Keep on workin': Unconditional basic income in the lab[1]

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Abstract

Our real effort lab-experiment features an unconditional basic income (UBI) scheme and compares it with a conventional redistribution mechanism and a system without redistribution. The participants choose between three options: working for themselves, the group, or enjoying leisure. Surprisingly, we find that neither the choice of options nor work efforts are sensitive to the transfer scheme, even although the UBI in our experiment constitutes a natural reference point for participants' earnings. However, the UBI significantly reduces the dispersion of incomes at only a small cost in terms of forgone production and budget deficit.

JEL classification: J22, C91

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1 Introduction

Typically, individuals are assumed to determine labor supply by trading off the benefits (monetary income) and costs (disutility of labor) such that their overall utility is maximized. As such, payment schemes within firms as well as taxation and redistribution by the state are of paramount importance in determining labor supply. A vast body of literature thus investigates labor supply under different regimes of remuneration[4], taxation and redistribution[5]. A primary result is that labor supply decreases as initial income increases. Moreover, it is shown that labor supply increases if net wages increase, since higher net wages

[4] Lazear (1986) has addressed the impact of fixed salaries and piece rates in a seminal paper featuring a theoretical model. Since then, empirical evidence from both the field (i.e. Shearer, 2004, Lazear, 2000) as well as laboratory experiments (i.e. Dohmen and Falk, 2011) has been made available. An important finding of this work is that output in variable payment schemes is higher. However, this result seems to be driven largely by selection of workers of different productivity into the different payment schemes. Another stream of literature has found that payments based on relative performance in tournaments increase work effort (see i.e. van Dijk et al., 2001).

[5] See, for example, Creedy (2006) for an extensive survey and van de Ven (2011), Oliver et al. (2010), Steiner and Wakolbinger (2010), Cai et al. (2008) or Dearing et al (2007) for recent applications analyzing pensions, income taxes, social assistance or family benefits.

make labor more rewarding and leisure more expensive[6]. These findings result in the view that labor supply is best to be stimulated with wage subsidies for low-wage earners, low marginal tax rates, and low transfer income (see, i.e., Sinn et al., 2006, for a German application).

Our study relates to these issues since we examine labor supply under as well as welfare and distributional consequences of different distributional schemes including one with an unconditional basic income (UBI). While our study is, as described above, closely related to the literature on the impact of payment schemes within firms and distributional mechanisms on work effort and labor supply, it is to the best of our knowledge the first to explicitly address the labor supply-, welfare- and distributional effects of an UBI in a laboratory experiment.

Our primary goal is to contribute to the discussion of labor supply effects of unconditional basic incomes (UBI) and negative income taxes. Concerning the latter, a series of experiments in the 1970ies has shown that making work both less rewarding and less essential by increasing negative income tax rates provokes only a slight reduction in household labor supply (Hall, 1975). With respect to the latter, Marx and Peeters (2008) show in a pilot study that among persons who

[6] However, Greiner et al. (2011) find in a real-effort experiment that wage increases do not affect performance unless those wage changes are made transparent.

have won an UBI in a lottery, only few quit or reduce work or become self-employed. Thus, the transfer payments from an UBI or negative income taxes might not be as detrimental to labor supply due to a negative income effect as standard economic reasoning suggests, but may still have the effect of making the income distribution more equal.

While we mainly focus on the behavioral responses to marginal tax rates and income effects, we also contribute to the literature on the impact of reference-dependence and loss aversion. A prominent series of papers in this field has analyzed, for example, labor supply of New York City cabdrivers[7] and found that on days with a higher demand (due to e.g. bad weather, breakdowns in the public transport system etc.) and thus higher hourly earnings, the drivers do not tend to work more than on days with lower demand, although responding to the material incentives would require so. This behavior is explained with the existence of a reference point in the form of a daily target income. On each day, labor supply is stopped soon after the target income has been met, and it will be met earlier on days with higher hourly earnings. Thus, higher earnings make labor supply decrease rather than increase, which is not what one would expect according to the findings discussed above. More recently Abeler et al. (2011) have tested the

[7] Research on cab-driver labor supply has been initiated by Camerer et al. (1997) but also carried out by Chou (2002), Farber (2005, 2008) as well as Crawford and Meng (2010).

effect of reference points on the provision of real effort in a laboratory experiment in a prospect theory context of risky choices (Tversky and Kahneman, 1991). In this experiment, a random draw decides, after the real-effort task has been carried out, whether a participant would be paid according to the output she previously generated or according to a fixed, previously determined fee. Participants are found to match output to the fixed fee (i.e. they generate output such that the payment based on output and the fixed fee are approximately equal). This result is explained by reference point dependency of preferences, and loss aversion. Participants would suffer losses if they were assigned the fixed fee, but had previously generated output such that the variable payment was higher. Analogously, they suffered losses if they generated a variable payment lower than the fixed fee, and were paid this variable payment. Since losses loom larger than gains, participants avoid this and match the two potential payments. In a related study, Akerlof and Yellen (1990) find that workers reduce effort if their wages are lower than a perceived "fair wage"; i.e. their reference point.

In our study, the UBI is set such that it constitutes the natural reference point for expected earnings in economic laboratory experiments. Thus, we are able to check how reference dependent loss aversion together with marginal incentives shapes the impact of different distributional systems on work effort and labor supply.

Our paper is structured as follows: Section 2 introduces the experimental design, Section 3 derives and formulates our hypotheses, Section 4 presents the results and Section 5 concludes.

2 Experimental design

We conduct three treatments and apply a between subjects design such that each participant plays in just one treatment. In each session, 18 participants are randomly put in groups of three, the group members' identities are not disclosed at any time. The groups stay together for the whole experiment, which consists of 8 periods each lasting 5'. Before the beginning of each period the subjects have to (non-reversibly) choose one of the following three options:

- *Option 1*: Individual income generation (working for oneself)
- *Option 2*: Group income generation (working for the group)
- *Option 3*: Leisure

Options 1 and *2* are based on the rather tedious and repetitive real effort task of calculating the sum of five 2-digit integers on the screen. For each such calculation a gross piece rate of 30 Eurocent is paid in both options. The entry of an incorrect result on the screen prompts an error message calling for revision, the next set of five 2-digit integers is only displayed when the previous has been solved correctly given the 5' work time of the period has not yet elapsed. The net

payment (in Eurocent) for each calculation which equals $30(1-t)$, ($t \in (0,1)$ denotes the tax rate), is transferred to a subject's personal account in *Option 1* and to the group account in *Option 2*. The money accumulated on either account is summed up over the periods. In the end money on the personal account is paid out to the subject directly, while the money on the group account is split equally between the three group members. The tax revenue produced in a group is put on an account of its own. How it enters the subjects' payoffs will be lined out below.

In *Option 3* the execution of the experimental software is relegated to the background for the duration of the period enabling the subjects to anonymously use their computer workstation in whatever way they want (e.g. browse the web, check emails, social networking etc).[8]

After each period the subjects are informed about which option they had chosen and the number of correct calculations they have entered (given *Option 3* has not been chosen). No information about the other group members' choices and performance is given such that the unit of observation is on the subject level.

Treatment variation goes along the line of the (constant) tax rate applied in *Options 1* and *2* and the way the redistributive transfers are designed. We run three treatments:

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[8] Simply doing nothing is of course possible regardless of the option chosen but whenever options 1 or 2 were chosen the subject did solve calculations.

In the *CONTROL-Treatment* there is no taxation and redistribution on top of the (implicit) taxation and redistribution of *Option 2* via splitting off the group account.

In the *TAX-Treatment* a 50% tax rate is applied to the incomes generated in *Options 1* and *2*. The tax revenue accumulated is equally split and redistributed to the group members as a transfer payment. Note that compared to *CONTROL* the tax imposed on *Option 2* makes no difference in the net earnings a subject attains with solving calculations in this option.

In the *UBI-Treatment* the subjects unconditionally receive a payment of 15 Euro (cash in an envelope placed at the workstation cubicles) at the beginning of the session, further earnings attained are added and paid afterwards. As in *TAX* the incomes generated in *Options 1* and *2* are subject to 50% taxation. The tax revenue is however not transferred to the group members, but held back and explained to be used to finance the up-front payment. The 15 Euro the UBI payment is set to is generally communicated as the average hourly earnings of taking part in economic experiments at the University of Innsbruck. Students typically know this value since it is also prominently used in advertising for

participation in economic experiments. Thus, it can be seen as a plausible and immediately salient reference point for the payoff to be made in an experiment.[9]

We summarize the treatments by the constant implicit tax rate associated with *Options 1* and *2*. The implicit tax rate is given as the share of the gross piece rate paid for solving a calculation that does not enter the subjects' final payoff.

Table 1: Implicit tax rates

	Option 1 (working for oneself)	*Option 2* (working for the group)
CONTROL	0%	67%
TAX	33%	67%
UBI	50%	83%

Prior to the experiment, a 5 minute trial period of calculating integer sums as explained above, where each correct calculation is remunerated with 30 Cent, is carried out. This trial period gives the participants the opportunity to get familiar with the task. We also use the participants' performances in the trial period as a measure of their individual productivity[10].

[9] There is of course a variation of the pay-offs subjects typically make within as well as across different experiments but in general we have tried to calibrate the experiments such that this average is met.

[10] Due to learning, subjects get more productive as the experiment proceeds. We have captured this in the regressions below by introducing time dummy variables.

After the experiment we carry out a social orientation test[11]. For this test, the participants are randomly matched into pairs and asked to choose between 24 pairs of allocations specifying a payoff for the decider and the matched partner. The participants are paid the sum of the payoffs as active deciders and passive recipients. The results allow classifying the participants into eight different types. An overwhelming amount of 96 percent[12] of subjects are classified as either "individualist" or "cooperator".

The experiment as well as the trial period and the social orientation test are computerized using z-Tree[13] (Fischbacher, 2006), 108 subjects recruited from the database of the Center of Experimental Economics at the University of Innsbruck have participated in 6 sessions (2 per treatment) lasting about 75' each. The average payoff was 39.1 Euro.

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[11] This test has been developed by Messick and McKlintock (1968) and applied by Burlando and Guala (2005), Brosig (2002) and van Lange et al. (1997), among others. Since outlining this test in more detail is beyond the scope of this paper, the interested reader is referenced to the seminal paper (Messick and McKlintock,l 1968).

[12] Brosig (2002), for example, classified 97.2 percent as either "individualist" or "cooperator".

[13] z-Tree (Zurich toolbox for readymade economic experiments) is a software package which allows developing and carrying out economic experiments. It is widely used in the scientific community.

The payoffs attained in the experiment as well as the trial period and the social orientation test are added up to the subject's total earnings. The neutrally worded instructions explain in detail how choices in the experiment are mapped to payoffs, and establish common knowledge of this mapping. The instructions for the trial period, the experiment and the social orientation test are handed out sequentially.

3 Hypotheses

We observe the behavior of the subjects in two dimensions: The choice of option for each of the 8 periods and the output measured as the number of calculations solved given *Option 3* has not been chosen.

The choice of option is driven by the participants' ability in the task and their preferences over the own payoff, the distribution of the payoffs in the group and the psychological cost of effort associated with working on the task. An explicit elicitation and estimation of these preferences is beyond the scope of this paper. Generally however, solving a calculation in *Option 1* generates a higher income for a subject than in *Option 2* and in order to choose *Option 2* the other group members' payoffs need to enter the subjects' utility function positively. This is necessarily consistent with pure altruism but also with e.g. inequality aversion according to Fehr & Schmidt (1999), given suitable beliefs about the

other group members' choices and productivities.[14] Choosing *Option 3* requires the monetary income plus the eventual distributional benefit of *Options 1* and *2* to be set off by avoided effort costs and benefits from using the time for other means.

As the tax rates associated with *Options 1* and *2* differ across treatments the relative prices of the options in terms of earnings forgone when not choosing *Option 1* differ as well. Table 2 displays the opportunity costs per calculation of not choosing *Option 1*.

Table 2: Foregone profits per calculation

	Option 2 instead of *Option 1*	*Option 3* instead of *Option 1*
CONTROL	20 Eurocent	30 Eurocent
TAX	10 Eurocent	20 Eurocent
UBI	10 Eurocent	15 Eurocent

Options 2 and *3* are the most expensive in *CONTROL*. In *TAX* and *UBI* the opportunity costs of *Option 2* are the same and lower than in *CONTROL*, *Option 3* is more expensive in *TAX* than in *UBI*. Therefore, one can expect that *Options 2* and *3* are the least often chosen in *CONTROL*. *Option 3* is expected to be chosen most frequently in *UBI* while *Option 2* should be chosen with the same frequency in *TAX* and *UBI*.

[14] For an inequality averse subject choosing option 2 she needs to believe that the other group members end up with a lower pay-off.

Furthermore, if reference point dependent preferences and loss aversion associated with prospective earnings and work effort play a role, *Options 2* and *3* should be chosen more often in *UBI* than in *TAX* or *CONTROL*. This is because earnings below an "expected experimental payoff" would be perceived as a loss and earnings beyond expectation as a gain. As long as a subject's accumulated earnings in the experiment fall short of the usual average payoff[15], loss aversion should put more emphasis on generating income while in the domain of gains when the accumulated earnings exceed the reference point the trade-off between labor and leisure should be solved with less emphasis on generating income. While the reference point of the usual average experimental earning of 15 Euro per hour is relevant for all treatments, it is more salient in *UBI* since the supposed target income is met right from the start due to the up-front payment. An eventual income effect due to this payment would work in the same direction and make the choice of *Option 2* and *3* even more frequent in *UBI*. Our hypothesis on choice thus is this.

Hypothesis 1: The choice frequency of Option 2 and Option 3 is highest in UBI and lowest in CONTROL.

[15] On average the subjects had taken part in about 5 experiments previously. We therefore assume that they are familiar with the usual average earnings by own experience.

Once the choice for one of the production options has been taken the subjects' abilities in the task and their preferences about the monetary gain, the distributional consequences and the disutility of working determine the effort put up in a period. As can be seen from Table 1, the tax rates are highest in *UBI*, followed by *TAX* and *CONTROL*. Furthermore, for the same reasons as put forth above eventual reference point dependent preferences and eventual income effects of the fixed payment in *UBI* should pull the work effort further in this direction. Our hypothesis on work effort is thus.

Hypothesis 2: Work effort is lowest in UBI and highest in CONTROL.

4 Results

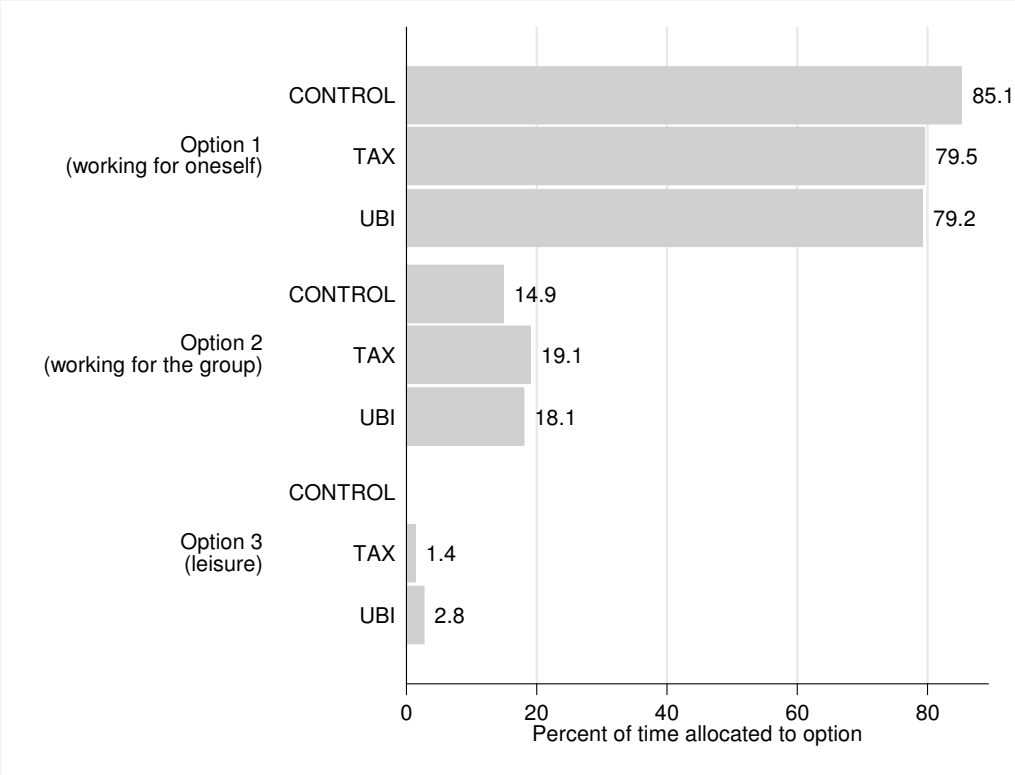
4.1 Choosing to work for oneself, the group, or enjoy leisure

Figure 1 shows the relative frequency of the option choices. Clearly, in all three treatments, participants choose *Option 1* most frequently, but a substantial share of 15 percent and more of choices is on *Option 2*. *Option 3* is, instead, rarely chosen.

The differences in option choices across treatments are in line with *Hypothesis 1*, apart from the fact that participants of *UBI* choose *Option 2* slightly less often than participants of *TAX*. In total however, we find that the differences across treatments are very low.

Consider *UBI*: Given the low price of leisure as well as the up-front payment, we had supposed that in line with the findings of Camerer et al. (1997) and others, *Option 3* would be chosen more often. However, the frequency of *Option 3* choices does not differ between *TAX* and *UBI* (Mann-Whitney Test, p = 0.96). In general, there are no significant differences in the option choices between any combinations of treatments.

Figure 1: Percentage of option choices



To identify the variables driving the option choices we have run a probit regression of the probability to choose *Option 1*. This will be important for the

observed output in the two production options. With heterogeneous subjects, work effort and output is determined not only by the different tax rates, but also by selection due to individual characteristics.

Table 3: Probit regression

	Dependent variable: P(*Option 1*)	
	marginal effect	standard error
# correct calculations (trial)	0.012	***(0.004)
# false calculations (trial)	0.087	*(0.055)
TAX	-0.273	(0.048)
UBI	-0.027	(0.048)
Female	-0.069	*(0.037)
Co-operator	-0.063	*(0.041)
Social Sciences	-0.012	(0.036)
Observations	822	
Clusters	104	
R-squared	0.07	

Robust standard errors in brackets

* significant at 10% ** significant at 5% *** significant at 1%

As can be expected, output in the trial period significantly raises the probability to choose *Option 1*. The more productive participants, who have higher opportunity cost, select themselves into *Option 1* more often.[16] Female

[16] Optimally solving the trade-off between speed and accuracy in doing the calculations apparently yields a certain number of errors. Therefore, not only the number of correct calculations but also the wrong ones are positively significant.

gender and the classification as co-operator however reduce the probability to select *Option 1*.

4.2 Productivity

Table 4 illustrates the behavior of subjects in the dimension of work effort once the option choice is made. It shows that the highest average output occurs in *CONTROL* while the lowest occurs in *UBI*, although the distribution of the option choices does not differ. Moreover, in all three treatments, average output is lower in *Option 2* than in *Option 1*, which can be due to the subjects responding to the higher tax rates as well as due to selection. The observed behavior is in line with *Hypothesis 2*; however, the differences across treatments are low and seldom significant. This pattern is very similar to our observations with respect to the distribution of the option choices and implies that neither redistribution scheme induces strongly significant welfare losses.

Consider again the comparison between *UBI* and *TAX*. While participants of *UBI* face higher marginal tax rates and are given the substantial up-front payment, the average output (across *Options 1* and *2*) statistically does not differ from *TAX* (Mann-Whitney Test, $p = 0.48$). Moreover, there is no significant difference between *TAX* and *CONTROL* (Mann-Whitney Test, $p = 0.32$), but the difference between *UBI* and *CONTROL* is significant at the 10-percent level

(Mann-Whitney Test, p = 0.07). However, we do not find a significant difference across treatments in a regression (see further below).

Table 4: Average number of correct calculations per subject and option

	Option 1 (working for oneself)	*Option 2* (working for the group)	Total
CONTROL	13.67	13.09	13.65
TAX	12.84	11.31	12.73
UBI	12.37	10.68	12.02

Furthermore, we have also run an OLS-regression to explain the output provided by the subjects which confirms the result that treatment variation does not cause significant changes in behavior.

Table 5: OLS-Regression # of correct calculations per subject

	Dependent variable: # of correct calculations	
	coefficient	standard error
correct entries in trial period	7.153	***(0.707)
percentage of choices of *Option 2*	-0.503	**(0.220)
UBI	6.675	(9.894)
CONTROL	-10.517	(8.682)
(percentage of choices on *Option 2*)**UBI*	0.131	(0.362)
(percentage of choices on *Option 2*)**CONTROL*	0.667	**(0.305)
Female	2.280	(5.940)
Co-operator	14.440	**(7.101)
Constant	38.670	***(10.548)
Observations	98[#]	
R-squared	0.64	

Robust standard errors in parentheses

* significant at 10%, ** significant at 5%, *** significant at 1%
#10 subjects have been excluded from the analysis because they have chosen *Option 3* or could not be classified as individualist or cooperator

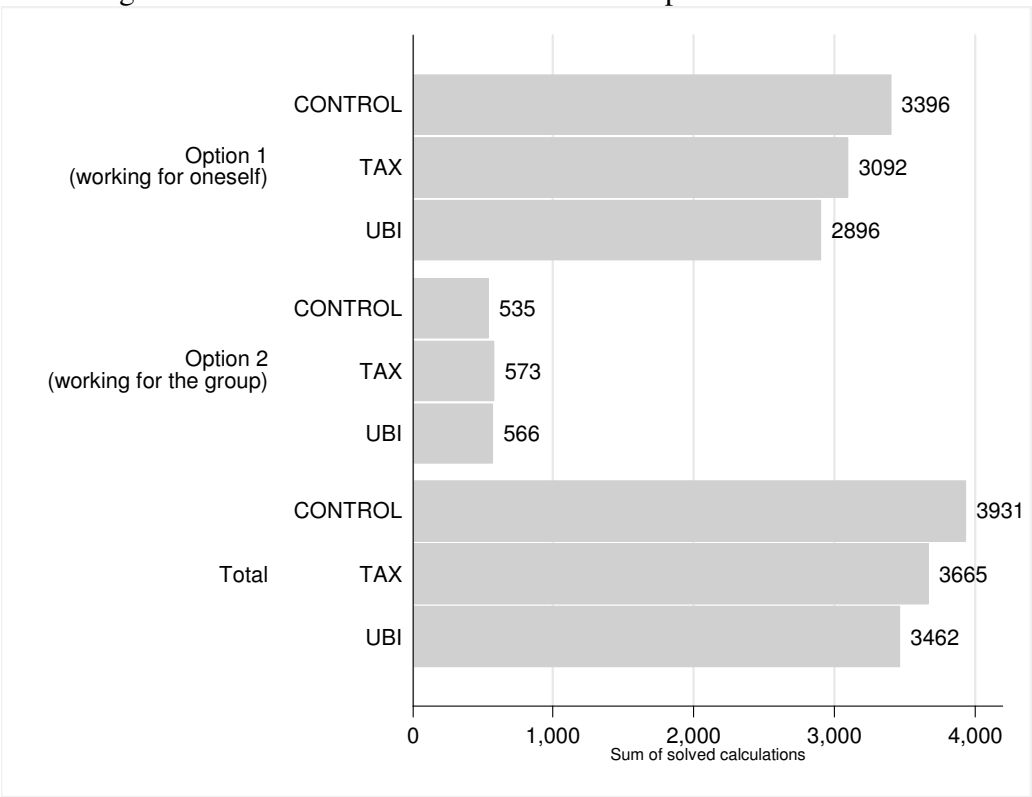
The dependent variable is the total number of correct calculations per participant. Independent variables are the number of correct calculations in the trial period, the percentage of choice of *Option 2* as well as dummy variables for the treatments, gender and social preferences. The social value orientation-test (Messick and McClintock, 1968) allows us to classify the participants either as "individualist" or "cooperator" (only four participants could not be classified into one of those two categories). Moreover, we use interaction terms to allow different effects of the percentage of choice of *Option 2* across treatments.

Clearly, the participants' output in the trial period is a significant determinant of productivity. Moreover, the more often participants of *TAX* and *UBI* have chosen *Option 2*, the lower is their total number of correct calculations. The treatment dummy variables, however, are not significant, while co-operators supply significantly more labor than individualists.

4.3 Total labor

Figure 2 shows the total number of correct calculations across options and treatments. The visible pattern is that total labor supply is in line with our hypothesis. However, the magnitude of the response is low (only 6% between *TAX* and *UBI*).

Figure 2: Number of calculated sums across options and treatments



4.4 Distributional effects

Put together, we do not find significant behavioral responses to the taxation and redistribution schemes. However, those schemes shift the distribution of payoffs. Table 6 gives the summary statistics of the subjects' payoffs (in Euro).

Table 6: Summary statistics of payoffs

	Mean	Std. Dev.	Min	Max
CONTROL	32.76	11.6	8.8	66.8
TAX	30.55	10.19	14.55	63.95

UBI	29.43	6.67	19.45	48.45

In *TAX*, average tax revenue (which is fully redistributed) is 15.27 Euro per subject while in *UBI* the average tax revenue is 14.43 Euro. This implies that the up-front payment of 15 Euro has to be subsidized with about 60 Cent per subject. There is considerable variation in the payoffs which stems from the differences in participants' ability and the fact that some participants choose *Option 2* and share their income with while others don't. Using a Mann-Whitney test to compare the subjects' payoffs across the treatments we find no significant differences, which does not surprise as the option choices and the solved calculations hardly differ. Inspecting the distribution of the payoffs however shows a different picture. The Gini-coefficients are 0.193 (std. error 0.023) in *CONTROL*, 0.1780 (0.017) in *TAX* and 0.118 (0.014) in *UBI*. This suggests that the inequality of the payoff distributions is reduced from *CONTROL* to *UBI*. Using a Levene's test for the payoffs' variances across the treatments we find that there is no significant difference between *CONTROL* and *TAX* ($p = 0.37$) but there is a significant difference between *TAX* and *UBI* ($p = 0.04$) and *CONTROL* and *UBI* ($p = 0.003$).

Therefore, the UBI redistribution scheme significantly lowers inequality to only a little cost due to forgone output and a small budget deficit needed to finance the up-front payment.

5 Conclusion

We use a real-effort laboratory experiment to study labor supply under a conventional taxation redistribution scheme, an unconditional basic income (UBI) scheme, and a system without redistribution. In either scheme subjects have the possibility to work for themselves (*Option 1*), the group (*Option 2*), and/or enjoy leisure (*Option 3*) by accessing the internet and using it as they like.

In terms of option choice, we find that the output provided is inversely related to the marginal tax rates imposed on the production options and that the leisure option is hardly ever chosen. *Option 1*, which ceteris paribus is always less taxed than *Option 2*, is chosen substantially more often than the altruism dependent *Option 2*, but the output provided across the options is not significantly different.

Treatment effects are hardly present which is surprising especially in the case of our treatment with the unconditional up-front payment set equal to the commonly known, usually expected earnings in experiments at the location. We do not find reference point dependent provision of effort. The experimental participants still respond to the material incentives and seldom choose leisure, although the real effort task is tedious. Thus, the up-front payment seems not to create an income effect that backfires on the incentives to work on the real effort task. Given this pattern, the welfare effects of introducing taxation and redistribution are very small but reduce the inequality of the income distribution

produced. Inequality is the highest absent redistribution and it is the lowest with the unconditional up-front payment. The reduction in inequality brought by the unconditional basic income in our experiment comes at only a relatively small cost in terms of forgone output and budget deficit.

This can be seen as evidence in favor of the unconditional basic income as an instrument of the welfare state and is in line with previous findings of negative income tax experiments (Hall, 1975) or people who won an unconditional basic income in a lottery (Marx and Peeters, 2008). None of those studies found large reactions in labor supply.

As the laboratory environment is, however, artificial, several caveats concerning this conclusion have to be kept in mind. In particular, our results might be driven by an experimenter demand effect (subjects perceiving working on the task as what is expected from them) and/or an insufficiently low recreational value of the leisure option as the subjects nevertheless have to stay in the laboratory for the duration of the session. A robustness check replicating the *UBI-Treatment* with giving the subjects the free choice to leave the experiment with the up-front payment right away and not requiring them to stay would shed light on to which extent our findings are driven by the specific situation in the lab. We suggest this for further research.

6 Literature

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