



Department of Economics Finance & Accounting

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The Marginal Benefit of Manipulation: Investigating paternalistic interventions in the context of intertemporal choice

John S.M. Gustavsson
Department of Economics, Finance & Accounting
Maynooth University
Maynooth, Co. Kildare
Republic of Ireland

Abstract

Libertarian paternalism (LP) has gained popularity in recent years as an alternative way for governments to induce consumers into making “good” decisions. Many, however, question the ethics of such interventions, calling them a form of psychological manipulation, and instead argue interventions should focus on expanding the information set available to consumers and encouraging consumers to reason their way to the right decision. Such interventions are known as Autonomy-Enhancing Paternalism. The question remains how effective such interventions are relative to LP interventions. In this paper I introduce the term Marginal Benefit of Manipulation (MBoM), the difference between the treatment effect of an LP and an AEP intervention. I find that the AEP intervention does not succeed in altering behavior, but the LP intervention does not fare better and may backfire when participants are exposed to it repeatedly. Neither intervention had any significant effect on behavior beyond the immediate present.

1. Introduction

Libertarian paternalism¹ (LP) is a divisive topic in behavioral economics. First introduced by Thaler and Sunstein (2003), libertarian paternalism is a set of interventions intended to “nudge” consumers towards a certain action (or inaction) without limiting freedom of choice. This is done through “choice architecture”, a process through which an architect designs the choice process in such a way as to push consumers towards an action that the architect deems desirable. Examples include setting the desirable action as the default option, giving consumers a “cooling off” period during which they can reverse their decision free of charge, and public service announcements/informational campaigns intended to convince consumers to take (or avoid) a certain course of action.

While the idea of libertarian paternalistic interventions is appealing to some and anathema to others (see Mitchell 2005), two critical questions remain: Do such interventions work, and are they ethically justified?

In their book “*Nudge*”, Thaler and Sunstein cite evidence for the efficacy of libertarian paternalism from areas as diverse as cafeterias to retirement saving plans. By designing a menu in such a manner that the healthy options are easily seen while the unhealthy options are less visible, consumers can be induced to choose a healthy option, while still having the choice of not doing so. And by allowing workers to opt out of a retirement saving plan instead of having to opt in, under-saving can be reduced. This allows policy-makers to give

¹ The term is itself controversial, with some arguing that it is an oxymoron, and that no true libertarian could possibly support libertarian paternalism. Mitchell (2005) argues that since the cost of LP interventions are paid for by all consumers, but LP interventions only help those consumers who otherwise would have chosen “poorly”, LP effectively redistributes wealth from the rational to the irrational, something that runs contrary to libertarian ideology. The source of conflict stems from a different interpretation of “libertarian”, where critics take it to mean “in line with the libertarian political/economic ideology” and supporters take it to mean “relatively non-intrusive compared to other types of paternalism”. Mitchell is correct that no paternalism (libertarian or otherwise) can ever be acceptable to a hardline libertarian, but he ignores that supporters of LP make no such claim.

consumers a “nudge” (as they refer to it) in the “right” direction, without having to force them.

While at first look this approach appears less invasive than traditional paternalistic measures such as sin taxes, it has not escaped criticism. Klick and Mitchell (2006) argue that libertarian paternalism may remove opportunities as well as incentives for consumers to learn how to make rational decisions, effectively making consumers less discerning and in need of more paternalism, creating a vicious cycle. In a similar vein, Binder (2014) argues that nudges may put a consumer on a learning trajectory that he or she did not choose and that LP interventions may have dynamic effects that supporters have failed to investigate. Binder also argues that it is nearly impossible to determine what an “acceptable” level of rationality is and how big a deviation from the neoclassical model must be to justify a libertarian paternalistic intervention, creating a risk of a “slippery slope” situation. Additionally one may of course question whether or not the neoclassical model is even the best model to begin with (Berg & Gigerenzer, 2010). Finally, Binder argues that any libertarian paternalistic intervention will tend to be conservative in nature, aimed at promoting behaviors considered correct by the culture at the time. While Thaler and Sunstein do address some concerns about libertarian paternalism in their book, all these arguments are ignored and have been met by silence from supporters.

Defenders of libertarian paternalism argue that since framing is inevitable – the items on a menu have to be ordered in some way – one may as well frame in such a manner as to help the individual make a (from the perspective of the choice architect) good decision (Thaler and Sunstein, 2003). This implicitly assumes that intentions do not matter. To accidentally place the salad at the top of the menu (or to do it for any non-LP reason, such as standard profit maximization) is not the same as to do it on purpose to make people eat salad. The latter

creates a precedent for using psychological manipulation to help individuals do what is “right”, a precedent that can then be used to justify further interventions.

Proponents of libertarian paternalism often make the claim that what they are nudging consumers to do are the same things the consumers wished they had the willpower to do on their own; the nudges are so to speak in line with consumer metapreferences. Thaler and Sunstein in their aforementioned book note that the vast majority of smokers would like to quit, and so by imposing nudges that make it harder for them to smoke or to access cigarettes, we are really doing them a favor.

The problem with this argument is that metapreferences are not observable, which is why economists for the most part tend to ignore them. While it is true that most smokers who have been surveyed claim to want to quit, we have no way of knowing whether they actually want to quit or whether they are merely stating what they believe to be the most socially acceptable position. They may claim to want to quit because they don't want to have to explain themselves and/or because they think it's what the person asking wants to hear. Basing nudges on metapreferences means that we may unintentionally manipulate people into choices that are socially acceptable but not in line with their utility functions. Since metapreferences are so strongly influenced by cultural norms and beliefs one would, in order to accept this argument from LP proponents, essentially have to accept that all widely held beliefs are by definition correct – a rather extreme form of moral relativism and dictatorship of the majority.

This goes back to Binder's criticism of libertarian paternalism that it is inherently conservative and promotes whatever is considered correct behavior by the culture in the time and place where it is being applied. If nudges are based on metapreferences, which is almost certain given that they play such a prominent role in justifying their existence in the first

place, then it is virtually guaranteed that these nudges will serve to reinforce cultural beliefs and stigmas.

Another key criticism against libertarian paternalism, advanced by Binder and Lades (2015), is that it is unethical to use psychological biases in policy interventions, even when this is done to benefit the consumer, and that consumers are not actually taught to act in a more rational manner by LP interventions – they are merely tricked into doing so. They suggest an alternative, restricted version of libertarian paternalism, which they call “autonomy-enhancing paternalism” (AEP). In order for an intervention to qualify under the criteria of AEP, the intervention must not rely on psychological biases and must instead work to strengthen the individual’s autonomy (the ability to make an actual conscious decision) by, for example, providing more information (through public service announcements, etc.) or by preventing an individual from making a hasty decision, by for example introducing a mandatory waiting time between the purchase decision and the delivery of a good/service during which the individual can cancel the purchase. Traditional LP interventions such as the use of default options and framing are off limits as their efficacy stems from psychological biases (status quo bias and the framing effect, respectively).

While it is clear that an ethical case can be made in favor of AEP over LP, the question remains whether or not LP treatments are more effective than AEP treatments, and if so to what extent. This defines “*the marginal benefit of manipulation*” (MBoM), the difference between the treatment effect of an LP treatment and an AEP treatment. This term is appropriate since LP interventions rely on psychological manipulation of consumers, while AEP interventions do not. The additional benefit offered by using an LP intervention is therefore the marginal benefit of using manipulation. In this paper I conduct an experiment the ostensible goal of which is to reduce the individual time discount rate, to measure the MBoM by randomly assigning participants into three groups: An AEP treatment group, an LP

treatment group, and a control group. This random assignment allows the experiment to run under both the AEP and LP umbrella, allowing them to be compared directly. In the AEP treatment group, participants were presented with a list of arguments in favor of the larger-later option *and* a list of arguments in favor of the smaller-sooner option (see the appendix for a list of arguments), while in the LP treatment participants were instead given the larger-later option as the default option and had to tick a box if they wanted to choose the smaller-sooner option. In the control treatment participants did not receive any arguments in favor of either option, nor were there any default options.

While the efficacy of a treatment in the immediate term is interesting, it is equally interesting from a policy viewpoint to determine to what extent the effect of a treatment outlives the treatment itself. A treatment that causes a small but permanent effect may be considered preferable to a treatment that causes a bigger effect which disappears as soon as the treatment ends. For this reason in my study all participants were invited to take part in a follow-up survey which they could complete (at the earliest) seven days after completing the first survey. In the follow-up survey everyone received the same tasks and information as the control group received in the first survey. Allcott and Rogers (2012) find that the treatment effect can outlast the treatment itself in a study on reducing energy consumption, however this treatment was a combination of AEP (providing information on monthly energy usage) and LP (using social pressure by pointing out to those who consumed more than their neighbors that they were doing so) and hence there is no way to know whether the permanency was caused by the AEP or the LP component, or both. There is also the problem of the transaction cost; a consumer who has switched to an energy-saving device after receiving a monthly report is unlikely to switch back (at least immediately) after the monthly reports end, but this does not apply in all intertemporal choice situations. In my experiment, there was no cost associated with choosing a different option in the second survey than in the first survey (i.e.,

choosing the larger-later option in the first survey and the smaller-sooner option in the follow-up survey), which leads to a more accurate estimate of the permanency of the effect of the different treatments.

From the neoclassical model of time discounting (commonly known as the Discounted Utility [DU] model) introduced by Samuelson (1937) we would expect there to be no difference between the control group and the treatment groups as consumers have stable preferences (so framing does not affect them) and full information (so the AEP intervention adds nothing of value). Further we would not expect anyone who chooses the smaller-sooner option for the shortest delay (one week vs one month) to choose the larger-later reward for the longer delays as the implied annual interest rate on the shortest delay is higher than for any of the later delays, so a consumer with a constant discount rate (as per the DU model) who rejects the larger-later option in the tasks with the shortest delay would also reject it for the longer delays. Finally the DU model implicitly assumes there to be no domain-specific discounting, which in the context of this experiment means that participants should choose the same course of action regardless of whether the reward consists of money or vouchers. Hence, no participant should, for example, choose to the larger-later option when asked to choose between 30 euro in one week and 50 in one month and then choose the smaller-sooner option when asked to choose between a 30 euro Amazon voucher in one week and a 50 euro Amazon voucher in one month.

Hypotheses

I test two hypotheses: First, that LP and AEP both increase the likelihood that a participant opts for the larger-later options, and that the advantage of LP over AEP – the marginal benefit of manipulation – will be statistically insignificant.

Second, that the AEP treatment effect will still be present in the follow-up survey while the LP treatment effect will not. This hypothesis is based on Rogers and Frey (2014) who found

that adding information could permanently change individual decision making, while no evidence exists that default options are capable of this.

2. Methodology

To test the hypotheses concerning the relative efficacy of LP and AEP I conducted an online experiment using the platform SurveyMonkey between the 27th of April and the 3rd of June 2015 with the original survey conducted between the 27th of April and 27th of May, and the follow-up survey conducted between the 4th of May and 3rd of June. A total of 535 participants completed the experiment, with 263 of those completing the follow-up survey. Participants were recruited mainly through social media websites including Facebook, Reddit, Twitter and Craigslist, and through an email invitation sent out to all economics, finance and accounting students at Maynooth University.

Following the incentive structure used by Coller and Williams (1999), this study used real incentives, with three randomly chosen participants being paid based on one pre-selected task². Although the evidence is mixed on whether real incentives have any effect on the behavior of participants in intertemporal choice experiments (Frederick, Loewenstein and O'Donoghue, 2002), real incentives were used in this study since the current literature cannot rule out with any reasonable degree of certainty that they can influence discount rates. Limiting the number of paid participants to three was done purely due to budget limitations. The randomly selected participants were contacted via email and paid through PayPal. Participants were informed about the incentive structure before agreeing to take part in the experiment, but were neither informed of the hypotheses nor which task would determine the payment if they were one of the randomly chosen participants, as this may have biased the results. The “real” task was task number 4.

Participants were randomly assigned into one of three groups: the LP treatment group, the AEP treatment group, and the control group. Due to platform limitations no true randomization was possible. Instead, in the first part of the survey participants were asked in

² This experiment was self-funded.

what part of the month they were born and based on that answer were assigned to one of the groups.

The second part of the experiment differed depending on into which group participants fell. All participants were asked to choose between receiving 30 euro in one week or 50 euro in one month, 30 euro in one month or 50 euro in 6 months, and 30 euro in 6 months or 50 euro in 12 months, and the choices were repeated with 30 and 50 euro Amazon and Apple vouchers being used instead, giving a total of nine tasks (exchange rates for US dollars and Pound Sterling were provided). Amazon and Apple vouchers were used to compliment the money tasks to mitigate the issue of participant choices being affected by operative liquidity constraints. That is, liquidity constrained individuals could choose the smaller-sooner option not because they are inherently impatient, but because they suffer from a shortage of liquid funds. While some individuals may choose the smaller-sooner option for the money tasks for that reason, it is highly unlikely that any individuals desperately need an Apple product and cannot delay receiving a voucher for that reason.

While choice tasks such as those used by this experiment provide less precision than other tasks, such as matching, they are preferable since they are the closest equivalent to the type of intertemporal choices faced by most consumers on a daily basis; that is, a choice between one fixed amount now and another fixed amount at a specific later point. There are very few, if any, real life situations where consumers are asked to “match” how much money at a later point is the equivalent of a certain amount in the present. Thus, as discussed by Frederick, Loewenstein and O’Donoghue (2002), participants in experiments tend to rely on heuristics to solve matching tasks, and this overuse of heuristics appears to be an experimental artifact. Matching tasks also require more time and effort which may reduce the number of participants who complete the experiment. Rating tasks were also considered but ultimately rejected as they too do not resemble any real life situation and so are inferior in terms of

generalizability, and also because they may be sensitive to extremeness aversion (Tversky and Simonson, 1993). The main reason for limiting the number of intertemporal choice tasks to nine was to ensure a high response rate³ and also because a high number of tasks can increase the risk of participants not paying attention. Needless to say there are also very few real-life situations where participants are faced with dozens of intertemporal choice tasks at the same time, and had participants had to solve a large number of tasks, the generalizability of the experiment would have been reduced.

The AEP treatment group, those born in the last third of the month, was presented with a list of arguments in favor of the larger-later and the smaller-sooner option. The arguments, together with the rest of the survey, can be found in the appendix. Additionally, participants in this group were asked which arguments they found most convincing for the smaller-sooner and larger-later option respectively to get a better idea about how individuals make intertemporal choice decisions.

The LP treatment group, those born in the second third of the month, was presented with the option to receive 50 euro (or a 50 euro voucher) in 1 month/6 months/12 months, or 30 euro (or a 30 euro voucher) in 1 week/1 month/6 months. For the latter option to be availed of, a box had to be ticked. By requiring participants to make an active choice to receive the smaller-sooner option, this treatment relied on the default option bias, also known as the status quo bias, to nudge participants toward the larger-later option.

The control group, those born in the first third of the month, was neither provided with a default option nor presented with any arguments in favor of either option.

The third part of the survey was identical for all participants and consisted of a set of demographic questions covering age, country of residence, marital status, gender, education,

³ Galesic and Bosnjak (2009) showed that there is a negative relationship between the number of questions and the survey completion rate.

saving and attitude toward saving and email address (participants were not required to share any identifying information). The survey also asked participants whether or not it was morning or evening at the time they were taking the survey, and asked them to rate Apple and Amazon on a scale from 1 (dislike) to 5 (strongly like). For participants to be included in the statistical analysis of the Apple/Amazon voucher discount rates, they had to have indicated that they at least somewhat liked the company (rated 2 or higher) as time discounting for losses (Thaler, 1981) and less desirable rewards (Tsukayama & Duckworth, 2010) have been shown to differ from that of more desirable rewards.

Additionally in the final part of the survey participants were asked two questions to weed out inattentive and less serious participants. These questions were “Is water wet?” and “What is two plus three?” Participants who answered either of these questions incorrectly had their answers removed from the data analysis to strengthen the credibility of the results and conclusions from the study (this was inspired by de Haan & Linde (2011) who used a similar procedure).

As a final question, participants were asked to provide feedback and/or ask any questions they may have in a comment field. The purpose was two-fold: By allowing participants a chance to give feedback, future experimental designs may be improved, and also the feedback question may reveal that some participants had no idea what they were doing and/or did not take the survey seriously, in which case their answers would be removed, just like with the participants who failed to answer the trick questions correctly. Participants were also asked to indicate if they wanted to find out what the experiment discovered and all participants who indicated that they did and who had provided their email addresses received a summary of the conclusions by email.

All participants who provided their email addresses were invited to take part in the second survey exactly one week after they took part in the first survey. The second survey once again asked participants during what part of the month they were born, but the second part of the survey was identical regardless of what participants answered, as the purpose of the follow-up survey was to measure the permanence of the treatment effect(s) from the first survey.

The rewards, as mentioned above, were set at 30 and 50 euro or the voucher equivalent. Thaler (1981) showed that discount rates are negatively correlated with the size of the reward (*“the magnitude effect”*); meaning very small rewards would cause an overwhelming number of participants to choose the smaller-sooner option. Large rewards solves this problem, but creates an additional two: Participants may not be used to making decisions involving large amounts of money, and this inexperience may affect their decision-making, while 30 and 50 euro are amounts that most people spend quite frequently. Also, had larger rewards been used, hypothetical incentives would have been necessary due to the limited budget of the study.

This experiment was conducted online to allow for a larger, more diverse sample. This was achieved since the experiment had 535 participants from all age groups and several countries. The internet also allowed for a greater degree of anonymity than what can be provided by a regular lab experiment, potentially reducing the observer effect. Finally, since taking part in an online experiment requires less participant effort and time expenditure, online experiments can attract even those who would not volunteer to take part in a lab experiment, reducing the self-selection problem associated with many economic experiments. Conducting an experiment online is also associated with certain drawbacks. Wright (2005) identifies the following. First, there is a risk that participants could take part of the experiment multiple times, a risk that was mitigated by making it impossible to take the experiment more than once from the same computer. While this is not a fool-proof measure, due to the relatively low incentives used in this experiment, it is unlikely that many participants found it

worthwhile to take the experiment several times. Second, participants may be suspicious of financial incentives used in an online experiment (they may be afraid of being scammed), a risk which was mitigated by assuring participants before the experiment that they would not have to share any bank account details to receive payment. Third, participants taking part in an online experiment may be more easily distracted during the course of the experiment than they would have been during a lab experiment, something which may affect their decision-making. This, however, may not be a disadvantage, as real-life economic decisions are often taken in “noisy” environments (i.e. shopping centers) where participants are distracted, and so this actually strengthens the generalizability of the experiment. Also having participants take part in an experiment from the comfort of their own home with their own computers should increase the likelihood that they act naturally, again strengthening the generalizability of the experiment. Finally, unlike in a lab experiment, there is no way to pay participants in an online experiment immediately upon completion of the experiment. Hence, measuring very short-term discount rates is not possible in studies which use real incentives, such as this one. For that reason the shortest delay in this experiment was one week. While this means that some of the “present bias” (Laibson, 1997) is lost, this is not a major concern as discount rates appear to be falling for at least one year from the present (Frederick, Loewenstein and O’Donoghue, 2002), and therefore most of the present bias is preserved even though participants cannot choose to receive the reward immediately.

3. Results

These results were obtained by running Firth-logistic regressions with the responses to the different choice tasks as the Y-variables (the binary independent variables made the data suitable for logistic regression). As there were 9 different choice tasks, there are 9 Y-variables. The coefficients are odds ratios representing the likelihood of a participant choosing the larger-later option relative to the control group – hence a coefficient of 2 should be interpreted as participants in that group being 200 % as likely as those in the control group of choosing the larger-later option for that specific delay and reward.

While running regressions the issue of complete separation, meaning one x-variable (the LP treatment variable) perfectly corresponded with the y-variable, was encountered for two of the variables (the twelve-month delay with Amazon and twelve-month delay with Apple vouchers). Complete separation means that normal logistic regression is impossible to perform since the result cannot be defined (because the maximum likelihood goes to infinity). For this reason, I opted to use the Firth-logistic estimator which is a penalized maximum likelihood estimator that has been shown (Firth, 1993; Heinze and Schemper, 2002) to be able to provide finite (meaning maximum likelihood never goes to infinity), consistent estimates of regression parameters even when separation exists by modifying the score function. Firth-logistic regression methodology was used for all Y-variables, including those without separation, to make the results easier to compare. Another option would have been to use exact logistic regression, but I opted against this as the sample size is relatively large and exact logistic regression is only computationally feasible for small samples (see Altman, Gill and McDonald, 2004 for a more detailed discussion on Firth and exact logistic regression).

After estimating the regression parameters, Wald tests were used to determine whether or not the LP treatment effect differed (at a 10 % significance level) from the AEP treatment effect.

The final sample size is 376 for the original survey and 263 for the follow-up survey. I decided to remove those who either said they disliked Apple or Amazon, or who had answered “I’d rather not say” to any of the demographic questions. While it would have been possible to leave in the participants who disliked Amazon and Apple for the money task regressions, this would have led to the regressions having different sample sizes hence making them more difficult to compare. Regressions were run under these conditions as well – leaving in everyone except the “don’t knows” for the relevant questions and those who did not like Apple or Amazon for the Apple and Amazon regressions respectively, but the results remained largely the same⁴.

In the regression output, below, LP refers to libertarian paternalism, AEP to autonomy-enhancing paternalism, MBoM is the marginal benefit of manipulation (that is the LP coefficient minus the AEP coefficient) and Wald refers to the probability of achieving the Wald statistic when testing the hypothesis that LP equals AEP. Regressions were run with no control variables, only demographic control variables (age, education, location, gender and marital status), only personal choice control variables (whether or not an individual saves, thinks he/she should save more, participated in the experiment in the evening – and hence was more likely to be tired – and whether or not he/she provided his/her email address and hence could receive payment for his/her participation) and both. As can be seen below, the inclusion of control variables did not have any great effect on the coefficients of the treatment variables.

⁴ The exception being when regression was run with demographic control variables only, when LP performed significantly better. This may be because the LP variable picks up some of the yessave variable as those who stated that they saved were overrepresented in the LP treatment group.

Summary

Terminology:

control	1 if participant in control group, 0 otherwise. This is the control variable among the treatment variables.
lp	1 if participant in libertarian paternalist treatment group, 0 otherwise.
aep	1 if participant in autonomy-enhancing paternalist treatment group, 0 otherwise.
onemmoney	1 if participant chose 50 euro in one month, 0 otherwise.
onemamazon	1 if participant chose a 50 euro Amazon voucher in one month, 0 otherwise.
onemapple	1 if participant chose a 50 euro Apple voucher in one month, 0 otherwise.
sixmmoney	1 if participant chose 50 euro in six months, 0 otherwise
sixmamazon	1 if participant chose a 50 euro Amazon voucher in six months, 0 otherwise.
sixmapple	1 if participant chose a 50 euro Apple voucher in six months, 0 otherwise
twelvemmoney	1 if participant chose 50 euro in twelve months, 0 otherwise.
twelvemamazon	1 if participant chose a 50 euro Amazon voucher in twelve months, 0 otherwise
twelvemapple	1 if participant chose a 50 euro Apple voucher in twelve months, 0 otherwise.
Ageover23	1 if participant is over the age of 23, 0 otherwise.
nopostgrad	1 if participant does not hold a postgraduate degree, 0 otherwise.
single	1 if participant is single or not cohabiting with partner, 0 otherwise
male	1 if participant is male, 0 otherwise.

ireland	1 if participant resides in Ireland, 0 otherwise.
sweden	1 if participant resides in Sweden, 0 otherwise.
canada	1 if participant resides in Canada, 0 otherwise.
uk	1 if participant resides in the United Kingdom, 0 otherwise.
ausnz	1 if participant resides in Australia or New Zealand, 0 otherwise.
usa	1 if participant resides in the USA, 0 otherwise. This is the control group for the geographical variables.
euro	1 if participant resides elsewhere in Europe, 0 otherwise
other	1 if participant resides elsewhere in the world, 0 otherwise.
ysmmovation	1 if participant wants to save more but lacks motivation, 0 otherwise.
ysmforget	1 if participant wants to save more but forgets to do so, 0 otherwise. This is the control group for this group of variables (reasons participants who want to save more do not).
ysmmomoney	1 if participant wants to save more but feels he/she cannot afford to, 0 otherwise.
yessave	1 if participant saves, 0 otherwise.
savemore	1 if participant feels he/she should save more, 0 otherwise.
email	1 if participant provided his or her email address, 0 otherwise.
time	1 if participant took part of the experiment in the evening, 0 otherwise.

Table 1: Data summary

To find the percentage of the sample that belonged to a specific group, simply multiply by 100 (i.e. 49.2 % of the sample belonged to the age group 18-23).

Variable	Mean	Variable	Mean
control	0.391	twelvemoney	0.699
lp	0.309	twelvemamazon	0.926
aep	0.301	twelvemapple	0.928
onemmoney	0.803	age1823	0.492
onemamazon	0.918	nopostgrad	0.846
onemapple	0.886	single	0.707
sixmmoney	0.529	male	0.553
sixmamazon	0.646	ireland	0.231
sixmapple	0.691	sweden	0.066
Variable	Mean	Variable	Mean
canada	0.051	ysmforget	0.045
uk	0.072	ysmnomoney	0.449
ausnz	0.029	yessave	0.612
euro	0.043	savemore	0.67
usa	0.46	email	0.771
other	0.04	time	0.745

As seen above, participants from vastly different backgrounds – in terms of age, nationality, education level etc, – took part in the experiment. This is one relatively unique feature of this

experiment as most experiments are only open to students (or only open to non-students), resulting in a homogenous sample not very representative of the overall population.

While this sample was more representative than most experiments, it should be noted that very few participants came from outside the western world, which is unfortunate but difficult to avoid as knowledge of English and access of internet tends to be lower outside the west. Caution is therefore advised before extrapolating any conclusions from this study to non-western populations and cultures.

Table 2: Summary of regression output controlling for personal choice and demographic variables

Regression equation: $\text{Odds}_{\text{FIRTH}} = p([\text{reward}] = 1) / 1 - p([\text{reward}] = 1) = e^{\beta_n X}$ (where X includes lp, aep, ageover23, nopostgrad, Ireland Sweden, Canada, UK, Ausnz, euro, other, male, single, yessave, savemore, ysmmotivation, ysmnomoney, email and time)

One week vs one month, money					One week vs one month, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.293	1.018	1.276	0.029	Coef	0.944	1.393	-0.449	0.442
SD	0.825	0.312			SD	0.425	0.662		
P> z 	0.021**	0.954			P> z 	0.898	0.485		

One week vs one month, Apple vouchers					One months vs six months, money				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	3.026	1.547	1.478	0.165	Coef	1.750	1.111	0.639	0.108
SD	1.35	0.581			SD	0.465	0.291		
P> z 	0.013**	0.245			P> z 	0.035**	0.686		

One month vs six months, Amazon vouchers					One months vs six months, Apple vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.517	1.262	0.255	0.53	Coef	1.638	1.106	0.532	0.198
SD	0.412	0.337			SD	0.47	0.303		
P> z 	0.125	0.382			P> z 	0.085*	0.713		

Six months vs twelve months, money					Six months vs twelve, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.1	0.935	0.165	0.586	Coef	0.012	0.73	-0.717	0.004
SD	0.311	0.258			SD	0.017	1.444		
P> z 	0.736	0.808			P> z 	0.002**	0.873		

Six months vs twelve months, Apple vouchers				
	LP	AEP	MBoM	Wald
Coef	0.009	0.722	-0.714	0.003
SD	0.013	1.424		
P> z 	0.001**	0.869		

Table 3: Summary of regression output controlling only for demographic variables

Regression equation: $\text{Odds}_{\text{FIRTH}} = \frac{p([\text{reward}] = 1)}{1 - p([\text{reward}] = 1)} = e^{\beta X}$ (where X includes lp, aep, ageover23, nopostgrad, Ireland Sweden, Canada, UK, Ausnz, euro, other, male and single)

One week vs one month, money					One week vs one month, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.324	0.99	1.334	0.02	Coef	1.011	1.284	-0.273	0.625
SD	0.821	0.302			SD	0.442	0.597		
P> z 	0.017**	0.973			P> z 	0.98	0.59		

One week vs one month, Apple vouchers					One month vs six months, money				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.887	1.494	1.393	0.171	Coef	1.828	1.093	0.736	0.065
SD	1.281	0.558			SD	0.478	0.284		
P> z 	0.017**	0.282			P> z 	0.021**	0.733		

One week vs one month, Amazon vouchers					One month vs six months, Apple vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.655	1.256	0.399	0.339	Coef	1.697	1.064	0.633	0.121
SD	0.443	0.333			SD	0.479	0.290		
P> z 	0.06*	0.39			P> z 	0.061*	0.821		

Six months vs twelve months, money					Six months vs twelve months, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.153	0.958	0.194	0.529	Coef	0.012	0.774	-0.762	0.003
SD	0.32	0.263			SD	0.017	1.546		
P> z 	0.608	0.877			P> z 	0.002**	0.898		

Six months vs twelve months, Apple vouchers				
	LP	AEP	MBoM	Wald
Coef	0.009	0.730	-0.721	0.002
SD	0.013	1.457		
P> z 	0.002**	0.875		

Table 4: Summary of regression output controlling only for personal choice variables

Regression equation: $\text{Odds}_{\text{SFIRTH}} = p([\text{reward}] = 1) / 1 - p([\text{reward}] = 1) = e^{\beta X}$ (where X includes lp, aep, yessave, savemore, ysmmotivation, ysmnomoney, email and time)

One week vs one month, money					One week vs one month, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.083	0.957	1.126	0.0313	Coef	0.959	1.262	-0.303	0.578
SD	0.723	0.284			SD	0.424	0.583		
P> z 	0.034**	0.882			P> z 	0.925	0.614		

One week vs one month, Apple vouchers					One month vs six months, money				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.812	1.431	1.381	0.159	Coef	1.646	1.104	0.542	0.142
SD	1.242	0.527			SD	0.421	0.278		
P> z 	0.019**	0.331			P> z 	0.052*	0.696		

One month vs six months, Amazon vouchers					One month vs six months, Apple vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.442	1.286	0.156	0.688	Coef	1.512	1.085	0.427	0.266
SD	0.383	0.335			SD	0.424	0.29		
P> z 	0.168	0.335			P> z 	0.14	0.76		

Six months vs twelve months, money					Six months vs twelve months, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.025	0.897	0.128	0.649	Coef	0.011	0.758	-0.747	0.003
SD	0.284	0.243			SD	0.015	1.518		
P> z 	0.929	0.689			P> z 	0.002**	0.89		

Six months vs twelve months, Apple vouchers				
	LP	AEP	MBoM	Wald
Coef	0.011	0.760	-0.749	0.003
SD	0.016	1.523		
P> z	0.002**	0.891		

Table 5: Summary of regression output with no control variables

Regression equation: $\text{Odds}_{\text{FIRTH}} = p([\text{reward}] = 1) / 1 - p([\text{reward}] = 1) = e^{\beta X}$ (where X includes lp and aep)

One week vs one month, money					One week vs one month, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.068	0.920	1.148	0.023	Coef	1.018	1.246	-0.228	0.675
SD	0.708	0.271			SD	0.439	0.569		
P> z	0.034**	0.778			P> z	0.967	0.63		

One week vs one month, Apple vouchers					One month vs six months, money				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.756	1.405	1.351	0.157	Coef	1.681	1.089	0.592	0.103
SD	1.211	0.516			SD	0.422	0.271		
P> z	0.021**	0.354			P> z	0.038**	0.733		

One month vs six months, Amazon vouchers					One month vs six months, Apple vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.543	1.268	0.275	0.485	Coef	1.536	1.057	0.48	0.199
SD	0.404	0.328			SD	0.422	0.279		
P> z	0.097*	0.358			P> z	0.118	0.835		

Six months vs twelve months, money					Six months vs twelve months, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.071	0.913	0.159	0.576	Coef	0.011	0.769	-0.759	0.003
SD	0.291	0.246			SD	0.015	1.542		
P> z 	0.799	0.735			P> z 	0.001**	0.896		

Six months vs twelve months, Apple vouchers				
	LP	AEP	MBoM	Wald
Coef	0.011	0.769	-0.758	0.003
SD	0.016	1.542		
P> z 	0.002**	0.896		

What these results indicate is that while LP has a positive treatment effect initially, this effect wears off rapidly and reverses towards the end of the survey (see the twelve month Apple and Amazon voucher regressions). While it is of course technically possible that LP only works on intertemporal choices involving relatively short time periods, there is no intuitive reason to believe this to be the case. Instead, I believe this may be to some extent explained by participants at first “going along” with the default option, but then towards the later choice tasks they may realize that they’ve picked the same option for each task, and choose the smaller-sooner option as a way to “diversify” their consumption bundle (though further research is necessary to confirm whether this is the case). This would suggest that while LP can work in modifying a consumer choice in one situation, changing the overall consumption bundle is more difficult – expressed differently, impatient consumers will always be impatient in the end, even if a choice architect temporarily tricks them into making patient choices. For comparison; risk compensation has been documented (Hedlund, 2000; Eaton and Kalichman,

2007; Streff and Geller, 1988) to occur when government regulation and interventions (such as increasing access to HIV intervention technologies) intended to make people safer is introduced; the targeted group responds by increasing risky behaviors (such as having unprotected sex), reducing the efficacy of the intervention. These data suggest that an impatient consumer who is nudged into being patient will engage in “intertemporal compensation” and act impatiently at a later stage, somewhat similar to how a consumer forced to wear a seat belt may risk compensate by driving faster.

It is also possible that participants realize towards the later tasks that they are being manipulated, and that this provokes a negative visceral reaction causing them to choose the smaller-sooner option for the later tasks. Expressed differently, people do not like being manipulated, and act in the opposite way as to what the manipulator intended once they realize they are being manipulated.

Defenders of libertarian paternalism may invoke that participants merely get bored towards the end of the survey as they are being asked almost the same question several times and pick another option just to “mix things up”; however the presence of real incentives means that participants have good reason to stay focused and not pick randomly, and any participant who got bored could exit the experiment by clicking the upper right hand corner as it was conducted online. It is worth noting again that the preference reversal was strong enough to cause complete separation in the dataset; it is highly unlikely that virtually every single participant got bored and behaved in the exact same way in response to this boredom.

The only task in which LP fares significantly better than AEP regardless of control variables is the one week vs one month money task, while LP also fares significantly better (at a 10 % significance threshold) than AEP for the one month vs six months money task when controlling for demographic variables. That these tasks are both money tasks may suggest that

LP has a greater effect on consumer decisions involving money rather than less liquid assets such as vouchers, however I do not see any intuitive reason why this would be the case.

It should be noted that very few participants choose the smaller-sooner option for the two final tasks, which makes statistical inferences somewhat more difficult; however the LP coefficient is significant even at a 0.5 % significance level for both tasks, making it hard to dismiss this as merely a statistical anomaly.

The AEP treatment turned out to have no effect whatsoever on the choices made by participants. The likeliest explanation for this is that the AEP treatment provided participants with arguments in favor of both options. A naturally impatient participant may focus on the arguments in favor of the smaller-sooner option and use them as an “excuse” to follow his or her natural inclination, and vice versa for a patient participant. As such, the treatment may have merely reinforced the choices the participants were already leaning towards. It is worth noting that providing arguments for both options is not necessary for a treatment to qualify as AEP; this treatment is therefore a very un-intrusive treatment even by AEP standards.

Also worth noting is that those in the AEP treatment group who stated that they thought the best argument in favor of the larger-later option was the high effective interest rate were significantly more likely to choose the larger-later option in 4/9 tasks. Hence, the AEP treatment appears to have worked at least for a subsection of the sample, although it is of course impossible to rule out that these individuals might have been able to calculate the implied interest rate on their own (and thought of doing so) and made the same choices. More research is necessary on this topic.

In one version of the model interaction variables were included but for the most part they turned out to be insignificant, the most notable exception being those in the LP group who lacked postgraduate education who were significantly more likely to choose the larger-later

option in three of the nine tasks (for another two tasks the interaction variable fell just short of the 10 % threshold). This suggests that nudges are more effective on less-educated consumers, which intuitively makes sense as those who hold advanced college degrees would be expected on average to be of a more analytical mindset and so naturally less inclined to “go along” with the default option. Again more research is necessary to establish whether or not this is the case.

The first hypothesis stated that both treatments would have a positive equal effect. These results indicate that LP has a positive treatment effect initially, but a strongly negative effect towards the end. The Wald test only rejects the null hypothesis that the AEP and LP variables are equal for 3 out of 9 tasks⁵ (at a 10 % significance threshold) when all the controls are applied, and for 2 of those tasks the AEP fares better (these are the two final tasks where the preference reversal occurs in the LP group). Hence, while the effect is not positive, I cannot conclude that LP fares better than AEP overall.

On a final note, these results contradict the Discounted Utility model: Participants frequently turned down the larger-later option during the tasks with the shortest delay but choose it when delays were longer. The LP treatment clearly affected the way participants chose, and many participants chose differently when dealing with money compared to vouchers even when the delay was the same.

⁵ This refers to the model with both demographic and personal choice controls. When only demographic controls are imposed, the Wald test rejects the null hypothesis for 4 out of 9 tasks.

Table 6: Summary of regression output, follow-up survey

Regression equation: $\text{Odds}_{\text{SFIRTH}} = \frac{p([\text{reward}] = 1)}{1 - p([\text{reward}] = 1)} = e^{\beta_1 \text{lp}} e^{\beta_2 \text{aep}}$

One week vs one month, money					One week vs one month, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.172	0.464	0.707	0.037	Coef	1.129	1.064	0.065	0.921
SD	1.172	0.179			SD	0.643	0.577		
P> z 	0.734	0.047			P> z 	0.831	0.909		

One week vs one month, Apple vouchers					One month vs six months, money				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	2.546	2.546	1.767	0.036	Coef	0.965	0.607	0.358	0.149
SD	1.432	0.315			SD	0.301	0.18		
P> z 	0.097	0.536			P> z 	0.909	0.092		

One month vs six months, Amazon vouchers					One month vs six months, Apple vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.588	1.068	0.52	0.263	Coef	1.214	0.817	0.397	0.263
SD	0.536	0.331			SD	0.418	0.26		
P> z 	0.171	0.832			P> z 	0.573	0.525		

Six months vs twelve months, money					Six months vs twelve months, Amazon vouchers				
	LP	AEP	MBoM	Wald		LP	AEP	MBoM	Wald
Coef	1.468	0.935	0.532	0.183	Coef	1.781	1.316	0.465	0.404
SD	0.478	0.282			SD	0.607	0.415		
P> z 	0.238	0.824			P> z 	0.09	0.384		

Six months vs twelve months, Apple vouchers				
	LP	AEP	MBoM	Wald
Coef	1.332	0.766	0.566	0.114
SD	0.455	0.238		
P> z 	0.401	0.39		

In the follow-up survey the LP treatment fares better than the AEP treatment, with a positive correlation at a 10 % significance level for two tasks while the AEP treatment has a negative correlation at a 5 % significance level in one task and is otherwise insignificant. It is possible that participants find it easier to remember the arguments in favor of the smaller-sooner option as they are relatively straight-forward, and so they stay with them longer than the arguments for the larger-later option. However, given the number of tasks, it cannot be ruled out that this is merely a coincidence, especially as the tasks for which the treatment effect lasts are different both in delay and domain. One limitation with the follow-up survey is that participants were not asked any demographic questions, nor were they asked how much they liked Amazon/Apple, and so, unlike in the main survey, there is no way to control for demographic variables or remove data from participants who disliked Amazon and/or Apple.

My second hypothesis stated that the AEP treatment effect would still be present in the follow-up survey while the LP treatment effect would not, but I have to concede that the data firmly reject this hypothesis.

4. Conclusions

These results do not support the implicit assumption of LP proponents that their methods are inherently superior to the less intrusive AEP interventions. While the AEP treatment was inefficient, the LP treatment was outright counterproductive in the later stages of the experiment, undoing its positive effect in the early stage. Binder's (2014) prediction that LP interventions may turn out to have adverse dynamic effects appears to be correct judging from this experiment. Neither treatment showed any greater degree of permanency.

Proponents of libertarian paternalism may argue that the preference reversal seen in the later stages of the survey is an experimental artifact. I do of course intend to conduct further experiments to find out whether preference reversal occurs consistently; that being said, there are several reasons to believe that this result is not an experimental artifact: First of all, the likelihood of experimental artifacts is reduced substantially by the design of the experiment which, as explained in previous sections, allowed participants to take part in the experiment from the comfort of their own homes, thereby inducing more natural behavior. The cost of acting "defiantly", picking the smaller-sooner option instead of the larger-later option, was also much higher (an individual that did so lost out on hundreds of percent of interest) in this experiment than in most real world situations, and so it is reasonable to assume it would actually be even more common in a real world setting. Finally, individuals face a number of intertemporal choices on an everyday basis (both economic and otherwise), and as such there are several situations where "nudges" would be used if libertarian paternalists had their way. Hence, it is not at all unreasonable to imagine an individual being exposed to several LP interventions during a single day, and as this experiment shows, even if the first intervention works this is far from a guarantee that later interventions will; the consumer choice basket appears to be sturdier than LP proponents have understood. There is no way in the real world

to ensure that an individual is only exposed to one or a few nudges, which means there is no way to ensure that this preference reversal does not occur.

Proponents may also assert that they would not advocate that interventions be done in this manner; that the preference reversal results from the intervention being carried out without the knowledge and consent of the participants involved. Had participants merely been informed that one option had been set as the default option for their own good, they would have understood and probably been grateful for the favor done to them by the choice architect, they may say. However, in the real world, it is virtually unheard of for libertarian paternalist interventions to follow these standards, and no literature exists supporting the notion that ethical, transparent libertarian paternalism works.

Finally proponents might claim that these nudges were “obvious” and that such bold-faced manipulation of course would cause participants to realize that they were being tricked and react in a defiant manner by doing the opposite of what the manipulator clearly wanted them to do. However, in the real world while nudges may sometimes be more subtle, there are media outlets that would be more than happy to inform consumers of what they are being tricked into doing, making these nudges no harder to discover. Also an LP proponent who makes this argument would have to concede that consumers do not like being manipulated even for their own good, which is one reason why many opponents consider LP to be unethical.

Beshears et al. (2009) reviewed the literature on the effect of a default option on retirement saving in the United States and conclude that the literature supports the idea that enrollment rates increase when the default option is to enroll. However, they do not discuss any literature on how many of those enrolled through automatic enrollment later go on to drop out of the plans they have enrolled in (assuming that is possible). It should be noted however that even if

the dropout rate among those who had been automatically enrolled were no higher than among those who had to make an active choice to be enrolled, this does not in and of itself contradict the idea that nudges cause preference reversal as there is nothing that prevents a preference reversal from occurring in another domain than the one where the nudge occurred in (e.g. through a decrease in private saving).

If repeated nudges do in fact cause preference reversal and “compensation” – regardless of the reason – there are severe policy implications. In the real world, unlike in an experiment, we do not know exactly when and in what situation the preference reversal will occur, and whether or not the resulting new consumption bundle will indeed be superior (from a social and/or individual viewpoint) to the old consumption bundle. If a nudge causes someone to save more (a patient decision) but also smoke more (an impatient decision), the net effect may very well be negative. Even if the preference reversal is limited to the domain of the nudge, and even if we assume that it is no greater than the initial positive effect of the nudge, we still cannot be certain that the nudge will increase utility; a consumer that is nudged to save more now and as a result saves less tomorrow will only be better off provided that saving more now and less tomorrow makes sense. An example when this may not be the case is when a consumer is low-paid today but high-paid tomorrow; in this case, saving today makes little sense as the marginal utility of consumption is likely to be high, and a nudge that causes this consumer to save today and spend tomorrow will be detrimental to the consumer’s lifetime utility.

I must however also conclude that this experiment does not support the idea that AEP would have a greater permanency than LP (which was my second hypothesis). It is possible that a “stronger” AEP treatment (for example; providing arguments only in favor of the larger-later option) may have produced better results, but that is a topic for future research.

Future research will further investigate the effect of LP and AEP interventions on different demographic groups and will also focus on determining whether or not transparency reduces the efficiency of LP interventions.

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Appendix: Survey

Welcome!

My name is John Gustavsson and I'm a research 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 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 of you who answer this survey will be paid in accordance with how you answer one of the scenarios. The three who are paid will be randomly selected; your answers have no bearing on your likelihood of being one of them. The final part of this survey contains demographic questions (age, gender, country of residence, education 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.

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 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.

Everyone who takes this survey will be invited back (by email) to take a shorter version of the survey again after 1 week. Retaking the survey is not mandatory. If you're interested in taking part of 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 10-20 minutes to complete, obviously depending on how much time you spend thinking about your decisions.

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!

What time of the month is your birthday?

First third of the month

Second third of the month

Last third of the month

Intertemporal choice scenarios [Libertarian Paternalist treatment group]

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound Sterling as of this writing.

In one month, you are going to receive 50 euro. If you would rather receive 30 euro in one week, please tick this box.

In one month, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one week, please tick this box.

In one month, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one week, please tick this box.

In six months, you are going to receive 50 euro. If you would rather receive 30 euro in one month, please tick this box.

In six months, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one month, please tick this box.

In six months, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one month, please tick this box.

In 12 months, you are going to receive 50 euro. If you would rather receive 30 euro in six months, please tick this box.

In 12 months, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in six months, please tick this box.

In 12 months, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in six months, please tick this box.

Intertemporal choice scenarios [Autonomy-enhancing paternalist group]

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

- 1) Choosing the “later” option means you have something to look forward to.
- 2) Saving money means you’ll be safe 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 later option now, it’ll be easier to do the same in the future – you can establish a positive precedent for yourself.
- 4) The annual interest rate in the first three scenarios (see below) is 742961 % (based on a four-week month) in the second and last third of the scenarios it is 241 % and 178 % respectively.

However, you should also keep in mind that:

- 1) If you choose to receive the money or voucher sooner, you’ll also be able to enjoy it sooner and have the freedom to choose whether you use them now or later. The “later” option prevents you from using the money/voucher sooner, but if you choose the sooner option you can always choose to use it later. Basically, the “sooner” option gives you more freedom.
- 2) Choosing the “later” option means taking a risk, as you could end up needing the money (or voucher) sooner than you thought, and choosing the “later” options means you won’t have it.

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound sterling as of this writing.

You are given a choice between receiving 50 euro in one month, or 30 euro in one week.

Which option do you choose?

50 euro in 1 month

30 euro in 1 week

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

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Which option do you choose?

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30 euro in 1 month

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A 50 euro voucher in 6 months

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A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

Which argument in favour of the “later” option did you find to be the most convincing?

The “something to look forward to”-argument

The “rainy day”-argument

The “positive precedent”-argument

The effective interest rate-argument

No difference

Which argument in favour of the “sooner” option did you find to be the most convincing?

“Freedom” argument

“Risk” argument

No difference

Intertemporal choice scenarios [control group]

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound sterling as of this writing.

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Which option do you choose?

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Demographic questions

Please state your age

18-23

24-35

35-64

65+

I'd rather not say

What country do you live in? If you'd rather not say, just write N/A in the box

[Comment field]

What is your relationship status?

Single/not living with partner

Married/civil union/living with partner

I'd rather not say

Is water wet?

Yes

No

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

If Yes, why don't you?

Not enough money

Lack of motivation

Forgetfulness

I don't want to save more

I'd rather not say

What gender do you identify as?

Male

Female

I'd rather not say

What is the highest level of education you have achieved?

High-school/post-primary school or less

Undergraduate degree

Postgraduate degree

I'd rather not say

Do you currently save?

Yes, through a pension plan

Yes, privately/both privately and through a pension plan

No

I'd rather not say

What is two plus three?

3

5

1000

69

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

More

Less

Same

No opinion

Is it currently morning or evening where you live?

Morning

Evening

How do you feel about Apple on a scale from 1 (dislike) to 5 (strongly like)

[Pull-down menu with numbers 1-5]

How do you feel about Amazon on a scale from 1 (dislike) to 5 (strongly like)

[Pull-down menu with numbers 1-5]

Please provide your email address in the field below (this is voluntary but encouraged)

[Comment field]

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

[Comment field]

Follow-up survey

Welcome!

One week ago, you participated in a survey I did on inter-temporal choice. In case you forgot, my name is John Gustavsson, and I'm a research student at the National University of Ireland, Maynooth at the Department of Economics, Finance & Accounting, and this research will form part of my thesis. The survey you are about to take is similar (but shorter as there are no demographic questions). Once again, your results are anonymous, and your 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 have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie. This survey will take approximately 10 minutes to complete, depending on how much time you spend thinking about each decision.

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!

Once again, could you please tell me if your birthday is...

In the first third of the month

In the second third of the month

In the final third of the month

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Which option do you choose?

50 euro in 1 month

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