Complex Tax Incentives - An Experimental Investigation*

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Abstract

This paper studies how complexity influences choices. We analyze how the complexity of the decision environment affects the impact of newly introduced incentives. To answer this question, we conduct a real-effort laboratory experiment. Subjects decide how much effort to exert; they receive a piece rate and face a number of taxes. In one treatment the tax system is simple; in the other treatment it is highly complex. The payoff-maximizing effort level and the incentives around this optimum are, however, identical across treatments. We introduce the same sequence of new tax rules in both treatments. We find that subjects in the complex treatment adjust their effort provision less in response to the new taxes than subjects in the simple treatment. Many subjects in the complex treatment even re-choose their previous effort level, ignoring the new rule entirely. These results suggest that the effect of a newly introduced tax will be attenuated in tax systems with higher levels of complexity.

Keywords: Complexity, taxation, attention, salience, laboratory experiment

JEL Codes: C91, D03, H31, J22

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1 Introduction

Economic decision makers are often faced with highly complex decision environments. Income tax systems are full of non-linearities, thresholds and withdrawals, as are many consumer price schedules, e.g. for utilities or telecommunication, and most income transfer programs (e.g., Liebman and Zeckhauser, 2005). Yet, economic theory by and large assumes that decision makers see through all complexity and decide on the basis of the true underlying implied incentives and constraints (Mullainathan and Thaler, 2000). According to this assumption, complexity per se does not influence behavior.

There is, however, growing evidence that many people do not behave in a fully rational way but are rather boundedly rational (see, e.g., Conlisk, 1996; Rabin, 1998; Mullainathan and Thaler, 2000; Kahneman, 2003; and DellaVigna, 2009 for perspectives from different literatures in economics and psychology). These people will have difficulties to cope with a complex decision environment and may not always be able to choose the first-best optimum that they would choose could they see through the complexity of the decision environment.

In this paper, we investigate whether and, if so, how the complexity of the economic decision environment influences decisions. In particular, we analyze how the complexity of the pre-existing incentive system influences the reaction to changes in economic incentives. The setting we consider is that of effort provision under tax regimes that differ in the level of complexity. Existing tax systems are usually highly complex and there is indirect evidence that people are not able to react optimally to such tax systems: they do not bunch at kinks to the extent that would be expected if marginal incentives were fully understood (Saez, 2010); labor supply seems to react to the average tax rate in the previous year (Feldman and Katuscak, 2009); EITC-eligible individuals adjust their labor supply if they receive personalized advice on their tax incentives (Chetty and Saez, 2009). Yet, there is little direct evidence about whether and how complexity of the tax system at large affects behavior.

1 There are different ways of measuring the level of complexity of existing tax systems. Slemrod and Sorum (1985) estimate that the filing costs of filing federal and state taxes for Minnesota tax payers are in the order of 5 to 7 percent of revenue raised. Another simple metric one could use to illustrate the complexity of a tax system is pages of tax law. The legal regulations pertaining to income taxation in Germany, for example, reach more than 1,200 pages of text (for some light bedtime reading see, e.g, Einkommensteuerrecht, 2012, dtv, Munich).
An ideal data set to study this question would contain observations of behavior in tax systems that only differ in the level of complexity. For lack of such exogenous variation in complexity when using field data we chose to conduct tightly controlled real-effort laboratory experiments. In the experiment, we can exogenously change the complexity of the decision environment and can introduce new tax rules; we are therefore able to draw causal inferences about the impact of complexity on decision making.

In our experiment, subjects work on a real-effort task in a setting that mimics a progressive tax system. The design features two between-subjects treatments: in the simple treatment (ST), the tax system features very few, simple rules that determine incentives. The complex treatment (CT) implements a tax system that features economic incentives that are almost identical to the ones in ST, in particular, the optimal effort choice and the payoffs around the optimum are identical; however, there are many more tax rules—some of which partially cancel each other—leading to a tax system with a higher degree of complexity. In our experiment, the complex treatment mimics the large number of distinct rules that characterize real tax systems; the simple treatment is designed to come closer to implementing the assumptions economists typically make when modelling behavioral responses to tax incentives, for instance, that individuals broadly understand the incentives they face and know their marginal tax rate. In each of three rounds of the experiment, subjects received a piece rate for each unit of (real-effort) output produced; they also had to pay a number of taxes and received a number of subsidies. In each round, an additional tax or subsidy was introduced changing the payoff maximizing number of units of output. These additional rules were identical across the two treatments. This design allows for an analysis of how the behavioral response to (tax) incentives changes due to ceteris paribus changes in background complexity.

We find that in the complex treatment, subjects choose the payoff-maximizing number less often, more generally choose output levels further away from this optimum, and, as a consequence, make less money than subjects in ST. This means that subjects are indeed influenced by the complexity of the decision environment. How does the complexity of the decision environment affect the impact of changes in incentives? Subjects in the complex
treatment under-react to the newly introduced tax rules and do not adjust their effort provision strongly enough towards the new optimal choice. For some subjects this under-reaction is so strong that they re-choose their previous decision, ignoring the new tax rule completely. Overall, we find that the fraction of subjects in the complex treatment who do not adjust their decision despite a change in incentives from round to round is 8.5 to 11 percentage points higher than the corresponding fraction in ST. An economic interpretation of this finding is that an increase in the complexity of the decision environment lowers individuals’ price elasticities.

In order to further interpret our findings, it is important to understand the psychological mechanisms underlying the behavior observed in the experiment. A growing literature provides evidence suggesting that mental resources are limited and can be depleted, for instance, by exerting cognitive effort (see, e.g., Baumeister, Bratslavsky, Muraven, and Tice, 1998; Pocheptsova, Amir, Dhar, and Baumeister, 2009). In our experiment, subjects in the first round of CT are faced with a cognitively demanding decision problem which has the potential to affect decision-making in the subsequent rounds of the experiment by depleting cognitive resources. Several pieces of evidence suggest that the depletion of mental resources is indeed an important mechanism to understand behavior in our experiment. First, subjects in CT need substantially longer to reach a decision in the first round compared to subjects in ST suggesting that more cognitive effort was needed to tackle the complex decision problem. Second, in the post-experimental questionnaire subjects in CT report higher stress levels regarding the decision-making process. Third, subjects in CT also report lower levels of confidence in their having chosen the payoff-maximizing effort level. That subjects in CT are less confident about their (previous) choice makes it all the more remarkable that they then choose exactly the same effort level again. This behavior, however, is in line with the first round depleting the mental resources of subjects in CT who then are not able or willing to exert enough effort in the second and third round but rather re-choose their previous effort level.

A key difference between our experiment and the existing literature is that we change the complexity of the whole tax system that subjects face; new tax rules that are introduced each
round are identical in the complex and the simple treatment. Previous studies varied the salience of one tax rule or one part of a price schedule to see how salience and complexity influence decisions. Chetty, Looney, and Kroft (2009), for example, show in several ways that consumers under-react to changes in non-salient taxes. Work by Finkelstein (2009) shows that automating toll collection—which lowers the salience of the toll—leads to a reduction in the toll elasticity of driving. Our design allows us to study how the reaction of subjects to a new tax rule depends on the complexity of the tax system in which the new rule is embedded. As our results indicate, higher levels of background complexity mute the reaction to new tax rules and lead to a higher prevalence of individual-level inertia. On a more general level, our findings therefore support the view that complexity of the decision environment is an important catalyst of behavioral anomalies such as the status quo bias (Samuelson and Zeckhauser, 1988; Kahneman, Knetsch, and Thaler, 1991; Fleming, Thomas, and Dolan, 2010).

Beshears, Choi, Laibson, and Madrian (2012) point out a different mechanism through which complexity affects decision-making; they provide evidence that lowering the complexity of the decision decreases the tendency to stick to an exogenously imposed default and argue that decision-makers’ tendency for procrastination prevents active choice in the presence of high complexity. This effect is different from what we find as all subjects in our experiment were forced to choose and actively chose the same effort provision again.

Our findings help to inform how complexity can be explicitly used as a tool by policymakers. A key take-away of our study is that high levels of complexity of an existing tax system reduce the effectiveness of new (tax) policies. This could be desirable if the goal is to shroud the economic impact of the tax—for instance if the efficiency costs of taxing a good are large due to a high price elasticity—but could be harmful in the case of a tax on some socially undesirable activity (e.g., polluting). Congdon, Kling, and Mullainathan (2011) discuss further intriguing examples for how complexity can be used to achieve social goals (e.g., by acting as a screening device).

Finally, the psychological mechanism that we argue underlies the effects we observe suggests that higher levels of tax complexity have non-negligible economic costs: first, there are time
costs of tax compliance (Slemrod and Sorum, 1985) as well as direct mental costs. Second, and plausibly more important, higher levels of complexity reduce the quality of decision-making both in the domain affected by the complexity as well as indirectly in other domains by depleting mental resources.

The rest of the paper is organized as follows: the next section describes the design of the experiment. We present results in the third section.

2 Experimental Setup

2.1 Overview

In our experiment, subjects work on a real-effort task in a setting that mimics a progressive tax system. Subjects have to move sliders on the screen and get a piece rate for each correctly positioned slider; they also have to pay taxes and receive subsidies depending on the total number of sliders they position. In three rounds, subjects decide how many sliders they want to position: they see the tax rules that apply to their decision, commit to a number of sliders and then position the committed number. The only difference between the three rounds is that one additional tax rule is introduced after each round. All previously applicable rules remain valid. Subjects can be in one of two treatments, the simple treatment (ST) or the complex treatment (CT). The only difference between the treatments is the number of tax rules in the first round (and therefore in the following rounds): 2 in the simple treatment or 22 in the complex treatment. The decision environment in CT is therefore much more complicated than in ST. The number of sliders that maximize payoff and the marginal payoff around this optimum is, however, identical across treatments. The newly introduced tax rules are also identical across treatments.
In the first round of ST, subjects initially face a piece rate of 100 points and a subsidy of 15 points per unit of output produced, i.e., per correctly-positioned slider. The payoff per unit of output is reduced by 2 points per additional unit of output so that, e.g., the second unit of output delivers a piece rate of 113 points. Schedule A in Figure 1 displays the marginal taxes subjects face in the first round of ST. The orange line denotes the baseline piece rate subjects receive for each slider. The blue line shows marginal taxes including all taxes and subsidies. Monetary payoff is maximized at the point where the baseline piece rate and the schedule of marginal taxes intersect. In this case, this corresponds to 58 sliders as indicated by the vertical dashed line in blue—the net payoff for each additional slider is negative.

In the two subsequent rounds, additional taxes or subsidies that have constant levels per unit of output are levied while the progressivity of the tax system is not changed. This leads to parallel shifts of the marginal tax schedules. The number of units of output that subjects need to produce to maximize their monetary payoff thus changes in each round. Schedules B (red)
and C (green) in Figure 1 display the marginal taxes that subjects in the simple treatment face in rounds 2 and three. All subjects in ST face schedule A in the first round. As a robustness check, we randomize the order in which subjects in ST face schedules B and C so that half of the subjects in ST face the tax schedules in the order A-B-C and the other half in the order A-C-B. The number of units of output that maximize an individual’s payoff are 42 for schedule B, and 25 for schedule C. Panel 1 of Table 1 shows an overview of the parameters of the tax schedules subjects face in ST. Throughout the experiment, subjects are not told the number of units of output that maximize their payoff.

Figure 2: Screenshot of the Tax Schedules for Round 1 in ST and CT

The key difference between the complex treatment and the simple treatment is that a number of additional tax rules are in place. We have instituted a mix of additional taxes and subsidies some of which are in place for a limited range of output. If one defines a single rule as a linear tax which applies to a sequence of adjacent sliders, then tax schedule A of ST contains 2 rules and tax schedule A of CT contains 22 rules. The tax schedules B and C each add one additional so-defined rule. Figure 2 shows an overview of tax schedule A in ST and CT as seen by subjects in the first round of the experiment (see instructions in Appendix C for the exact wording of all tax schedules). Schedule A in Figure 3 displays the marginal taxes subjects in CT face in the first round. Again, the orange line denotes the baseline piece rate subjects receive for each unit of output. The blue line shows marginal taxes—including all taxes and
subsidies—for each unit of output. As some of the additional rules in CT cancel each other out, the schedule displayed in Figure 3 does not show the distinct effects of each additional rule implemented in CT but aggregates all rules to display the overall marginal tax levied on each unit of output as generated by all rules of the schedules in the complex treatment. The figure thus understates the full complexity of the tax schedule in CT as perceived by subjects.

Figure 3: Marginal Taxes for Different Tax Schedules in the Complex Treatment

The figure displays marginal taxes and the piece rate per slider in schedules A, B, and C in the complex treatment (bold lines) as a function of sliders positioned correctly on the x-axis. The bold orange line indicates the piece rate per slider; the three bold lines denote marginal taxes under the different schedules in the complex treatment. For comparison, the three short-dashed lines denote marginal taxes under the different schedules in the simple treatment. The dashed vertical lines indicate the number of sliders at which payoffs are maximized for a given tax schedule.

The additional rules implemented in the second and third round of the experiment do not differ between the complex and simple treatment and only differs across subjects depending on the order of tax schedules that was assigned. This allows for an analysis of how the introduction of the same additional tax rule can have differential effects depending on the initial complexity of the tax schedule.

We have designed the tax schedules in CT and ST to be as similar to each other as possible in terms of economics incentives while still changing the level of complexity. Figure 3 displays
marginal taxes for all schedules in both treatments and reveals that the tax schedules in CT can be seen as perturbations of the schedules in ST. Firstly, the number of units of output that maximize payoff in schedules A through C are identical across the complex and the simple treatment. Secondly, the payoff generated at each respective optimum is also identical in both treatments. Lastly, the tax schedules in the complex treatment are designed so that the changes in the marginal tax around the payoff-maximizing points for each schedule are also identical across treatments. As Figure 3 shows, the tax schedules in both treatments are locally identical in a neighborhood of at least four units of output around the payoff-maximizing points for each schedule (even though many more rules need to be taken into account in the complex treatment). This implies that local deviations from the optimum are as costly in the simple as in the complex treatment. An overview of the tax parameters in the two treatments is given in Table 1.

Table 1: Parameters of the Tax Schedules in the Simple and Complex Treatment

Panel 1: Simple Treatment

<table>
<thead>
<tr>
<th>Schedule</th>
<th>Number of Applicable Rules*</th>
<th>Payoff-Maximizing Number of Sliders</th>
<th>Total Payoff At Optimum</th>
<th>Increase in Marginal Tax Per Slider</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>2</td>
<td>58</td>
<td>3,364</td>
<td>2</td>
</tr>
<tr>
<td>B</td>
<td>3 or 4</td>
<td>42</td>
<td>1,764</td>
<td>2</td>
</tr>
<tr>
<td>C</td>
<td>4 or 3</td>
<td>25</td>
<td>625</td>
<td>2</td>
</tr>
</tbody>
</table>

Panel 2: Complex Treatment

<table>
<thead>
<tr>
<th>Schedule</th>
<th>Number of Applicable Rules*</th>
<th>Payoff-Maximizing Number of Sliders</th>
<th>Total Payoff At Optimum</th>
<th>Increase in Marginal Tax Per Slider At Optimum**</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>22</td>
<td>58</td>
<td>3,364</td>
<td>2</td>
</tr>
<tr>
<td>B</td>
<td>23 or 24</td>
<td>42</td>
<td>1,764</td>
<td>2</td>
</tr>
<tr>
<td>C</td>
<td>24 or 23</td>
<td>25</td>
<td>625</td>
<td>2</td>
</tr>
</tbody>
</table>

*The indeterminacy of the number of rules in schedules B and C is caused by the randomization of the order in which subjects face tax schedules B and C.

We define a rule as a linear tax which applies to a sequence of adjacent sliders.

** Increase in the tax per slider measured in a neighborhood of at least 4 sliders around the payoff-maximizing number of sliders.
2.2 Timeline

At the beginning of the experiment, subjects are familiarized with the real effort task they will work on in the experiment. We implement a real effort task that was developed by Gill and Prowse (2012). During the task, subjects see a single screen showing 48 sliders (see Figure 10 in Appendix A). Initially, the sliders are positioned at 0 (Figure 10 (a)). Subjects can adjust the position of each slider in a range from 0 to 100. Output is defined as the number of sliders positioned at exactly 50 (Figure 10 (b)). It is attractive because it is remarkably simple and does not require pre-existing knowledge or, e.g., mathematical skills. Moreover, there is little randomness in output and little room for guessing.

The introduction of the task is followed by several control questions that test whether subjects understand their potential payoff for a given a number of correctly positioned sliders in several hypothetical tax and subsidy regimes.

First Round

At the beginning of the main part of the experiment, subjects are informed that they have ten minutes to read and understand the rules upon which their payment for the first round of the experiment is based and then need to explicitly commit to a number of sliders—ranging from 0 to 96—they will position correctly in this round of the experiment. A reminder of the time limit is briefly shown after 9:30 minutes.

Subjects can make a choice after the time was over but they cannot see the tax rules anymore.

After subjects have committed to a number of sliders, they start working on the slider task until they reach the specified number of sliders.

Second and Third Round

In the second and third round, subjects are informed that all rules from the previous round are still in place and that they have 4 minutes to read and understand the one additional rule

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\(^2\)See Gill and Prowse (2011) for a comprehensive description and discussion of the task.
that will affect their earnings in this round (a reminder is again shown 30 seconds before this time limit). All previous rules are also displayed.

**Productivity Test and Questionnaire**

After the final round, subjects take part in a brief test of their productivity on the slider task. They are paid a constant piece rate of 2 points per slider and can work for a total of up to 15 minutes without having to specify the number of sliders they will position in advance.

Finally, subjects take part in a brief questionnaire in which we elicit some demographic information, conduct a test of cognitive reflection (based on Frederick (2005)), and ask some questions about subjects’ behavior in the experiment.

### 2.3 Procedural Information

A total of 277 subjects participated in the experiment which was conducted at the Centre for Decision Research and Experimental Economics (CeDEx) laboratory at the University of Nottingham in November 2011. As we expected a higher level of dispersion of decisions in CT, we randomly assigned 197 subjects into CT and 80 into ST to increase the statistical power of our analysis. Subjects were recruited from a database of registered volunteers using ORSEE (Greiner, 2004); the experiment was implemented using z-Tree (Fischbacher, 2007). Subjects received a show-up fee of £2.50; points earned in the experiment were converted into currency at a rate of 1p per 7 points. If the total number of points aggregated across all rounds of the experiment was negative for a participant only the show-up fee was paid. The average payment per subject was £9.02.\(^3\) The average duration of the experiment was 50 minutes. All instructions and the survey can be found in Appendix C.

\(^3\)Approximately US$ 14 or EUR 10.50 in November 2011.
3 Results

In this section, we present results of the experiment and discuss possible explanations for the observed behavior. First of all, we show that subjects do not behave in a fully rational way.

**Result 1:** Subjects’ choices are influenced by the level of complexity. Choices in the complex treatment are more spread out and thus, on average, further away from the optimum. As a consequence, subjects in CT earn less money.

Figure 4 depicts histograms of the choices for schedule A in the two treatments. Histograms for schedules B and C are shown in the appendix (Figures 6 and 7) and tell the same story: almost 40 percent of subjects in the simple treatment choose exactly the optimum (in the sense of payoff maximizing) marked by the dashed line in the figure. The Simple Treatment was designed to come closer to implementing the usual economic assumptions, e.g., that individuals broadly understand the incentives they face and know their marginal tax rate. It is thus reassuring that most subjects in this treatment indeed behave more or less optimally. In contrast, only 1.7 percent of choices in CT are optimal. This is only slightly better than random choice which would predict a success rate of about 1.0 percent.
Table 2 shows that this difference in behavior is highly significant. In columns 1–3, the dependent variable is a dummy which equals 1 if the subject chose the optimal effort level in a given round. Each subject enters the regression three times but we do not assume that an individual’s decisions are independent: standard errors computed by block bootstrap clustered at the subject level are shown in the table. In column 1, we regress the dependent variable only on a dummy for being in the complex treatment. The p-value of the CT dummy is below 0.001. The dummy stays significant when we control for the choice order (A-B-C or A-C-B) and for an interaction of CT and choice order (column 2) and when we additionally add controls for age, gender, and IQ (column 3).4

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4We combine three sets of information to derive our measure of IQ. We know subjects’ math grade in their final high school year. It has been shown that math grade correlates highly with Spearman’s g, the quantity that IQ-tests aim to measure (Deary and Der (2005)). Subjects also complete the Cognitive Reflection Test which also correlates with IQ (Frederick (2005)). Finally, subjects answer a set of questions to test their financial numeracy (similar to the ones in Gerardi, Goette, and Meier, 2010). Our measure of IQ is the principal factor of a factor analysis of these three variables, standardized to have mean 0 and standard deviation 1.
### Table 2: Effect of Complexity

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Distance to optimal choice</th>
<th>Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>1 if CT</td>
<td>-0.375***</td>
<td>-0.393***</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.137)</td>
</tr>
<tr>
<td>1 if Choice Order A-B-C</td>
<td>0.03</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>CT * A-B-C</td>
<td>0.017</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.114)</td>
<td>(0.119)</td>
</tr>
<tr>
<td>Age</td>
<td>0.003</td>
<td>0.22</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.447)</td>
</tr>
<tr>
<td>1 if Female</td>
<td>-0.067***</td>
<td>2.544</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(1.413)</td>
</tr>
<tr>
<td>IQ Measure</td>
<td>0.054***</td>
<td>-2.544***</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.025)</td>
</tr>
<tr>
<td>Constant</td>
<td>5.138***</td>
<td>6.687***</td>
</tr>
<tr>
<td></td>
<td>(1.865)</td>
<td>(2.806)</td>
</tr>
<tr>
<td>N.Obs.</td>
<td>831</td>
<td>831</td>
</tr>
</tbody>
</table>

Notes: Probit, tobit and OLS estimates. The dependent variable is, in Columns 1–3, a dummy for choosing the optimal effort level (probit, marginal effects shown); in Columns 4–6, the distance to the optimal choice (tobit with lower limit at 0); and in Columns 7–9, the earnings from the decision measured in British pence per round (OLS). Each subject enters the regression three times; standard errors computed by block bootstrap clustered at the subject level are in parentheses. Significance at the 1, 5, and 10 percent level is denoted by ***, **, and *, respectively.
Subjects in the complex treatment do not only choose the optimum less often, their choices are generally further away from the optimum. We calculate the absolute distance to the optimal choice for each decision and regress this distance on a CT dummy using tobit regressions with 0 as the lower bound (Table 2, columns 4–6). The p-value of the CT dummy is below 0.001 and stays below 0.001 when we add controls for the choice order, an interaction of CT and choice order, and for age, gender and IQ. As a consequence of being further away from the optimum, subjects in CT obtain lower earnings than subjects in ST. Total profits over the three rounds are 23.0 percent lower in CT (column 7). In columns 7–9, we regress subjects’ earnings from each decision on a dummy for CT using OLS regressions. The p-value of the CT dummy is always below 0.001, also when we control for choice order and the additional control variables, as in the previous regressions.  

Subjects in CT realize that they have more difficulty in dealing with the more complex situations. In the post-experimental questionnaire, they agree significantly more than subjects in ST with the statements “Figuring out how many sliders I wanted to position was very stressful.” and “I would have needed more time to figure out how many sliders to position.” and agree less with the statement “I think that I chose the number of sliders that maximized my monetary payoff.”

These results validate our treatment manipulation and also confirm that subjects in general are influenced by the complexity of the decision environment. Next, we explore how subjects try to cope with the complex environment.

**Result 2:** Complexity induces inertia into subjects’ decision-making: Subjects in the complex treatment take their previous round’s decision as a point of departure

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5To take account of the fact that the earnings are capped at the optimal level, one could use tobit regressions with an upper limit at this optimal earnings level. Since this level varies between the three rounds, we recode the earnings variable as maximal earnings - earnings and run tobit regressions with a lower limit at 0. All p-values of the CT dummy stay below 0.001 and the point estimates increase strongly to around 600. Moreover, if the total earnings of a subject was negative, it was set to zero. This happened for 4.3 percent of subjects. Results do not change if we recode earnings according to realized final earnings.

6On a scale from 1 "Disagree completely" to 6 "Agree completely", subjects in CT average 4.4 in the first question and 5.0 in the second, while subjects in ST average 3.2 and 3.0. The third question was asked separately for the three rounds; for each round, subjects in CT report a lower agreement (3.2, 3.3, 3.7) than subjects in ST (4.4, 4.4, 4.7). These differences are highly significant in tobit regressions with and without the controls used in Table 2 (all p<0.001).
and do not adjust their choice far enough in reaction to new incentives.

In Table 3, columns 1–3, we take the change in effort level from the previous round, i.e., from the first to the second and the second to the third, as dependent variable. To make the effort changes comparable across treatments, the effort change in the second decision in the choice order 312 is multiplied by -1. It is thus always optimal to reduce the effort from stage to stage (given an optimal choice in the previous stage). We regress this variable on a dummy for being in the complex treatment and the various control variables mentioned above. Since effort choices and also changes in effort choices are spread out over the whole range of possible choices (see Figures 4, 6, and 7) we use median regressions to limit the influence of extreme outliers. We include both effort changes that a subject makes in the regression and use bootstrapped standard errors clustered on an individual subject.

We find that subjects in CT react less strongly to new incentives. The median change in effort by subjects in the simple treatment is -17 which is actually the optimal reaction if they started from an optimal choice. The median change in effort by subjects in the complex treatment is only -12. The positive coefficient for the CT dummy shows that subjects in CT react less to the new incentive. This difference is highly statistically significant and remains significant when we add controls for the choice order and an interaction of CT and choice order in column 2 and when we additionally add controls for age, gender and IQ in column 3.

This result could be driven by individual-level inertia, i.e., deliberately more cautious or conservative choices by subjects in CT around the previous round’s choice. This would be curious, as subjects in CT have little reason to believe that their previous round’s choice was optimal (and, as mentioned above, in fact they don’t believe this). Alternatively, subjects in CT might just have a larger decision error: as it is harder to find the optimum, their decisions will be more noisy and this noise may bias the estimates of labor supply adjustment towards 0. Consider, for instance, the following simple model of decision errors: a fraction $\theta$ of individuals sees through the tax system and is able to choose the payoff-maximizing option. A fraction $1 - \theta$ of individuals does not see through the tax system and randomly chooses from the available options. Let the available options be given by the interval $[0, 1]$. Let
$m_1$ denote the payoff-maximizing option in situation 1 and $m_2$ denote the payoff-maximizing option in situation 2. The average choice in situation 1 is then given by $\theta m_1 + (1 - \theta) \cdot 0.5$, and similarly, $\theta m_2 + (1 - \theta) \cdot 0.5$ in situation 2. The observed difference between the choices is then given by $\theta (m_2 - m_1)$. Clearly, a decrease in $\theta$—brought about, for instance, through an increase in the complexity of the environment—will attenuate the observed change in choice behavior between the two situations. More generally, we would expect an attenuation of changes in behavior whenever higher degrees of complexity lead to a larger fraction of individuals choosing an option independently of the actual optimum. For instance, if a more complex choice environment leads to a larger fraction of individuals choosing an action based on some anchor that remains constant when economic incentives change, we will see less change in response to price changes.

It is possible to distinguish between the two hypotheses of inertia on the one hand and versions of decision error as described above on the other hand. If individual-level inertia drives the effect and if this inertia differs between subjects, one would expect a mass of subjects choosing the exact same decision as in the previous choice, namely all subjects with very high inertia. If instead decision error as modeled above drives the effect, one would not expect such a spike. Our data support the first hypothesis.

**Result 3:** Subjects in the complex treatment are more likely to leave their previous effort choice unchanged.

In Table 3, columns 4–6, the dependent variable is a dummy which equals 1 if a subject chose the same effort as in the previous round. The estimates show that subjects in ST almost never choose the same effort as in the previous round. This happens only once out of the 160 effort changes in ST. In contrast, 9.1 percent of effort changes in CT equal zero. It is actually the modal choice in CT to choose the same behavior again. These treatment differences are highly significant and robust to the inclusion of the aforementioned control variables. Figures 8 and 9 in the appendix depict histograms of the change of effort from round to round. One can clearly see the spike at zero in the complex treatment.
Note that our design makes it hard to detect such an effect as subjects are forced to re-enter their choice for every decision. Choosing the same level of output as in the previous round cannot be driven by a mechanical default effect as subjects had to actively enter a choice. The high frequency of this extreme form of inertia speaks against a simple decision-error explanation, and rather suggests that the increased inertia in the complex treatment is driven by individual-level inertia.\textsuperscript{7} In the limit, this increased inertia leads the well-known status-quo effect (e.g., Samuelson and Zeckhauser (1988)). Our results show that increased background complexity can trigger this effect. Such inertia is very much in line with subjects having only a limited stock of mental resources and this stock being depleted by previous mental effort like deciding in the first round of CT. The next result also supports this interpretation.

\textbf{Result 4:} \textit{Subjects in CT take longer for the first decision than subjects in ST.}

\textit{This difference is much smaller for the second decision and vanishes for the third.}

Figure 5 shows the cumulative distributions of decision times of subjects in CT and ST. The top panel shows the time in the first round. In this round, subjects had 600 seconds to make a choice; a reminder of this time limit was briefly shown after 570 seconds. Subjects could make a choice after the time was over but they could not see the tax rules anymore. The lower panel shows decision times in the second and third round. Here, the rules were masked after 240 seconds and a reminder was shown after 210 seconds.

\textsuperscript{7}Somewhat redundantly, note that a decision error that is distributed as a function of the range of possible choices and independent of previous decisions would induce an additional bias towards the middle of the range, i.e., towards a choice of 48 (choices had to be between 0 and 96). This in turn would mean a downward bias for schedule A in which the optimal decision was 58 and an upward bias for schedule B and C in which the optimal decisions were 25 and 42, respectively. In fact, there is only a significant bias for schedule B but this bias goes in the opposite direction of what this kind of decision error would predict. Additionally, this decision error would not predict the spike of choices at last round’s choice.
Table 3: Change in effort level from previous round

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Change in effort level from previous round</th>
<th>1 if subject chose same effort as in previous round</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5) (6)</td>
<td></td>
</tr>
<tr>
<td>1 if CT</td>
<td>5.000*** 6.000** 6.786** 0.085*** 0.111*** 0.112***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.525) (2.718) (3.240) (0.018) (0.026) (0.027)</td>
<td></td>
</tr>
<tr>
<td>1 if Choice Order A-B-C</td>
<td>1.000 2.237 0.012 0.012</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.846) (2.737) (0.013) (0.014)</td>
<td></td>
</tr>
<tr>
<td>CT * A-B-C</td>
<td>-3.000 -3.837 -0.052 -0.052</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.347) (3.787) (0.035) (0.035)</td>
<td></td>
</tr>
<tr>
<td>Age</td>
<td>1.585**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.736) (0.008)</td>
<td></td>
</tr>
<tr>
<td>female</td>
<td>1.800</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.677) (0.025)</td>
<td></td>
</tr>
<tr>
<td>IQ Measure</td>
<td>-0.128</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.088) (0.012)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-17.000*** -17.000*** -50.201*** 0.006 0.000* -0.058</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.447) (1.833) (15.528) (0.006) (0.000) (0.166)</td>
<td></td>
</tr>
<tr>
<td>N.Obs.</td>
<td>554 554 554 554 554 554</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Quantile (median) regression (columns 1–3) and OLS estimates (columns 4–6). The dependent variable in columns 1–3 is the change in effort level from the previous round; all effort changes except the one in the second decision in the choice order A-C-B are multiplied by -1 to make them comparable; coming from an optimal effort level, an effort increase is always optimal. The dependent variable in columns 4–6 is a dummy equaling 1 if the subject chose the same effort as in the previous round. Each subject enters the regression twice, standard errors computed by block bootstrap clustered at the subject level are in parentheses. Significance at the 1, 5, and 10 percent level is denoted by ***,**, and *, respectively.
The solid vertical lines mark the time limit after which the individual tax rules were masked in each round. Subjects could still make their choice after the time limit. They were reminded of the time limit 30 seconds before the limit (dashed line).

One would expect that subjects react to a more complex environment by thinking longer about their decision. This is indeed the case for the first round. As one can see from Figure 5, subjects in CT take on average more than twice as long as subjects in ST (511 seconds vs. 225 seconds). Table 4 shows that this difference is highly significant (column 1) and remains significant if we control for the additional variables described above (columns 2 and 3). This understates the true underlying need for additional time, as many subjects in CT are forced to shorten their deliberation time and to make a choice once they reach the end of the allotted time. In contrast, average decision times in the second and third round are much

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Subjects in CT do not deliberate longer in the first round because they are slower in general. Since subjects were allocated randomly to treatments, there should be no difference in innate characteristics of subjects across the treatments. And indeed, subjects do not differ in the speed with which they tackle decisions and tasks that do not differ across treatments. For example, they take the same time to answer the control question (p=0.667) and also take similar times to move the sliders in the real-effort task (p=0.097, p=0.287, and p=0.678 for first, second, and third round, respectively).
closer together. The difference in the second round is still significant (columns 4–6) but the difference in the third round is not significant anymore (columns 7–9).

If subjects had only a limited stock of mental resources, one would expect exactly this kind of behavior. Even though the complexity of the problem in the second and third round is higher in CT than in ST, the depletion of mental resources increases the marginal cost of deliberation so much that subjects in CT reduce their mental effort over time down to the levels found in ST. Note that the achievable gains of prolonged thinking are higher in CT as most subjects in ST find the optimum and cannot gain from any additional mental investment; most subjects in CT are far from the optimum when they stop thinking and could gain considerable amounts of money by continuing to search for a better effort level.

Robustness Check: Real-effort costs

The idea behind having a real-effort task linked to the effort choice was to make the experiment less abstract and more psychologically meaningful. In addition to the financial rewards and costs, such a real-effort task will introduce an effort cost of actually moving the sliders which might be heterogenous across subjects and might therefore influence the experimental results. It turns out, however, that in our case the real-effort costs are not big enough to overturn the experimental incentives. After the main part of the experiment, subjects faced an additional phase in which they had the opportunity to move as many sliders as they wanted, up to a maximum of 144 sliders, for a piece rate of 2 points each. 2 points was the marginal incentive around the optima in the main part of the experiment. In this free-choice phase, 82 percent of subjects work to the maximum, i.e., they move 144 sliders. The average number of sliders moved is 134. The real-effort cost must therefore be below 2 points per slider for most subjects and should not hinder subjects from choosing the (financially) optimal effort level. Moreover, the behavior of subjects in CT and ST does not differ in this phase (p=0.205) and subjects in CT and ST do not differ in their agreement or disagreement with the statement “After having decided on the number of sliders, actually positioning the sliders was very stressful.” (p=0.299). We therefore conclude that the real-effort cost is too small to have influenced the second and third round, respectively).
### Table 4: Decision Time

Dependent variable: 

<table>
<thead>
<tr>
<th></th>
<th>Decision time first round</th>
<th>Decision time second round</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>1 if CT</td>
<td>285.490***</td>
<td>273.140***</td>
</tr>
<tr>
<td></td>
<td>(16.419)</td>
<td>(22.058)</td>
</tr>
<tr>
<td>CT * A-B-C</td>
<td>24.700</td>
<td>18.271</td>
</tr>
<tr>
<td></td>
<td>(33.886)</td>
<td>(34.740)</td>
</tr>
<tr>
<td>Age</td>
<td>1.247</td>
<td>-2.497</td>
</tr>
<tr>
<td></td>
<td>(4.422)</td>
<td>(3.455)</td>
</tr>
<tr>
<td>1 if Female</td>
<td>-16.575</td>
<td>-16.933</td>
</tr>
<tr>
<td></td>
<td>(16.636)</td>
<td>(10.477)</td>
</tr>
<tr>
<td>IQ Measure</td>
<td>26.659***</td>
<td>5.013</td>
</tr>
<tr>
<td></td>
<td>(8.584)</td>
<td>(5.435)</td>
</tr>
<tr>
<td>Constant</td>
<td>225.201***</td>
<td>237.582***</td>
</tr>
<tr>
<td></td>
<td>(13.577)</td>
<td>(17.161)</td>
</tr>
<tr>
<td>N.Obs.</td>
<td>277</td>
<td>277</td>
</tr>
</tbody>
</table>

Notes: OLS estimates. The dependent variable is the time subjects took to decide in the three rounds of the experiment (measured in seconds). Standard errors computed by block bootstrap clustered at the subject level are in parentheses. Significance at the 1, 5, and 10 percent level is denoted by ***, **, and *, respectively.
effects of the treatment.

References


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Greiner, B. (2004): “An online recruitment system for economic experiments,”


### 4 Appendix A: Figures

Figure 6: Histogram of Choices for Schedule B

The dashed line marks the optimal choice at 42.
Figure 7: Histogram of Choices for Schedule C

The dashed line marks the optimal choice at 25.
Figure 8: Histogram of Effort Changes from First to Second Round

The dashed line marks the optimal change in effort coming from an optimal effort in the previous round. Which effort change is optimal depends on the choice order.
Figure 9: Histogram of Effort Changes from Second to Third Round

The dashed line marks the optimal change in effort coming from an optimal effort in the previous round. Which effort change is optimal depends on the choice order.

5 Appendix B: Task Description

Figure 10: Schematic Representation of a Slider (Gill and Prowse, 2011)

(a) Initial position. (b) Positioned at 50.

6 Appendix C: Instructions

Screen 1:
Thank you for participating in this experiment. For your arrival on time, you will receive £2.50 that will be paid to you at the end of the experiment in addition to all earnings from the experiment. If you use the computer in an improper way you will be excluded from the experiment and from any payment. Please turn off mobile phones now and leave them turned off throughout the experiment. If you have a question during the course of the experiment, please raise your hand and we will come to your place and answer your question in private. Please refrain from communicating with other participants throughout the experiment.

During the experiment you can earn points; points will be converted into pence at a rate of 7 to 1; that means that you receive 1p per 7 points. The sum of all points that you earned will be paid out to you at the end of the experiment.

The experiment consists of four parts in which you will position sliders on the screen and a questionnaire after the main part of the experiment.

Only once the experimenter tells you so, press OK to proceed to the next screen.

Screen 2:

In the four parts of the experiment, your task will consist of positioning sliders on screens containing 48 sliders. Each slider is initially positioned at 0 and can be moved as far as 100. Each slider has a number to its right showing its current position. You can use the mouse or keyboard (arrow keys) in any way you like to move each slider. You can readjust the position of each slider as many times as you wish.

Your payment depends on the number of sliders positioned at exactly 50. We will call a slider positioned at 50 a “correct slider”. You will always get a piece rate per correct slider but you will also have to pay taxes and/or receive subsidies depending on the number of correct sliders.

Do you have any questions at this point?

Before the main experiments starts, please answer a couple of example questions on the next screens.

Please press OK now to proceed to the next screen.

Control Questions

Control Question 1

Consider the following example:
You receive a piece rate of 20 points per correctly positioned slider.

A constant tax of 5 points is levied on each slider. That means that for each correct slider, you will have to pay a tax of 5 points.

Please answer the following questions and click OK. If one of your answers turns out to be not correct, please try again or ask the experimenter for help.

- Suppose you had positioned 3 sliders correctly. What is the total amount in piece rates that you would receive for the three sliders together?
- Suppose you had positioned 3 sliders correctly. What amount of taxes do you have to pay for the third slider?
- Suppose you had positioned 3 sliders correctly. What are your net earnings (piece rate - taxes) for the third slider?

Control Question 2

Consider the following example:

You receive a piece rate of 20 points per correctly positioned slider.

There is an increasing tax per slider. This tax is 2 points for the first slider and goes up by 2 points per additional slider. That is, it implies a tax of 4 points for the second slider, 6 points for the third slider, and so on.

Please answer the following questions and click OK. If one of your answers turns out to be not correct, please try again or ask the experimenter for help.

- Suppose you had positioned 4 sliders correctly. What amount of taxes do you have to pay for the fourth slider?
- Suppose you had positioned 4 sliders correctly. What are your net earnings (piece rate - taxes) for the fourth slider?

Control Question 3

Consider the following example:

You receive a piece rate of 20 points per correctly positioned slider.

There is an increasing subsidy per slider that you get on top of the piece rate. This subsidy is 2 points for the first slider and goes up by 2 points per slider. That is, it implies a subsidy of 4 points for the second slider, 6 points for the third slider, and so on.
• Suppose you had positioned 4 sliders correctly. What amount of subsidies do you receive for the fourth slider?

• Suppose you had positioned 4 sliders correctly. What are your net earnings (piece rate + subsidies) for the fourth slider?

Control Question 4

Consider the following example:

You receive a piece rate of 20 points per correctly positioned slider.

There is an increasing tax per slider. This tax is 6 points for the first slider and goes up by 6 points per additional slider. That is, it implies a tax of 12 points for the second slider, 18 points for the third slider, and so on.

• How many sliders should you position correctly to maximize your financial earnings?

Hint: Your payment is maximized if you position sliders correctly up to just before the point at which the taxes per slider are higher than the piece rate.

Control Question  Sliders [What’s missing here?]

Instructions - Round One

The main part of the experiment starts now.

In this part of the experiment, you will work on the slider task. You will first learn about the piece rate and the particular taxes and subsidies that are relevant for this stage. You will then decide how many sliders you want to position, taking the level of the piece rates and all taxes/subsidies into account.

You have 10 minutes to read these rules and decide on the number of sliders before the actual slider task begins.

Once you have decided how many sliders you want to position, there is no time constraint for actually positioning the sliders.

You receive a baseline piece rate of 100 points per slider positioned correctly. But you also have to pay a number of taxes and can receive a number of subsidies depending on the number of sliders positioned correctly. All taxes and subsidies are added together.
Your payment is maximized if you position sliders correctly up to the point at which the taxes per slider are higher than the piece rate plus the potential subsidies.

If you have any questions, please raise your hand. If not, please press OK to see the taxes and subsidies for this stage.

**Tax Rules A (Complex Treatment)**

You receive a constant **subsidy** for each slider positioned correctly. This subsidy remains constant at 7 points per slider.

You receive a **subsidy** for sliders 1 through 10. This subsidy is 20 points for the first slider positioned correctly and decreases by 2 points per additional slider (until slider 10). That is, you receive a subsidy of 18 points for the 2nd slider, 16 points for the 3rd slider, and so on. Thus, the subsidy is zero for the 11th slider and remains at zero for all additional sliders.

There is an increasing **tax** starting at the first slider that applies to all additional sliders. This tax is 0 points for the first slider and goes up by 2 points per slider. That is, it implies a tax of 2 points for the second slider, 4 points for the third slider, and so on.

There is a constant **tax** for the following sliders: 12 through 17; 32 through 37; 48 through 52; 78 through 83. This tax is constant at 5 points per slider positioned correctly for the ranges mentioned above and is zero otherwise. Three is another constant tax of 7 points per slider for the sliders 31 through 34. And there is 20 point tax for each slider 7 through 11.

You receive a **subsidy** of 10 points for the following sliders: 6, 7, 8, 16, 17, 18, 19, 20, 21, 69, and 70. You receive a **subsidy** of 15 points for the sliders 51-53 and a **subsidy** of 20 points for sliders 35-37.

You receive an increasing **subsidy** starting at the 6th slider that applies to all additional sliders. This subsidy does NOT apply to sliders 1 through 5. This subsidy is 2 points for the 6th slider and goes up by 2 points per additional slider. That is, you receive a subsidy of 4 points for the 7th slider, 6 points for the 8th slider, and so on.

There is a constant **tax** of 5 points per slider for all sliders after the 66th slider. That is, this tax is zero for all sliders up to and including the 66th slider, and at 5 points for the 67th slider and all additional sliders.

There is an increasing **tax** starting at the 10th slider that applies to all additional sliders. This tax does NOT apply to sliders 1 through 9. This tax is 2 points for the 10th slider and goes up by 2 points per slider. That is, it implies a tax of 4 points for the 11th slider, 6 points for the 12th slider, and so on.
There is a tax of 10 points per slider for the following sliders: 20, 21, 48, 49, 74, 75, 90, and 91.

**Tax Rules A (Simple Treatment)**

You receive a constant subsidy or each slider positioned correctly. This subsidy remains constant at 15 points per slider.

There is an increasing tax starting at the first slider that applies to all additional sliders. This tax is 0 points for the first slider and goes up by 2 points per slider. That is, it implies a tax of 2 points for the second slider, 4 points for the third slider, and so on.

**Instructions for Slider Task - Round One**

You have decided to position ___ sliders correctly. You will have to do so on the next screens. The number of sliders you have already positioned and the remaining time are shown on the top of the screen. Once you have positioned ___ sliders correctly, press OK to proceed to the next stage of the experiment.

**Note:** For a slider to count as “correct slider” it has to be positioned exactly at 50.

If you decided to position more than 48 sliders, please position all 48 sliders on the screen and press OK. Then a second screen will be shown with the remaining sliders.

Please click OK now to start positioning the sliders.

**New Instructions - Round Two**

In the next stage of the experiment, all tax and subsidy rules from the previous stage are still applicable. However, there is an additional rule applicable and this rule also determines your payment.

**You have 4 minutes to read these rules and decide on the number of sliders before the actual slider task begins.**

Once you have decided how many sliders you want to position, there is no time constraint for actually positioning the sliders.

**New Tax Rules - Round Two**

ABC: In addition to the previous rules, there is a constant tax for each slider. This tax remains constant at 32 points per slider.
ACB: In addition to the previous rules, there is a constant tax for each slider. This tax remains constant at 66 points per slider.

**Instructions for Slider Task - Round Two**

You have decided to position ___ sliders correctly. You will have to do so on the next screens. The number of sliders you have already positioned and the remaining time are shown on the top of the screen. Once you have positioned ___ sliders correctly, press OK to proceed to the next stage of the experiment.

**Note: For a slider to count as “correct slider” it has to be positioned exactly at 50.**

If you decided to position more than 48 sliders, please position all 48 sliders on the screen and press OK. Then a second screen will be shown with the remaining sliders.

Please click OK now to start positioning the sliders.

**New Instructions - Round Three**

In the next stage of the experiment, all tax and subsidy rules from the previous stage are still applicable. However, there is an additional rule applicable and this rule also determines your payment.

**You have 4 minutes to read these rules and decide on the number of sliders before the actual slider task begins.**

Once you have decided how many sliders you want to position, there is no time constraint for actually positioning the sliders.

**New Tax Rules - Round Three**

ABC: In addition to the previous rules, there is a constant tax for each slider. This tax remains constant at 34 points per slider.

ACB: In addition to the previous rules, there is a constant subsidy for each slider. This subsidy remains constant at 34 points per slider.

**Instructions for Slider Task - Round Three**

You have decided to position ___ sliders correctly. You will have to do so on the next screens. The number of sliders you have already positioned and the remaining time are shown on the top
of the screen. Once you have positioned ___ sliders correctly, press OK to proceed to the next stage of the experiment.

Note: For a slider to count as “correct slider” it has to be positioned exactly at 50.

If you decided to position more than 48 sliders, please position all 48 sliders on the screen and press OK. Then a second screen will be shown with the remaining sliders.

Please click OK now to start positioning the sliders.

Instructions Productivity Test

In this last part of the experiment, you will work on the slider task again. In contrast to before, you do not have to commit in advance to how many sliders you will do.

You will be paid 2 points per slider positioned at exactly 50. There are no taxes or subsidies in this stage.

You can work for a maximum of 15 minutes on this task but even if you have time left, you can only do up to 144 sliders and earn the respective piece rates.

Please click OK to start this stage of the experiment.

Feedback

The main part of the experiment is now over. Your total earnings from the four stages is ___ points.

We would now like to ask you to fill in a short questionnaire. After the questionnaire, the experiment is over and you will be paid.