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The Marginal Cost of Transparency: Do honest nudges work?

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Abstract

Libertarian paternalism (LP), a term which refers to the practice of “nudging” consumers into making “good” decisions, has grown steadily in popularity in recent years as an alternative to sin taxes and other traditional forms of paternalism. Critics however believe that relying on psychological manipulation is inherently unethical as consumers are typically unaware of the nudge and the intention behind it. While proponents of LP insist that they want LP interventions to be conducted in an ethical manner, there is so far little evidence that LP interventions, when conducted in such a manner, still have the desired effect. In this paper I introduce the term Marginal Cost of Transparency (MCoT), the difference in treatment effect of an LP and what I call a Transparent Libertarian Paternalism (TLP) intervention; a type of LP intervention where consumers are made aware of the nudge and why it is there. The results indicate that the MCoT is not statistically significant from zero and that the answer to the question “Do honest nudges work?” is Yes. Moreover, the results indicate that Autonomy-enhancing paternalism (AEP), a type of paternalist interventions that work to enhance the autonomy of consumers (mainly by providing information) and unlike LP do not rely on psychological manipulation, fares at least as well as the LP/TLP treatments when stakes are high.

1. Introduction

Since the term was coined by Thaler and Sunstein (2003), libertarian paternalism has been a topic of debate among behavioral economists. The term refers to measures that intend to change consumer behavior for their own good (paternalism) without using coercive means (that is, using a “libertarian” approach). It can be thought of as an umbrella term, incorporating various types of so called “nudges”: Changing the order of items on a menu, changing the default option on a corporate pension plan (from opt-in to opt-out) and informing people in a neighborhood of their neighbors’ consumption patterns (such as how much energy the average person in their neighborhood uses) are just a few examples.

Although libertarian paternalism has been embraced by policymakers in several countries as an easy way to “fix” consumer behaviors that they perceive as flawed, libertarian paternalism has been met with far from universal acclaim in the academic community with critics questioning everything from the suitability of the term itself, to the efficacy and ethics of the methods used.

In this paper I conduct an experiment with the ostensible goal of inducing consumers to make more patient choices. As previously stated, for a nudge to be considered ethical the bias it corrects has to have been conclusively proven to be harmful. In the case of discount rates, it can be noted that a high discount rate is associated with among other things obesity (Komlos, Smith and Bogin, 2004), drug addiction (Kirby and Petry, 2004) and risky sexual behaviors (Chesson et al, 2006), all of which are known to negatively affect a person’s physical and mental well-being. I measure the MCoT by assigning participants to four different groups: An LP group, a TLP group, an AEP group and a control group. Conducting an experiment with random assignment allows for the effects of the LP and TLP treatments to be compared directly. The reason for including an AEP treatment in the experiment is that AEP is another potentially viable ethically superior alternative to LP.

One criticism leveled by Binder and Lades (2015), among others, is that most forms of libertarian paternalism use psychological manipulation and the exploitation of biases to achieve the goal of the “choice architect” (the policymaker designing the nudge), and usually without the consumer being aware of the nudge or why it is there. For example, while workers being enrolled in an opt-out retirement saving plan will be informed that they are being enrolled and be provided with information regarding the plan, the same cannot be said for consumers visiting a restaurant whose menu has been designed to induce them to choose the salad over the burger.

Binder and Lades (2015) proposed an alternative they named Autonomy-Enhancing Paternalism (AEP). AEP is technically a subset of LP but with stricter criteria: In order for an intervention to qualify under the AEP umbrella, the intervention cannot rely on the exploitation of psychological biases; instead it must enhance the individual’s autonomy (the ability to make a conscious decision) by, for example, providing more information (public service announcements, nutrition labels on menus, etc.) or by preventing an individual from making a hasty decision by, for example, introducing a mandatory waiting period between the purchase and delivery of a good/service (such as a payday loan) during which the individual can cancel the purchase. Common forms of libertarian paternalism such as changing the default option to the option the choice architect wants the consumer to choose is off limit under AEP, as is the use of framing in the menu example. Felsen, Castelo and Reiner (2013) showed that AEP interventions in general were more acceptable to consumers than LP interventions. Decisions taken under AEP interventions were described by participants as more “authentic” which supports the idea that LP not only infringes on autonomy but does so in a way that reduces utility.

In summary, one may say that while AEP at its core relies implicitly on the assumption of classic liberalism that consumers will do what is best for them if given all the necessary

information and enough time to make a decision, LP is based on the more pessimistic view common among behavioral economists where consumers cannot be relied on to make good decisions even if provided with full information.

While AEP has a clear ethical advantage over LP, it is not without disadvantages: It is conceivable that providing nutrition information on restaurant menus could cause a loss of utility for all consumers who are buying high (or even moderate) calorie meals *even* if their action is rational, by inducing guilt and/or shame that may not be rational. While these consumers may be aware that they are eating an unhealthy meal, having the nutrition information “pushed down their throats” may put a damper on the mood even if the meal is for example part of the celebration of a special occasion. Such an AEP intervention could also serve to worsen the conditions of those who suffer from eating disorders such as anorexia who are prone to obsess about the calories in the foods they consume. It is an open question what information consumers need to make good choices (and who is capable of deciding that and why), and what to do if different sets of consumers need different information (i.e. anorectics who do not need calorie information). This of course is a recurring problem in public policy when different groups require different things, but the same thing has to be applied to everyone, and neither AEP nor LP escapes from this dilemma (it is neither possible to provide nutrition labels only to those who need them, or to set the default option differently for different individuals).

Lusk (2014) criticizes libertarian paternalism by arguing that consumers in the real world have an incentive to self-regulate by engaging in activities that prevent their biases from harming them, such as by having their pension fund contributions directly deducted from their paychecks to ensure they do not accidentally spend that money. Lusk (2014) argues that nudges reduce (or even eliminate) the incentive to self-regulate, and consumers may come to rely on nudges to prevent them from making bad choices. This criticism may however also to

some extent apply to AEP interventions, as consumers may come to expect any potentially (not just obviously) harmful products or services to come with information detailing the hazardous effects.

Furthermore, as Berg and Gigerenzer (2010) points out, behavioral economists too often act like their neoclassical counterparts by ignoring the process that leads to a consumer decision being made. This is especially relevant when designing AEP interventions; what information should be provided, and at what stage? Simply running regressions may not necessarily tell us that.

Hence while AEP solves some of the ethical issues associated with libertarian paternalism, to apply it in an effective way may often require more information (that in some cases may not be available) than traditional nudges.

In their book *Nudge* (2003) Thaler and Sunstein assure the readers that they want nudging to be carried out in an ethical, transparent manner (they do not describe in any further detail what they mean by this). Curiously, however, they do not further define what an ethical nudge looks like, or what the “red lines” are that choice architects are not allowed to cross lest their nudges become unethical. Furthermore, the vast majority of nudging case studies they present to bolster their case lack any semblance of transparency, and there is no way to know whether the nudges would work as well or at all, had the choice architects been transparent about their work and intentions.

While Thaler and Sunstein (2003) do not define what an ethical nudge is, it is a question worth asking. An ethical nudge should be transparent, meaning the nudge is designed in such a way that consumers can tell that the nudge is there, why it is there, and have a clear path towards choosing another option than the one they are nudged towards. Finally, it should be correcting a bias (or other behavior) that has been conclusively proven to reduce utility. The

question remains however whether or not a nudge designed in this manner remains capable of altering consumer choices.

If it is in fact the case that transparency does not harm the efficacy of a nudge, then this provides a potential “third option”, a compromise of sorts between the LP and AEP approach: Honest nudging, or Transparent Libertarian Paternalism (TLP). A TLP intervention would be identical to an LP intervention, with the exception that consumers are explicitly informed (for example through a written disclaimer) that they are being nudged and why. This approach solves one of the major ethical issues with LP which is the lack of transparency and by extension lack of accountability of the choice architects.

The question remains however whether or not hidden nudges outperform transparent nudges, and if so to what extent. This defines the “*Marginal Cost of Transparency*” (MCoT), the difference between the treatment effect of a standard libertarian paternalist treatment and a transparent libertarian paternalist treatment.

As previously mentioned the experiment I conducted had three treatment groups (LP, TLP and AEP) and one control group: In the LP treatment group the default option was set to the larger-later option, meaning participants had to make an active choice (by checking a box) in order to receive the smaller-sooner option. The TLP treatment was identical except for a disclaimer in capital letters informing participants of the nudge and why it was there. This was done to allow consumers to retain a greater degree of autonomy which would be lost had they unknowingly been manipulated into choosing a certain option. In the AEP treatment there was no default option but participants are instead provided with a list of arguments in favor of choosing the larger-later option (the arguments and the disclaimer message can both be found in the appendix). In the control group there was no default option nor are participants provided with any arguments in favor of either option.

This study tests two main hypotheses: First of all that the TLP treatment will be statistically insignificant; in other words that transparency renders a nudge useless. Secondly, that the difference between the LP and AEP treatment will be statistically insignificant.

2. Methodology

To test the hypotheses regarding the relative efficacy of first and foremost TLP and LP and secondly LP/AEP I conducted an online experiment between the 18th of October and the 17th of November 2016. In total 1552 participants completed the experiment. Participants were mainly recruited through social media including Facebook, Reddit and Twitter, as well as the Swedish news website Avpixlat and an email invitation sent out to all students at the Department of Finance, Economics & Accounting at Maynooth University.

The incentive structure of this experiment was similar to the one developed by Coller and Williams (1999): Three participants were randomly selected to be paid based on their stated preference for one pre-selected task (task #7). These participants were not aware at the time they took part in the experiment that they would be paid nor were they or any other participants aware of which task was the “real” task. All participants were informed of the incentive structure before agreeing to take part in the experiment, but they were not informed of the hypotheses as that may have biased the results. The three selected participants were contacted via email and paid through PayPal.

While existing literature has been unable to show unambiguously that discount rates are influenced by real incentives, it also has not been able to rule it out (Frederick, Loewenstein and O’Donoghue, 2002). Therefore as a matter of caution this experiment used real incentives. This experiment was self-funded and limiting the number of paid participants was necessary due to budget limitations.

Participants were randomly assigned into one of four groups: The LP treatment group, the TLP treatment group, the AEP treatment group, and the control group. As the platform did not allow for true randomization, the first question asked participants during what part of the

month (first week, second week etc.) they were born, and based on their answers they were assigned to different groups.

In the second part of the experiment, all participants were told that they had won the lottery and were asked to choose between a prize of 20/50/250 euro¹ in 1 week/1 month/6 months and 40/100/500 euro in 1 month/6 months/12 months, which made for a total of nine intertemporal choice tasks. Different sized rewards were used as it has been shown (Thaler, 1981) that the discount rate tends to fall as the size of the reward goes up, hence it seems within reason to suspect that demographic and treatment variables may have different impact on different sized rewards (i.e. some may only affect the lowest rewards, some only the highest).

This experiment used choice tasks; that is, tasks where participants are asked to choose between a smaller-sooner reward and a larger-later reward. Such tasks provide less precision in measuring discount rates, yet they are preferable since they are the closest equivalent to the type of intertemporal choices faced by most consumers on a daily basis which consist of a choice between one fixed amount now and another fixed amount at a specific later point. One option would have been to use “matching” tasks where participants are asked to match how much money they would need at a certain point in the future for it to be equivalent to a specific amount of money today. However such matching tasks, while they do provide precise measurements of discount rates, are very rare outside of experiments; there are very few if any real life situations where consumers are asked to “match” a certain amount in the future with another amount today. Because of this, as discussed by Frederick, Loewenstein and O’Donoghue (2002), consumers tend to rely on heuristics when solving matching tasks that they would not rely on outside of the experiment. Finally, matching tasks are relatively

¹ Participants were provided with exchange rates for USD and SEK.

time consuming and may reduce the number of participants who actually complete the experiment and/or stay focused throughout its duration.

In addition to matching tasks rating tasks were also considered, but ultimately deemed inferior to choice tasks as they, just like matching tasks, do not resemble any real life situation and additionally they may be sensitive to extremeness aversion (Tversky and Simonson, 1993).

The number of tasks was kept at the relatively low number of nine for two reasons: First of all because survey completion rates have been shown to have a negative relationship with the number of questions (Galesic and Bosnjak, 2009) and secondly because the generalizability of the experiment would be reduced by too many tasks, as there are very few real life scenarios where a consumer would face dozens of intertemporal choices at once and little is known regarding whether consumers act differently when faced with a large number of choices compared to a small number.

In the libertarian paternalist treatment group, the default option was set to the larger-later option and participants had to make an active choice by checking a box if they wanted the smaller-sooner option (meaning the treatment relied on the status quo bias).

In the transparent libertarian paternalist treatment group, the default option was, just like in the LP treatment group, set to the larger-later option. However, participants in this treatment group were explicitly told about the default option and the purpose behind it (before the choice tasks there was a message written in all-caps conveying this information). As such, while the treatment still had a nudge, it had a greater degree of transparency and did not seek to unknowingly manipulate participants in the way the LP treatment did.

In the AEP treatment group, nudges were foregone entirely in favor of providing participants with a list of reasons why they should choose the larger-later option (the list, together with the rest of the survey, can be found in the appendix). Participants were also asked which

argument they found the most convincing, and those who stated that they hadn't read the list were not included in the regressions as they could not be considered part of the AEP group – one option would have been to include them in the control group, but this was rejected as there is no way to know whether they may have read (and as such been influenced) by a few of the reasons or whether they didn't read any at all.

Participants in the control group were neither provided with a list of arguments nor exposed to a default option.

The third and final part of the survey was identical for all participants and consisted of demographic questions covering age, marital status, gender, education, in which part of the world the participant resided and whether the participant was currently enrolled at university (participants were not required to provide any identifying information). This section also asked questions regarding saving and the participant's attitude towards it (see appendix for complete list). Notably, this survey did not ask for the annual income of participants, even though it is conceivable that it may affect the discount rate. This is for a number of reasons: First of all, a large number of participants – likely mainly those with low incomes – would be reluctant to provide that information. Secondly, what is considered a high income in one location may not be a high income in another location; a person making a high salary in Mexico may still make less than the average American. This issue exists even within countries; a salary high enough to afford a very comfortable lifestyle in rural US may not be nearly high enough to afford even a decent lifestyle in Manhattan or San Francisco. Thirdly, income is far from a perfect predictor of lifestyle; students for example generally have low incomes but also do not have the same expenses that adults out of college tend to have – students tend to save money by living in dorms or at home, most of them do not need a car as they live close to college, they receive student discounts in many shops etc. and may therefore appear poorer than they really are. The same can be said of retirees.

The final part of the survey also contained two trick questions designed to find out whether the participants had paid attention while reading the instructions of the survey (this was inspired by Haan and Linde, 2011). These questions were “How many participants who take this survey will be paid?” and “How many intertemporal choice scenarios (questions where you were asked to choose between a smaller-sooner and larger-later reward) were there on the previous page?” The answer to the first question was provided in the introduction to the survey, and to answer the second question the participant only needed to remember how many tasks he or she had just completed on the previous page.

The second-to-last question asked participants for their email address so that they could be contacted and paid if they were one of the three selected participants. The last question was a comment field where participants could leave feedback and request to take part of the findings from the experiment. The “feedback form” was included for two reasons: First of all because this feedback may be used to improve the design of future experiments. Secondly because the feedback of some participants may indicate that they did not understand the experiment and their role in it, and in that case their data could be removed from the experiment before statistical analysis took place.

As mentioned above the rewards in this experiment varied from 20, 50 and 250 euro for the smaller-sooner option to 40, 100 and 500 for the larger-later option (the smaller-sooner reward was always half of the larger-later reward). Given the magnitude effect (Thaler, 1981) we cannot expect smaller rewards to be discounted at the same rate as larger rewards, and it is conceivable to think that a treatment that works on a smaller (larger) reward may not work on a larger (smaller) reward, which is why this experiment used rewards of different sizes.

Conducting this experiment online allowed for a larger and more diverse sample than traditional experiments conducted on college campuses. This experiment has 1552 participants; more than one could reasonably fit into most rooms. The experiment was far

more diverse than most experiments, with hundreds of participants from all age group groups, both genders, married as well as non-married etc.

The internet also provides a greater degree of anonymity than traditional lab experiments, potentially reducing the observer effect that otherwise may lead to participants acting unnaturally, which would reduce the generalizability of the results. Finally participation in an online experiment requires less time and effort on behalf of the participant, meaning even those who would not find it worthwhile to participate in a lab experiment may take the time to participate in this experiment, which reduces the self-selection problem associated with economic experiments.

However, as identified by Wright (2005), an online experiment is also associated with drawbacks not present in a lab experiment: One and the same participant could potentially take the experiment multiple times, although this risk was mitigated by making it impossible to take the experiment multiple times from one and the same computer; meaning any “cheaters” would have to use separate computers or internet-connecting devices. Given the relatively low incentives in this experiment, it is highly unlikely that more than at most a few participants found this worthwhile.

Participants may also be suspicious of the financial incentive and may suspect that the experiment is a scam. However, this risk was mitigated by reassuring participants in the introduction to the experiment that they would not have to provide any banking details to receive payment. There is also the issue of distractions; while in a lab experiment participants tend to be in a quiet room with nothing else to do than completing their tasks, this is not the case with in online experiment where participants may be distracted by other web content such as popup notifications. However, this does not necessarily have to be a negative feature as economic decisions are usually taken in “noisy” environments such as shopping centers, as such the “environment” provided by this experiment is closer to the kind of environment

where real life intertemporal choices are made. This experiment also allows participants to take part from the comfort of their own homes, possibly making them more relaxed and prone to act naturally. Hence, it could be argued that what seems to be a disadvantage is actually an advantage as it improves the generalizability of the experiment.

Finally, conducting an intertemporal choice experiment online has the disadvantage of not being able to pay participants immediately upon completion as could be done in a lab experiment. As such, the shortest delay in an online intertemporal choice task cannot be zero if real incentives are to be used (as in this experiment), as many participants would certainly figure out that it would be impossible to pay rewards immediately and that tasks that gave the option of receiving money immediately were hypothetical. Therefore, the shortest delay in this experiment was set to one week. While this may mean that the experiment may fail to capture some of the “present bias”, this is not a grave concern as discount rates appear to be falling for at least one year from the present time (Frederick, Loewenstein and O’Donoghue, 2002) and so most of the present bias is likely to still be present even though participants cannot choose to receive the reward immediately.

3. Results

These results were obtained by running logistic regression using the responses to the intertemporal choice tasks as the dependent variables. As there were nine tasks, there are also nine dependent variables, all of them binary making them suitable for logistic regression. For the sake of simplicity the coefficients are expressed as odds ratios expressing the likelihood of a participant in the relevant group choosing the larger-later option for that particular task, relative to the likelihood of a participant in the control group making the same choice.

After estimating the regression parameters, Wald tests were used to determine whether or not the LP treatment effect differed significantly (at a 5 % level) from the TLP or AEP treatment effect, and whether the TLP treatment effect differed significantly from the AEP treatment effect.

From the total sample size of 1552 those who answered “I’d rather not say” to any of the demographic or personal choice questions were dropped if relevant to the regression (hence a person who did not wish to reveal their gender would be left in for regressions that did not include demographic variables). Additionally those in the AEP group who had indicated that they had not read the list of arguments provided were dropped.

There are a total of four sets of regressions below², first of all they differ in that two of the sets have no control variables while the other two sets contain control variables that 1) were correlated (at a 10 % significance level) with either of the treatment variables and 2) were not the type of control variable that may have been affected by the treatment (a participant who

² Regressions were also run with only demographic (age, gender, marital status, location, whether a participant was currently a full-time student, and highest level of education achieved) control variables, with only personal choice control variables (whether or not an individual saves, thinks he/she should save more, if so why he doesn’t, whether he/she thinks saving is a moral obligation, whether he/she budgets, and whether or not he/she thinks he is influenced by framing and by psychologically manipulative advertising), and with both demographic and personal choice variables.

has just been exposed to the LP treatment may for example be more likely to concede being affected by psychological manipulation). The other difference is that for two of the sets all participants who failed either trick question were dropped, while for the other two sets only those who failed both questions were dropped. The reason for including the latter two sets is that it seems reasonable to assume that some of those who failed only one trick question may still have taken the experiment seriously but have paid too little attention to remember the instructions (in which they could find the answers to the trick questions). In the real world many consumer decisions (especially those involving low amounts of money) are taken by consumers while they are not paying full attention, and the inclusion of these two sets can provide insights on the effect of different treatments on consumers decisions made while consumers are not being fully attentive.

In the regression output, below, LP refers to libertarian paternalism; TLP refers to transparent libertarian paternalism, AEP to autonomy-enhancing paternalism, MCoT to the Marginal Cost of Transparency, and Wald refers to the likelihood of achieving the Wald statistic when testing the hypothesis that LP equals AEP/TLP or that TLP equals AEP. Results significant at a 5 % level are in **bold**.

Table 1: Data summary

To find the percentage of the sample that belonged to a specific group, simply multiply by 100 (i.e. 27.51 % of the sample were in the control group).

Variable	Mean	Variable	Mean
control	0.2751289	other	0.0167526
lp	0.2113402	married	0.4162371
tlp	0.2164948	student	0.1082474
aep	0.2970361	incentiveright	0.5296392
onemonth40	0.9246134	icsright	0.3627577
onemonth100	0.9278351	yessavemore	0.4664948
onemonth500	0.9130155	nomoney	0.2474227
sixmonths40	0.7532216	motivation	0.1681701

sixmonths100	0.7951031	forgetfulness	0.0367505
sixmonths500	0.8530928	male	0.8305412
Variable	Mean	Variable	Mean
twelvemonths40	0.8627577	yesmanipulated	0.3054124
twelvemonths100	0.8820876	highschool	0.2893041
twelvemonths500	0.8853093	undergrad	0.2358247
age1823	0.0708763	postgrad	0.4104381
age2435	0.1475515	savepension	0.0760309
age3664	0.6204897	saveprivate	0.5831186
ageover64	0.1507732	yesframing	0.5921392
weuropeaus	0.8833763	yesmoral	0.4072165
nonweurope	0.0296392	yesbudget	0.5708763
northamerica	0.0579897	email	0.6082474

Table 2: Control variables included, participants who failed either trick question dropped

Odds_{LOGIT} = $p([\text{reward}] = 1) / (1 - p([\text{reward}] = 1)) = e^{\beta_n X}$ (where X includes lp, aep, tlp, age1823, ageover64, student, highschool, postgrad, yesbudget, email)

One week vs one month, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	3.0463	2.0095	6.9769	1.0368	0.5842	0.3822	0.153
SD	2.0675	1.1498	5.5519				
P> z 	0.101	0.223	0.015				

One week vs one month, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.2856	2.447	3.1745	-0.1613	0.9354	0.7005	0.7594
SD	1.5967	1.7053	2.2229				
P> z 	0.237	0.199	0.099				

One week vs one month, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.3887	1.7606	10.578	-0.3719	0.7083	0.0656	0.1075
SD	0.7657	1.0174	11.2783				
P> z 	0.552	0.328	0.027				

One month vs six months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.5726	2.4192	1.1568	-0.8466	0.3113	0.4248	0.0663
SD	0.5848	0.9479	0.3985				

P>|z| 0.223 **0.024** 0.672

One month vs six months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.9744	3.7564	1.48	-1.782	0.1885	0.4873	0.0449
SD	0.7725	1.674	0.5325				
P> z	0.082	0.003	0.276				

One month vs six months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.6449	1.8067	3.117	-0.162	0.8467	0.3043	0.2316
SD	0.7092	0.77562	1.4889				
P> z	0.248	0.168	0.017				

Six months vs twelve months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.024	1.7693	0.41	-0.7453	0.362	0.0531	0.0083
SD	0.514	1.026	0.1797				
P> z	0.962	0.325	0.042				

Six months vs twelve months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.541	4.1372	0.9093	-1.5962	0.519	0.0734	0.0254
SD	1.4311	2.7654	0.4049				
P> z	0.098	0.034	0.831				

Six months vs twelve months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.3383	1.3322	2.529	0.006	0.9929	0.2825	0.2776
SD	0.6348	0.6289	1.4148				
P> z	0.539	0.543	0.097				

These results suggest, contrary to the first hypothesis of this experiment, that there is no marginal cost of transparency. There is little indication that the transparent libertarian paternalist treatment fares any worse than the standard LP treatment. Only in one case (the first task) in two sets of regressions does the LP treatment outperform the TLP treatment as measured by the Wald test – whether this is merely statistical noise or due to there actually

being an initial cost of transparency that “wears off” rapidly as the participants move on through the tasks one cannot tell with certainty based on this experiment.

It is noteworthy however that the TLP treatment outperforms the LP treatment in the regression sets where all participants who failed either trick question have been dropped. This suggests that TLP works better when participants pay more attention, which at first seems counterintuitive as those who are not paying attention ought to be more likely to miss the disclaimer revealing the existence and purpose of the default option nudge. In other words, it appears the disclaimer has if anything a positive impact on the efficacy of the nudge.

This brings us to the question of how consumers can be nudged even when they know that they are being nudged. The likeliest explanation in my view is that during any LP treatment, some people will figure out that they are being manipulated and “lash out” against the choice architect by actively doing the opposite of what the architect wants. In this case, once a participant realizes that the choice architect is trying to manipulate him/her to choose the larger-later option, and out of resentment over this manipulation, he/she then chooses the smaller-sooner option. Gustavsson (2016) showed that the effect of an LP treatment could backfire if repeated enough times, presumably as more and more participants figure out what the choice architect is doing and lash out against it.

Why does this backlash not occur with the TLP? Quite simply nudging appears to be a case where honesty pays. By informing the participants that there is a nudge and why, participants no longer feel the need to “lash out” against the choice architect once they found out, as they don’t experience the same feeling of having been deceived and manipulated.

It also cannot be ruled out that participants are still affected by the default option on a psychological level even though they know why it is there, similar to how humans can

experience a placebo effect even when they know that they are taking a placebo (Schafer, Colloca and Wager, 2015).

Finally, it seems plausible that the disclaimer, which again states that there is a nudge to induce participants to choose the larger-later option, induces participants to think of reasons why they should choose the larger-later option, even though (unlike in the AEP group), no arguments are provided. It can be assumed that most people know of at least one reason to save of the top of their head, and the disclaimer may cause them to think of this reason(s).

It is worth keeping in mind that a large proportion of the participants in this survey are Swedish³. While English proficiency in Sweden is very high it cannot be ruled out that some participants in the TLP group did not understand the meaning of the disclaimer informing them about the nudge. It is however unlikely that this had any greater effect on the results as it is unlikely that many participants simultaneously had a such a poor grasp of English that they could not understand the disclaimer while simultaneously a good enough grasp of English to pass both trick questions.

Turning attention to the AEP treatment, there is a great difference between the two sets where participants needed only to have passed one trick question and those where they needed to have passed both. AEP has a great effect in the latter case, likely because (as discussed) these participants were paying more attention to the experiment, which likely translated to paying more attention to the list of arguments provided in the AEP treatment. It should not come as a surprise to anyone that in order for a list of arguments to be effective in convincing a consumer to pursue a certain course of action, the consumer has to pay attention to the arguments.

³ This was a side effect of extensive promotion of the experiment on Swedish-language websites.

The AEP treatment also appears to work better when rewards are large. This may be because participants are more likely to stop and consider their actions carefully when large amounts are at stake. This may have made the list of arguments they had just been provided more persuasive as with the smaller rewards participants may have simply not bothered to think too hard about the decision and instead just used their intuition.

In summary what these results suggest is that honest nudges do work, and that reasoning with consumers may work even better provided that stakes are high enough and that consumers are paying attention.

Moving on to the demographic variables, first it should be noted that the randomization was generally successful with few demographic groups being overrepresented among the treatment groups. The correlation coefficients indicate that those aged 18-23 and those who are students are significantly less likely to choose the larger-later option for seven out of nine tasks (after removing participants who failed both trick questions). There is a severe overlap making it difficult to distinguish between these two groups, and as such we cannot tell whether people aged 18-23 are less patient because they are students, or the students are less patient because they are aged 18-23. It is interesting however to note that not only are students less patient, but they also appear to think about saving in a different way, being less likely to think of saving as a moral obligation and also less likely to think that people in general should save more. One question in the last section of the survey asked participants whether their attitude towards saving had become more positive, more negative or stayed the same as they had become older, and an overwhelming majority responded that they had become more positive to saving as they had aged, which is consistent with these findings. Mischel and Metzner (1962) found that discount rates were negatively correlated with age at least until the age of 12 (their sample used children aged 5-12), while Steinberg et al (2009) found that discount rates keep falling at least until the age of 16. This study suggests that discount rates

continue to fall even once individuals have reached the age of legal adulthood, and may stabilize only sometime in the mid-early 20's.

It is also interesting to note that holding a postgraduate degree is positively correlated with the discount rate while holding an undergraduate degree is not. In economic terms an education is an investment, and so one would reasonably expect that anyone who invested more than the bare legal minimum in education would have a lower discount rate compared to those who don't. How then could it be that undergraduate degree holders are no more patient than those who do not hold a degree? There are several reasons that may contribute to this: First of all while attending college is an investment, it is far from certain that all students think of it that way. Some students may attend college not to increase their lifetime income, but instead to experience college life and achieve personal development (to these people, college is not a sacrifice as it leads to short-term gratification). Additionally many students may have been pressured (or possibly bribed) into attending college by their parents, in which case their attending college isn't a reflection on their own economic patience but rather that of their parents. Holding a postgraduate degree is not associated with a lower discount rate in the tasks with short delays (one week vs one month), which makes sense as the decision to pursue a postgraduate degree is typically made several months in advance of actually commencing the degree and so does not involve the "present bias" component of the discount rate.

Not surprisingly, participants who state that they save privately are more likely to choose the larger-later option, while those who save through pension plans are not. This may be because many pension plans have the contributions deducted automatically from the paychecks of the participants, meaning they don't have to make an active choice every month not to spend all the money that they are paid.

Counterintuitively, participants who state that they budget their consumption have higher discount rates than those who don't. At first appearance this may seem strange as budgeting is

a choice one would normally associate with patience and the ability to plan ahead. One possible explanation is that participants who budget their consumption are aware that they suffer from a present bias. They are, as O'Donoghue and Rabin (1999) put it, sophisticated consumers who are aware of their short-comings and attempt to plan ahead to stop their "future selves" from making the wrong decisions. This explanation is further supported by the fact that believing that one should save more than one currently does is associated with a higher, not a lower discount rate – clearly there are a lot of economically impatient participants who are aware that their behavior isn't optimal. Another explanation is that consumers with lower incomes may be more prone to budgeting (as they have to be more careful not to overspend), and these participants would also be more likely to choose the smaller-sooner option as they are liquidity constrained. These explanations are obviously not mutually exclusive.

Being married is positively correlated with choosing the larger-later options in for 3 out of 9 tasks. It may be that being in a team with another individual that one cares about induces more careful, patient decision-making, but it may also be the case that patient individuals are more likely to get married, that the maturity that comes with age acts both to reduce discount rates (as indicated by this experiment) and to increase the likelihood of getting married, or that having two incomes (as most married couples have) means they can afford to save. Further research is necessary in this area.

Contrary to the findings of Silverman (2003) and McLeish and Oxoby (2007), the results from this experiment indicate that men are more patient than women. It is conceivable to think that this may be due to women earning less than men and thus being more prone to being liquidity constrained (Arulampalam, Booth and Bryan, 2007). Liquidity constraint due to lower income is also a probable explanation as for why participants in the non-western parts of Europe are

less patient than those in Western Europe; these regions, being poorer, likely have more consumers who are liquidity constrained.

Cubitt and Read (2007) question whether time preferences can be properly elicited through experiments. The time preferences expressed in this experiment are largely consistent with the expectations of economic theory, with savers having lower discount rates than non-savers, retirees having high discount rates due to dis-saving, those with advanced degrees having low discount rates etc. Hence, while other experiments may suffer from a lack of generalizability due to homogenous samples, self-selection and a range of other issues, this experiment appears to have succeeded in replicating the kind of savings behavior one would expect from different groups, meaning it is likely that the effects from the different treatments as shown in this experiment would also carry over to the real world.

Conclusions

These results indicate that nudging can work even if conducted in an open, transparent manner. While that may be seen as a victory for libertarian paternalism, if we accept these results it also means that the way that nudges are commonly being used today – without transparency – is not just an ethically questionable way of changing consumer behavior, but an ethically questionable way that carries no gain as the same results can be achieved through transparent means.

Furthermore, these findings also suggest that AEP under the right circumstances can be even better than LP/TLP, and it can be argued that if the same or similar results can be obtained using an AEP treatment, then an AEP treatment should be used as it relies on informing consumers. TLP, while more ethical than LP, still will not teach a consumer anything he or she did not already know, and when the TLP nudge is gone, the consumer's behavior is almost certain to reverse.

Judging from the results of this experiment, the problem with AEP interventions is that they require consumers to pay attention for them to be effective while LP interventions seem to work regardless (which makes sense as AEP still requires an active choice). From a policy standpoint, this means that AEP interventions should mainly be used when one can be reasonably assured that consumers will be paying attention, and they should be designed in such a way as to grab attention. Finally, it is important to note that some AEP interventions may be less reliant on consumers paying attention to them (such as mandatory cooling off periods).

Future research will investigate whether there may be a marginal cost of transparency in contexts other than intertemporal choice as this experiment cannot tell us whether consumers would act the same way outside of an intertemporal choice context. This is something that needs to be ascertained before TLP interventions can be used widely by policy-makers.

Likewise, future experiments will focus on determining whether TLP and match LP using other nudges than the default option nudge. Future experiments will also be provided in several languages to ensure that all participants understand the instructions. Finally, future experiments will further investigate the link between age and savings behavior to find out whether there is a causal link between them.

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Appendix A: Survey

Welcome!

My name is John Gustavsson and I'm a PhD student at Maynooth University at the Department of Economics, Finance and Accounting.

This survey is an experiment that is part of the research I am doing for my doctoral thesis. In this survey, you will be asked a number of questions about how you value future income relative to present income – what we economists call “inter-temporal choice”.

You will be posed with a number of scenarios and asked how you would act in them (there will be two options in each scenario). These are not purely hypothetical scenarios; three (3) of you who answer this survey will be paid in accordance with how you answer one (1) of the scenarios. The three who are paid will be randomly selected; your answers have no bearing whatsoever on your likelihood of being one of them.

The final part of this survey contains demographic questions (age, gender, what part of the world you live in, education level, whether you are currently a student and marital status) as well as some questions on consumer behavior and attitudes. If you are uncomfortable with answering a demographic question, simply choose the option “I'd rather not say” (or write N/A in the box) which is provided for every demographic question. All data will be stored in a password-protected folder stored in the university system, and there will be no further use of the data beyond this study.

You will be asked to provide me with your email address at the end of the survey – this is so that I can contact you in case you are one of those who have been selected to be paid. You are not required to provide your email address, but if you don't I won't be able to pay you. You will not need to provide your bank account details to receive payment. The email addresses will be stored (in a separate password-protected folder) only until the selected participants

have been paid, while the rest of the data will be retained for research purposes. You may quit the survey at any time; if you quit before finishing the survey, your data will be deleted. You can also withdraw your data at any time by emailing me at the email address provided below.

It must be recognized that, in some circumstances, confidentiality of research data and records may be overridden by courts in the event of litigation or in the course of investigation by lawful authority. In such circumstances the University will take all reasonable steps within law to ensure that confidentiality is maintained to the greatest possible extent.

If you're interested in learning the findings of this study you're more than welcome to do so; simply indicate your interest when answering the final question.

If you have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie.

You must be 18 or older to participate in this survey. This survey will take approximately 15 minutes to complete, obviously depending on how much time you spend thinking about your decisions. Please read the descriptions on the next page carefully.

By proceeding, you agree to take part in this survey, and have your data stored under the conditions outlined above. Thank you for your participation!

Q1: What time of the month is your birthday?

Between the 1st and 7th of the month

Between the 8th and 14th of the month

Between the 15th and 21st of the month

After the 22nd of the month

CONTROL GROUP

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q2: You win the lottery and your prize is to receive either 40 euro in one month, or 20 euro in one week. What do you choose?

20 euro in one week

40 euro in one month

Q3: You win the lottery and your prize is to receive either 100 euro in one month, or 50 euro in one week. What do you choose?

50 euro in one week

100 euro in one month

Q4: You win the lottery and your prize is to receive either 500 euro in one month, or 250 euro in one week. What do you choose?

250 euro in one month

500 euro in one month

Q5: You win the lottery and your prize is to receive either 40 euro in six months, or 20 euro in one month. What do you choose?

20 euro in one month

40 euro in six months

Q6: You win the lottery and your prize is to receive either 100 euro in six months, or 50 euro in one month. What do you choose?

50 euro in one month

100 euro in six months

Q7: You win the lottery and your prize is to receive either 500 euro in six months, or 250 euro in one month. What do you choose?

250 euro in one month

500 euro in six months

Q8: You win the lottery and your prize is to receive either 40 euro in twelve months, or 20 euro in six months. What do you choose?

20 euro in six months

40 euro in twelve months

Q9: You win the lottery and your prize is to receive either 100 euro in twelve months, or 50 euro in six months. What do you choose?

50 euro in six months

100 euro in twelve months

Q10: You win the lottery and your prize is to receive either 500 euro in twelve months, or 250 euro in six months. What do you choose?

250 euro in six months

500 euro in twelve months

AEP TREATMENT GROUP

Below, you will be presented with a number of scenarios – you will be asked to choose between a smaller reward received soon, and a larger reward received later. Before you make your choices, here are a few things that I would like you to take into account:

- 1) Choosing the “later” option means you have something to look forward to.
- 2) Saving means you’ll be better off in the event of a “rainy day”
- 3) Every decision that we make is influenced by the choices we’ve made in the past. By choosing the larger-later option now, it’ll be easier to do the same in the future – you can establish (or strengthen an already existing) good habit.

4) The interest rate is 100 %, or to put it another way on an annual basis in the first three scenarios (one week vs one month, see below) the interest rate is 170 681%, while in the second (one month vs six months) and last third (six months vs twelve months) of the scenarios it is 428 % and 300 % respectively.

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q11: You win the lottery and your prize is to receive either 40 euro in one month, or 20 euro in one week. What do you choose?

20 euro in one week

40 euro in one month

Q12: You win the lottery and your prize is to receive either 100 euro in one month, or 50 euro in one week. What do you choose?

50 euro in one week

100 euro in one month

Q13: You win the lottery and your prize is to receive either 500 euro in one month, or 250 euro in one week. What do you choose?

250 euro in one month

500 euro in one month

Q14: You win the lottery and your prize is to receive either 40 euro in six months, or 20 euro in one month. What do you choose?

20 euro in one month

40 euro in six months

Q15: You win the lottery and your prize is to receive either 100 euro in six months, or 50 euro in one month. What do you choose?

50 euro in one month

100 euro in six months

Q16: You win the lottery and your prize is to receive either 500 euro in six months, or 250 euro in one month. What do you choose?

250 euro in one month

500 euro in six months

Q17: You win the lottery and your prize is to receive either 40 euro in twelve months, or 20 euro in six months. What do you choose?

20 euro in six months

40 euro in twelve months

Q18: You win the lottery and your prize is to receive either 100 euro in twelve months, or 50 euro in six months. What do you choose?

50 euro in six months

100 euro in twelve months

Q19: You win the lottery and your prize is to receive either 500 euro in twelve months, or 250 euro in six months. What do you choose?

250 euro in six months

500 euro in twelve months

Q20: Which argument in favor of choosing the larger-later option did you find the most convincing?

The “Something to look forward to”-argument

The “Rainy day”-argument

The “good habit”-argument

The interest rate-argument

No difference

I didn't find any argument convincing

I didn't read them

LP TREATMENT GROUP

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q21: You've won the lottery and your prize is to receive 40 euro in one month, or 20 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q22: You've won the lottery and your prize is to receive 100 euro in one month, or 50 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q23: You've won the lottery and your prize is to receive 500 euro in one month, or 250 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q24: You've won the lottery and your prize is to receive 40 euro in six months, or 20 euro in one month. If you would prefer to receive 20 euro in one month, please tick this box.

Q25: You've won the lottery and your prize is to receive 100 euro in six months, or 50 euro in one month. If you would prefer to receive 50 euro in one month, please tick this box.

Q26: You've won the lottery and your prize is to receive 500 euro in six months, or 250 euro in one month. If you would prefer to receive 250 euro in one month, please tick this box.

Q27: You've won the lottery and your prize is to receive 40 euro in twelve months, or 20 euro in six months. If you would prefer to receive 20 euro in six months, please tick this box.

Q28: You've won the lottery and your prize is to receive 100 euro in twelve months, or 50 euro in six months. If you would prefer to receive 50 euro in six months, please tick this box.

Q29: You've won the lottery and your prize is to receive 500 euro in twelve months, or 250 euro in six months. If you would prefer to receive 250 euro in six months, please tick this box.

TLP TREATMENT GROUP

BEFORE YOU PROCEED, BE AWARE THAT THE DEFAULT OPTION FOR THIS SECTION IS THE LARGER-LATER OPTION (RECEIVING 40/100/500 EURO AFTER A LONGER DELAY RATHER THAN 20/50/250 AFTER A SHORTER). DEFAULT OPTIONS ARE KNOWN TO AFFECT THE DECISIONS MADE BY CONSUMERS AND THE DEFAULT OPTION HAS BEEN SET THIS WAY TO HELP YOU MAKE GOOD, FORWARD-LOOKING CHOICES.

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 44 USD, 390 SEK or 36 Pound Sterling.

Q30: You've won the lottery and your prize is to receive 40 euro in one month, or 20 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q31: You've won the lottery and your prize is to receive 100 euro in one month, or 50 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q32: You've won the lottery and your prize is to receive 500 euro in one month, or 250 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

Q33: You've won the lottery and your prize is to receive 40 euro in six months, or 20 euro in one month. If you would prefer to receive 20 euro in one month, please tick this box.

Q34: You've won the lottery and your prize is to receive 100 euro in six months, or 50 euro in one month. If you would prefer to receive 50 euro in one month, please tick this box.

Q35: You've won the lottery and your prize is to receive 500 euro in six months, or 250 euro in one month. If you would prefer to receive 250 euro in one month, please tick this box.

Q36: You've won the lottery and your prize is to receive 40 euro in twelve months, or 20 euro in six months. If you would prefer to receive 20 euro in six months, please tick this box.

Q37: You've won the lottery and your prize is to receive 100 euro in twelve months, or 50 euro in six months. If you would prefer to receive 50 euro in six months, please tick this box.

Q38: You've won the lottery and your prize is to receive 500 euro in twelve months, or 250 euro in six months. If you would prefer to receive 250 euro in six months, please tick this box.

DEMOGRAPHIC QUESTIONS

Q39: Please state your age

18-23

24-35

36-64

65+

I'd rather not say

Q40: What part of the world do you reside in?

Western Europe

Eastern Europe

Southern Europe

North America

Central America

Australia/NZ/Oceania

Southeast Asia

Middle east

Africa

South America

I'd rather not say

Q41: Are you married?

Yes

No

I'd rather not say

Q42: Are you a full-time student (or a graduate of the class of 2016)?

Yes

No

I'd rather not say

Q43: How many participants who take this survey will be paid?

1

3
5
6
7

Q44: Thinking about your personal finances, do you think you should save more than you currently do?

Yes

No

Don't know

I'd rather not say

Q45: If Yes, why don't you?

I don't feel like I can afford it

Lack of motivation

Forgetfulness

Other/I'd rather not say

Q46: What gender do you identify as?

Male

Female

I'd rather not say

Q47: Do you think you are prone to be affected by psychologically manipulative tactics (such as those commonly employed by advertisers) when making consumer decisions

Yes

No

I don't know

Q48: What is the highest level of education that you have achieved?

High school/Post-primary school or less

Undergraduate degree

Postgraduate degree/Postgraduate diploma

I'd rather not say

Q49: Do you currently save regularly?

Yes, through a pension plan

Yes, privately/both privately and through a pension plan

No

I'd rather not say

Q50: How many intertemporal choice scenarios (questions where you were asked to choose between a smaller-sooner and larger-later reward) were there on the previous page?

7
6
9

12

15

Q51: Do you think that your consumer choices are affected by the order that the options (such as, items in a shop) are presented in?

Yes

No

I don't know

Q52: Do you think people in general should save more, less or about the same as now?

More

Same

Less

No opinion

Q53: Do you think that saving is a moral obligation for those who are able to save?

Yes

No

Q54: Do you usually plan your consumption ahead of time (budgeting)?

Yes

No

I'd rather not say

Q55: Thinking back, do you think your attitude towards saving and whether it's important has changed as you've grown older?

Yes, I'm more positive to saving today than when I was younger

Yes, I'm more negative to saving today than when I was younger

No

I'd rather not say

Q56: Please provide your email address in the field below (this is voluntary but it's necessary for you to have a chance to be paid as I need to be able to get in touch with you).

Q57: Do you have any comments, questions or feedback in general? If you would like to take part of the findings from this study, please indicate this here.

Appendix B: Terminology

Terminology:

control	1 if participant in control group, 0 otherwise.
lp	1 if participant in libertarian paternalist treatment group, 0 otherwise.
aep	1 if participant in autonomy-enhancing paternalist treatment group, 0 otherwise.
tlp	1 if participant in transparent libertarian paternalist treatment group, 0 otherwise.
Onemonth40	1 if participant chose 40 euro in 1 month over 20 euro in 1 week, 0 otherwise.
Onemonth100	1 if participant chose 100 euro in 1 month over 50 euro in 1 week, 0 otherwise.
Onemonth500	1 if participant chose 500 euro in 1 month over 250 euro in 1 week, 0 otherwise.
Sixmonths40	1 if participant chose 40 euro in 6 months over 20 euro in 1 month, 0 otherwise.
Sixmonths100	1 if participant chose 100 euro in 6 months over 50 euro in 1 month, 0 otherwise.
Sixmonths500	1 if participant chose 500 euro in 6 months over 250 euro in 1 month, 0 otherwise.
Twelvemonths40	1 if participant chose 40 euro in 12 months over 20 euro in 6 months, 0 otherwise.
Twelvemonths100	1 if participant chose 100 euro in 12 months over 250 euro in 6 months, 0 otherwise.

Twelvemonths500	1 if participant chose 500 euro in 12 months over 250 euro in 6 months, 0 otherwise
Age1823	1 if participant is between the ages 18-23, 0 otherwise.
Age2435	1 if participant is between the ages 24-35, 0 otherwise.
Age3664	1 if participant is between the ages 36-64, 0 otherwise.
Ageover64	1 if participant is above the age of 64, 0 otherwise.
weuropeaus	1 if participant resides in Western Europe, Australia or New Zealand, 0 otherwise.
nonweurope	1 if participant resides in Europe but not in western Europe, 0 otherwise.
northamerica	1 if participant resides in North America, 0 otherwise.
other	1 if participant resides elsewhere, 0 otherwise.
married	1 if participant is married, 0 otherwise.
student	1 if participant is a student or a graduate of the class of 2016, 0 otherwise.
incentiveright	1 if participant gave the correct answer to the trick question asking how many participants were to be paid, 0 otherwise.
icsright	1 if participant gave the correct answer to the trick question asking how many intertemporal choice scenarios they had completed, 0 otherwise.
yessavemore	1 if participant thinks he or she should save more, 0 otherwise.
nomoney	1 if participant states the reason he or she does not save more is due to lack of money, 0 otherwise.
motivation	1 if participant states the reason he or she does not save more is due to lack of motivation, 0 otherwise.

forgetfulness	1 if participant states the reason he or she does not save more is due to forgetfulness, 0 otherwise.
male	1 if participant is male, 0 otherwise.
yesmanipulated	1 if participant states he or she believes that he or she believes he or she is affected by psychologically manipulative tactics in advertisement, 0 otherwise
highschool	1 if the highest level of education attained by the participant is a high school degree or less, 0 otherwise.
undergrad	1 if the highest level of education attained by the participant is an undergraduate degree, 0 otherwise.
postgrad	1 if the highest level of education attained by the participant is a postgraduate degree, 0 otherwise.
savepension	1 if participant saves through a pension plan, 0 otherwise.
saveprivate	1 if participant saves privately or both privately and in a pension plan, 0 otherwise.
yesframing	1 if participant states he or she believes that his or her choices are affected by framing, 0 otherwise.
yesmoral	1 if participant states he or she believes it is a moral obligation for those who can to save, 0 otherwise.
yesbudget	1 if participant states that he or she plans his or spending ahead of time, 0 otherwise.
email	1 if participant provided his or her email address, 0 otherwise.

Appendix C: Additional regressions

Table 3: Control variables included, participants who failed both trick questions dropped

Odds_{LOGIT} = $p([\text{reward}] = 1) / (1 - p([\text{reward}] = 1)) = e^{B_n X}$ (where X includes lp, aep, tlp, age2435, age3664, undergrad, yesbudget)

One week vs one month, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.4556	0.7028	1.769	0.7528	0.0965	0.6894	0.0227
SD	0.6494	0.2479	0.7324				
P> z	0.4	0.317	0.168				

One week vs one month, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.575	0.9774	1.1708	0.5974	0.3253	0.5353	0.6525
SD	0.7423	0.3827	0.4504				
P> z	0.335	0.953	0.682				

One week vs one month, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	0.7172	0.805	1.8736	-0.088	0.7467	0.0201	0.038
SD	0.2565	0.2817	0.7646				
P> z	0.353	0.535	0.124				

One month vs six months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.6963	1.6406	0.9477	0.0557	0.894	0.011	0.0115
SD	0.3919	0.3593	0.1845				
P> z	0.022	0.024	0.782				

One month vs six months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.2599	1.6805	1.0127	0.5794	0.2907	0.0018	0.0288
SD	0.5821	0.3902	0.2067				
P> z	0.002	0.025	0.951				

One month vs six months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.2405	1.2234	1.4811	0.0171	0.9617	0.5328	0.481
SD	0.3358	0.3139	0.3747				
P> z	0.426	0.432	0.12				

Six months vs twelve months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.4374	1.2472	0.557	0.1902	0.6693	0.0012	0.0028
SD	0.4462	0.3602	0.1353				
P> z 	0.242	0.444	0.016				

Six months vs twelve months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.3943	1.5685	0.8717	0.8258	0.253	0.0027	0.0379
SD	0.817	0.4516	0.2135				
P> z 	0.011	0.118	0.575				

Six months vs twelve months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	0.9786	0.6883	1.2055	0.2903	0.2286	0.4926	0.0427
SD	0.2909	0.185	0.3404				
P> z 	0.942	0.165	0.508				

Table 4: No control variables, participants who failed both trick questions dropped

Odds_{LOGIT} = $p([\text{reward}] = 1) / 1 - p([\text{reward}] = 1) = e^{\beta_n X}$ (where X includes lp, aep, tlp)

One week vs one month, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.3178	0.7385	1.4797	0.5793	0.1353	0.7825	0.0587
SD	0.5189	0.2494	0.5534				
P> z 	0.483	0.369	0.295				

One week vs one month, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.7291	1.1038	0.9875	0.6253	0.3287	0.1974	0.7666
SD	0.7582	0.419	0.3447				
P> z 	0.212	0.795	0.971				

One week vs one month, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	0.7352	0.8776	1.5679	-0.1424	0.6019	0.0396	0.1218
SD	0.2454	0.2997	0.5803				
P> z 	0.357	0.702	0.224				

One month vs six months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.0571	1.8153	1.1033	0.2417	0.5976	0.0038	0.0158
SD	0.443	0.3749	0.1999				
P> z	0.001	0.004	0.587				

One month vs six months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.6906	1.7628	1.1227	0.9278	0.1143	0.0004	0.0391
SD	0.6603	0.3846	0.2139				
P> z	0	0.009	0.543				

One month vs six months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.5062	1.3307	1.4588	0.1755	0.6534	0.7195	0.9046
SD	0.3846	0.3249	0.3408				
P> z	0.109	0.242	0.106				

Six months vs twelve months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.6833	1.4	0.6339	0.2833	0.5594	0.0004	0.002
SD	0.4882	0.3807	0.1416				
P> z	0.073	0.216	0.041				

Six months vs twelve months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.4353	1.6266	0.9007	0.8087	0.2434	0.0014	0.0288
SD	0.7682	0.449	0.2072				
P> z	0.005	0.078	0.65				

Six months vs twelve months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	0.9727	0.7373	1.0524	0.2354	0.3185	0.7788	0.1745
SD	0.2731	0.1935	0.2792				
P> z	0.921	0.246	0.847				

Table 5: No control variables, participants who failed either trick question dropped

Odds_{LOGIT} = $p([\text{reward}] = 1) / 1 - p([\text{reward}] = 1) = e^{\beta X}$ (where X includes lp, aep, tlp)

One week vs one month, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.6707	2.1366	5.9268	0.5341	0.7467	0.3655	0.2312

SD	1.6007	1.1881	4.6209
P> z	0.101	0.172	0.022

One week vs one month, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.6118	2.6471	2.0912	-0.0353	0.9871	0.7758	0.7627
SD	1.79	1.8138	1.2984				
P> z	0.161	0.155	0.235				

One week vs one month, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.0268	1.7381	9.7619	-0.7113	0.3758	0.0359	0.1191
SD	0.5148	0.9918	10.35				
P> z	0.958	0.333	0.032				

One month vs six months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.6918	2.5806	1.0705	-0.8889	0.2983	0.1998	0.0218
SD	0.5858	0.9645	0.3423				
P> z	0.129	0.011	0.831				

One month vs six months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	2.0886	3.7121	1.3364	-1.6235	0.2323	0.2619	0.0241
SD	0.7909	1.6187	0.4566				
P> z	0.052	0.003	0.396				

One month vs six months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.7260	1.9562	2.6969	-0.2301	0.7899	0.3667	0.5232
SD	0.7061	0.8207	1.2050				
P> z	0.182	0.11	0.026				

Six months vs twelve months, 40 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.0013	1.9349	0.4464	-0.9336	0.2583	0.0667	0.0057
SD	0.4789	1.0888	0.1847				
P> z	0.998	0.241	0.051				

Six months vs twelve months, 100 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.8992	4.0123	0.8642	-2.1132	0.3031	0.1311	0.0203
SD	0.9869	2.6492	0.3671				
P> z 	0.217	0.035	0.731				

Six months vs twelve months, 500 euro

	LP	TLP	AEP	MCoT	WaldLP/TLP	WaldLP/AEP	WaldTLP/AEP
Coef	1.2126	1.2305	2.0597	-0.0178	0.9768	0.3378	0.3511
SD	0.5624	0.5704	1.0681				
P> z 	0.678	0.655	0.164				