Benefit Losses Loom Larger than Taxes: The Effects of Framing and Loss Aversion on Behavioural Responses to Taxes and Benefits

Silvia Avram
Institute for Social and Economic Research
University of Essex

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Non-technical summary

Traditionally, the workhorse model of labour supply behaviour has assumed that individuals react to public taxes and benefits only because these affect the net pay rate or the resources available outside work. More recent evidence from behavioural public economics suggests that individuals may be responsive not only to the monetary incentives of taxes and benefits but also to the way they are framed.

This paper provides evidence that individuals perceive and react differently to direct taxes on future earnings and implicit taxation via loss of benefits, even when these two different forms of taxation have identical effects on the financial rewards from work. In a lab experiment, 148 subjects are randomly allocated to three treatment groups and asked to supply real effort in a simple but tedious task in return for pay. The three treatment groups provide the same net pay rate schedule but differ in the way the payment structure is framed. The frames mimic direct taxation and loss of benefits (approximated by the show-up fee), with the control group being presented a neutral, non-tax-benefit related language.

Results indicate that the tax frame strongly affects the amount of time subjects choose to work but not the effort they supply per unit of time. Subjects in the benefit loss frame are three times more likely to stop working before the maximum allocated working time elapses compared to subjects in the tax frame. Further analyses suggest that one mechanism potentially explaining the differential behaviour is loss aversion, i.e. the human propensity to place greater emphasis on avoiding losses compared to realizing gains. In the benefit loss frame, (strongly) loss- averse individuals are much more likely to stop working compared to their less loss averse peers while in the other treatment groups differences are much smaller. From a policy perspective, findings point to the possibility of reducing work disincentives coming from income-tested benefits by altering their framing or design.

Benefit Losses Loom Larger than Taxes: The Effects of Framing and Loss Aversion on Behavioural Responses to Taxes and Benefits*

Silvia Avram

ISER, University of Essex

Abstract

A substantive body of research highlights the existence of framing effects in labour supply responses to taxation challenging traditional models that assume taxes only influence behaviour via the budget constraint. Using a lab experiment, this paper examines the presence of differential responses to identical marginal tax schedules coming from direct taxation and from benefit withdrawal respectively. In an incentivised real-effort task, subjects supply time and effort while facing an incentive structure that is framed as taxation or benefit withdrawal respectively, while yielding the exact same budget constraint. Results indicate that subjects in the benefit withdrawal condition are more likely to reduce working time compared to both subjects in the tax treatment and a control group where the incentive structure is described without using the language of taxes and benefits. The effect is driven by loss-averse individuals suggesting that benefit streams may be subject to an 'endowment effect'. The findings have clear implications for welfare policy design.

JEL: C91, D80, I38

Keywords: framing effects, behavioural responses to taxation, loss-aversion, experiments

Contact: Silvia Avram, Institute for Social and Economic Research (ISER), University of Essex, Wivenhoe Park, Colchester, CO4 3SQ, E-mail: savram@essex.ac.uk

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1. Introduction

The traditional view on labour supply responses to taxes and benefits asserts that individuals react to changes in the net budget constraint generated by the presence of tax/ benefit instruments (Brewer et al., 2010; Meghir and Phillips, 2010). Although successive extensions of the basic model have incorporated many features that make modelling more realistic such as rigidities on the demand side, partner earnings, fixed costs of working etc., the fundamental assumption remains that individuals react only to the shape of the net budget constraint and not to the way the budget constraint is generated. A corollary of this assumption is that the framing of taxes and benefits does not matter, only their effect on the net budget.

More recently, a number of studies have introduced non-standard elements such as loss-aversion or misperception/salience to explain puzzling empirical findings that cannot be accounted for by the usual workhorse model (Camerer et al., 1997; Fehr and Goette, 2007; Crawford and Meng, 2011). A growing body of evidence suggests that labour supply is sensitive to framing effects. In particular, individuals appear to have a reference dependent utility function that places much more weight on losses compared to gains (Abeler et al., 2011; Goette et al., 2003). They also tend to focus on some elements of the net budget such as the gross wage and misperceive others such as implicit or explicit tax rates, changes in the tax base etc. (Fochmann et al., 2013; Gamage et al., 2010; Blaufus et al., 2010). If individual responses to taxes and benefits are subject to framing effects, taxes that have the exact same effect on the net budget constraint can no longer be considered equivalent. Since different types of taxes tend to affect different groups of individuals, evidence on this point is relatively scarce. However, the non-equivalence of various types of taxes has been confirmed in a number of experimental studies (Weber and Schram, 2013; Blumkin et al., 2008; Kerschbamer and Kirchsteiger, 2000; Sausgruber and Tyran, 2005). In this paper, I extend the literature on framing effects and in particular the non-equivalence of taxes by examining behavioural responses to taxation and benefit withdrawal. In an experimental setting, I use an incentivised real effort task to measure responses to a tax schedule that is framed either as a tax on future earnings or as a withdrawal of a current benefit. I also examine the extent to which loss aversion can explain any difference in behaviour. The rest of the paper proceeds as follows. Section 2 reviews the existing evidence on tax framing and labour supply decisions and discusses the hypotheses. Section 3 details the experimental design. Section 4 gives an overview of the experimental data and presents descriptive statistics on labour supply by treatment group and loss-aversion. Section 5 presents the main results while Section 6 concludes.

2. Tax framing and the labour supply decision

Previous scholarly work has looked into the potential effect of tax framing (as opposed to tax design) on behaviour. Most studies have focused on the salience aspect of taxation in a variety of settings covering consumption (Chetty et al., 2009; Finkelstein, 2009) , wealth (Cabral and Hoxby, 2012) and labour supply (Fochmann and Weimann, 2013; Blaufus et al., 2010; Fochmann et al., 2013). These studies usually conclude that consumers/ owners/ workers tend to underreact to taxes, especially when their presence (or the presence of some elements of the tax schedule) is not conspicuous. Thus, framing effects tend to be attributed to misperception of at least some elements that are relevant for maximizing utility.

A related strand of research looking into tax equivalence shows that framing effects go beyond salience. Several studies document a differential (consumption or labour supply) behavioural response depending on how a tax or a subsidy are framed (Blumkin et al., 2008; Gamage et al., 2010; Weber and Schram, 2013; Gallagher and Muehlegger, 2011). There is less evidence on why the differential behavioural response occurs and what features of the tax/subsidy instrument are responsible for it[1].

This paper contributes to the literature on framing effects in taxation by examining the existence of a differential response to direct taxes on future earnings and implicit taxes materialized through the withdrawal of income tested benefits. In addition, it investigates the plausibility of a mechanism other than salience/ misperception generating tax non-equivalence, namely loss aversion. Prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992) suggests that individuals treat losses and gains asymmetrically: losses affect their utility function more than gains of a similar magnitude. An implication of loss aversion is the so-called ‘endowment effect’, i.e. the well-documented stylized fact that individuals appear to place a greater value on an object when they already possess it compared to when the same object is not part of their endowment (Kahneman et al., 1991;

Time discounting and misperception are some of the explanations that have been put forward.

Kahneman et al., 1990; Knetsch, 1989; Bateman et al., 1997). The 'endowment effect' potentially has implications for tax-benefit policy. In particular, if the existing stream of benefits is more likely to be integrated into the current endowment compared to future earnings (because of a perceived entitlement to the benefit stream but not to future earnings), individuals will be more likely to reduce their labour supply in the face of benefit withdrawal (such as to avoid loss of benefits) compared to direct taxation (which a loss of future rather than already realized income).

To test this hypothesis, I use an incentivised, real-effort experiment where I compare labour supply under three different conditions, neutral, tax frame and benefit withdrawal frame, while keeping the shape of the net budget constraint constant. A measure of loss aversion has also been collected during the experiment via a set of incentivized lotteries to test whether loss-averse individuals are more likely to reduce their labour supply in the benefit withdrawal condition. The next section describes the experimental design.

3. Design of the experiment

Studying non-equivalence between taxes and benefit withdrawal using observational data is exceedingly problematic due to the fact the two types of taxes usually apply to different populations. Implicit taxation resulting from the withdrawal of income tested benefits is salient only for the low income population whereas (progressive) tax systems usually impose the highest marginal tax rates on individuals/ families with higher incomes. To overcome any potential selection biases, this study relies on an experimental framework.

Use of lab experiments to study the labour supply decision has grown exponentially in the last twenty years (Charness and Kuhn, 2011; Falk and Heckman, 2009). Albeit still somewhat removed from the reality in the field, lab experiments can credibly replicate most elements deemed important in labour supply decision theory. In particular, both the disutility of effort and the incentive structure can be replicated and indeed manipulated in the lab. Following established practice, this study uses an incentivised real-effort task to simulate the context in which the labour supply decision is made.

Five experimental sessions have been run during late June and early July 2014 at the ESSEXLab facility located at the University of Essex. In total, 149 subjects have taken part in the sessions. Out of these, 148 have generated valid data. Subjects have been recruited from the student and staff population at the University of Essex. The entire experiment (including instructions) has been programmed in Z-tree (Fischbacher, 2007).

The labour task

The main part of the experiment is concerned with replicating the labour supply decision. After receiving detailed instructions, subjects were faced with the decision of how much time and effort to put into a labour task. The labour task used in the experiment is the ‘slider task’ first proposed by Gill and Prowse (2012; 2011) and later used by other authors (see for example Abeler and Jäger, 2013). Subjects are presented with a series of sliders that can be moved using the mouse on a continuum ranging from 0 to 100. Initially, all sliders are positioned at 0. To earn money, subjects are required to position a slider exactly in the middle of the continuum, i.e. at 50. A number on the right of each slider indicates the current position of the slider. For a full description of the slider task see Gill and Prowse (2011). Albeit somewhat artificial[2], the ‘slider task’ has a number of advantages. It is repetitive and tedious so as to generate positive disutility from work, while not requiring any prior knowledge or skills. Because of its simplicity and straightforwardness, it is likely to be only minimally affected by learning and/or ability. It also has the advantage that it is identical across repetitions, involves little randomness and there is no scope for guessing. As such, it can be used to precisely measure effort exerted by the subject, ensuring enough heterogeneity is generated across subjects.

All subjects participate in the slider task and face the exact same incentive structure. For each correctly positioned slider up to and including the 250th, subjects are paid £0.01. For each subsequent correctly positioned slider (starting with the 251st), they receive £0.02. There is no limit on the number of sliders that can be correctly positioned, and hence no (explicit) ceiling on pay. However, total ‘working’ time is limited to a maximum of one hour. At the top of the screen, subjects can at all times see how many sliders they have positioned correctly and how much time they can continue working on the slider task.

[2] Artificiality /meaningless in a task has both advantages and disadvantages; on the one hand, it may lower work incentives and potentially increase the reservation wage (Ariely D, Kamenica E and Prelec D. (2008) Man's Search for Meaning: The Case of Legos. *Journal of Economic Behavior & Organization* 67(3/4): 671-677.) but it has also been used to avoid the possibility that subjects offer labour as a way to reciprocate the experimenter.

To mimic a choice between labour and leisure, subjects have a choice between positioning sliders and being paid for it (work) and leaving the experiment early (leisure, which is unpaid). The leisure option is important in the context of a lab experiment where subjects may provide effort in solving the task at hand because of a lack of real 'alternatives' (leisure in this case would be doing nothing). To circumvent the problem of having to offer a 'leisure' alternative which subjects may value differently depending on their tastes and preferences, subjects are allowed to choose the amount of time they are willing to provide effort[3]. More specifically, subjects can stop working at any time before the maximum one hour allotted working time elapses. Once they have decided to stop working, they participate in a series of nine incentivised lotteries designed to measure loss aversion (see below) and answer five simple demographic questions, after which they are free to leave the experiment and collect their payment. To ensure they have understood the task, all subjects are required to correctly position three sliders before they can begin the actual labour task (for a full set of experimental instructions see Appendix 6).

A potential drawback of allowing subjects to leave early is the generation of peer-effects in the decision to stop working. Since subjects in the same session can see other subjects leave, they may be prone to imitate this behaviour and stop working as well. In this case, the decision to stop working would not be independent across individuals. Appendix 3 shows this is not the case. It plots the distribution of working time within session. Working times below the maximum allowed one hour do not cluster together in any of the sessions.

The incentive structure

Subjects have been allocated randomly to one of three experimental conditions which determined the framing of the incentive structure. Which treatment a subject experienced was randomly determined by the computer. 64 subjects have been assigned to the BASELINE condition, 34 the TAX treatment and 50 to the BENEFIT treatment.

[3] This strategy has successfully been used in other studies (Abeler J, Falk A, Goette L, et al. (2011) Reference Points and Effort Provision. *American Economic Review* 101(2): 470-492, Abeler J and Jäger S. (2013) Complex Tax Incentives-An Experimental Investigation. *IZA Discussion Paper Series.* Bonn IZA, Fochmann M, Weimann J, Blaufus K, et al. (2013) Net Wage Illusion in a Real-Effort Experiment. *Scandinavian Journal of Economics* 115(2): 476-484.)

Each subject underwent one treatment only. Thus, the experiment follows a between-subjects design. This has the advantage of avoiding contamination and order effects when the same subject is exposed to more than one treatment (Charness et al., 2012). It also facilitates using early departure from the experiment as the (implicit) leisure option. To avoid any contamination from behavioural reactions to changes in treatments, no practice rounds have been carried out prior to starting the main labour task. Instead, the subjects have been given a clear explanation of how their labour supply is mapped onto earnings. To make sure they understand the monetary incentives structure, subjects had to correctly answer four control questions before they could move on to the main task (see Experimental instructions in Appendix 6).

All subjects are paid a show up fee of £2.5 in addition to any earnings. After completing the labour task, subjects are also awarded a fixed £5 payment, irrespective of how much time and effort they put into the labour task. Unlike the show-up fee, which is known from the start of the experiment (and part of the EssexLab payment rules), the £5 payment is only revealed to the subjects once they complete the labour task. Its purpose is to endow subjects with enough money so that any losses experienced while playing the incentivized lotteries can be covered using experimental earnings only. The payment is not linked to actual participation in the lotteries. Instead, it is framed as a fixed payment paid on account of reaching the lottery stage.

Baseline condition

The framing of the incentive structure in the baseline condition is meant to be ‘neutral’. Subjects are simply told they will be paid £0.01 per correctly positioned slider for the first 250 sliders and £0.02 per slider thereafter. The exact language used in the baseline condition is reproduced in Annex 6.

Tax condition

In the tax condition, subjects earn money but have to pay tax on their earnings. They earn £0.02 for every correctly positioned slider but have to pay a tax of 50% on their earnings received for the first 250 correctly positioned sliders. There is no tax starting with the 251st slider. The exact instructions given to the subjects in the tax treatment can be found in Appendix 6.

Benefit condition

In the benefit condition, subjects earn £0.02 for every correctly positioned slider. However, they have to pay back the experimenter £0.01 per slider from their show-up fee until this fee is exhausted (which will happen once they have correctly positioned 250 sliders). Thus, an implicit marginal tax rate of 50% is imposed on earnings received for the first 250 sliders. However, instead of an explicit tax on future earnings, the reduction in the rate pay is framed as a show-up fee withdrawal.

Loss aversion measurement

To further investigate the role of loss aversion in generating differential responses to taxes and benefits, I use a series of incentivized lotteries to measure loss aversion. Since the ground breaking work of Kahneman (Kahneman and Tversky, 1979), loss aversion has been broadly interpreted as the increased weight individuals place on avoiding losses compared to increasing their gains. More recently, a variety of formal models for defining and measuring loss aversion have been proposed (Abdellaoui et al., 2008; Köbberling and Wakker, 2005; Brooks and Zank, 2005). These models usually require subjects to choose between many lotteries both in the gain and in the loss domains, and hence may undesirably increase both the length of the experiment as well as cognitive load. To minimize these shortcomings, I use a simplified version of the Abdellaoui et al. (2008) model put forward in Gächter et al. (2010) which assumes probability weights of one (at least around the 50% threshold) and no risk aversion when only small amounts are involved. The risk neutrality for small stakes is based on Rabin's argument that risk aversion for small stakes implies (by extrapolation) implausibly large risk aversion levels for higher but still moderate gambles (Fehr and Goette, 2007; Rabin, 2000). Gächter et al.(2010) show that this simple measure of loss aversion correlates well with another widely used indicator namely the willingness-to-accept/ willingness to pay gap.

Subjects are required to accept or reject nine risky lotteries. If they choose to reject the lottery, they do not gain or lose anything. If they accept the lottery, they may win with a probability of 50% £5 or they may lose an amount with a probability of 50%. The amount they may lose increases from £1 in the first lottery to £5 in the ninth lottery in increments of £0.50. Assuming risk neutrality, an individual who is not loss averse would be expected to accept all nine lotteries (since all lotteries have a positive expected value). However, most subjects do not accept all lotteries. The lottery where they switch from acceptance to rejection (the switching point) can be used as a rough indicator of loss aversion.

To ensure incentive compatibility, one lottery is selected at random and played out leading to real gains or losses. To avoid total experimental earnings being negative, subjects are endowed with £5 before the lottery section of the experiment begins (see also above). Subjects are made aware that this payment is certain and not linked in any way with lottery related behaviour and that all lottery earnings are in addition to the £5 payment (see Annex 6 for more details on exact instructions).

4. Working time, effort and earnings: some descriptive statistics

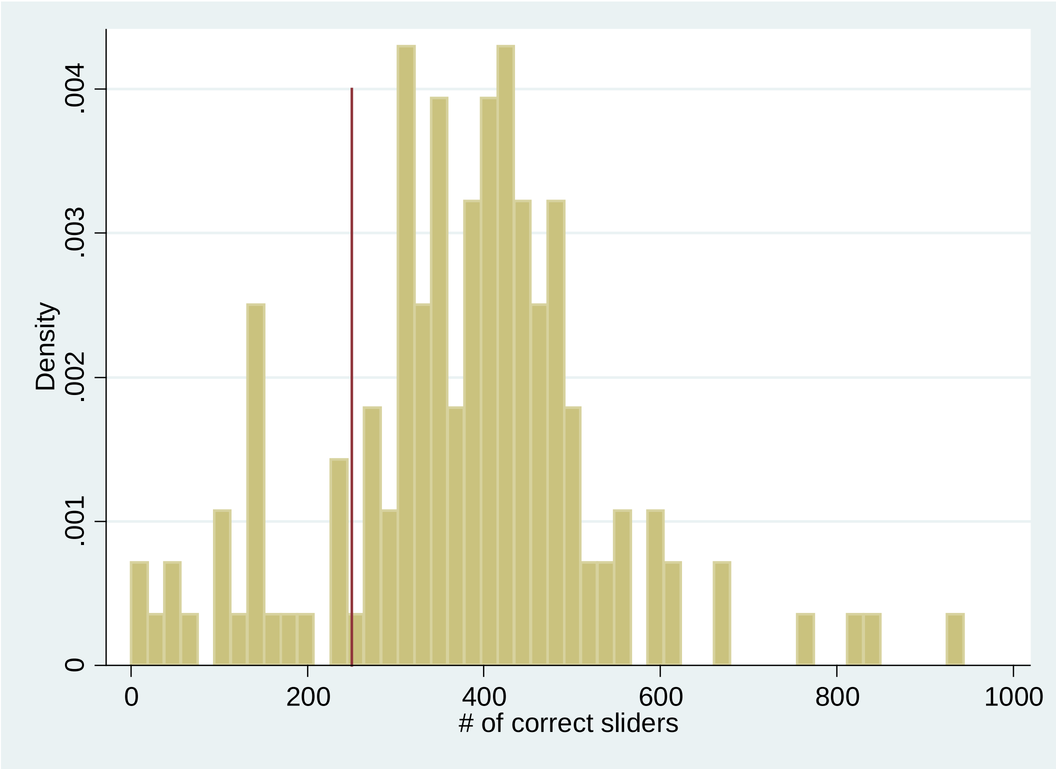
On average, subjects earn approximately £13, including the £2.5 show-up fee, labour related payments, lottery earnings and the £5 bonus offered before the lottery play. Labour-related earnings totalled on average approximately £5 and ranged between £0 and £16.38.

Individual work effort in the labour task can be captured by several measures. The overall effort is captured by the total number of correctly positioned sliders. This measure can be decomposed into two margins, namely working time and work intensity. Subjects choose both the amount of time they will spend on the labour task as well as how much effort they will put in per unit of time.

Figure 1 presents the distribution of correctly positioned sliders using all 148 subjects. As shown in the graph, there is wide variation in this measure of effort ranging from 0 to 944. As predicted by the classic income-leisure trade-off model, there is a dip in the density of correctly positioned sliders around the 250th threshold. This suggests that, on average, subjects do seem to perceive the sub-optimality of stopping just before the 250th slider.

On average, subjects position 383 sliders and work for 54 minutes. Out of the 148 subjects, 114 (77%) chose to work the maximum allotted time. Work intensity is on average 6.7 sliders per minute, ranging between 0 and 15.8. As expected, work intensity is higher among those who continue working for the maximum allocated time compared to those who stop early (7 vs. 5.6 sliders) with the difference being statistically significant at the conventional 5% level.

Figure 1: Distribution of number of correctly positioned sliders

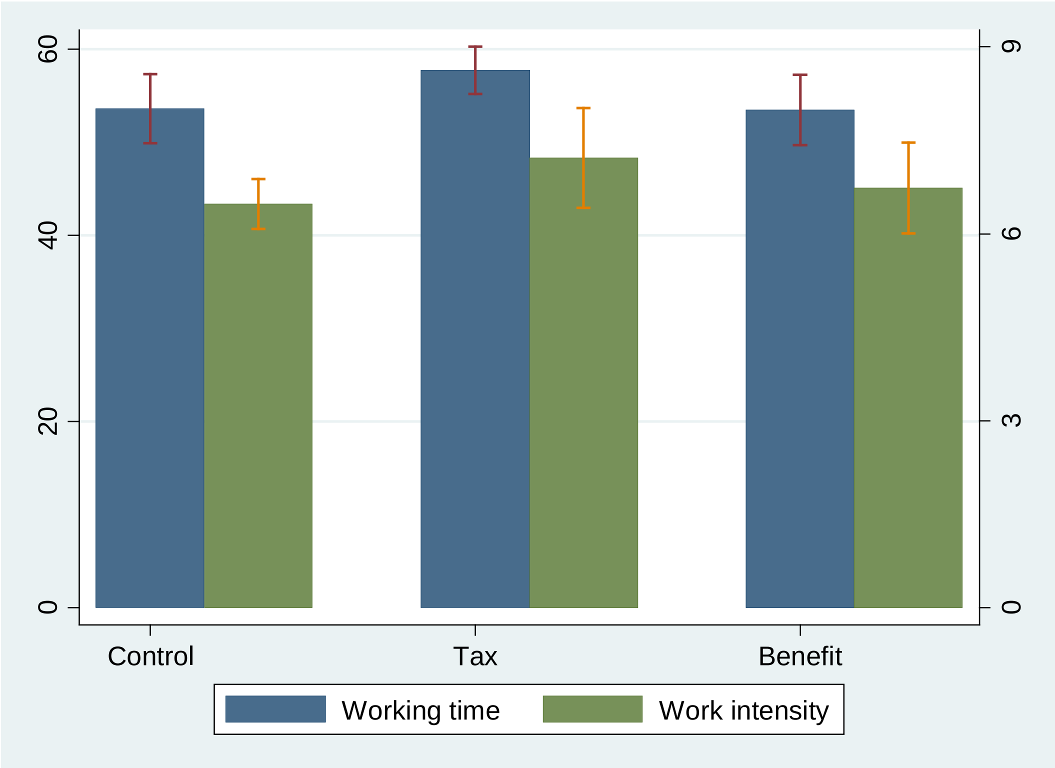


Note: Red line indicates the 250th slider where the effective piece rate doubles

Source: Own calculations.

Figure 2 plots average working time and average work intensity by treatment group. Both working time and work intensity are highest in the TAX treatment group. Only 14% of subjects experiencing the tax frame stopped early and on average work intensity is 7.21 sliders per minute. By contrast, working time is lowest in the BENEFIT condition. Out of the subjects in the group, 32% decide to stop working before the maximum 60 minutes elapse. Thus, the probability to stop early is twice as large in the BENEFIT treatment compared to the TAX treatment, a difference significant at the 10% level. Subjects in the BASELINE condition behave more similarly to the TAX group with approximately 20% deciding to stop working early. Differences between the three groups are much smaller in the case of work intensity. Subjects in both the BENEFIT and BASELINE complete on average 6.7 and 6.5 sliders per minute respectively. Finally, the average number of completed sliders is highest in the TAX group (422) and lowest in the BASELINE group (355) with the BENEFIT group in between (377). Only the difference between the TAX group and the BASELINE group is statistically significant (at the 5% level)

Figure 2: Mean working time and mean work intensity by treatment group

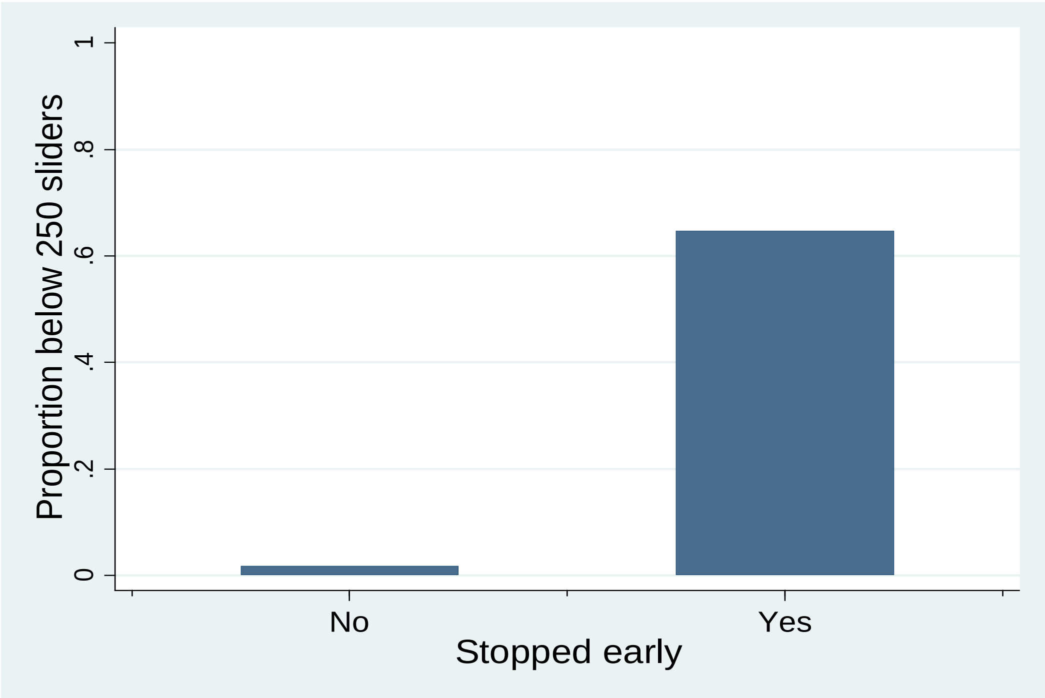


Notes: Left axis: probability of stopping work before the full 60 minute working period lapses; Right axis: number of correctly positioned sliders per minute; error bars computed based on 95% confidence intervals.

Source: Own calculations

One implication of the chosen experimental design is that framing effects should affect labour supply only up to and including the 250th slider. Beyond 250 sliders, all three groups are paid a uniform rate of 0.02£ per slider. It follows that differential labour supply responses to the three tax frames should be minimal (or at least much smaller) among subjects who exceeded the 250th slider threshold (although the proportion of subjects who surpass the 250th threshold is endogenous to the treatment). Figure 3 shows that in fact, over two thirds of those who stop early do so before reaching the 250th slider. Conversely, less than 10% (12 out of 124) of those who position more than 250 sliders correctly stop before the maximum allocated time is up. Thus, working time patterns are consistent with subjects reacting to the framing of the incentive structure.

Figure 3: Proportion not reaching the 250th slider by working time



Source: Own calculations

Table 1: Working time and work intensity by loss aversion and treatment

	Baseline		Tax		Benefit	
	LA<=6	LA>6	LA<=6	LA>6	LA<=6	LA>6
% stopping early	0.16	0.22	0.09	0.014	0.20	0.47
Difference	0.07		0.05		0.26**	
Mean work intensity	6.48	6.42	7.12	7.64	6.33	7.15
Difference	0.06		0.52		0.81	
Mean # sliders	352	348	424	440	363	381
Difference	-4.22		15.9		17.9	
N	53		29		46	

Note: LA=number of rejected lotteries (median value=6); **p<0.05.

Source: Own calculations.

Finally, a first impression of the potential role of loss aversion in shaping the behavioural response to the framing of incentives can be obtained by comparing working time and work intensity among individuals with varying degrees of loss aversion within treatment. As detailed in Section 3 above, loss aversion is measured by the number of lotteries a subject rejects to play. The loss aversion measure varies from 0 (no loss aversion) to 9 (most loss averse). In the subject pool, the median number of rejected lotteries is 6 (while the range is from 0 to 9). Based on their loss aversion measure, subjects have been categorized as less loss averse if they have a loss aversion measure smaller or equal to 6 (approximately 60% of subjects) and more loss averse if their loss aversion measure is strictly higher than 6 (approximately 40% of subjects). Table 1 displays mean work intensity, mean number of correctly positioned sliders and the probability of stopping early by treatment and loss aversion status. In the case of work intensity and number of correctly positioned sliders, differences between less loss averse and more loss-averse individuals are relatively similar across treatments. In the case of working time, albeit more loss-averse individuals are slightly more likely to stop early in every treatment, the difference becomes very large only in the BENEFIT treatment. This pattern suggests that loss-averse individuals may be particularly susceptible to be influenced by the benefit withdrawal frame.

5. Tax benefit frames, loss aversion and work effort

To better pinpoint the potential effect of framing on labour supply behaviour, a series of regression models have been estimated. I look at two outcomes separately, i.e. working time and work intensity. Because the vast majority of subjects choose to work for the maximum allowed, working time is measures by a dummy variable that takes the value of 1 if the individual chooses to stop working before the allocated 60 minutes elapse and 0 otherwise. Work intensity is measured as before as the number of correctly positioned sliders per minute. Control variables include session fixed effects as well as a series of variables that have been collected as part of the experiment. These are gender, age (in years), experience in participating in lab experiments, rated difficulty of the task (on a scale of 1 to 5), initial comprehension of the incentive structure (as measured by time needed to answer the verification questions), academic major (re-categorized in science related subjects vs. all the rest), and the average number of correctly positioned sliders per screen. Although, subjects were instructed to move to the next screen once they have filled in the current one, there was no requirement to correctly position all 48 sliders before moving on. Consequently, many subjects chose to move to the next screen before all sliders had been correctly positioned. In principle, positioning sliders should have been equally difficult whether subjects chose to switch screens often or not. However, it is possible that subjects might have felt that they will be more productive by switching screens which in turn may have affected their productivity and willingness to continue working. Conversely, switching screens often might be a signal that the subject was not very patient and disliked the task but continued to work nonetheless. To account for any confounding effects coming from these potential behaviours, models have been estimated with and without the average number of correctly positioned sliders per screen. It is possible that instead of reflecting effort, work intensity is better understood as a measure of productivity. If subjects gain more information about their own productivity levels as the labour task progresses, it is possible that they use this information to approximate potential labour earnings at the end of the working time period and thus to decide when to stop working. As a sensitivity check, working time models have been estimated with and without work intensity. The final set of models examines treatment effects by loss aversion status.

Table 2: Treatment effects on working time (odds ratios of stopping work before 60 minutes elapse)

	Model A	**Model B**	**Model C**	**Model D**	**Model E**	**Model F**
BASELINE	1.64	1.37	1.20	1.07	2.64	2.23
TAX	(ref. group)	(ref group)	(ref group)	(ref group)	(ref group)	(ref group)
BENEFIT	2.91*	2.82*	3.03*	3.19*	2.38	2.65
Baseline X loss aversion					2.33	2.76
Tax X loss aversion					1.46	1.39
Benefit X loss aversion					9.71**	13.49**
Session fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls		Yes	Yes	Yes	Yes	Yes
#sliders/screen			Yes	Yes	Yes	Yes
Work intensity				Yes		Yes

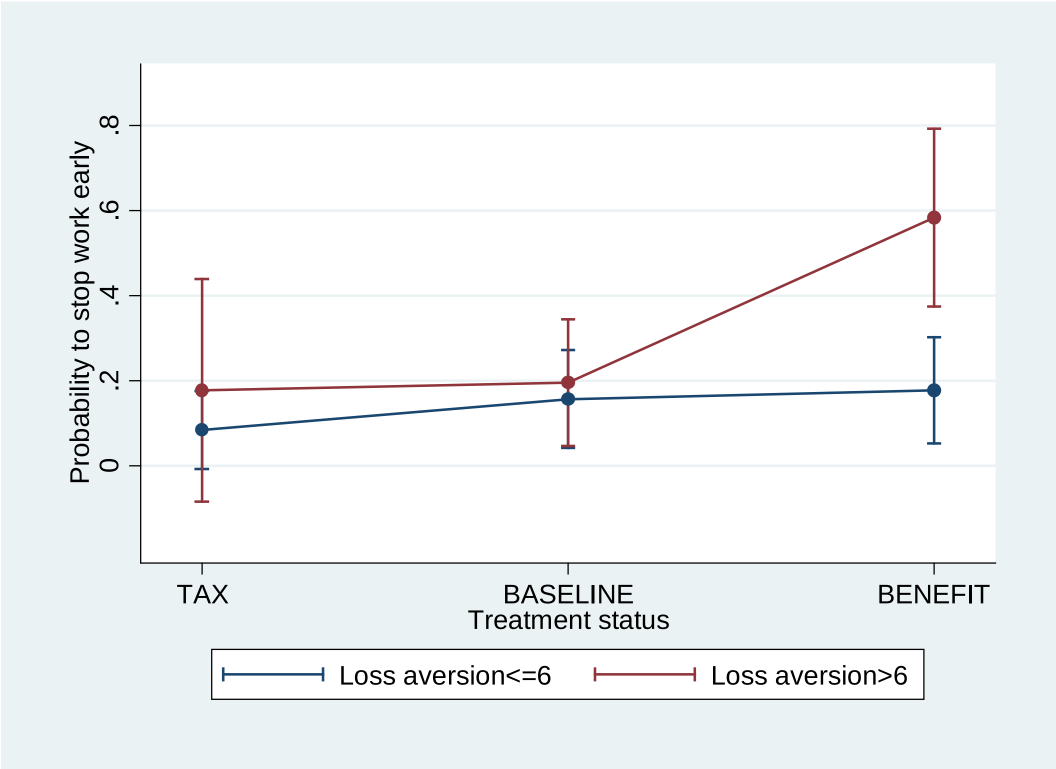
	Model A	Model B	Model C	Model D	Model E	Model F
N	145	143	143	146	123	123

Note:* significant at the 10% level; ** significant at the 5% level. Loss aversion is measured by a binary variable taking a value of 1 if the subject rejects more lotteries than the median. The full set of coefficients is available in Appendix 1.

Source: Own calculations.

Table 2 presents odds ratios of stopping work early (for a complete list of coefficients see Appendix 1). Including session fixed effects only (Model A) subjects in the BENEFIT group are almost three times more likely to stop working early compared to subjects in the tax group. Adding more control variables to the estimation (Models B, C and D), does not alter significantly the treatment effect. Subjects in the BENEFIT group always remain more likely to quit working early compares to subjects in the TAX treatment. The size of the effect varies slightly depending on the exact specification but hovers around the 300% mark. Adding work intensity to the estimation slightly increases the treatment effect on working time, but the substantive result remains unchanged. Models E and F show odds ratios for stopping work early by loss aversion status. As before, loss aversion is a binary variable with a value of one indicating the respondent rejected more than six lotteries (the median value). More loss averse individuals appear to be more likely to stop working early only in the TAX and BENEFIT frames. In the BASELINE frame, less loss averse individuals appear to be as likely to stop early as more loss averse subjects. In the TAX condition, loss-averse individuals are approximately 40% more likely to stop early, although the difference is not statistically significant. Conversely, in the BENEFIT treatment, the difference is very large and statistically significant. More loss-averse subjects in the BENEFIT frame are approximately 2.2 times as likely to stop working early compared to less loss-averse ones in the same group and approximately seven times more likely compared to less loss-averse individuals in the TAX treatment. Figure 4 shows the average marginal probability of stopping early by loss aversion status and treatment group using the specification in Model F.

Figure 4: Probability of stopping early by treatment group and loss aversion score



Note: Values represent average marginal effects; error bars corresponding to 95% CI.

Source: Own calculations

Few of the control variables included in the model appear to influence the decision to stop early. Individuals who switch screens less often (meaning they complete more sliders per screen) are less likely to stop early suggesting that frequent screen switching may be a sign of impatience. Supporting the hypothesis that experiment participants observe their own work intensity and interpret it as a productivity signal, individuals with higher work intensity are also less likely to stop early.

Table 3 displays results from OLS regressions estimating treatment effects on work intensity (a full set of coefficients can be found in Appendix 2). The framing of incentives appears to have little or no effects irrespective of specification. Work intensity is slightly higher both in the TAX and BENEFIT conditions compared to the BASELINE, in line with previous experimental results that found a ‘fiscal illusion effect’, i.e. higher gross (but not net) wages being correlated with higher work effort (Fochmann et al., 2013). The differences are however small and not statistically significant. The estimated coefficients are relatively stable across models. Compared to the TAX treatment, subjects in the BASELINE treatment position, on average, 0.6 fewer sliders and subjects in the BENEFIT group 0.2-0.4 fewer sliders, respectively. Finally, more loss-averse individuals appear to work slightly harder, completing about 0.5-0.6 more sliders per minute. However, the differences are again small and statistically insignificant.

Table 3: Treatment effects on work intensity (OLS coefficients)

	Model A	**Model B**	**Model C**	**Model D**
BASELINE	-0.50	-0.67	-0.59	-0.57
TAX	(ref group)	(ref. group)	(ref group)	(ref group)
BENEFIT	-0.23	-0.41	-0.34	-0.45
Baseline X loss aversion				0.57
Tax X loss aversion				0.47
Benefit X loss aversion				0.65
Session fixed effects	Yes	Yes	Yes	Yes
Additional controls		Yes	Yes	Yes
#sliders/screen			Yes	Yes
N	145	143	123	123

Note: significant at the 10% level; ** significant at the 5% level. Loss aversion is measured by a binary variable taking a value of 1 if the subject rejects more lotteries than the median. The full set of coefficients is available in Appendix 2.

Source: Own calculations

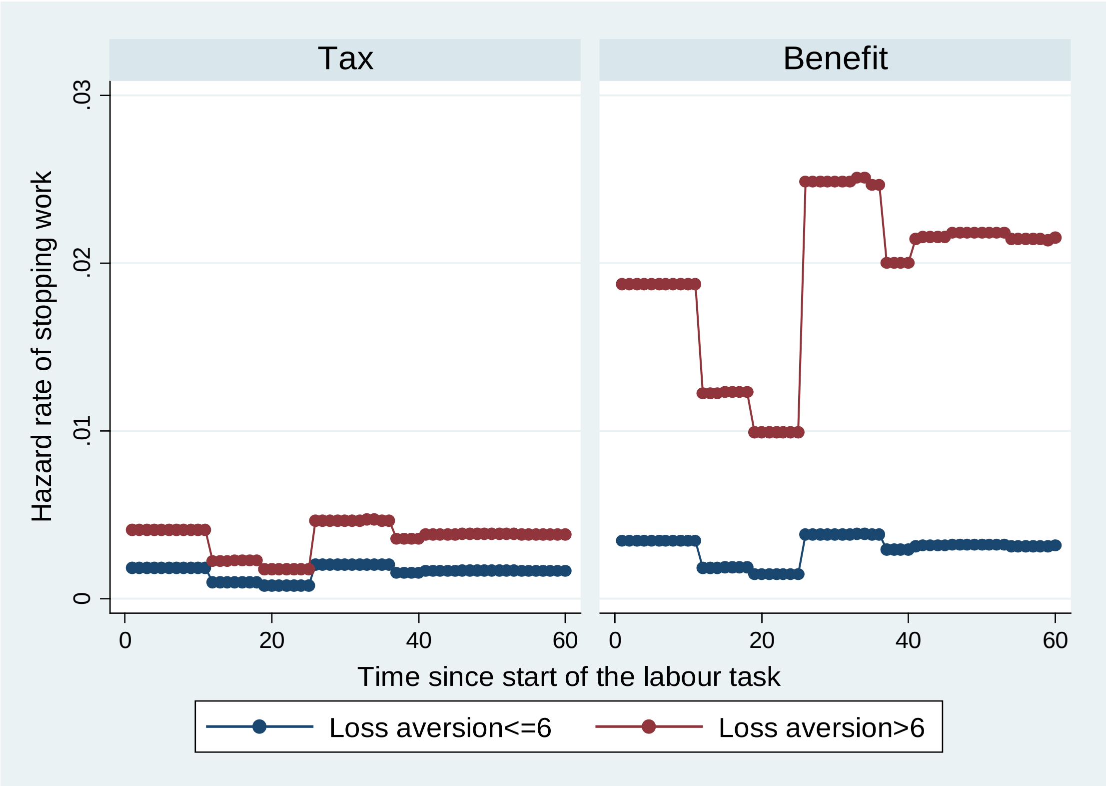
Women, subjects who rate the task as more difficult, as well as subjects who initially show lower levels of comprehension of the pay structure have lower levels of work intensity. Individuals who switch screens less often are also more likely to have higher work intensity levels. The other control variables do not appear to play a role.

Robustness checks

Out of the 148 subjects participating in the labour task, three have consistently chosen to use only one slider out of the 48 present on the screen. Although the estimation takes into account the average number of positioned sliders per screen, it is possible that these three outliers unduly influence the final set of results. To check this possibility, all models have been re-estimated after the three outliers have been dropped. Results are presented in Appendices 1 and 2. Albeit significance levels drop in some cases, the magnitude of the estimated coefficients is very similar to that in the main set of results presented above and the substantive conclusions remain. Incentive framing appears to have no effect on work intensity but it does affect working time. Subjects in the BENEFIT group stop work earlier with loss-averse individuals appearing to be particularly sensitive to this frame.

Using the series of nine lotteries to measure loss-aversion requires individuals to be consistent and have only one switching point. Most individuals in the experiment do indeed switch only once but 20 individuals have more than one switching point. While a definitive interpretation of this behaviour has not been established in the literature, some authors suggest inconsistency in lottery choices is a sign of confusion or misinterpretation of the task requirements (Jacobson and Petrie, 2009). As such, labour supply patterns among inconsistent individuals may reveal information about the mechanisms behind treatment effects. If differential behaviour in the BENEFIT frame is due to misunderstanding or inattention at the task at hand, any effects should be enlarged among inconsistent participants. Appendix 4 shows this is not the case. Due to the very small number of cases, only raw differences are calculated. Inconsistent subjects in the BENEFIT condition are approximately 50% more likely to stop working early compared to subjects in the TAX treatment. By contrast, consistent subjects in the BENEFIT treatment are approximately 3.8 times more likely to stop early. Thus, the framing effect is considerably *smaller* among inconsistent individuals (albeit the sample of inconsistent individuals is very small).

Figure 5: Predicted hazard rates of stopping work by treatment and loss aversion status



Source: Own calculations

Another possibility of analysing the experimental data is to model explicitly the time until work is stopped, treating subjects who work for the entire allocated 60 minutes as censored. This approach has the advantage of using all the information about working times not just whether the person stopped before the maximum allocated time was exhausted or not. It also enables me to control for any effects that the passage of time might have on working times independently of the treatment. The hazard rate of stopping work has been modelled using a discrete time piecewise specification that in addition to three time dummies, also controls for session fixed effects, gender, age, experience with participating in experiments, rated difficulty of the task, initial comprehension of the incentive structure, academic major and number of correctly positioned sliders per screen. As before, treatment effects are allowed to differ between less and more loss-averse individuals. Figure 5 plots the predicted conditional probabilities of stopping work by treatment and loss aversion status (a full set of regression coefficients is available in Appendix 5). In line with the simple logit results, subjects in the

BENEFIT treatment are more likely to stop work at any time but only if their loss aversion scores are above median. This difference is significant at the 1% level.

6. Discussion and conclusions

Using an incentivised real effort task, this study has examined differences in the behavioural responses to direct taxation of future earnings and implicit taxation via benefit withdrawal. Subjects were asked to perform a tedious, boring but relatively simple task (the so called 'slider-task) in exchange for experimental earnings. Subjects choose both the amount of time they were willing to put into the labour task and the effort they exerted per time unit. The net incentives were directly proportional with the amount of effort and were kept constant across treatment groups. Their framing was however varied.

Results indicate that participants who were exposed to a benefit withdrawal frame were more likely to stop working early compared to participants experiencing a direct tax frame. The difference in the probability to stop early is large (approximately 100-200%) in all specification and statistically significant in most. More elaborate models indicate that more loss-averse individuals are more likely to respond to the benefit withdrawal frame by stopping work early compared to individuals in other groups. This pattern suggests that loss aversion may play a role in shaping behavioural responses to social and fiscal policy, although it is possible that other individual features correlated with loss aversion are responsible for the differential response.

Albeit not conclusive, results presented in this paper partially contradict the finding that instrumental objects are not affected by loss aversion (Kahneman et al., 1990). Further research is needed to disentangle under what circumstances instrumental objects (and hence income) are treated more like consumption objects.

Finally, the experimental findings of this study have important implications for fiscal and social policy design. If loss averse individuals reduce their labour supply more in reaction to benefit withdrawal compared to direct taxation, policy makers may want to re-examine the framing as well as the design of income-tested benefits.

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Appendix 1: Odds ratios of stopping work early

Model	A	B	C	D	E	F	G	H	I	J	K	L
BASELINE	1.61	1.38	1.34	1.18	2.64	2.65	1.64	1.37	1.20	1.07	2.23	2.23
TAX	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group
BENEFIT	2.91*	2.82*	3.03*	3.19*	3.46	3.25	2.83*	2.70	2.49	2.68	2.38	2.65
BASELINE X loss aversion					2.74	3.28					2.33	2.76
TAX X loss aversion					1.44	1.44					1.46	1.39
BENEFIT X loss aversion					7.74**	13.62**					9.71**	13.5**
Session 1	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group
Session 2	0.72	0.48	0.52	0.45	0.70	0.56	0.70	0.47	0.45	0.41	0.59	0.52
Session 3	2.75	2.64	2.74	3.34*	2.61	4.15	2.68	2.53	2.37	2.84	2.36	3.50
Session 4	1.11	0.96	0.91	1.04	0.88	1.15	1.19	1.00	1.08	1.03	1.20	1.15
Session 5	1.22	1.23	0.96	0.51	0.71	0.31	1.19	1.19	0.63	0.43	0.41	0.25
Female		1.27	1.44	1.15	1.53	1.32		1.28	1.76	1.38	2.05	1.63
Age		1.02	1.01	0.99	1.01	0.98		1.03	1.01	0.99	1.01	0.98
Science		0.64	0.62	0.56	0.49	0.43		0.67	0.72	0.62	0.60	0.47
Rated difficulty		0.88	0.85	0.73	1.27	1.10		0.88	0.75	0.71	1.15	1.10
Experience		1.02	1.01	1.00	1.03	1.02		1.02	1.01	1.00	1.03	1.03
Non-		1.05	1.02	0.80	1.17	1.05		1.05	0.96	0.79	1.14	1.03

comprehension												
#sliders/screen			0.95**	0.96*	0.93**	0.95**			0.91**	0.93**	0.89**	0.92**
Work intensity				0.59**		0.48**				0.66**		0.55**
Outliers excluded	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
N	148	146	146	146	126	126	145	143	143	143	123	123

Note: age measured in years; science measured as a binary variable taking a value of 1 if the subject declares her major to be biological sciences, computer science and engineering, finance, economics and business or mathematical sciences and 0 otherwise; rated difficulty measured on a 5 point scale; experience measured as number of previous participations in experiments; non comprehension measured as a binary variable taking a value of 1 if the subject needed more than120 seconds to answer the control questions and 0 otherwise

Source: Own calculations

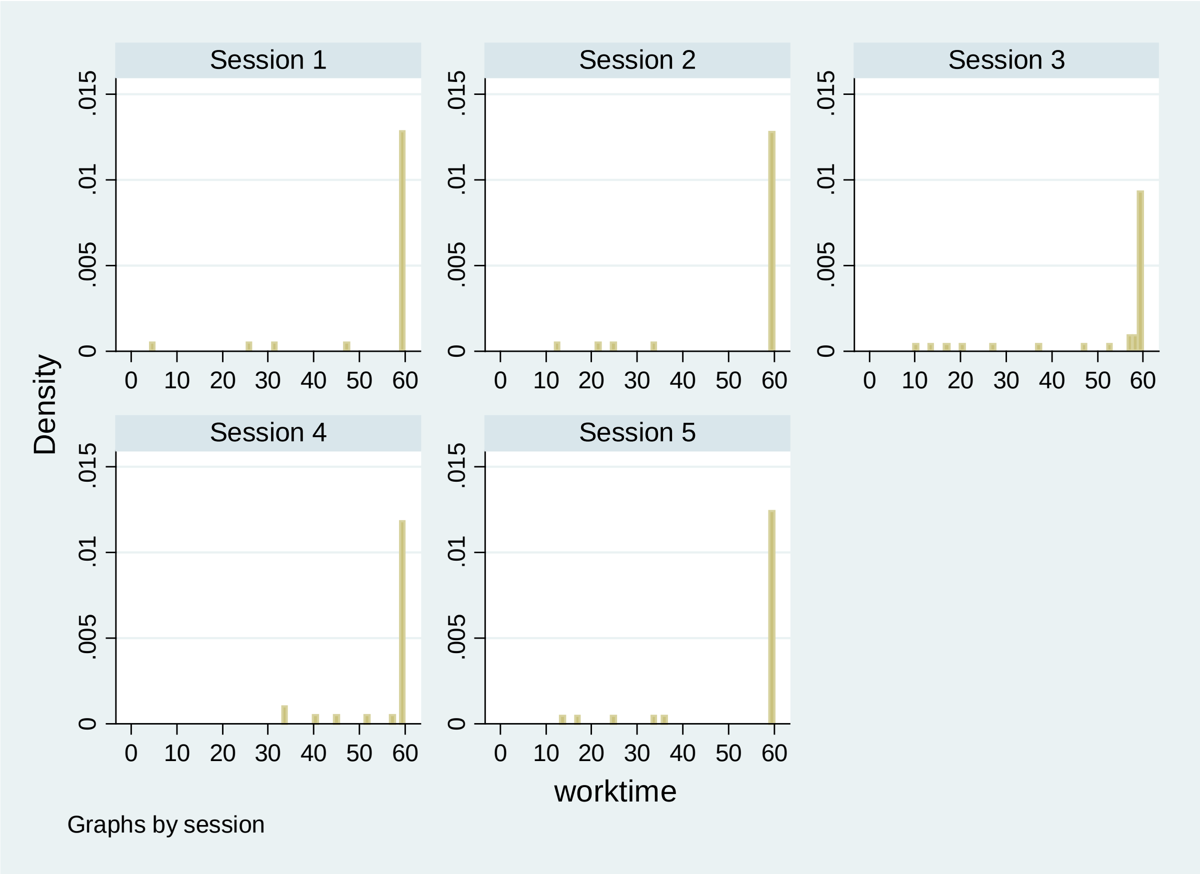
Appendix 2: OLS regression coefficients of work intensity

Model	A	B	C	D	E	F	G	H
BASELINE	-0.50	-0.67	-0.59	-0.57	-0.63	-0.73	-0.61	-0.62
TAX	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group
BENEFIT	-0.23	-0.41	-0.34	-0.45	-0.26	-0.39	-0.17	-0.28
BASELINE X loss aversion				0.57				0.50
TAX X loss aversion				0.47				0.34
BENEFIT X loss aversion				0.65				0.50
Session 1	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group	Ref group
Session 2	0.13	0.42	0.35	0.45	0.10	0.40	0.39	0.47
Session 3	-0.04	-0.04	0.01	0.34	-0.09	-0.04	0.19	0.49
Session 4	-0.15	0.05	0.12	0.15	-0.40	-0.11	-0.13	-0.15
Session 5	-1.65**	-1.39**	-1.17**	-1.09*	-1.68**	-1.40**	-0.89*	-0.77
Female		-0.76**	-0.85**	-0.83**		-0.73**	-0.92**	-0.90**
Age		-0.04*	-0.03	-0.03		-0.04**	-0.03	-0.02
Science		0.17	0.18	0.32		0.09	-0.02	0.06
Rated difficulty		-0.29*	-0.26	-0.21		-0.25	-0.16	-0.11
Experience		-0.05	-0.04	-0.05		-0.05	-0.04	-0.05
Non-comprehension		-0.83**	-0.79*	-0.77*		-0.83*	-0.69*	-0.68
#sliders/screen			0.05**	0.05**			0.09**	0.09**
Outliers excluded	No	No	No	No	Yes	Yes	Yes	Yes
N	148	146	145	126	145	143	143	123

Note: age measured in years; science measured as a binary variable taking a value of 1 if the subject declares her major to be biological sciences, computer science and engineering, finance, economics and business or mathematical sciences and 0 otherwise; rated difficulty measured on a 5 point

scale; experience measured as number of previous participations in experiments; non comprehension measured as a binary variable taking a value of 1 if the subject needed more than120 seconds to answer the control questions and 0 otherwise.
Source: Own calculations

Appendix 3: Distribution of leaving times (in minutes) by session



Source: Own calculations.

Appendix 4: Treatment effects for consistent and inconsistent individuals in the lottery task

	Working time (odds ratios of stopping before the maximum allocated time elapses)		Work intensity (OLS coef.-s-sliders per minute)	
	Consistent	Inconsistent	Consistent	Inconsistent
BASELINE	2.02	0.56	-0.79	-0.40
TAX	Ref. group	Ref. group	-	
BENEFIT	3.79*	1.5	0.61	0.79
N	128	20	128	20

Source: Own calculations

Appendix 5: Hazard Ratios of stopping work

	Model A	Model B
TAX		
BASELINE	1.23	2.66
BENEFIT	1.44	1.93
TAX x loss aversion	1.11	2.35
BASELINE x loss aversion	0.26	1.02
BENEFIT x loss aversion	1.55**	7.15**
Duration	Yes	Yes
Age	Yes	Yes
Science	Yes	Yes
Rated difficulty	Yes	Yes
Experience	Yes	Yes
Non-comprehension	Yes	Yes
#sliders/screen	Yes	Yes
Outliers excluded	No	Yes
N	6882	6702

Notes: **p<0.01; duration is modelled via three dummies: below 25 minutes, 25-40 minutes and more than 40 minutes.
Source: Own calculations.

Annex 6: Experimental instructions

Common introduction:

“Please turn off your mobile phones and keep them turned off for the entire duration of the experiment.

Please note that communication with other participants is not allowed. Failure to comply may result in exclusion from the experimental session and all associated payments.

Before the experiment begins, there will be an opportunity to ask questions. If you would like to ask a question after the experiment begins, please raise your hand and the experimenter will come to your booth.

By participating in this experiment you have the opportunity to earn money. For showing up on time, you will receive a fixed payment of 2.5£. In addition to this fixed payment, you will be paid a variable amount depending on the decisions you will take during the experiment.

All payments will be made at the end of the experiment based on your randomly allocated Participant Number (PN). You can find your participant number by opening the brown envelope you received when entering the lab. Please keep this envelope as you will need it to receive your payment.

*o receive your payment you **MUST** complete the questionnaire at the end of the experiment. This is when you will be asked for your Participant Number (PN) which will be used to pair you with your earnings during the experiment.”*

Baseline condition

“You can earn money by correctly positioning sliders on the computer screen. The amount you earn depends on the number of sliders you position at exactly 50. We will call a slider positioned at exactly 50 a 'correctly positioned' slider. You can check the position of a slider by looking at the number displayed immediately to its right.

For each correctly positioned slider you will be paid 1 penny up to and including the 250th slider. Starting with the 251st slider you will be paid 2 pence per correctly positioned slider.

There will be 48 sliders on the screen. If you have positioned them all correctly and wish to continue with the task, please press the "Continue working" button .This will open a new screen with 48 new sliders which you can continue positioning at 50 to increase your earnings. There is no limit on the number of sliders you can position. However, this stage of the experiment will automatically end after one hour. Thus, you can continue to position sliders and earn money for doing it for up to one hour.

You can stop working at any time. In this case, you will be directed to a short questionnaire. After filling in the questionnaire, you are free to leave the experiment and collect your earnings. To stop working at any time press the "Stop working" button at the bottom left of the screen. This will end the first stage of the experiment and will direct you to the questionnaire in the second stage.

Before proceeding, please answer the following questions."

Tax condition

"You can earn money by correctly positioning sliders on the computer screen. The amount you earn depends on the number of sliders you position at exactly 50. We will call a slider positioned at exactly 50 a 'correctly positioned' slider. You can check the position of a slider by looking at the number displayed immediately to its right.

You will be paid 2 pence for each slider you position correctly. However, you will also have to pay taxes on the amount of money you earn. For the first 250 sliders you position correctly, you will pay a 50% tax. There is no tax applicable starting from the 251th slider onwards

There will be 48 sliders on the screen. If you have positioned them all correctly and wish to continue with the task, please press the "Continue working" button .This will open a new screen with 48 new sliders which you can continue positioning at 50 to increase your earnings. There is no limit on the number of sliders you can position. However, this stage of the experiment will automatically end after one hour. Thus, you can continue to position sliders and earn money for doing it for up to one hour.

You can stop working at any time. In this case, you will be directed to a short questionnaire. After filling in the questionnaire, you are free to leave the experiment and collect your earnings. To stop working at any time press the "Stop working" button at the bottom left of the screen. This will end the first stage of the experiment and will direct you to the questionnaire in the second stage.

Before proceeding to the session, please answer the following questions."

Benefit condition

"*You can earn money by correctly positioning sliders on the computer screen. The amount you earn depends on the number of sliders you position at exactly 50. We will call a slider positioned at exactly 50 a 'correctly positioned' slider. You can check the position of a slider by looking at the number displayed immediately to its right.*

For each slider you position at 50 you will be paid 2p. However, for each slider you position correctly you will also have to pay the experimenter 1p from your show-up fee of 2.5£ until this amount is exhausted. After exhausting the show-up fee, you will not have to pay anything to the experimenter anymore but will continue to earn 2p per correctly positioned slider.

There will be 48 sliders on the screen. If you have positioned them all correctly and wish to continue with the task, please press the "Continue working" button .This will open a new screen with 48 new sliders which you can continue positioning at 50 to increase your earnings. There is no limit on the number of sliders you can position. However, this stage of *the experiment will automatically end after one hour. Thus, you can continue to position sliders and earn money for doing it for up to one hour.*

You can stop working at any time. In this case, you will be directed to a short questionnaire. After filling in the questionnaire, you are free to leave the experiment and collect your earnings. To stop working at any time press the "Stop working" button at the bottom left of the screen. This will end the first stage of the experiment and will direct you to the questionnaire in the second stage.

Before proceeding to the session, please answer the following questions."

Verification questions

"Suppose there are four subjects such as yourself participating in this session, which will be referred to as A, B, C and D. Please look at the table below. The table lists the number of sliders each participant has correctly positioned at the end of this session. How much did each participant earn during the session? Please fill in the table. You will only be able to proceed once you have answered correctly all questions. If you don't know the correct answer, please raise your hand and an experimenter will come to explain."

Baseline condition

	A	*B*	*C*	*D*
No of correctly positioned sliders	*200*	*300*	*500*	*700*
Earnings (in pence)				

Tax condition

	A	*B*	*C*	*D*
No of correctly positioned sliders	*200*	*300*	*500*	*700*
Gross earnings (in pence)				
Taxes paid (in pence)				

Benefit condition

	A	*B*	*C*	*D*
No of correctly positioned	*200*	*300*	*500*	*700*

sliders				
Gross earnings (in pence)				
Amount paid to the experimenter from the show up fee (in pence)				

Control sliders

Please position the following 3 sliders at 50.

You will only be able to proceed once you have correctly positioned all sliders. If you cannot position the sliders, please raise your hand an experimenter will come and help.

Note that you can only use the left button of the mouse. Both the keyboard arrows and the mouse scroll wheel have been disabled.

Introduction to the labour task

The main part of the experiment is about to start.

You have a maximum of 1 hour to position sliders and earn money for correctly positioned ones. The amount of remaining time (in seconds) will be shown on the top right corner of the screen.

If you have positioned all 48 sliders on the screen and wish to continue, press the 'Continue working' button.

Once you have decided to stop working, press the 'Stop working' button.

Do you have any questions?

If you do not have any questions, please press the OK button to continue.

The first part of the experiment is over.

The total number of sliders you have positioned correctly is: <No sliders >

Your earnings (in £) during this round are: <Profit >

Press continue to proceed to the second part of the experiment

Lottery stage

For successfully reaching this stage of the experiment, you receive 5£. This is in addition to any experimental earnings in the previous round and in addition to the show-up fee.

The next stage of the experiment consists of 9 lotteries.

or each lottery, you can choose whether to accept the lottery in which case the lottery may be played out meaning you could win or lose money or to reject the lottery in which case you receive 0£ for this stage and your current earnings are unaffected.

After you will have made your choices, one lottery will be selected at random.

If you indicated that you accept the lottery, the lottery will be played out and winnings paid or losses subtracted from your current earnings according to the result.

If you have indicated you reject the lottery, you will receive 0 £ for the lottery stage of the experiment and your previous earnings (including the 5£ bonus for reaching this stage) will be unaffected.