DO EMISSION TRADING SCHEMES FACILITATE EFFICIENT ABATEMENT INVESTMENTS? AN EXPERIMENTAL STUDY

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Do Emission Trading Schemes Facilitate Efficient Abatement Investments? An Experimental Study[1]

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Abstract

Cap-and-trade programs, such as the EU carbon Emission Trading Scheme, are currently the most prominent market-based method used to reduce carbon emissions. Cap-and-trade programs are, on theoretical grounds, considered to be a cost-efficient method. Experimental evidence, however, shows that experimental subjects make highly inefficient abatement choices and that permit allocation methods (allocating permits for free or against payment) bias subjects to too much or too little abatement. The experimental evidence thus suggests that cap-and-trade programs may in practice be more costly than theory predicts. This study, however, challenges this interpretation and shows that, when they are price takers (as in thick markets) and have ample opportunities for learning, subjects quickly learn to make accurate decisions and that these decisions are not affected by the permit allocation method.

Abstrakt

Cap-and-trade programy, jakým je například Evropská směrnice o obchodování s emisemi (EU ETS), jsou v současné době nejvýznamnější trhovou metodou určenou ke snížení emisí uhlíku. Tyto programy jsou z teoretického hlediska považovány za nákladově efektivní metodu. Experimentální pokusy avšak nasvědčují tomu, že subjekty dělají vysoce neefektivní volby ohledně velikosti snížení emisí a že metody, které jednotlivé emisní limity přidělují (ať už zdarma či za úplatu), vedou subjekty k příliš velkému nebo příliš malému snížení emisí. Tyhle výsledky tudíž naznačují, že v porovnání s teorií jsou v praxi cap-and-trade programy nákladnější. Tato studie však tuhle interpretaci zpochybňuje a ukazuje, že v případě, že subjekty jsou cenoví příjemci a mají dostatek příležitostí k učení, se tyhle subjekty rychle naučí dělat správná rozhodnutí a že tato rozhodnutí nejsou ovlivněny metodou, která emisní limity přiděluje.

Keywords: Abatement, Cap-and-Trade, Experimental Economics, Emission Trading System, Carbon Permits, Experience effects
JEL Classification: C91, D62, Q54, Q55, Q58

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1 Introduction

The main policy objective of a cap-and-trade program is the cost-efficient abatement of the emissions of pollutants. This paper focuses on the cost effectiveness of abatement of greenhouse gas emission in the largest cap-and-trade program in the world: the EU carbon Emission Trading Scheme (EU-ETS).[3] I focus specifically on abatement by investment in cleaner technology.

Theory predicts that a cap-and-trade program is cost efficient (Montgomery 1972). Cost-efficient abatement requires that it be carried out by firms with the lowest abatement costs. A cap-and-trade program creates a market for permits and firms are allowed to emit pollutants only when they surrender permits. Permits can be traded and often also banked. With perfect competition in the permit market, an equilibrium permit price results which equalizes demand and supply. This equilibrium price achieves cost efficiency as emitters with abatement costs lower than the price abate, while those with abatement costs higher than the price do not abate. Firms that hold a surplus of permits sell the surplus, while those that have a shortage of permits make up their shortfall by buying on the permit market. Empirical data from permit markets created by cap-and-trade programs are, so far, not detailed enough to allow strong conclusions (Ellerman 2010) and are generally fraught with identification problems. Economics experiments are an alternate method to test permit markets in a controlled environment and can address design issues that may affect efficiency and effectiveness (Roth 2002).

While experimental research on cap-and-trade programs exists, the literature on abatement is scant as most experiments on cap-and-trade programs do not address abatement investment. To date the research on abatement investment under cap-and-trade programs consists of Betz and Gunnthorsdottir (2009); Camacho-Cucna, Requate, and Waichman (2012); Chesney, Taschini and Wang (2011);

[3] See Ellerman (2010) for a detailed description.

Gangadharan, Farrell and Croson (2012); and Grimm and Ilieva (2013). Contrary to the theoretical results of Montgomery (1972), most of the experimental research finds that subjects make highly inefficient abatement choices, suggesting that a cap-and-trade program may not deliver abatement for the lowest costs possible (Betz et al. 2009; Gangadharan, et al. 2012; Grimm et al. 2013). Betz et al. (2009), Gangadharan et al. (2012) and Grimm et al. (2013) find high error rates in the choices of subjects. Gangadharan et al. (2012) and Grimm et al. (2013) find that experimental subjects generally overinvest in abatement (over-abatement). Gangadharan et al. (2012) finds that 61% of firms invest in abatement as opposed to the theoretical optimum of 16.7%.[4] Grimm et al. (2013, p.18) find abatement levels significantly higher than the theoretical optimum. Camacho-Cucna et al. (2012) present subjects with the dichotomous option to invest or to abstain from investment and find that the percentage of correct choices is 81% among subjects who are predicted to invest and 72% among subjects who are predicted to abstain from investment. These success percentages are not as high as they might seem at first sight. Due to the dichotomous nature of the task a random choice would have lead to a success percentage of 50%. Betz et al. (2009) find an allocation bias in the abatement choices. Overall, the experimental evidence suggests a high rate of error and, consequently, a relatively low efficiency for cap-and-trade programs.

The allocation bias found by Betz et al. (2009) is in line with the theoretical predictions of Baldursso and von der Fehr (2004) and Gagelmann (2008). Baldursso and Von der Fehr (2004) and Gagelman (2008) predict that when firms are risk averse, the method of permit allocation affects the outcomes of a cap-and-trade program. Risk-averse firms which are short of permits and must pay for them (we refer to such firms as being "under-allocated") will over-invest in abatement

[4] Gangadharan et al. (2005) represented the problem in a fundamentally different way than other experiments on cap-and-trade programs and abatement. They presented abatement as an investment in the higher efficiency of the production process, thus leading to higher profits.

(also referred to as over-abatement), while those that receive permits for free or have an overabundance of permits (we refer to such firms as being "over-allocated") will under-invest (also referred to as under-abatement) relative to the cost-minimizing solution. The intuition behind this result is that risk-averse, under-allocated subjects perceive the payment for certificates as the more risky parameter and thus over-abate, while over-allocated firms perceive abatement as the more risky element and thus under-abate.

In line with these theoretical predictions, Betz et al. (2009) find that under-allocated subjects over-abate, while over-allocated subjects under-abate relative to the cost-minimizing solution. Betz et al. (2009), however, find no significant relation between abatement choices and risk preferences. While Grimm et al. (2013) find that, overall, subjects over-abate, they report that under-allocated subjects abate significantly less. Their regression analysis, however, uses an – arguably – inappropriate clustering of the data, and it is therefore not clear if the reported relationship remains significant with a more conventional manner of clustering.[5] Moreover, Grimm et al. (2013) did not measure risk preferences. Camacho-Cucna et al. (2012) find mixed evidence for the effect of allocation on investment. They run a pooled probit estimation of abatement on a set of variables including risk preferences. They find that risk aversion has a significant negative effect on abatement when permits are allocated for free and subjects are over-allocated, thus supporting the theory of Baldursso and von der Fehr (2004) and Gagelmann (2008). Risk aversion is positive when permits are allocated by auction and subjects are under-allocated, but not significant.

[5] Grimm et al. (2013) cluster data on the subject level, which presumes that observations are independent on this level and that they have 80 independent data points per treatment. However, subjects are clustered within 5 different groups and interact within, but not between groups. Data should thus be clustered on the group level, resulting in 5 independent data points per treatment. Indeed, Grimm et al. (2013, p.18) report that "(w)e gathered 5 independent observations per treatment". The reported data analysis thus overestimates the number of independent observations by a factor of 16 (80 independent observations instead of 5), which may have inflated the level of significance in their analysis.

Overall, experiments report a prevalence of inefficient abatement choices. In contrast, in an experiment by Wråke, Myers, Burtraw, Mandell, and Holt (2010), subjects made highly efficient decisions.[6] Subject had to make production choices given the level of the permit price. The production choices can be interpreted as abatement-by-reduction (Requate 2005) – when the carbon price is high, subjects reduce pollution by choosing a lower level of production. Error rates fell, depending on the treatment, from between 15% and 45% in the first round to between 0% and 10% in the tenth round. A possible explanation for the low error rate of the decisions is that subjects had ample opportunity for learning. This was not so in the experiments of Betz et al. (2009), Gangadharan et al. (2012), and Grimm et al. (2013). Their experiments consisted of a relatively few number of rounds, between four and eight. Moreover, in their experiments rounds were not independent, as banking[7] was allowed and abatement investments were irreversible. Banking and irreversible abatement investments changes the marginal costs of production in later rounds, thus creating a path-dependency of later rounds on the decisions in earlier rounds. Inefficient decisions may thus have been caused by unwary choices in the first round by inexperienced subjects. Due to banking and irreversible investments, participants thus had little opportunity to experiment and learn from their investment choices.[8]

In addition, having prices determined by trading and auction with a relatively low number of subjects, sometimes as low as eight, may have introduced uncertainty regarding prices due to strategic bidding or bounded rationality.[9] Cap-

[6] While Wråke et al. (2010) address a different type of abatement; it is presented here as the study may suggest how to address the potential external validity problems with the studies of abatement by technology switching.

[7] Banking of permits means that permits need not to be used in the period they have been issued or bought, but can be saved and used in a later time period, possibly several years later.

[8] Exceptions are, as mentioned before, Wråke et al. (210) and Camacho-Cucna et al. (2012). Camacho-Cucna et al. (2012) ran four training rounds before running six rounds of their main experiment and the rounds were independent – they did not influence one another by banking or irreversible investments.

[9] It is well established that participants in experiments make bids that are far from optimal: see Kagel & Levin (mimeo) for an overview.

and-trade markets such as the EU-ETS are very large and liquid, and the market liquidity is progressively increased by the growth of carbon future markets (Ellerman 2010). The bidding behavior of a small group may thus not be representative for such large and liquid markets.[10]

Table 1a and 1b below summarize the design and results of the previous experiments. The results column also specifies the allocation bias: the effect of Over-Allocation (OA) versus Under-Allocation (UA) on the abatement choice. The last row in Table 1a shows the design and a preview of the results of the experiment in this paper. Table 1b summarizes the design of Wråke et al. (2010).

Table 1a Overview previous experiments on abatement investment in ETS

Abatement by technology switching	**Are rounds independent? (origin of dependency)**	**Number of market subjects (HHI: competitiveness)**	**Independent observations**	**Rounds**	**Results: Efficiency (Allocation Bias)**
Gangadharan et al. (2012)	Dependent (irreversible investment and banking)	28 (357: very low concentration)[11]	1	6 rounds	Relatively low (Over-abatement by both UA and OA firms)
Betz & Gunnthorsdottir (2009)[12]	Dependent (irreversible investment and banking)	8 (1250: moderately concentrated)	1?	8 rounds	Relatively low (Over-abatement by UA and under-abatement by OA, but no correlation with risk attitudes)
Grimm et al. (2013).	Dependent (banking)	16 (625: low concentration)	5	4 rounds	Relatively low (Over-abatement)
Camacho-Cucna et al. (2012)	Independent	18 (556: low concentration)	3	6 rounds + 4 training rounds	Moderately, the reported success percentages (77%, 85% and 80%) are only moderately higher than the success percentage for random choice (50%) (Over-abatement by UA

[10] The Herfindahl-Hirschman Index (HHI) for allowances holding is very low in the EU-ETS. For example, the 2008 HHI indices for firms' original allowance allocations, their allowance surplus and their allowance deficit were very low: 136, 94 and 228 respectively (Ellerman et al., 2010, p.129; Alberola, 2008). Furthermore, the secondary market in allowances is growing steadily in volume and sophistication (Ellerman et al., 2010, p. 289).
[11] Gangadharan et al. (2005) assigned the 28 participants to 6 different types of producers, but they did not provide details of how the 28 participants were divided over the different producer types.
[12] Betz & Gunnthorsdottir (2009) do not report the number of participants and sessions in their experiment. Their Table seems to indicate that the eperiment consists of at least 1 session with 8 participants.

					and under-abatement by OA)
This paper	Independent	price-taking assumption (0: perfect competition)	24	20 rounds	Relatively high (Over-abatement by UA with high risk-aversion in the first 10 rounds, no bias in the last 10 rounds)

Table 1b Abatement by reduction in ETS

Abatement by reduction of production	Are rounds independent? (origin of dependency)	Number of market subjects (HHI: competitiveness)	Independent observations	Rounds	Results: Efficiency (Allocation Effect)[13]
Wråke et al. (2010)	Independent	price-taking assumption (0: perfect competition)	12	10 rounds	Relatively high in the last five rounds (more errors by OA than UA in the first five rounds, no difference in the last five rounds)

Table 1a and 1b summarize the main differences in design and results between Wråke et al. (2010) and the other experiments. However, we cannot directly compare Wråke et al. (2010) with the other experiments, as Wråke et al. (2010) allow only abatement by output reduction and not by choosing a cleaner technology. This leads to the question of whether the inclusion of the main design elements of Wråke et al. (2010), independent rounds and perfect competition on the permit market, in an experiment allowing abatement by choosing a cleaner technology would also result in high efficiency and an absence of an allocation effect or bias. Measuring risk preferences would furthermore allow the testing of the theoretical predictions of Baldursso and von der Fehr (2004) and Gagelmann (2008).

The present experiment addresses this question by testing the efficiency and effect of allocation on abatement-choices by choosing a cleaner technology, drawing on the methodology of Wråke et al. (2010) by providing subjects with the

[13] As Wråke et al. (2010) report the error percentages separately for under-allocated and over-allocated subjects, but do not distinguish between errors of overproduction (under-abatement) and those of underproduction (over-abatement), I refer to the effect of allocation for this study simply as an “allocation effect”.

opportunity of learning through a considerable number of independent rounds (investment is not irreversible and there is no banking) and in a highly competitive market (price-taking assumption). Assuming a highly competitive market is realistic for large cap-and-trade programs such as the EU-ETS (Ellerman et al. 2010). As the task in the experiment is more complicated than the one in Wråke et al. (2010), the experiment consists of more rounds (20 versus their 10 rounds). In the remainder of this paper, I describe the experimental design in section 2, procedures in section 3, the results in section 4, and I conclude in section 5.

2 Experimental design

In the experiment I run two main treatments: Under-Allocation (UA) and Over-Allocation (OA). [14] Experimental subjects represent firms that produce a fixed number of 30 units every round, bringing a fixed earning of 135 ECU, but for which they must surrender 30 permits under the default technology (no abatement). Firms can, against paying a fixed "installation" cost, choose from a set of cleaner technologies that reduce emissions, and thus reduce the number of permits they are required to surrender. Cleaner technologies can reduce emissions by 10%, or a multiple of 10%, with a maximum reduction of 100%. At the end of a round, the excess (shortage) of permits is sold (bought) for the permit price. The permit price is announced at the beginning of each round.

Firms in the treatment "over-allocated" ("under-allocated") receive 30 (zero) permits for free and thus never buy (sell) permits, but can increase (decrease) the number of permits they sell (buy) by choosing a cleaner technology. Under risk-neutral preferences and profit-maximizing behavior, both under-allocated and over-

[14] Two more treatments have been tested that were highly risky in the sense that subjects were informed of the realized permit price only after they had made their abatement decision. Subjects were informed that the permit price was chosen at random from the set of integers between 1 and 9 with equal probabilities. The outcomes of these treatments were less clear, possibly because of the rather high risk, and I therefore discuss the main results of these treatments in Appendix A.

allocated subjects are predicted to maximize $\pi = 135 - p_{PERMIT} \cdot (30 \cdot abatement) - c[abatement]$. In the equation p_{PERMIT} is the price of a permit, *abatement* is the percentage reduction in emissions (and thus in permits to be surrendered) and *c*[*abatement*] is the cost of the technology to realize the reduction in emissions. Table 2 shows the optimal technology as a function of the permit price.

Table 2 Technologies, abatement and costs

Technology	**Abatement**	**Cost ($c[abatement]$)**	**Permit price for which the technology is optimal**
Default	0%	0	1
1	10%	3	1
2	20%	7	-
3	30%	10	2, 3
4	40%	20	-
5	50%	30	4, 5
6	60%	45	5, 6
7	70%	65	7, 8
8	80%	90	9
9	90%	130	-
10	100%	190	-

The cost of cleaner technologies is modeled by a strictly convex carbon abatement cost function. For the experiment, the cost function is chosen to be $c[x] = -5 + 6 \cdot 2^{5x/100}$, rounding to the nearest integer for numbers below 10, to the nearest multiple of five for numbers above 10, and cost 0 for the default technology. Table 2 presents the technologies-abatement percentages, their costs and the carbon prices for which they are the profit-maximizing choices.

Table 3 Hypotheses

H1: High efficiency	The error rate is low in rounds 11-20
H2: Learning effect	The fraction of errors is lower in rounds 11-20 than in rounds 1-10

H3: No allocation effect	a) Over-Allocated and Under-Allocated subjects are not different in the proportion of errors they make. b) Over-Allocated subjects don't abate more than Under-Allocated subjects
H4: No risk preferences effect	Controlled for risk preferences, Over-Allocated subjects don't abate more than Under-Allocated subjects

Table 3 summarizes the four hypotheses. I conjecture that with a design using independent rounds and perfect competition, choices will be highly efficient after subjects had the chance to familiarize themselves with the task (Hypothesis 1) and that decisions exhibit a pronounced learning effect over rounds (Hypothesis 2). As the price is announced beforehand, subjects do not face risk and their decisions are thus predicted not to be affected by allocation (Hypothesis 3) or risk preferences (Hypothesis 4).

3. Procedures

The experiment was programmed in ZTREE (Fischbacher 2007). The experimental sessions were conducted in June and October 2012 at the experimental laboratory LEE of the University of Economics in Prague.[15] Subjects were students at the University of Economics in Prague. In total two sessions were run, one for each of the two treatments. In each treatment 24 subjects took part, resulting in 24 independent observations per treatment. In total, 48 subjects participated in the main experiment.[16] A treatment contains 20 periods and lasts up to one hour. The same experimenter read the (English language) instructions to the subjects for all sessions.

In the one-hour long experiment subjects earned, on average, CKZ 370, which is equal to €15 (equivalent to an EU-27 average purchasing parity of about €20). The

[15] For more info, see http://www.vse-lee.cz/eng.
[16] A total of 48 subjects participated in the two other "risky" treatments.

minimum earning was 130 and the maximum earning was 530 Koruna. The experiment was thus well incentivized. All subjects took part in one and only one session, thus observations are independent across treatments.

The consolidated instruction can be found in the Appendix. Subjects can see on the screen how many permits they need to buy or have left for sale. They can also see the permit price for the round. As mentioned before, at the end of a round, after subjects have made their technology decisions, the number of permits they have in excess (are short of) is sold (bought) automatically for the permit price. All prices are quoted in Experimental Currency Units (ECUs), which are converted to Czech Crowns at the end of the experiment.

Table 4 Subgroups and the presented Permit Price (PP)

		PP								
		1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Presented PP	**1**	1	2	3	4	5	6	7	8	9
	2	4	5	6	7	8	9	1	2	3
	3	7	8	9	1	2	3	4	5	6

The permit price is drawn randomly – using one seed – from the uniform distribution over the set {1, 9}. To reduce the probability of an atypical sequence of permit prices, the 24 subjects in each treatment are subdivided into three subgroups of eight subjects each. The subjects in the first subgroup are presented with the randomly drawn permit price (*PP*). The subjects in the second and third subgroups are presented with, respectively, *MOD*(*PP + 3, 9*) and *MOD*(*PP + 6, 9*). Table 4 gives an overview of the permit price presented to the subgroups. This procedure guarantees that, in every round, for equal proportions of the subjects, the permit price is in the low region (range 1-3), the middle region (range 4-6), and the high region (range 7-9). This makes it less likely that an atypical sequence of permit prices could affect the results.

At the end of the experiment, I categorized all subjects according to their risk aversion through an additional test, similar to the procedure in Holt & Laury (2002). Subjects had to choose between a series of safer and riskier options. In the first choice, the safer option had a higher expected value (EV) than the corresponding riskier one. With every choice, the EV of the riskier choice grew faster than the EV of the corresponding safer one. The riskier option had a higher expected value than the corresponding safer one for the fifth choice (see Table 5 below). Standard theory predicts that an agent will switch, if at all, only once from the safer to the riskier option across the 10 choices. A subject that makes (more than/less than) five safe choices is categorized as being risk neutral (risk averse/risk loving). I define variable *RA* for Risk Attitude as the number of safe choices minus five. A subject that has a score on *RA* equal to (larger than /smaller than) zero is thus categorized as risk neutral (risk averse/risk loving). Following standard practice, subjects that make inconsistent choices, such as switching from the riskier option to the safer one, are excluded from the analysis.

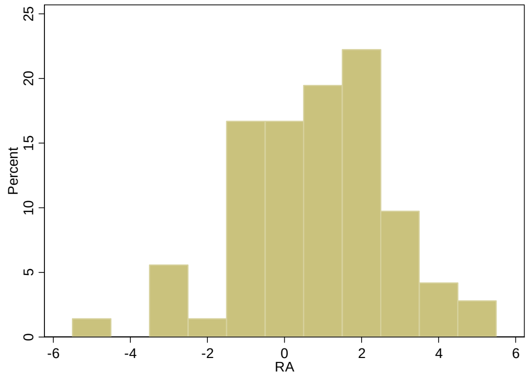
Table 5 Overview of the options in the test for risk preferences

	Safer Option					**Riskier Option**					
Decision	**Probability High Payoff**	**High Payoff**	**Probability Low Payoff**	**Low Payoff**	**Expected Value**	**Probability High Payoff**	**High Payoff**	**Probability Low Payoff**	**Low Payoff**	**Expected Value (EV)**	**Difference (Riskier - Safer)**
1	0.1	200	0.9	160	164	0.1	385	0.9	10	47.5	-116.5
2	0.2	200	0.8	160	168	0.2	385	0.8	10	85.0	-83.0
3	0.3	200	0.7	160	172	0.3	385	0.7	10	122.5	-49.5
4	0.4	200	0.6	160	176	0.4	385	0.6	10	160.0	-16.0
5	0.5	200	0.5	160	180	0.5	385	0.5	10	197.5	17.5
6	0.6	200	0.4	160	184	0.6	385	0.4	10	235.0	51.0
7	0.7	200	0.3	160	188	0.7	385	0.3	10	272.5	84.5
8	0.8	200	0.2	160	192	0.8	385	0.2	10	310.0	118.0
9	0.9	200	0.2	160	212	0.9	385	0.2	10	348.5	136.5
10	1.0	200	0.1	160	216	1.0	385	0.1	10	386.0	170.0

4. Results

In line with the literature, most subjects were soundly risk-averse. Figure 1 shows the risk preferences of subjects. Three out of 48 subjects made inconsistent choices (switching from the riskier option to the safer one) and were thus excluded from the analysis. Including these three subjects leaves the results which follows basically unchanged.

Figure 1 Risk preferences



Values of Risk Attitude (RA) larger than zero indicate attitudes that are risk-averse, values smaller than zero risk-loving attitudes

Figure 2 Results

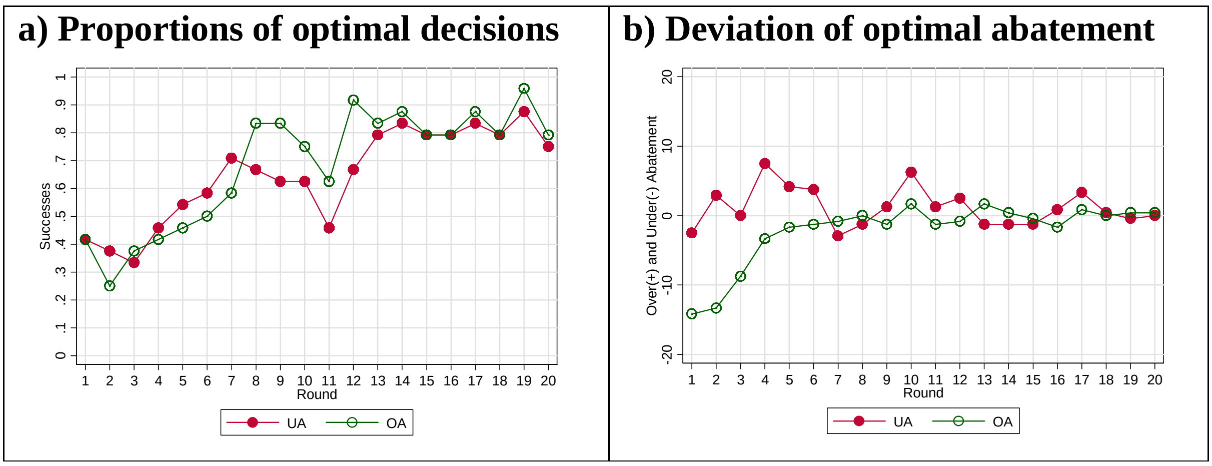


Figure 2a shows the proportion of optimal abatement decisions per round, separately for the Under-Allocated (UA) and Over-Allocated (OA)subjects. The proportion of optimal decisions is very low in the earlier rounds, the average over the first five rounds for under-allocated and over-allocated subjects together is only 0.4. The low proportion of optimal decisions is in line with the earlier experimental literature. However, we see that over the number of rounds, the curve increases steeply, resulting in a relatively high proportion of optimal decisions after approximately seven or eight rounds. In the last 10 rounds, the proportion of optimal decisions is on average 0.8. This score is only slightly lower than in Wråke et al. (2010), and is probably in the same ballpark when we correct for the fact that, when choosing at random, the probability that the subjects made the optimal decision was in Wråke et al. (2010) much higher (33%, 1 out of 3) than in this experiment (9%, 1 out of 11). The high proportion of optimal decisions in the last 10 rounds confirms Hypothesis H1.

The proportion of optimal decisions strongly increases in the experiment, thus suggesting a pronounced learning effect. Indeed, a Wilcoxon signed-rank test confirms that the increase in the proportion of optimal decisions from the first 10 to the last 10 rounds is highly significant ($p<0.001$), and it doesn't matter if this test is performed for all subjects taken together or for only the over- or under-allocated subjects. This confirms Hypothesis H2.

Using the eyeball test, there is no clearly visible substantial difference between the UA and OA treatments in Figure 2a, indicating that there is no allocation effect or bias. Indeed, a Wilcoxon signed-rank test comparing the proportions of correct decisions between the UA and OA treatment finds no significant difference ($p=0.78$), and it doesn't matter if this test is performed including only the first five rounds, only the first 10 rounds, or all 20 rounds: The lowest p-value is 0.46. This confirms Hypothesis H3a: There is no significant difference between the UA and OA treatments.

Figure 2b shows the deviation from the optimal abatement level, the abatement chosen minus the optimal abatement, averaged for each round separately over the under-allocated and over-allocated subjects. Positive (negative) values thus represent over-abatement (under-abatement). The eyeball test suggests that in the early rounds (the first six rounds), under-allocated subjects are more prone to over-abatement, while the over-allocated are more prone to under-abatement. The effect seems to disappear in the later rounds (rounds 7-20). I discuss this effect in more detail below.

Table 6 OLS regression results

	Model 1	Model 2	Model 3
Round	1-20	1-10	11-20
VARIABLES	*Optimal* (proportion of optimal abatement decisions)	*Deviation* (abatement level chosen minus optimal level)	*Deviation* (abatement level chosen minus optimal level)
UA	0.04	2.85	-0.10
	(0.13)	(2.64)	(0.73)
RA	-0.03	-2.02*	-0.13
	(0.04)	(1.14)	(0.28)
*UA*x*RA*	-0.05	4.14***	0.33
	(0.07)	(1.45)	(0.43)
Round	0.03***		
	(0.00)		
*Round*x*UA*	-0.00		
	(0.01)		
*Round*x*RA*	0.00		
	(0.00)		
*Round*x*UA*x*RA*	0.00		
	(0.00)		
Constant	0.39***	-2.59	0.10
	(0.09)	(2.26)	(0.50)
Observations (independent)	920 (46)	460 (46)	460 (46)
R-squared	0.13	0.06	0.00

To analyze the data further I run a regression using OLS with errors clustered on the subject level (Froot 1989):

$$Optimal = \alpha_1 \cdot UA + \alpha_2 \cdot RA + \alpha_3 \cdot UA\text{x}RA + \alpha_4 \cdot Round + \alpha_5 \cdot Round\text{x}UA + \alpha_6 \cdot Round\text{x}RA + \alpha_7 \cdot Round\text{x}UA\text{x}RA + \varepsilon$$

In the regression equation, *Optimal* stands for the deviation from the optimal choice, *UA* is a dummy that assumes the value one when subjects receive zero permits for free and are thus Under-Allocated (*UA*) and zero otherwise, *RA* indicates Risk Attitude and, as mentioned above, a value of *RA* equal to (larger than /smaller than) zero indicates risk-neutral (risk-averse/risk-loving) attitudes. The variable *Round* stands for the number of the played rounds and is thus a proxy for experience. Furthermore, interaction variables have been included to account for all possible interaction effects between these variables.

Table 6, Model 1, shows that, apart from the constant, only the round is significant ($p<0.01$). The coefficient on the round played is substantial: every additional round of play increases the proportion of correct decisions by 3 percentage points, thus indicating a pronounced learning effect. This further supports Hypothesis H2. As all variables other than the round and the constant are insignificant, allocation (*UA*) and risk aversion (*RA*) (or their interactions with one another and with the round) do not affect the level of correct decisions or the speed of learning. This further supports Hypothesis H3a. These findings are, however, in contrast to those of Wråke et al. (2010), who found quicker learning for the subjects that had to pay for permits (*UA*).

To determine if there is an allocation bias, I run the regression:

$$Deviation = \alpha_1 \cdot UA + \alpha_2 \cdot RA + \alpha_3 \cdot UA\text{x}RA + \alpha_4 + \varepsilon$$

In the regression equation, *Deviation* stands for the deviation from the optimal level of abatement: a positive (negative) value thus indicates over-abatement (under-abatement). The results of this regression are shown in Table 6. I run the regression separately for the data of rounds 1-10 (Model 2) and rounds 11-20 (Model 3).

Table 6, Model 3, shows that, in the regression over rounds 11-20, none of the variables is significant. The insignificance of the dummy variable *UA* indicates that under-allocated subjects do not abate more than over-allocated subjects. This confirms Hypothesis H3b. The insignificance of the variable *RA* indicates that risk aversion in itself has no effect on abatement choices. The insignificance of the interaction of under-allocation and risk aversion *UA*x*RA* indicates that under-allocated subjects with high risk aversion do not abate more than other subjects. Thus, allocation does not affect abatement, even when risk preferences are controlled for. This is in line with expectations as there was no risk in the treatment: all relevant information was available. The results thus supports hypothesis 4.

However, Table 6, Model 2, shows that, in the regression over rounds 1-10, when subjects are still relatively unfamiliar with the task, two variables are significant. The coefficient on *RA* is negative and significant (p=0.081), indicating that an increase in risk aversion results in more *under-abatement*. The coefficient of the interaction of the method of allocation and risk aversion (*UA*x*RA*) is positive and highly significant (p<0.01), indicating that, for under-allocated subjects, an increase in risk aversion results in more *over-abatement*. This is an effect that is predicted by Baldursso and von der Fehr (2004) and Gagelmann (2008) when agents face risky choices. However, as mentioned before, the treatments does not contain risk. A possible explanation may be that, because of being unfamiliar with the task, subjects perceive this task as being risky or uncertain. As a result, over-allocated subjects seem to perceive abatement as the most risky parameter in the task and thus under-abate (over-abate) when they are risk-averse (risk-loving). Under-allocated subjects seem to perceive the payment for certificates as the most risky parameter and thus over-abate (under-abate) when they are risk-averse (risk-loving). Their decisions thus follow the pattern as predicted by Baldursso and von der Fehr (2004) and Gagelmann (2008). As mentioned before, the effect of the unfamiliarity dissipates and is absent in the regression for rounds 11-20, where none of the variables is significant. A likely explanation is that subjects with experience no longer perceive the task as risky.

5 Conclusion

The two main findings in this experiment are that experimental subjects make highly inefficient decisions that are biased by allocation in the first few rounds, and that they make highly efficient abatement decisions unbiased by allocation after about six to 10 rounds.

The first finding, the occurrence of highly inefficient and biased decisions in the early rounds is in line with earlier experimental studies on abatement. The earlier studies generally did not provide subjects with many rounds to become familiar with the task. Moreover, the rounds were mostly not independent due to the possibility of banking and long-lasting investments that affect costs and payoffs in successive rounds, which hampers learning. This experiment supports the earlier experimental literature in suggesting that subjects make highly inefficient decisions that are biased by allocation, but adds, motivated by the second finding, the caveat that this is the case only for relatively inexperienced subjects.

The second finding, that subjects, after having acquired experience with the task, made highly efficient abatement decisions unbiased by allocation, is a new finding. These highly efficient and unbiased abatement decisions were observed after subjects had played as many as six to 10 rounds. That the first rounds in an experiment may be less meaningful due to confusion of the subjects is not a novel idea. It is usual practice in experimental economics focused on IO topics to disregard a considerable part of the earlier rounds and to focus the analysis of the data on the latter part of the rounds.[17]

In addition, I find that the decisions of subjects in the first 10 rounds follow the pattern of the theoretical predictions by Baldursso and von der Fehr (2004) and Gagelmann (2008): under-allocated subjects (who need to buy permits) have a tendency to over-abate and this tendency increases in relation to their risk averseness. The pattern disappears with experience. A possible explanation is that subjects may have perceived the task as risky due to their incomplete understanding in the early rounds, and allocation and risk preferences therefore affected their decisions.

This study thus suggests that the inefficient and biased decisions reported in previous experimental studies may be a product of the lack of understanding of the subjects due to a lack of opportunities for learning. The lack of sufficient learning may negatively affect the internal validity of complicated studies with interdependent rounds (such as rounds allowing banking or long-lasting investments that affect costs and payoffs in successive rounds). Where possible, the design of such complicated studies should be adapted to provide opportunities for learning.

The findings of this study are mostly in line with those in Wråke et al. (2010), who, in a different (simpler) setup without abatement by investment in a cleaner technology, also found an initial high proportion of inefficient decisions that transformed into a rather high proportion of efficient decisions after a learning period. The results in this study contrast with those in Wråke et al. (2010) in that no difference in the speed of learning is found between under-allocated and over-allocated subjects.

The finding that subjects are able learn to make very accurate abatement

[17] See, for example, Brandts et al. (2008) and Van Koten and Ortmann (2013).

decisions bodes well for cost-efficient abatement under cap-and-trade programs. Moreover, allocation, free or paid, has been found to have no effect on the accuracy of decision making, not even for relatively inexperienced subjects. This suggests that policy makers may enjoy the industry support provided by free allocation without paying the cost of decreased abatement efficiency.

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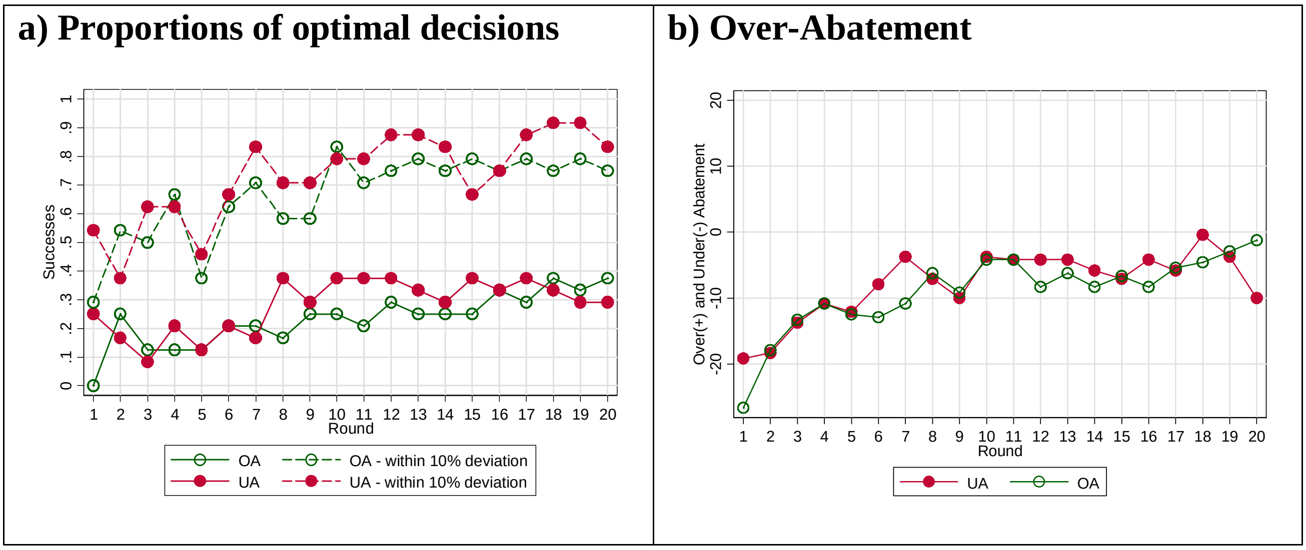
7 Appendix

A. Additional analysis

In addition to the main treatments in the paper, two treatments were run where subjects were informed that the permit price was chosen at random from the set of integers between one and nine, and were told the realized permit price only after they had made their abatement decision. Figure A1 shows, per round, the proportion of optimal decisions and the average amount of over-abatement.

Figure A1



The lower points connected by solid lines in Figure A1.a show the proportion of optimal decisions for each round. The under-allocated and over-allocated subjects taken together have a very low proportion of optimal decisions in the first rounds, on average 0.15 in the first five rounds. The proportion increases slightly over the duration of the experiment to an average of 0.33 in the last five rounds. This may not be surprising, as the treatment is considerably more difficult: Subjects must maximize an expected outcome over all nine possible permit prices. The higher points connected by dashed lines in Figure A1.a show the proportion of optimal decisions with a tolerance of a mistake of 10 percentage points. Proportions are now much higher, from an average of 0.50 in the first five rounds, to an average of 0.81 in the last five rounds.

However, with or without a tolerance of 10 percentage points, we see again – as in the analysis in the main text - a strong learning effect, illustrated by the increase in the proportion of optimal decisions. This is confirmed by Wilcoxon signed-rank tests comparing the outcomes of the last 10 rounds with those of the first 10 rounds. These tests are highly significant, either with (p<0.001) or without (p<0.006) the tolerance of 10 percentage points.

Using the eyeball test for Figure A1.a suggests that the proportion of efficient decisions is not different between under-allocated and over-allocated subjects. Indeed, a Wilcoxon signed-rank test is insignificant, either with (p<0.48) or without (p<0.44) the tolerance of 10 percentage points.

The results are thus mostly in line with the earlier findings: choices are highly inefficient in the early rounds but become, allowing a tolerance of 10 percentage points, highly efficient in the later round. Allocation has no effect on the proportion of efficient decisions or the speed of learning.

Figure A1.b shows the over-abatement for under-allocated and over-allocated subjects for each round. In the early rounds both under-allocated and over-allocated subjects under-abate in the earlier rounds. The degree of under-abatement weakens until it is close to zero in the last five rounds. Using the eyeball test for Figure A1.b suggests that, in contrast to the results in the main text, there is no difference between under-allocated and over-allocated subjects for the proportion of efficient decisions. A possible explanation is that the very high risk in the treatments made over-allocated as well as under-allocated subjects perceive abatement as the more risky element in the earlier rounds. As a result both groups of subjects under-abated in the earlier rounds.

B. Consolidated Instructions

The following consolidated instructions integrate the instructions of all treatments in "Do Emission Trading Schemes Facilitate Efficient Abatement Investments?". The treatments conditions are explained in detail in the paper and are here referred to abbreviated as:

- C: Certain. Subjects are shown the permit price before they make a decision
- R: Risky. Subjects are not shown the permit price before they make a decision, but afterwards.
- UA: Under-allocated. Subjects received zero permits for free
- OA: Over-allocated. Subjects receive 30 permits for free

The conditions are combined into four treatments:

1. C-UA: Certain & Under-allocated
2. C-OA: Certain & Over-allocated
3. U-UA: Risky & Under-allocated
4. U-OA: Risky & Over-allocated

All text outside brackets [] is the baseline text common to all treatments. Bracket [] identify text that is specific to a treatment. After the opening bracket the treatment is indicated (C-UA, C-OA, R-UA, or R-OA), followed by one space after which the text specific to a treatment follows.

A) The main experiment

Welcome to the experiment!

General rules
Please turn off your mobile phones now.

If you have a question, raise your hand and the experimenter will come to your desk to answer it.

You are not allowed to communicate with other participants during the experiment. If you violate this rule, you will be asked to leave the experiment and will not be paid (not even your show-up fee).

Introductory remarks
You are about to participate in an economics experiment. The instructions are simple. If you follow them carefully, you can earn a substantial amount of money. Your earnings will be paid to you in cash at the end of the experiment.

The currency in this experiment is called "Experimental Currency Units", or "ECU"s. At the start of the experiment, you will receive a start capital of 30 ECU. At the end of the experiment, we will exchange ECUs for Czech Crowns as indicated below. Your specific earnings will depend on your decisions. You will not interact with the other participants in the room.

Your exchange rate will be: 1 Czech Crown for [R-UA 3 ECU] [R-OA 16 ECU] [C-UA 5 ECU] [C-OA 20 ECU].

This experiment will take approximately 60 minutes. There are 20 paid rounds in this experiment.

You are allowed to write on these instructions.

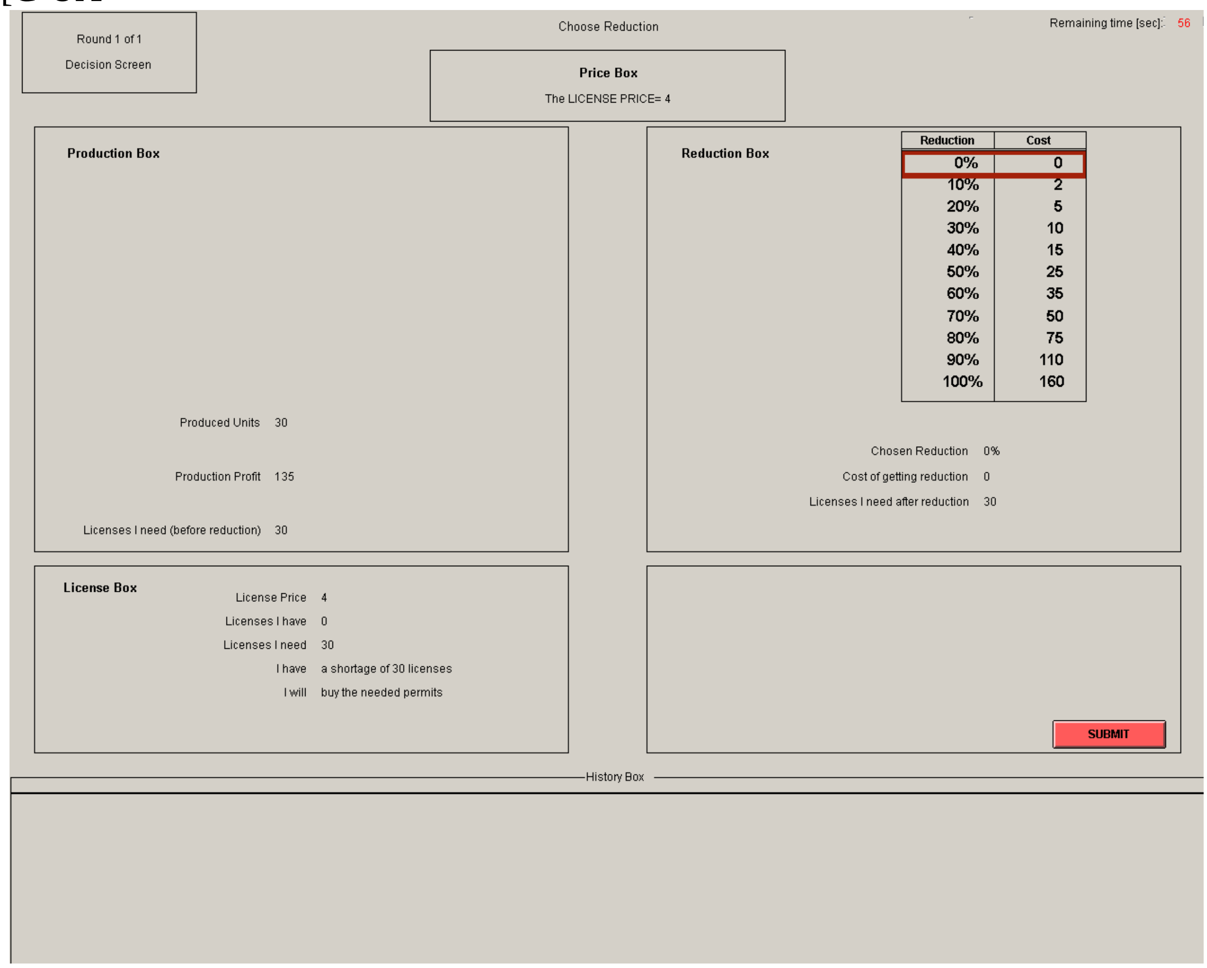
In this experiment, in each round, you will need a number of licenses. Each round, you will have to make a decision whether you want a reduction on the number of licenses needed, and, if yes, how large a reduction. We explain this in detail below.

Your decisions are valid for the present round only and thus affect only your profit for the present round.

Figure 1 shows you an example of the computer screen you will be using to make your decisions during the experiment. Note that in the upper left corner is written the Round (in the example in Figure 1 the Round is 1), and that the screen is the DECISION SCREEN. In the upper right corner you can see the time you have to make your decision ('Remaining time').

Figure 1

[C-UA



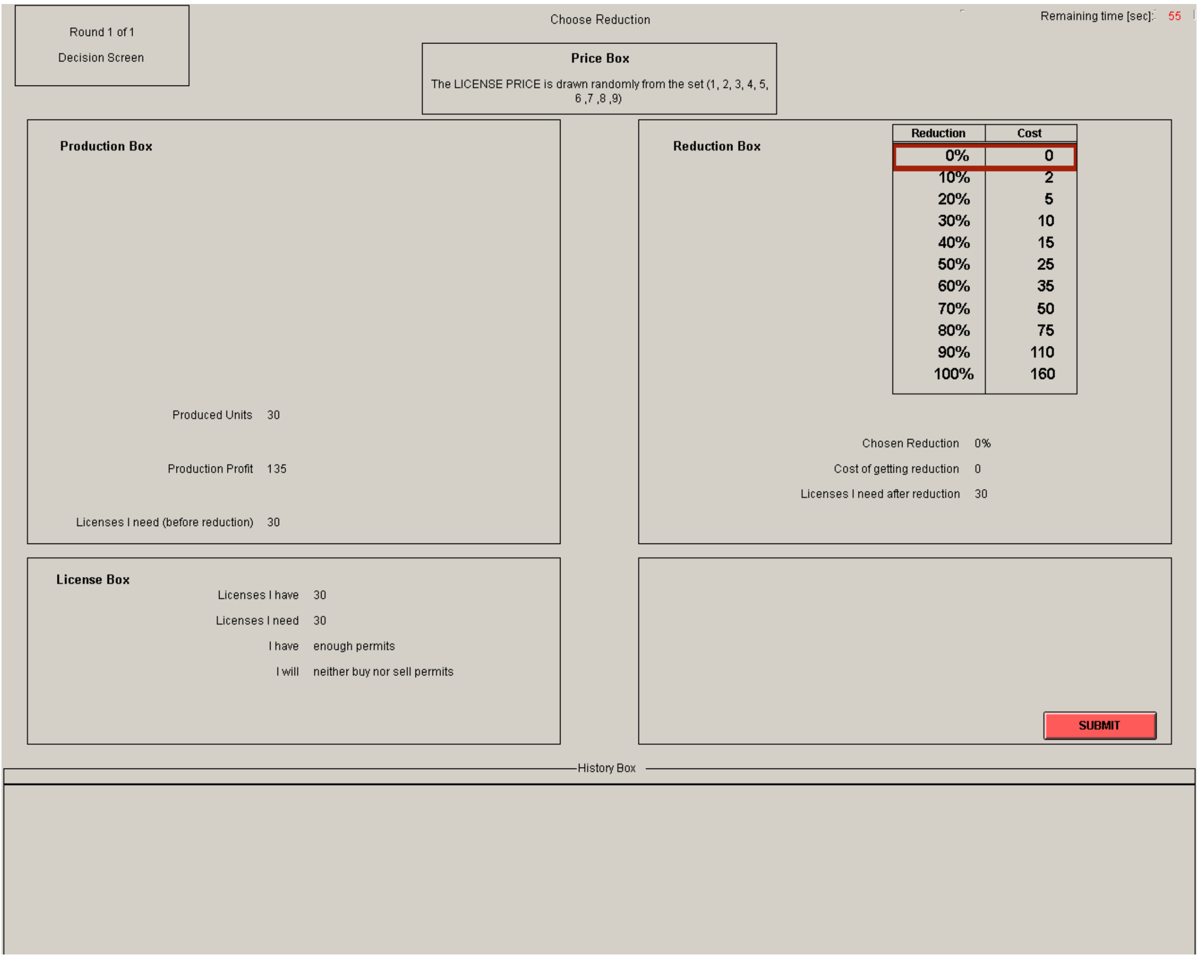
C-UA]

[C-OA


Round 1 of 1
Decision Screen
Choose Reduction
Remaining time [sec]: 55
Price Box
The LICENSE PRICE= 4
Production Box
Produced Units 30
Production Profit 135
Licenses I need (before reduction) 30
Reduction Box
Reduction Cost
0% 0
10% 2
20% 5
30% 10
40% 15
50% 25
60% 35
70% 50
80% 75
90% 110
100% 160
Chosen Reduction 0%
Cost of getting reduction 0
Licenses I need after reduction 30
License Box
License Price 4
Licenses I have 30
Licenses I need 30
I have enough permits
I will neither buy nor sell permits
SUBMIT
History Box

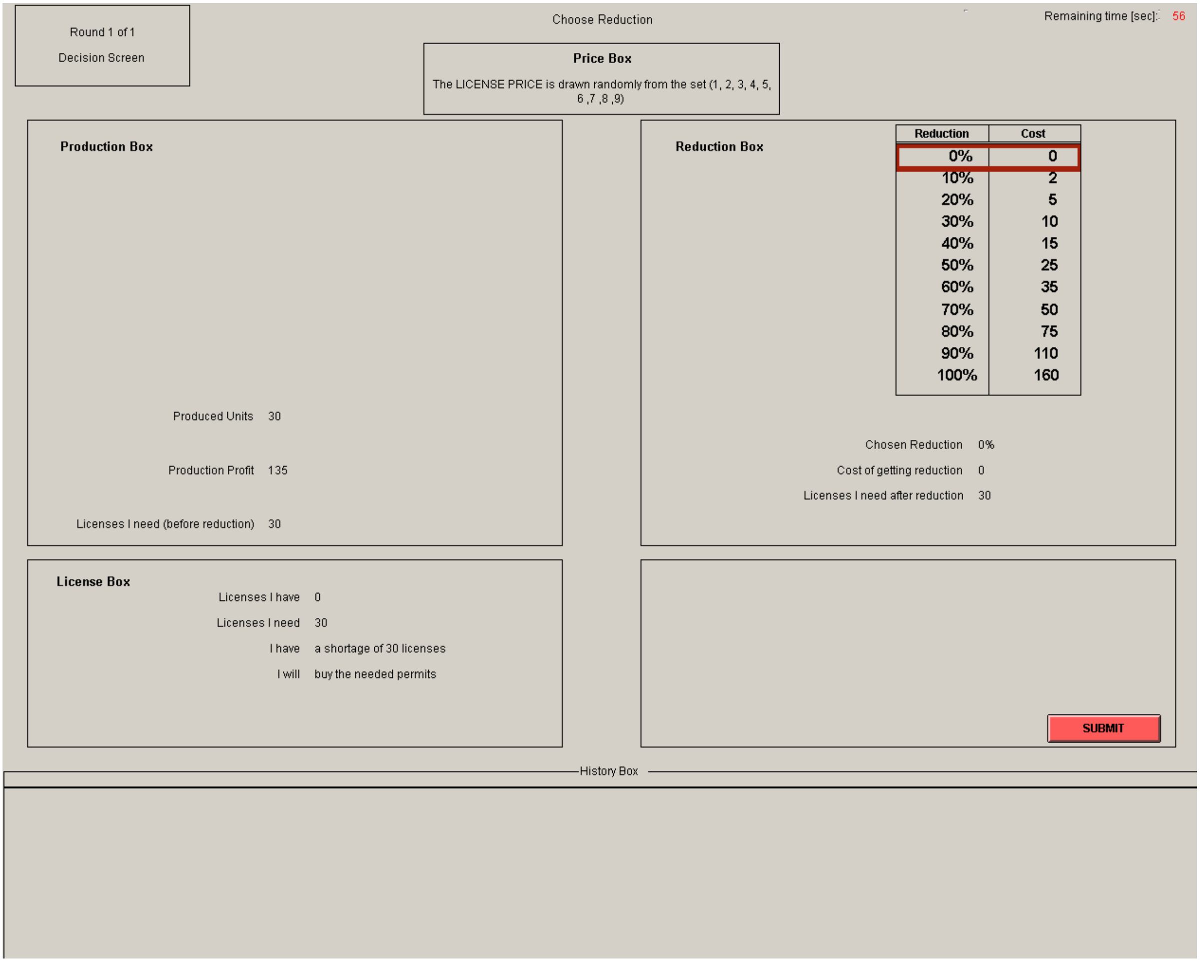
C-OA]

[R-OA


Round 1 of 1
Decision Screen
Choose Reduction
Remaining time [sec]: 55
Price Box
The LICENSE PRICE is drawn randomly from the set (1, 2, 3, 4, 5, 6 ,7 ,8 ,9)
Production Box
Produced Units 30
Production Profit 135
Licenses I need (before reduction) 30
Reduction Box
Reduction Cost
0% 0
10% 2
20% 5
30% 10
40% 15
50% 25
60% 35
70% 50
80% 75
90% 110
100% 160
Chosen Reduction 0%
Cost of getting reduction 0
Licenses I need after reduction 30
License Box
Licenses I have 30
Licenses I need 30
I have enough permits
I will neither buy nor sell permits
SUBMIT
History Box

R-OA]

[R-UA



R-UA]

Production Box (see Figure 1, the large upper left box)
In each round, you automatically produce and sell an imaginary good X. In each round, the automatic production earns you a profit equal to 135 ECU, but it obliges you to have and hand over 30 licenses.

Reduction Box (see Figure 1, the large upper right box)
You can reduce the number of licenses you need in the present round by choosing a reduction in the Reduction Box. You see an example in Figure 1 on the upper right.. Under **"Reduction"**, you can choose between reduction rates of 0% (no reduction), 10%, 20%, 30%, 40%, 50%, 60%, 70%, 80%, 90%, and 100%. The price for a reduction is given in the same row of the reduction rate: thus 0% costs 0, 10% costs 2, 20% costs 5, 30% costs 10, and so on.

For example, you will need 30 licenses if you choose a reduction rate of 0%, 27 licenses if you choose 10%, 24 licenses if you choose 20%, 21 licenses if you choose 30%, and so on.

You choose your reduction rate by clicking on one of the percentages listed under "**Reduction**" in the Reduction Box. By clicking on the percentage, a thick rectangle appears around the row you chose. Figure 1 gives an example where a player chose a reduction of 0%.

The reduction rate you choose will be valid only for the present round.

License Box (see Figure 1, the large lower left box)
In the License Box you see the number of licenses you have and the number of licenses you need (taken in account the reduction rate you have chosen).

[F At the start of every round, you will have 30 licenses. If you have more licenses than you need after you made your reduction choice, the surplus licenses will be sold automatically for the License Price. F]
[P At the start of every round, you will have 0 licenses.

If you have fewer licenses than you need to hand over, the missing licenses will be bought automatically for the License Price. P]

The automatic [P buying P] [F selling F] of licenses happens at the end of the round, after you have made your decisions and pressed the SUBMIT button.

Every round the License Price is set equal to a random number drawn from the set {1, 2, 3, 4, 5, 6, 7, 8, 9}. Each number has an equal probability to be drawn for a round.
[C The License Price is shown in the Price Box on the top of the screen; see Figure 1. C]
[U This is shown in the Price Box on the top of the screen; see Figure 1. The License Price is shown only AFTER you have made your decision. U]

You may change your reduction as many times as you like. You make your choice final by pressing the red SUBMIT button. The experiment continues after all subjects have pressed the SUBMIT button. Please press the SUBMIT button within 60 seconds.

Results box
Next, you will see the final results. In the lower right part of the screen will be a Result Box which will show you your Production Profit, License Result and Final Profit.

When you have inspected the results, press the red NEXT ROUND button. The experiment continues once all subjects have pressed the NEXT ROUND button. Please press the red NEXT ROUND button within 1 minute.

To repeat: Your decisions are valid for the present round only and thus affect only your profit for the present round. You will not interact with the other participants in the room.

Do you have any questions at this point?

B) The Holt-Laury test (measurement of risk preferences)

These are the instructions for a decision experiment that is related to the one you just participated in.

The currency in this experiment is again called "Experimental Currency Units", or "ECU"s. Your exchange rate for this decision experiment will be: 1 Czech Crown for 2 ECU.

You will not interact with the other participants in the room.

The next screen will ask you to make ten decisions. In Figure 1 you see an example of the decisions you will be asked to make.

Figure 1

	Option A		-	Option B	
Decision 1	probability of 1/10 of 200 ECU,	probability of 9/10 of 160 ECU	○ ○	probability of 1/10 of 385 ECU,	probability of 9/10 of 10 ECU
Decision 2	probability of 2/10 of 200 ECU,	probability of 8/10 of 160 ECU	○ ○	probability of 2/10 of 385 ECU,	probability of 8/10 of 10 ECU
Decision 3	probability of 3/10 of 200 ECU,	probability of 7/10 of 160 ECU	○ ○	probability of 3/10 of 385 ECU,	probability of 7/10 of 10 ECU
Decision 4	probability of 4/10 of 200 ECU,	probability of 6/10 of 160 ECU	○ ○	probability of 4/10 of 385 ECU,	probability of 6/10 of 10 ECU
Decision 5	probability of 5/10 of 200 ECU,	probability of 5/10 of 160 ECU	○ ○	probability of 5/10 of 385 ECU,	probability of 5/10 of 10 ECU
Decision 6	probability of 6/10 of 200 ECU,	probability of 4/10 of 160 ECU	○ ○	probability of 6/10 of 385 ECU,	probability of 4/10 of 10 ECU
Decision 7	probability of 7/10 of 200 ECU,	probability of 3/10 of 160 ECU	○ ○	probability of 7/10 of 385 ECU,	probability of 3/10 of 10 ECU
Decision 8	probability of 8/10 of 200 ECU,	probability of 2/10 of 160 ECU	○ ○	probability of 8/10 of 385 ECU,	probability of 2/10 of 10 ECU
Decision 9	probability of 9/10 of 200 ECU,	probability of 1/10 of 160 ECU	○ ○	probability of 9/10 of 385 ECU,	probability of 1/10 of 10 ECU
Decision 10	probability of 10/10 of 200 ECU,	probability of 0/10 of 160 ECU	○ ○	probability of 10/10 of 385 ECU,	probability of 0/10 of 10 ECU

Each Decision is a paired choice between "Option A" and "Option B." You will make a choice by selecting the radio button next to your choice. Option A is always the left radio button and Option B is the right radio button.

Before you start making your choices, it is important that you understand how your choices will affect your earnings for this part of the experiment.

After you have made all of your choices, the computer will randomly generate two numbers, each from the set {1, 2, 3, 4, 5, 6, 7, 8, 9, 10}. These numbers will be reported on the screen.

The first number is the "Decision number" and it will select one of the ten decisions to be used. The second random number is the "Payoff number". The "Payoff number" determines your payoff for the option you chose, A or B, for the particular decision selected. Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used. Obviously, each decision has an equal chance of being used in the end.

Now, please look at the ten Decisions in Figure 1.
- If the random "Decision number" is 1, Decision 1 will be earnings relevant. If the random "Decision number" is 2, Decision 2 will be earnings relevant, and so on.

Now, please look at Decision 1 at the top. . Option A pays 200 ECU if the "Payoff number" is 1, and it pays 160 ECU if the "Payoff number" is 2-10. Option B pays 385 ECU if the "Payoff number" is 1, and it pays 10 ECU if the "Payoff number" is 2-10. The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, the "Payoff number" will not be needed since each option pays the highest payoff for sure, so your choice here is between 200 ECU or 385 ECU.

To summarize, you will make ten decisions: for each row you will have to choose between Option A and Option B. When you are finished, press the OK button, and the computer will generate the two numbers between 1 and 10 and display these numbers and your payoff.

Earnings for this decision experiment will be added to your other earnings, and you will be paid all earnings in cash when we finish.

Are there any questions? Raise your hand if you have a question.

Now you may press the OK button and begin making your choices for Decisions 1-10. Please do not talk with anyone while we are doing this.

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