale. *Procrastination score* measures participant's procrastination tendencies.

To check whether inconsistent choices bias overall results, I replicate the regression analysis without both strictly and weakly inconsistent choices and present results in Table 4.4.3. First, it should be noted that dropping participants with inconsistent answers decreases the sample by around 34% (strictly inconsistent) and 26% (weakly inconsistent). As with the full sample, results are mostly consistent with the previous ones: being in the treatment group and facing higher interest rates increases the proportion of effort tasks allocated to an earlier date. However, the significance of low SES and its interaction with the treatment drops with the restricted sample: the only significant result at 5% level is the interaction term in a sample with weakly inconsistent answers. It should be noted that in the same sample, a better mood appears to increase the willingness to work earlier. Separate estimations for the one-inflated beta model confirm the results seen in the full sample.

4.5 Conclusions

This paper presents experimental evidence of salient subjective socioeconomic status impact on intertemporal preferences regarding effort. In an online experiment, a subsample of participants had their SES primed with MacArthur's scale of subjective socioeconomic status, while the remaining group answered questions unrelated to SES. Afterwards, both groups were asked to make 15 intertemporal choice questions regarding a split of 150 real effort tasks across 3 pairs of working dates and under 5 increasing interest rates. I find that SES priming affects only low SES individuals: in a control group, these participants were less likely to allocate work to a later date by approximately 3.8%, but in the treatment, the number of effort tasks allocated to the future increased by 7.3% and the result is generally robust when divided by decision time frames and interest rates. Low SES participants also made 8.14% more present biased decisions compared to the control group, while for high SES individuals this difference is just 1.44%. While the present bias parameter β is not significantly different from 1 for both SES groups in control and high SES group in treatment, it falls below 1 ($\beta = 0.937$) for primed low SES participants. Moreover, the weekly discount factor δ shows that these individuals discount effort more compared to high SES participants in treatment and low SES participants in control. Overall, regression analysis showed that treatment and low socioeconomic status increased the share of effort task budget allocated to an earlier date; however, their interaction produced a negative effect. This result holds for interior solutions since a separate - one-inflated beta regression - a model for decisions of allocating all their budget to an earlier date showed that only interest rate and gender had a significant impact on the choice. One of the potential mechanisms behind such effect could come from negative utility associated with one's status (the following work by Bartoš et al. (2018)): priming task makes one's socioeconomic status salient and it might be that dissatisfaction with the low status makes people seek to counteract this negative utility by engaging in activities that bring positive or avoid activities that bring negative utility. Since immediate effort brings about disutility, postponing the work to the future might work as a way of counterbalancing the (negative) effect of salient low SES.

These results suggest that reminders of one's socioeconomic status can have a disproportionate impact on intertemporal decision making. While salient status might not impact those who are living relatively well-off, it can push individuals of lower social standing to make more present-biased decisions. However, future studies would benefit from a further investigation on the precise mechanisms driving such results forward, especially a potential role of negative affect in mediating the effect of salient (low) SES and work decisions. Moreover, as division by socioeconomic status into low SES and high SES groups was done by the median split, more heterogeneous division in terms of status could help to pinpoint more precisely who are the most susceptible for this effect among all the socioeconomic groups. We must be careful with result interpretation as well: although the control group was not asked questions directly related to their socioeconomic status, it is possible that questions about age and gender indirectly activated SES-related thoughts. If such was the case, results for the control group could be downward biased.

Appendix

4.A Appendix A

Figure A-1: Distribution of the share of experimental budget allocated to an earlier date

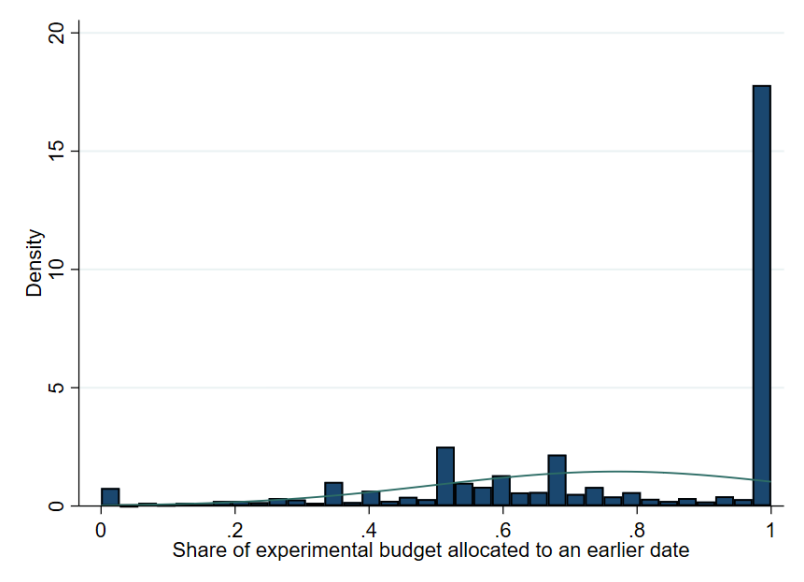


Table A-1: Attrition

	(1) Dropout b/se
Treatment	-0.089* (0.04)
Control mean	0.181
N	367
Adj. R^2	0.015
vce	robust

Note: Estimates obtained via OLS regressions. Robust standard errors in parentheses. *, **, *** denote level of significance (10%, 5%, and 1% respectively). Outcome variable is a dummy on decision to drop out (0 means participant completed the study, 1 means participant dropped out at any point after being assigned to the control treatment).

Table A-2: Aggregate behavior by interest rate

	(1) N	(2) $t = 0$	(3) $t \neq 0$	(4) Diff.	(5) St. Err.	(6) t value	(7) p value
Panel A: control group							
Aggregate	145	0.756	0.761	-0.005	0.01	-0.45	0.649
r = 0%	145	0.704	0.717	-0.013	0.015	-0.9	0.38
r = 11%	145	0.744	0.75	-0.006	0.013	-0.45	0.647
r = 25%	145	0.766	0.766	0.00	0.012	0.00	0.997
r = 43%	145	0.777	0.777	0.00	0.011	0.00	0.993
r = 75%	145	0.79	0.794	-0.004	0.011	-0.35	0.724
Panel B: treatment group							
Aggregate	171	0.779	0.788	-0.009	0.007	-1.3	0.192
r = 0%	171	0.738	0.747	-0.009	0.011	-0.9	0.377
r = 11%	171	0.756	0.769	-0.014	0.011	-1.2	0.225
r = 25%	171	0.784	0.789	-0.005	0.009	-0.5	0.61
r = 43%	171	0.8	0.816	-0.016	0.008	-2.00	0.048
r = 75%	171	0.818	0.82	-0.002	0.007	-0.35	0.733

Note: able reports results from two-sample t tests regarding share of the effort task budget allocated to an earlier date (when $t = 0$ in Column(2) and when $t \neq 0$ in Column (3). For each panel, t tests are performed separately for allocations under different interest rates and for overall mean allocation ("Aggregate") for that group

Table A-3: Static preference reversals by interest rate, SES, and experimental condition

	(1) N_1	(2) $t = 0$	(3) $t \neq 0$	(4) Diff.	(5) St. Err.	(6) t value	(7) p value
Panel A: Control group's allocations to an earlier date (low SES)							
Aggregate	76	0.778	0.775	0.003	0.014	0.25	0.813
r = 0%	76	0.739	0.741	-0.001	0.018	-0.05	0.946
r = 11%	76	0.773	0.766	0.008	0.018	0.4	0.68
r = 25%	76	0.783	0.785	-0.002	0.017	-0.1	0.922
r = 43%	76	0.79	0.778	0.012	0.015	0.75	0.442
r = 75%	76	0.804	0.804	0.00	0.017	0.00	0.985
Panel B: Control group's allocations to an earlier date (high SES)							
Aggregate	69	0.732	0.746	-0.013	0.015	-0.9	0.364
r = 0%	69	0.664	0.69	-0.025	0.022	-1.1	0.266
r = 11%	69	0.712	0.733	-0.021	0.018	-1.15	0.248
r = 25%	69	0.746	0.745	0.002	0.018	0.1	0.925
r = 43%	69	0.764	0.776	-0.013	0.015	-0.9	0.376
r = 75%	69	0.774	0.783	-0.009	0.016	-0.55	0.584
Panel C: Treatment group's allocations to an earlier date (low SES)							
Aggregate	89	0.741	0.758	-0.017	0.009	-1.85	0.071
r = 0%	89	0.69	0.713	-0.023	0.016	-1.45	0.157
r = 11%	89	0.722	0.731	-0.009	0.015	-0.6	0.557
r = 25%	89	0.744	0.764	-0.02	0.011	-1.95	0.052
r = 43%	89	0.764	0.786	-0.022	0.009	-2.45	0.016
r = 75%	89	0.783	0.794	-0.011	0.009	-1.1	0.266
Panel D: Treatment group's allocations to an earlier date (high SES)							
Aggregate	82	0.821	0.822	-0.001	0.011	-0.05	0.95
r = 0%	82	0.789	0.784	0.005	0.013	0.4	0.709
r = 11%	82	0.791	0.81	-0.019	0.017	-1.1	0.265
r = 25%	82	0.829	0.817	0.012	0.015	0.8	0.431
r = 43%	82	0.84	0.849	-0.008	0.013	-0.65	0.518
r = 75%	82	0.856	0.849	0.007	0.009	0.75	0.466

Note: Table reports results from two-sample t tests regarding share of the effort task budget allocated to an earlier date by low and high SES participants. Panels A and C tabulate results from decisions of low SES individuals ($SES < 5$); panels B and D contain represent decisions by high SES individuals ($SES \geq 5$). For each panel, t tests are performed separately for allocations under different interest rates and for overall mean allocation ("Aggregate") for that group.

Table A-4: Aggregate allocations by interest rate, SES, and experimental condition

	(1) N_1	(2) N_2	(3) Low SES	(4) High SES	(5) Diff.	(6) St. Err.	(7) t value	(8) p value
Panel A: Control group's allocations to an earlier date when $t = 0$								
Aggregate	76	69	0.778	0.732	0.046	0.044	1.05	0.296
$r = 0\%$	76	69	0.739	0.664	0.074	0.053	1.4	0.166
$r = 11\%$	76	69	0.773	0.712	0.061	0.048	1.3	0.201
$r = 25\%$	76	69	0.783	0.746	0.037	0.046	0.8	0.415
$r = 43\%$	76	69	0.79	0.764	0.026	0.047	0.55	0.57
$r = 75\%$	76	69	0.804	0.774	0.03	0.046	0.65	0.507
Panel B: Control group's allocations to an earlier date when $t \neq 0$								
Aggregate	76	69	0.775	0.746	0.03	0.042	0.7	0.48
$r = 0\%$	76	69	0.741	0.69	0.051	0.051	1	0.328
$r = 11\%$	76	69	0.766	0.733	0.033	0.046	0.7	0.488
$r = 25\%$	76	69	0.785	0.745	0.041	0.045	0.9	0.366
$r = 43\%$	76	69	0.778	0.776	0.003	0.045	0.05	0.96
$r = 75\%$	76	69	0.804	0.783	0.022	0.044	0.5	0.627
Panel C: Treatment group's allocations to an earlier date when $t = 0$								
Aggregate	89	82	0.741	0.821	-0.081	0.034	-2.4	0.018
$r = 0\%$	89	82	0.69	0.789	-0.1	0.043	-2.3	0.022
$r = 11\%$	89	82	0.722	0.791	-0.069	0.039	-1.75	0.078
$r = 25\%$	89	82	0.744	0.829	-0.086	0.036	-2.4	0.018
$r = 43\%$	89	82	0.764	0.84	-0.076	0.036	-2.1	0.036
$r = 75\%$	89	82	0.783	0.856	-0.073	0.036	-2.05	0.044
Panel D: Treatment group's allocations to an earlier date when $t \neq 0$								
Aggregate	89	82	0.758	0.822	-0.065	0.034	-1.9	0.059
$r = 0\%$	89	82	0.713	0.784	-0.072	0.044	-1.65	0.102
$r = 11\%$	89	82	0.731	0.81	-0.079	0.038	-2.05	0.041
$r = 25\%$	89	82	0.764	0.817	-0.053	0.037	-1.4	0.16
$r = 43\%$	89	82	0.786	0.849	-0.063	0.035	-1.8	0.072
$r = 75\%$	89	82	0.794	0.849	-0.055	0.037	-1.5	0.136

Note: Table reports results from two-sample t tests regarding share of the effort task budget allocated to an earlier date by low and high SES participants. Panels A and C tabulate results from decisions involving only present (now vs 2 weeks and now vs 4 weeks); in panels B and D only decisions regarding future (2 weeks vs 4 weeks) are considered. For each panel, t tests are performed separately for allocations under different interest rates and for overall mean allocation ("Aggregate") for that group.

Table A-5: Change in SES impact on allocation shares

	(1)	(2)
Past (negative)	-0.038	-0.074*
	(0.05)	(0.04)
Future (negative)	-0.052	-0.105
	(0.08)	(0.09)
Past x Future		0.206
		(0.18)
Constant	0.779***	0.802***
	(0.07)	(0.07)
Covariates	Yes	Yes
N	2565	2565
R^2	0.0310	0.0392
Clusters	171	171

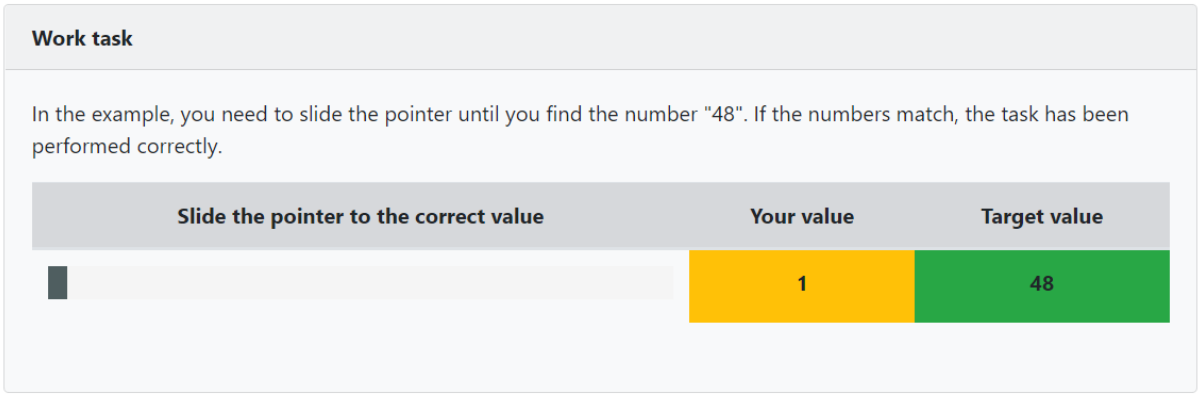
Note: OLS regression with robust standard errors in parentheses.*, **, *** denote significance at the 10%, 5% and 1% levels respectively. Dependant variable is share of effort task budget allocated to an earlier date. *Past (negative)* is a dummy variable where value 1 indicates lower SES as compared to a year ago. *Future (negative)* is a dummy variable with value 1 indicating that participant expects her SES to become worse in a year.

4.B Appendix B

4.B.1 Instructions

For this experiment, you will have to complete a work task 150 times, but you don't have to do it right away. Now you'll just have to decide when you want to do it.

The work task is this: you have to slide the pointer on the line so that the number in the yellow column matches the number in the green column.



If you complete all 150 tasks correctly you can receive a bonus payment of £10 in addition to the basic prolific payment for this study. If you do not complete all 150 tasks correctly, you will not receive the bonus payment, but will still get the Prolific participation fee.

However, first you will have to choose a work plan to complete the tasks.We will ask you to choose:
- how many of the 150 tasks you want to complete today and how many in 2 weeks.
- how many of the 150 tasks you would like to complete today and how many in 4 weeks.
- how many of the 150 tasks you would like to complete in 2 weeks and how many in 4 weeks.

Beware: if tasks are postponed in time, they will increase in number. The rate of increase in the number of tasks varies for each work plan and is called a "task rate".

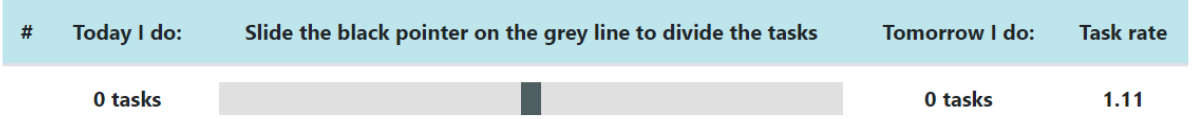
Task rate

If instead of doing the 150 tasks today you choose to do all of them in 4 weeks, you will have to do 11% more - so 17 more tasks - for a total of 167 tasks.

How do I choose the work plan?

To choose when to do the tasks, simply move the slider below from right to left and vice versa. The further to the left is the slider, the more tasks you do on a sooner date, the further to the right is the slider, the more tasks you do on the later date.

Give it a try by moving the slider in the example below.



You will be asked to make choices about 15 work plans. We will then randomly draw just one of them. Then, your prolific ID will be entered into a lottery where 1 in 100 participants will win a £10 bonus. If you are one of the lottery winners, then you will be asked to complete your work plan.

Work plan.

For example, if your work plan asks you to do 100 tasks today and 110 in 2 weeks, you will immediately receive an invitation for a new study in which you must complete 100 tasks within

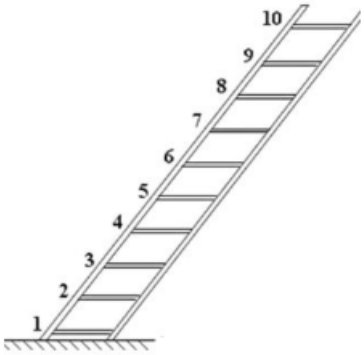
24 hours and receive a new invitation in 2 weeks in which you must complete the remaining 110 tasks.

If you complete both studies, you will win a £10 bonus.

If you are not drawn to receive the bonus, you will still be paid the usual flat fee, according to the Prolific rules, for this study.

4.B.2 Treatment group

Think of this ladder as representing where people stand in your country. At the top of the ladder are the people who are the best off – those who have the most money, the most education, and the most respected jobs. At the bottom are the people who are the worst off – those who have the least money, least education, the least respected jobs, or no job. The higher up you are on this ladder, the closer you are to the people at the very top; the lower you are, the closer you are to the people at the very bottom.



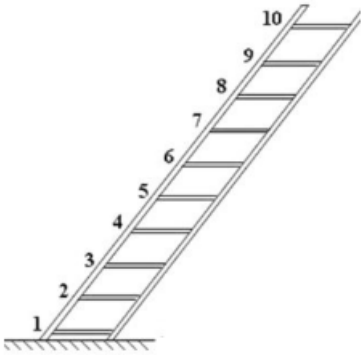
Q1. Where would you put yourself on the scale now?
Scale from 0 to 10

Q2. Now, think about last year at this same time. Where were you on the ladder a year ago? *Scale from 0 to 10*

Q3. Now, think about next year at this same time. Where do you think you will be on the ladder a year from now?
Scale from 0 to 10

4.B.3 Control group

Think of this ladder as representing the importance people attribute to eating breakfast. At the top of the ladder are the people who believe the breakfast is a very important meal of the day – they tend to eat relatively a lot for breakfast. At the bottom are the people who are believe the breakfast is not important at all – they tend to skip breakfast completely. The higher up you are on this ladder, the closer you are to the people at the very top; the lower you are, the closer you are to the people at the very bottom.



Q1. Where would you put yourself on the scale now?
Scale from 0 to 10

Q2. Now, think about last year at this same time. Where were you on the ladder a year ago?
Scale from 0 to 10

Q3. Now, think about next year at this same time. Where do you think you will be on the ladder a year from now?
Scale from 0 to 10

Intertemporal choice task

Ending questionnaire

Procrastination. 6 item version of the Irrational Procrastination Scale (IPS).

Items (order randomized):

1. I put things off so long that my well-being or efficiency unnecessarily suffers
2. My life would be better if I did some activities or tasks earlier
3. When I should be doing one thing, I will do another
4. At the end of the day, I know I could have spent time better
5. I delay tasks beyond what is reasonable
6. I procrastinate

Answer scale: 1-5 Likert scale (1 = "Very seldom or not true of me" 5 = "Very often true or true with me")

Mood

1. How would you describe your mood today in general? Scale from 1 (Very negative) to 10 (Very positive)

Goal of the study

1. 'What do you think this study was about?' (Open question)

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