The effects of fee imposition on the efficiency of resource utilization: an experimental study

by

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Abstract

The paper presents the results of an economic experiment where it is tested whether the imposition of resource fees increases efficiency in the utilization of limited resources. Two methods of collecting fees are compared. Laboratory subjects (university students) played the roles of company managers. Their companies produce a good which requires a limited input in production, *viz.* tradable production permits. Three treatments were compared: a) no fee imposed (grandfathering); b) a constant resource tax is imposed in each time period per unit of production permits; c) a certain amount of permits are withdrawn from companies in each period and reallocated by auction. Our results indicate that imposition of resource fees has an impact on distribution of permits and that fees enhance efficiency compared to the grandfathering case. Furthermore, withdrawal and reallocation by auction reduces efficiency compared to the case of no resource fee. The most probable explanation is that it matters what subjects face taxation and how it is presented to them.

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1 Introduction

In a seminal article Montgomery (1972) established the now classical result that a system of tradable pollution licenses will minimize costs of achieving a set abatement target. How licenses or permits are initially distributed is irrelevant to this result. In particular, it does not matter whether licenses are auctioned fully or partially or whether (non-distortionary) fees are levied on them, the end result is the same: trade will take place until marginal costs of abatement are equal across firms and costs are minimized. Of course Montgomery's result, *mutatis mutandis*, not only applies to tradable pollution permits, but to any activity regulated by tradable quantity instruments, be they called licenses, permits or quotas. Such regulation has in the last few years increasingly been used by governments in many countries. Examples are markets with permits for pollution release, fishing quotas, quotas for agricultural production, frequency spectra and transport franchises.

Little if any research using data from real resource markets has been done in order to test Montgomery's theorem. This is no wonder, since it is difficult to find reliable data where the effect of fees or auctions on different market results can be isolated. Experimental testing therefore lends itself well to this situation; in the laboratory, it is possible to compare behavior in markets which are identical in all other aspects than those regarding fee imposition. In this paper we use experimental methods to study one aspect of the issues related to Montgomery's theorem; in particular, we focus on the question whether taxation or auctioning of permits affects allocative efficiency. More specifically, we investigate whether imposition of fees – where we use the term 'fees' to include both taxation and permit auctions – influences the distribution of permits between firms with different production costs. Our study is rooted in a long tradition of experiments on market behavior and is *inter alia* based on the work of Smith, Suchanek and Williams (1988).

We are not aware of previous work on the issue we study in this paper. There are, however, some recent experimental studies that are related to our work. In a study of compliance in emissions trading programs with imperfect monitoring Murphy and Stranlund (in press) found that firms with higher initial allowances tended to retain more permits and be more compliant than those with a lower initial allocation; a similar effect was not observed in a perfect monitoring and compliance treatment indicating that some sort of transactions costs – in a wide sense – are created by the introduction of imperfect monitoring and the related uncertainty. Interestingly, such an effect was not found in a study of imperfect enforcement and banking by Cason and Gangadharan (in press). The experimental study of tradable fishing allowance markets by Anderson and Sutinen (in press) is closest in experimental setup to ours, and a related paper by Anderson, Freeman and Sutinen (2005) on industry consolidation under tradable fishing licences is also conceptually relevant to our work. None of these papers, however, focus on the effect of permit fees on allocative efficiency as we do here.

When tradable permit or quota systems have been introduced in practice in an existing activity such as electricity generation or fishing this has usually been done by grandfathering, i.e. by allocating permits in accordance with historical use of the resource being regulated without charging payment. Grandfathering is often criticized on equity grounds; the reason for the prevalence of this practice is most likely the need to acquire political support from the affected industry.[1] However, the view – in accordance with Montgomery's theorem – that this entails no efficiency losses has traditionally been the prevailing one in the economics literature. Dissenting

[1] See Oates and Portney (2003) for an overview of the political economy of environmental policy.

views have, however, been gaining weight in recent years and it is increasingly claimed that free allocation of permits will indeed lead to efficiency losses. Underlying such claims are of course some assumptions that are excluded in Montgomery's static, friction-free, full-information model. Most often these assumptions have implications for dynamic, rather than allocative efficiency and should not matter in a static setting.[2]

It has also, however, been argued that allocative efficiency is improved by imposing resource fees. For example Stavins (1995) shows that transactions costs can inhibit trading so that an inefficient initial allocation of permits is, at least partially, maintained and abatement costs are not minimized.[3] Auctioning permits, on the other hand, would lead to an efficient initial allocation as firms with the highest valuations of permits will bid highest. It should be noted that, in contrast to auctioning, imposing a fixed tax on permits would not have any effect on an inefficient initial distribution of permits in Stavins' setup; for taxation to be able to 'push' inefficient firms to sell their permits there must be an effective distinction between paid costs and non-realized opportunity costs of holding allowances rather than selling them. On a related note Baldursson and von der Fehr (2004) show that when up-front investment in abatement is required and agents are risk averse, efficiency is achieved by partial auctioning of allowances.

Our approach was initially motivated by local debate – sometimes quite heated – in Iceland on the effects of fishing fees and whether and then how they should be imposed.[4] Indeed, some features of our experimental design reflect some policy options that were suggested in this

[2] For example, Hahn and McGartland (1989) show that permit auctions create incentives to develop new options in production or pollution reduction technology. Kling and Zhao (2000) analyze a model where auctions affect entry and exit of firms; see also Pezzey (2003) for a comparison of views on the long-run efficiency of emission taxes and auctioned permits. Taking a general-equilibrium perspective rather than the partial-equilibrium perspective of Montgomery's theorem, the double-dividend literature (e.g. Bovenberg and Goulder, 1996) points out that revenues from auctioned quotas can be used for reducing distortionary taxes. These literatures are not directly relevant to our experiment.

[3] The results of Montgomery and Stavins are of course intimately related to the Coase (1960) theorem, i.e. that in the absence of transactions costs well-defined property rights and free trade lead to an efficient allocation of rights.

[4] For some aspects of this debate, see the papers in Arnason and Gissurarson (1999), and Matthiasson (2001).

debate. In particular, we assume that a tradable permit system for regulating production is in place with a predetermined maximum aggregate quantity; permits are long-lived and production takes place over a number of periods. Two particular ways of collecting fees are compared: a fixed tax on number of permits held and partial withdrawal and reallocation of permits by auction in each period. However, the experiments were run with neutral terminology and our approach is rather general and should therefore be applicable to any market where a basic production input is available in limited quantity and is allocated to producers by freely transferable utilization permits. We do not look at fee imposition as an administrative tool to achieve a certain target utilization, but focus on a system where the collective utilization of a resource, which is here equal to total production, is determined in advance, as in e.g. sulfur trading in the U.S. Sulfur Trading Program, the European Union Greenhouse Gas Emission Trading Scheme (EU ETS) or the Icelandic fishing quota system.[5]

Our experiment is set up so that dynamic issues such as entry and exit of firms or general equilibrium issues such as the double dividend effect are not relevant. The level and methods of collecting fees are such that in the absence of transactions costs and with perfect information the equilibrium distribution of permits should be same, irrespective of whether and then how resource fees are collected. Yet, our results indicate that – controlling for price effects – collecting resource fees does have an impact on efficiency and the distribution of permits. In particular, fees appear to enhance efficiency compared to the grandfathering case. However, in contradiction to the transactions cost theory of Stavins, withdrawal and reallocation of permits by auction reduces efficiency compared to the case of no resource fee.

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[5] Grandfathering was used for initial allocation in all these systems.

As has been established in economics experiments, individuals' and companies' behavior is not always rational in the traditional interpretation of economic theory. In this context, we maintain that two primary factors are potentially important in relation to our subject matter. On one hand, fee imposition can be important if companies react in different ways to real and inevitable cost, such as taxation, and the opportunity cost of having a utilization permit, in this case the market price of the licence. Experiments dealing with auction markets indicate that this could be the case (Phillips, Battalio and Kogut, 1991). On the other hand, the original allocation could influence individuals' valuation (Thaler, 1980) so that there is a tendency for those who receive a generous original allocation to retain their permits in excess of what can be expected on the basis of expected present value income from the permits and their market price. Our results indicate that it is the former explanation which is the most likely one. Furthermore, it appears that presentation of fee imposition is important, since taxation on the one hand and withdrawal and auctions on the other have opposite effects.

The remainder of the paper is organized as follows. Section 2 describes the experimental structure and procedures. Section 3 contains the main results, and we discuss these in the fourth and last section.

2 A simple utilization permit market

2.1 The structure of the experiment

In order to study the effect of fee imposition on efficiency in resource utilization, we designed an experimental market, described below. Participants in the experiment were given the role of companies which produced and sold an unspecified product and traded production permits. Eighteen six-person groups made up of students from the business and economics, engineering and science faculties of the University of Iceland participated in the experiment, which was conducted in several sessions in May and October 2004.[6] Thirty-six of these 108 participants were present twice so that the effect of acquired experience on the results could be assessed. Participants were paid for their contribution in direct proportion to the profit they made on behalf of their company. No one was paid less than 800 ISK, the highest payment was 4,097 ISK, and the average payment was 2,252 ISK.[7]

Six participants played a part in each individual game or experiment. Each had the role of running a business which was allocated a certain amount of production permits and an initial fund which could be used to purchase permits.[8] Each game ran for 15 rounds, in addition to two practice rounds which are not included in the data set. Each round was divided into a market phase and a production phase. In the market-phase, there was an open market with production permits in real time (a minimum of three minutes) where all participants could bid to buy or sell permits, one at a time, as well as accept other bids. This market was organized as a double auction which has proved successful in numerous experiments (Sunder, 1995). In this part of the experiment, participants were to decide on their desired production quantity and market it in the stipulated period. It was only possible to select a quantity corresponding to an integer smaller than or equal to the number of permits that each participant had in their possession. For reasons of simplification, we assumed that there was perfect competition in the product market, so that every company received a fixed price of 75 e$ for produced units. At the end of each period, participants were told how much profit they had made during this time, and given a statement of funds and production permits. In one treatment of the experiment, Treatment C, there were two more factors in addition to the aforementioned ones. In that case, each period began with three

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[6] In addition, twenty people participated in a preparatory experiment conducted at the Bifrost School of Business.
[7] Corresponding to approximately 11 $/10 €, 60 $/50 €, and 33 $/27 €, respectively.
[8] Most similar experiments use larger markets (i.e. with more participants). However, preliminary testing showed that there was little difference between results based on whether the group consisted of six or eight people.

production permits (20% of the total) being withdrawn from participants and right thereafter, the same permits were reallocated by auction. These two additional factors are described in more detail below.

The six firms were grouped into three pairs where each pair of firms had the same production costs. However, since within each pair the initial allocation of permits was different none of the participants had exactly the same business conditions. As can be seen in Table 1, Companies 1 and 2 had the lowest cost per unit, or 35 e$ for the first three units but 45 and 55 e$ for the fourth and fifth units, respectively. Companies 3 and 4 had, relatively, medium high costs and Companies 5 and 6 fairly high unit costs. In each period, the companies could produce at most equally many units to the number of their production permits. Companies could not own more than five permits, so that the maximum production in each company was thus also five units.

Had it not been for the production limitation imposed by the permit system, the companies could have manufactured 30 units in total without a loss, since production cost per unit never exceed the output price. On the other hand, the total number of production permits was only 15. The initial allocation of permits was as unequal as possible, so that the odd-numbered companies were not allocated any permits, while the even-numbered ones received five permits, which was the maximum number allowed for one company to own. This organization meant that the original allocation of licences was neutral towards the distribution of production options between companies.

Table 1: Cost per unit for companies and allocation of production permits

	Unit cost					*Permit allocation*	
Firm	*1. unit*	*2. unit*	*3. unit*	*4. unit*	*5. unit*	*Initial*	*Efficient*
1	35	35	35	45	55	0	4-5
2	35	35	35	45	55	5	4-5
3	35	45	55	65	65	0	2-3
4	35	45	55	65	65	5	2-3
5	55	65	65	75	75	0	0-1
6	55	65	65	75	75	5	0-1

Each group participated in one of three treatments of the experiment. In Treatment A, the baseline treatment, there was no fee imposed on production licences and profit (rent) which resulted from rationalization in production was retained by the companies themselves. In Treatment B, the companies paid a tax in every period for each permit held at the end of the period. The tax was fixed at 15 e$ per permit. In Treatment C, three permits, or a fifth of the total, were withdrawn in the beginning of each period. This was done by a company owning one permit having a 20% chance of losing it, a company owning two licences having a 40% chance, and so on, so that a company with five permits always lost one permit. Irregular loss/profit because of relatively great/little depreciation was evened out with monetary transfers, so that the financial effects of depreciation would be largely similar to everyone losing 20% of their permits. The three permits were then sold in a sealed bid (Vickrey) auction. All companies were obligated to bid for one permit. The three highest bidders received one permit each and paid for it the amount of the fourth highest bid. The experimental treatments were alike in all other ways.

The participants' possibilities of making a profit were fairly disparate depending on their assigned role, or which treatment of the experiment they took part in. For example, the sum of e$ available for distribution among participants was much greater in Treatment A than the other treatments. This was equalized by determining the relative value of experimental dollars to ISK for each and every role and also the experimental treatments (A, B or C). All participants thus had an *a priori* equal opportunity to make a profit in terms of expected value and theoretical predictions of prices.

When the experiment was repeated with experienced participants, the structure was altered slightly in order to help the participants to get a better grip on the game and for faster convergence to equilibrium. The main change was that instead of having 15 rounds, the game was shortened to four rounds and then repeated four times (the paradigm for this structure comes from Anderson and Sutinen, in press). The model's parameters were changed slightly in this version. For example the product price was raised to 80 e$ and production cost was lowered somewhat. Despite a higher contribution margin, the tax was lowered down to 11 e$ to take into account the effect of fewer rounds on auction revenues (which were lowered). These changes do not qualitatively affect the theoretical prediction of behavior which is described below. To simplify the exposition, we only refer to the parameters of the former experiment with inexperienced participants below, except where the difference is crucial.

2.2 Predicted behavior

If production were centralized in the small economy that is created in the experiment, and the total quantity of production were limited to 15 units, as is done in the experiment, then all production options with a unit cost of 45 e$ or lower would be used; also, three additional units (out of six) would be produced at a cost of 55 e$. By this arrangement, the total cost of producing 15 units would be minimized (see Table 1).

In competitive equilibrium, where all firms base their decisions on maximising profit, take prices as given and assume the same behavior from other participants in the market, the result should be the same as in the centralized solution: the 15 allocated permits should be traded until the 15 units are produced with minimum cost. Therefore, the 12 most expensive units – the ones marked by a shadowed background in Table 1 – should not go into production.[9] (These units have a production cost higher than 55 e$.) For each such produced unit, the cost of manufacturing the 15 units which carry a permit increases, and an inefficiency results. This does not agree with premises of competitive equilibrium, because companies with more efficient production options should be willing to pay a high enough price for production permits that it would be more lucrative to sell the licence than to use it to produce an inefficient unit. Also, some companies should produce three additional units with a 55 e$ production cost.

As mentioned above, participants were paid for their contribution in direct relation to the profit they made on behalf of their company. Thus, the participants had a strong incentive to maximise their profit. However, it cannot be assumed that everyone behaves according to profit maximization in an experiment like this one, although based on previous experience, there is a strong tendency in that direction.[10] In order to ensure that inevitable deviation from profit maximization has as little effect as possible, the production options are organised so that all the companies can stay at the margin and produce at 55 e$ a unit, even though the most efficient division of production permits (see the last column of Table 1) calls for an uneven possession (of permits). Due to the shortage of licences, three of the companies at most can utilize these production options.

Since only three out of six permits with 55 e$ costs should be produced, the competitive equilibrium does not provide a unique prediction of the division of licences and thus each firm's production. Rather, the permit holding of each company can take two values which agree with efficient allocation. In competitive equilibrium, the efficient firms (no. 1 and 2) have four or five

[9] For all the six companies to maximise their profit is as such an unneccesarily rigid premise. Adequate conditions are that three of the companies maximise profit and the other ones choose either the amount which maximises profits or one unit less.

[10] See further Camerer and Hogarth (1999), which i.a. deals with the effects of different amounts on incentives in economics experiments.

units, the mid-range companies (no. 3 and 4) have two to three units, and the inefficient companies zero to one unit. Nevertheless, there is a unique solution for the price of production permits, which is equal to the profit of the marginal units multiplied by the number of remaining periods.

It was not expected in advance that participants in the experiment would find the competitive equilibrium described above immediately, i.e. in the first round, as could be assumed if all neo-classical premises held. There are mainly two things that stand in the way of rapid adaptation to competitive equilibrium. On one hand, participants do not individually have enough information to be able to calculate what the market price of permits should be in equilibrium. Each one only has information of the product price, the development in the quota market, and their own (discontinuous) cost function. The companies can nevertheless assess their own profit if they would increase/decrease their licence possession by one permit. If they follow the simple rule to buy a permit when the benefit of owning an additional one exceeds its market price, and in the same way to sell it when the benefit of the last production unit falls short of the market price of one licence, it should not be long before all business opportunities are taken advantage of and competitive equilibrium is reached. All the same, the process of adapting to equilibrium can be slow (Smith, 1962).

When decisions are made the companies will not only look at production cost but they must also take into consideration the fact that production permits are assets which are transferred between rounds and that it can be expected that price formation occurs in a similar way as in comparable asset markets. Economics experiments have demonstrated that there is a strong tendency for a price bubble to form in experimental asset markets, but eventually the bubble bursts and price collapses.[11] The concept of a 'price bubble' here refers to a development where prices rise far more than normal compared to the underlying variables (e.g. marginal operating profit). Our experiment is different from traditional experiments on asset prices insofar that in our experiment operating profits – corresponding to dividends in asset market experiments – are non-random and known. Hence, the net income from owning a permit is certain, although the development of permit prices and the resulting capital gains can be erratic. However, according to Porter and Smith (1995), uncertainty regarding dividend payment is not an important explanation for price bubbles. Rather, it is primarily speculation.[12] Therefore, on the basis of previous experimental evidence, price bubbles can be expected to form in our production permit market. Permit price can take on a life of its own, so to speak, and thus create trade with production licences independent of production efficiency. These two factors, i.e. asymmetric information on one hand and price bubble formation on the other, can hinder the market from converging to competitive equilibrium and efficient distribution of production licences.

2.3 Hypotheses

As mentioned above, our primarily concern is the question of whether fees levied on utilization permits have an effect on efficiency. It is therefore necessary to define an efficiency measure. Let R_{ijt}, C_{ijt} and $\Pi_{ijt} = R_{ijt} - C_{ijt}$ stand for income, cost and operating surplus of company $i \in I_j$ in Treatment $j \in \{A, B, C\}$ at time *t*, respectively. The total surplus of companies $j \in \{A, B, C\}$ at time *t* is thus $\Pi_{jt} = \sum_{i \in I_j} \Pi_{ijt}$. Maximum possible surplus is

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[11] See Porter and Smith (2003) for an overview of such experiments.

[12] According to Smith et al. (1988), speculation on a market such as this one is not a result of irrationality. Although everyone trading in a particular stock has exactly the same distribution of future returns and full and mutual information is accessible, this is not enough to create unity in market participants' expectations. In their opinion, the main reason for bubble formation is individuals' uncertainty regarding behavior of other market participants.

denoted by $\Pi^*_{jt} > 0$. Note that this surplus can only be made when production is at full capacity, i.e. 15 units. We will now define the efficiency measure *E* as the ratio of realized and optimal aggregate surplus.

$$E_{jt} = \frac{\Pi_{jt}}{\Pi^*_{jt}}. \tag{1}$$

Clearly $E_{jt} \leq 1$ with equality only if production is efficient, i.e. $\Pi_{jt} = \Pi^*_{jt}$.

As pointed out above, we are mainly interested in whether there is a difference in efficiency between treatments. The initial statistical null hypothesis is thus that mean efficiency is the same in all three treatments of the experiment. Presumably, results from the last few periods are most reliable, since participants have at that point gained a considerable amount of experience. Permit prices are not directly comparable between experiments, since fee imposition has a direct influence on price formation. Yet, below, we consider relative price deviation from the theoretical forecast of permit price. In all cases, the price forecast is based on the premise that companies maximise expected profit and look at marginal profit earned on production units taking taxes levied or withdrawal of permits into consideration. Also, the number of remaining rounds is taken into account. The forecast for production permits in the basic treatment (A) is thus:

$$\hat{P}_{A,t} = \sum_{i=t}^{T} s = s \times (T - t + 1), \tag{2}$$

where *s* is per-unit-surplus, *T* is the total number of rounds and *t* represents the decision period. As mentioned above, the experiment was changed slightly when it was repeated with experienced participants. In the first implementation (inexperienced subjects), the hypothetical marginal profit in each round was 20 e$ and the number of rounds was 15. In that case, the price forecast is 300 e$ in the first round, decreasing by 20 e$ in each round. In the second implementation (experienced subjects), the marginal profit in each period was 27 e$ and there were only four rounds. The price forecast was therefore initially 108 e$, decreasing by 27 e$ in each round.

In the tax treatment (B), τ e$ were subtracted from participants' income for each permit they owned in a round. The price forecast is thus:

$$\hat{P}_{B,t} = \sum_{i=t}^{T} (s - \tau) = (s - \tau) \times (T - t + 1) . \quad (3)$$

The tax, τ, was 15 e$ in the first implementation and 11 e$ in the second one. The post-tax profit was 5 e$ at the margin, which means that the price forecast starts at 75 e$ in the first round and there is a linear decrease to 5 e$ in the 15th round in the first implementation. In the second implementation, the marginal profit was 16 e$; the price forecast started at 64 e$ and steadily decreased to 16 e$ in the last round.

The price forecast in the auction treatment (C) is a bit more complicated since the production permit value is partly determined by their relative depreciation *f*. The result is thus:

$$\hat{P}_{C,t} = s \sum_{i=t}^{T} (1 - f)^{T-i} = s \frac{1 - (1 - f)^{T-t+1}}{f} . \quad (4)$$

The depreciation rate *f* was always set equal to 0.2, which means that the price forecast started at just over 96 e$ and ended at 20 e$ in the first implementation, and in the second one, it started at 80 e$ and was 27 e$ in the end. Figures 1 to 4 below illustrate price forecasts in the three treatments, and also give examples of the participants' real behavior.

Note that according to price forecasts (*ex ante*), the tax treatment and the auction treatment involve comparable fee imposition on production permits. However, since imposed fees are fixed at a certain currency amount in the tax treatment but are a proportion of the production permit price in the auction treatment, they are in general not equal in the actual experiments (*ex post*).

3 Results

3.1 Production permit prices

Before we turn to the main research question of this article in more detail, we must briefly address how trade was conducted and how prices and volumes in the experimental markets developed. This was fairly diverse from one experiment to another. However, individual experiments can be roughly grouped according to price development.

The experiment was conducted twenty-four times in total. An evident *price bubble* formed on five occasions, in the end imploding or bursting. The development of bids and prices in experiment 3A, illustrated in Figure 1, is typical for this category. In the figure, it is clear how the price was originally quite low, i.e. only one-third of the predicted amount. Later, the price increased substantially in the first four rounds, and peaked at 500 e$, by which time it was double the predicted price according to the underlying variables. After that, the price remained high compared to the price forecast (see the horizontal lines in the figure) until it began to plunge quickly in the twelfth round.

Experiment 4B is characteristic for another kind of development which can be referred to as *stable excess price*, see Figure 2. The price started rather high above the price forecast, stayed above it for most of the following periods and remained many times higher than the forecast. The price then dropped drastically in the final round and came close to the fundamental price. A similar development took place altogether six times.

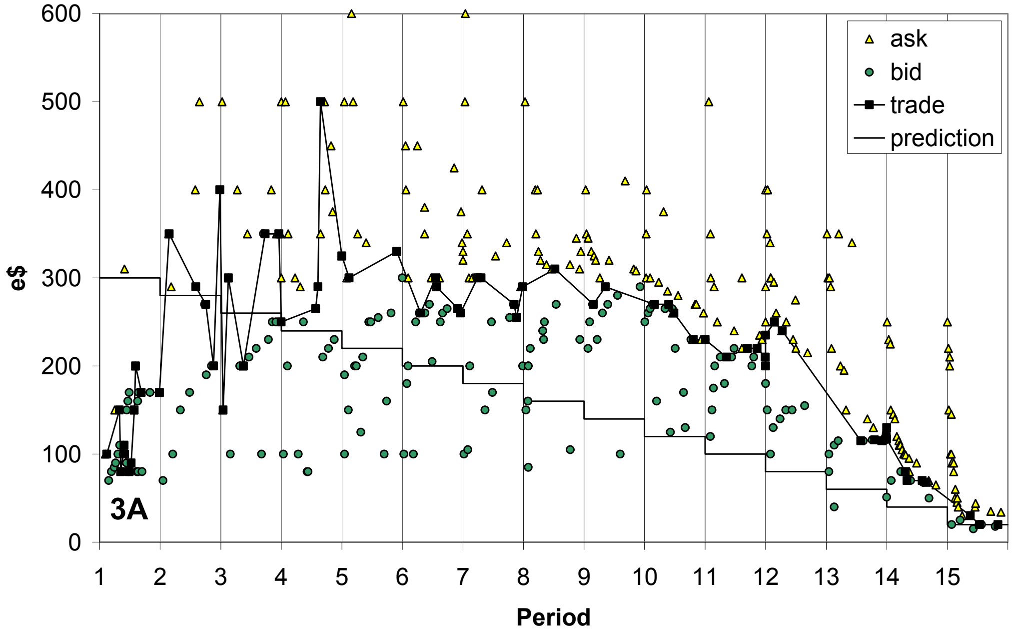

Figure 1: Outcome of asks, bids and transaction prices compared to predicted price path in session 3A (no-fee treatment).

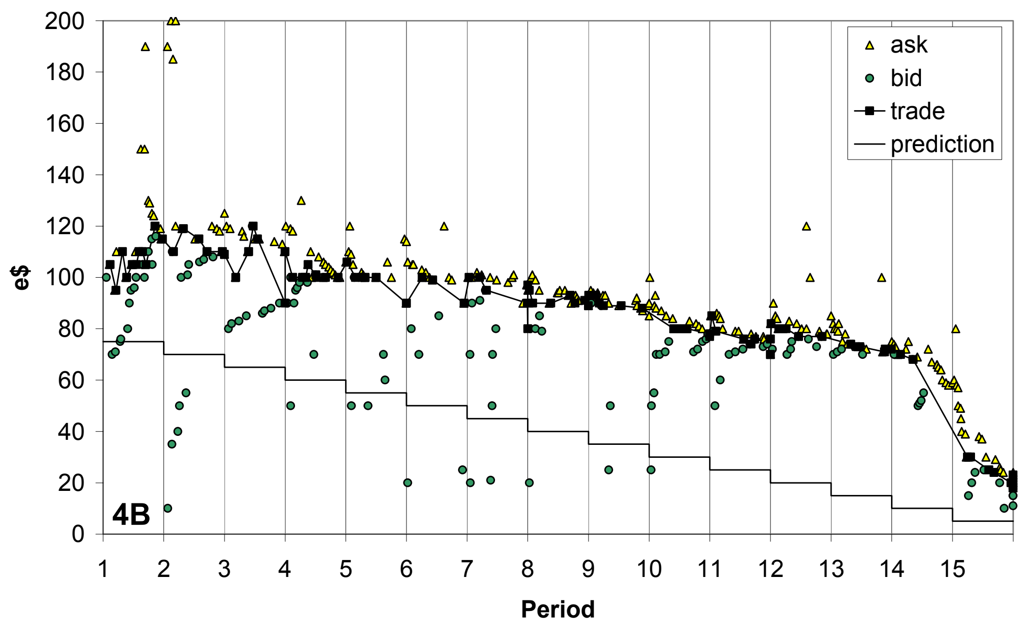

Figure 2: Outcome of asks, bids and transaction prices compared to predicted price path in session 4B (tax treatment)

In seven instances price development was comparable to the data from experiment 6C, shown in Figure 3. What is characteristic of this development is a relatively *stable price*, apart from the first and last periods. Moreover, the price starts out below the forecast, but in the end it is substantially above it, as the forecast price decreases during the experiment.

Finally, market behavior was very close to *predicted behavior* in six instances. An example of this is shown in Figure 4. This is an experiment with participants taking part for the second time who are considered relatively experienced in the game. The second time the experiment was carried out, the structure was changed so that the number of rounds was reduced to four, and instead, the game was repeated several times. These are the last six experiments (7A to 8C). As mentioned above (Section 1.1), the amounts were also altered slightly so that price forecasts are not completely in agreement with what is depicted in Figures 1 to 3. No qualitative change occurred internally within the treatments. As is clear from Figure 4, the market price development was relatively close to the predicted price in the last two rounds.

The examples are too few and the behavior categorization too rough for any generalizations to be made regarding the likelihood of one behavior or another. In almost all treatments, there are examples of a particular behavior. The exception is Treatment A which at no time shows a steady overprice. On the other hand, there are more examples of price bubbles in Treatment A than in other treatments. The difference between these two categories of behavior is however not great between treatments and therefore it is possible to hypothesise that the treatment as such – i.e. whether and how fees are imposed on utilization rights – does not have a substantial effect on what sort of price behavior is to be expected.

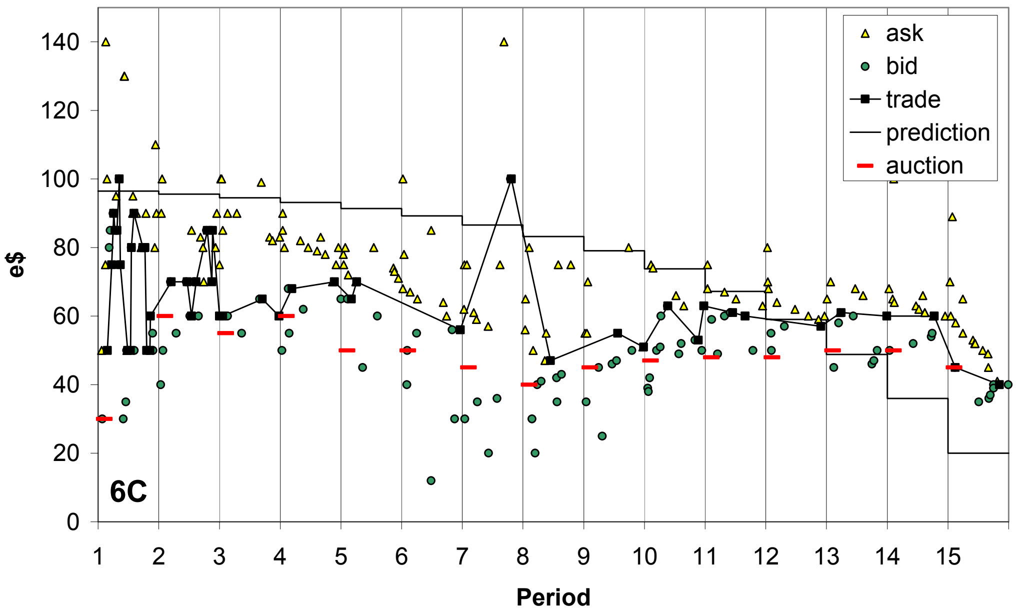

Figure 3: Outcome of asks, bids and transaction prices compared to predicted price path in session 6C (auction treatment)

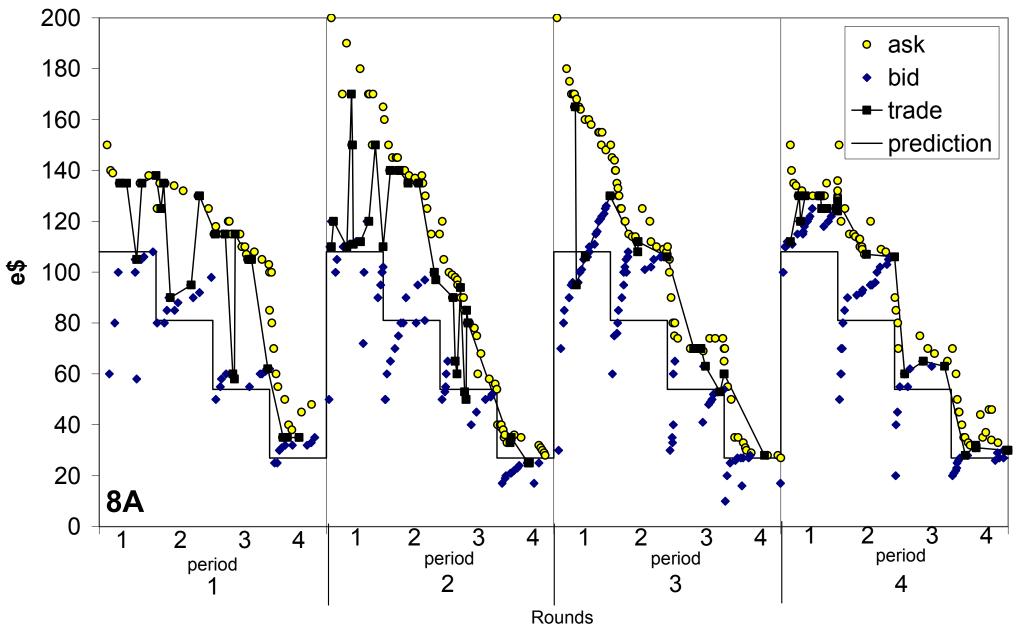

Figure 4: Outcome of asks, bids and transaction prices compared to predicted price path in session 8A (no-fee treatment, experienced subjects)

In addition to price development and price forecasts, Figures 1 to 4 depict selling and buying bids made in each session in chronological order (read from left to right). Figure 3, which shows examples of Treatment C, also shows the price paid for redistributed permits in auction. As is clear in the figure, the auction price was always slightly lower than the market price, which is characteristic for this treatment in the experiment.

3.2 Efficiency development

As discussed above, price development was rather varied from one experiment to another. The same applies to trade and allocation of production permits. In Table 5, the ownership development of production permits in all the experiments can be seen. Note the fact that in order to reach the most efficient position, the participants in roles 1 and 2 had to possess four to five permits, participants in roles 3 and 4 needed to have two to three permits, and participants in roles 5 and 6 zero to one permit. In order to clarify the picture to some extent, green indicates an efficient position, yellow represents a one or two unit deviation and red depicts even greater deviations.

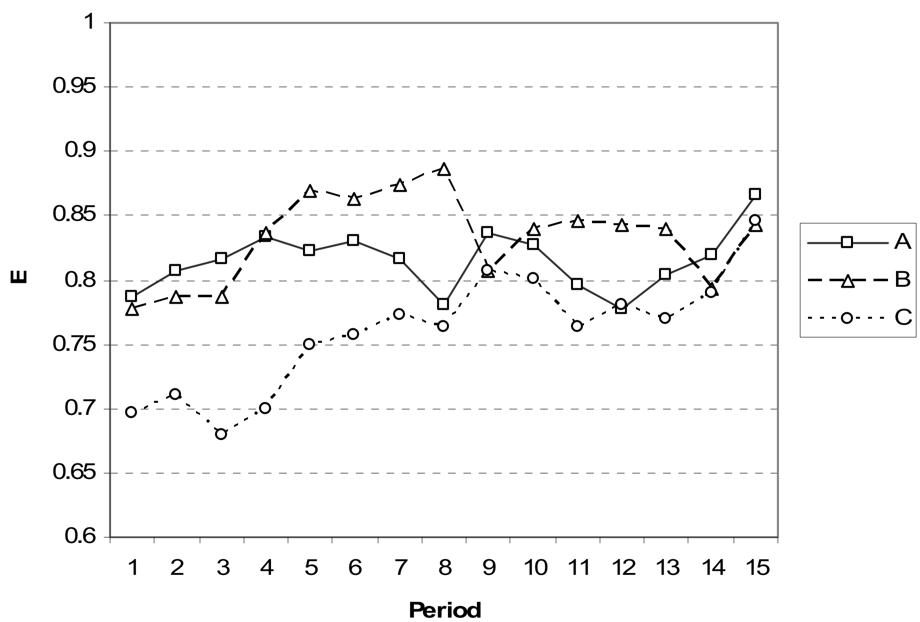

Figure 5: Mean efficiency in each treatment (inexperienced subjects)

Table 5 does not invite easy conclusions. For example, there seems to be no systematic difference between the three treatments of the experiment. Behavior seems to be fairly variable in all instances. It is also non-discernable whether efficiency increases with time or not. If we consider every experiment in each treatment and calculate average efficiency according to (1), it is clear that there is a considerable difference, both between experimental treatments as well as temporal development. Figure 5 depicts the average efficiency in the first 18 experiments (inexperienced subjects).[13] During all the periods, the auction treatment (C) seems to produce the lowest efficiency, but the tax treatment (B) is on average the most efficient. However, efficiency rises steadily in the auction treatment and becomes similar to that in other treatments in rounds 14 and 15. The increase in efficiency over time is much less pronounced in the other two experimental treatments. Figure 6 shows the development of average efficiency for each treatment with experienced participants. Evidently, experience is important and valuable. Efficiency is in general much higher than in Figure 5, and its increase over time is fairly clear. We cannot draw conclusions from the difference between individual experimental treatments in this instance, as there are only two experiments behind each mean number.

[13] Recall that perfect efficiency corresponds to E=1.

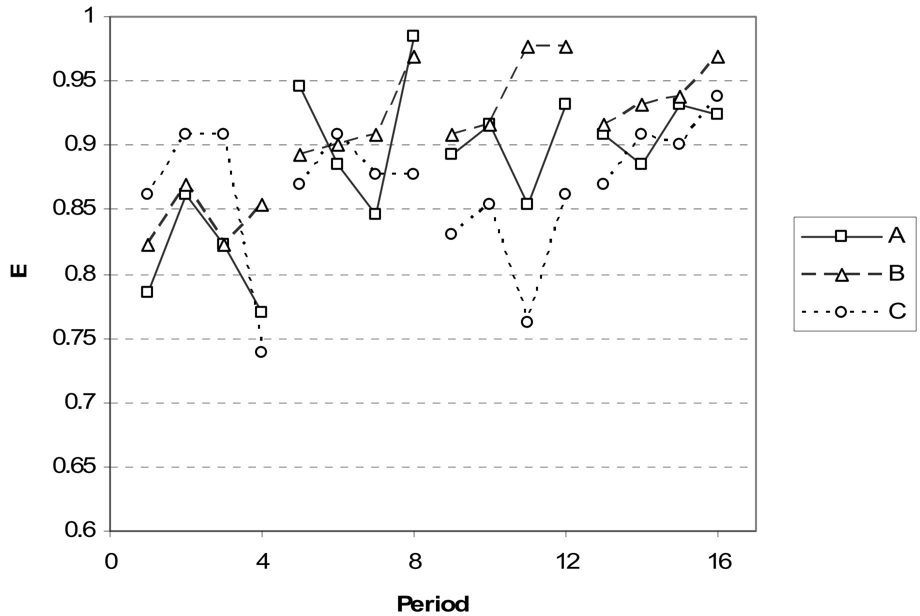

Figure 6: Mean efficiency in each treatment (experienced subjects)

More formally, we may investigate whether there is a significant difference in efficiency between treatments by comparing the distribution of results with a nonparametric test. The results from such tests, on the one hand the Wilcoxon/Mann-Whitney test for the same probability distribution in two treatments at a time, and on the other hand the Kruskal-Wallis test for the same probability distribution in all three treatments at the same time are shown in Table 2. For inexperienced participants, the measurements are divided in three groups of five periods each, but for experienced participants, we test each round separately. It is evident from Table 2, that the null hypothesis (H_0) of a common distribution is rarely rejected in favor of the bilateral hypothesis (H_1) with traditional significance levels (p-value less than 5% or 10%). Treatment C – the auction treatment – differs significantly in the first group of the experiments with inexperienced participants and in the third round with experienced participants.

The above comparisons do not allow us to draw conclusions on the superiority of one arrangement over another in terms of efficiency, even though there are weak indications that the auction treatment, as it is defined in the experiment, is less efficient than both the tax treatment and the baseline (no fee).

Table 2: Significance levels (p-values) in non-parametric tests of equal medians in all treatments

Inexperienced subjects		***period***	
Hypothesis	***1-5***	***6-10***	***11-15***
Equal medians in A and B	0.98	0.15	0.66
Equal medians in A and C	0.00	0.76	0.29
Equal medians in B and C	0.01	0.10	0.19
Equal medians in all treatments*	0.00	0.19	0.36

Experienced subjects		***round***	
Hypothesis	***2***	***3***	***4***
Equal medians in A and B	0.96	0.12	0.32
Equal medians in A and C	0.29	0.03	0.49
Equal medians in B and C	0.16	0.00	0.32
Equal medians in all treatments*	0.32	0.00	0.31

* the Kruskal-Wallis is used here, in other instances the Wilcoxon/Mann-Whitney test is used.

3.3 The interaction between price and efficiency

Although efficiency in the three treatments is mostly (statistically) similar when participants have gained experience, one must not jump to the conclusion that fee imposition does not have an effect on efficiency. Other aspects than efficiency are very variable between individual experiments and can create 'noise' which makes comparison with relatively few measurements potentially difficult. Price development is one of the components which is worthy of special attention in this context.

Price and price expectations can have a great influence on development of trade with production permits. As in other property markets, participants can profit from trade with production permits, on one hand by retaining a certain number of permits and producing and selling in the market, and on the other hand by being resourceful and buying permits at a low price and selling when it is high. Although a price forecast predicts a steady decrease in production permit prices as their remaining utilization period becomes shorter, realized price can equally well go up, at least temporarily. Expected price increases create a possibility for profits from speculation, and if enough participants focus too much on exchange profit from production permits price formation may to a limited extent be connected to fundamentals.

If all participants would think in the same way, a price bubble should, however, not affect the number of production permits they choose to retain at the end of each round, but if the group of participants is diverse, there is a likelihood of deviations occurring from the price forecast and permit ownership. This can for example happen when a participant that has high production costs (roles 5 and 6) expects prices to go up, at least temporarily. If other participants in the same experiment, particularly those with low production costs (roles 1 and 2) do not hope for higher prices, the situation can easily arise that the former retains more permits than is efficient. A similar state of inefficiency can also be brought about if the price is too low, as long as participants' expectations of price formation is heterogenous. Thus, here, we put forward the hypothesis that relative deviation from predicted prices has a negative effect on efficiency. Further to that, the question can be explored whether there is a significant efficiency difference between treatments, when the effects of price deviation has been taken into account.

In order to analyse the effect of price deviation and the organization of fee imposition, it is necessary to use linear regression. The main results from such an analysis, using a linear mixed-effects model, are displayed in Table 3.

In Model 1, we endeavour to explain efficiency in the first 18 experiments, when all participants were new to the project, by experimental variables as well as the absolute value of price deviation from the price forecast. For the sake of parsimony, we assume that the time effect is linear.[14] It appears that price deviation has a markedly negative effect on efficiency, but neither the tax treatment nor the auction treatment seem to have a significant effect. This result should be taken with caution since there is considerable autocorrelation in the equation's residuals. This is taken care of in Model 2, which is the same as Model 1 except that lagged values of efficiency have been added to the equation. With this change, autocorrelation is no longer significant and explanatory power increases. Furthermore, the tax treatment has significantly (at the 5% level) higher efficiency than the baseline treatment; as in Model 1 the auction treatment has lower efficiency than the baseline, but the difference is not statistically significant.

Model 3 is comparable to Model 1, but it uses data from experiments with experienced subjects. The only difference between the estimated equations as such is the addition of the variable *round*, which along with *period*, captures the effect of repetition. In this case, there are relatively few available observations, or 72. With that proviso, all coefficients of the equation come out as significant apart from the *round* effect. Treatment effects have the same sign as in Models 1 and 2 and are significant at the 5% level.

These results indicate that, as expected, price deviation from forecast price has a negative effect on efficiency of allocation of permits between participants. Furthermore, after controlling for the effects of price deviation, fee imposition on production permits has an effect on allocative efficiency. In particular, the tax treatment has significantly higher efficiency than the baseline, as opposed to the auction treatment, which has significantly lower efficiency. It is probable that the less pronounced results in Models 1 and 2 result from the participants' inexperience of the game, leading to irregular behavior that is difficult to interpret. When participants are more experienced, the effects of the fee imposition are to a large extent clear. Thus, it seems safe to reject the null

[14] A more general definition of time effect does not significantly affect the results.

hypothesis that fee imposition or its implementation does not affect efficiency. Montgomery's theorem therefore appears not to hold in our setting.

Table 3: Regression results for efficiency in markets with inexperienced and experienced subjects (linear mixed-effects model)

	Model 1 (Inexperienced)	***Model 2 (Inexperienced)***	***Model 3 (Experienced)***
Constant	0.783***	0.185***	0.879***
	(0.04)	(0.04)	(0.03)
Period	0.005***	0.002	0.013**
	(0.001)	(0.001)	(0.008)
Round			0.003
			(0.01)
Price deviation	- 0.023***	- 0.019***	- 0.052**
	(0.006)	(0.005)	(0.022)
Efficiency (-1)		0.723***	
		(0.04)	
Tax	0.042	0.027**	0.038**
	(0.05)	(0.01)	(0.02)
Auction	- 0.054	- 0.01	- 0.032**
	(0.05)	(0.01)	(0.02)
R^2	0.10	0.61	0.29
R^2 *(adj.)*	0.09	0.60	0.24
1º autocorr., ρ	0.73***	-0.016	-
No. obs.	270	252	72
Cross sections	18	18	6

Cross sectional parameters not shown. *) Significant at the 10% level; **) significant at the 5% level; ***) significant at the 1% level. Standard errors shown in parantheses.

3.4 Underlying reasons for the effects of fee imposition

The theory that fee imposition or its implementation does not matter to allocative efficiency builds on neo-classical principles concerning maximization of present value of profit as well as an assumption of rational expectations. There are several possible reasons why the theory fails. One possibility is that of endowment effects, i.e. that participants consider their original allocation an important point of reference to a normal situation, independent from financial incentive, and are thus ready to sacrifice money in order to maintain a situation which they think is normal and right.[15] These can clearly reduce efficiency, as other objectives than the maximization of profit are partly dominating. The combined results of endowment effects and fee imposition on trade with production permits are complex and not easily predictable, and different methods of fee imposition may well have an effect.

Independent from the question whether the endowment effect is present or not, there is the possibility that participants will not equate 'real' (paid) cost and opportunity cost. In this context, the opportunity cost of production permit ownership rests in its market value. Individuals' attitude to opportunity cost can vary, especially considering that participants may have different expectations of permit price development and that they have very different production possibilities.

Fee imposition increases direct cost and thus reduces profits, but simultaneously, opportunity cost decreases, because the production permit price is also reduced. These influences do not appear in the exact same way in the tax and auction treatments. In the former, the tax has the effect that the profitability of inefficient units (the shaded cells in Table 1) becomes negative. A large part of the opportunity cost of retaining permits is thus changed to paid cost with that method of fee imposition. In each round of the auction treatment, some participants lose a permit and in order to reach their former position, they need to buy the permits back, either in an auction market or in an after-market. The tax sum and the depreciation ratio is arranged so that expected fee imposition is equal between treatments. Apart from how the fee imposition is organised, there should not be a great difference between the two methods (i.e. Treatments B and C) with regards to income transfer. Effects of presentation of information are, however, often important in market

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[15] See e.g. Kahneman et al. (1991).

experiments and may play a part here. The chief difference in implementation between tax and auction treatments – i.e. in presentation of fees – is that in the tax treatment, participants that had some permits needed to make a decision about whether they were going to sell one or more permit or not, while in the auction treatment, the participants needed to decide how much they were ready to spend in order to acquire more permits or reclaim permits which they had lost before.

There is also a possibility that participants' uncertainty regarding permits loss because of withdrawal in auction sessions had an effect. Recall, however, from Section 2.1 that irregular income effects due to randomized withdrawal were evened out by transfers, so that the financial implications for each participant were close to 20% reduction of the value of individual ownership of production permits. Of course the possibility cannot be excluded that some participants did not fully realize the total implications of permit depreciation and transfers. Yet, informal interviews with participants at the end of auction sessions did not indicate an appreciable lack of comprehension of this mechanism.

In order to study the importance of such effects for individual behavior in our experiment, an ordered probit model was estimated, in which the dependent variable is the number of permits that the participant in question owned at the end of a particular round. As stated above, each participant could have from zero to five permits. The model estimates the likelihood that a certain number of permits is chosen using particular exogenous variables. In Model 4 in Table 4, the initial allocation (the variable *initial*), that is to say the number of permits allocated in the beginning of each session, and the production opportunities of participants are used as explanatory variables. Production opportunities are described by two dummy variables: the variable *efficient* takes the value 1 when the participant has role 1 or 2 in Table 1, i.e. has relatively low production costs, but otherwise the value 0; and the variable *inefficient* takes the value 1 when the participant has role 5 or 6 in Table 1, i.e. has relatively high costs; otherwise the variable takes the value 0. In addition, variables are included to capture development over rounds. Only the last five rounds are used for inexperienced participants and the last two rounds for experienced ones, when it can be assumed that initial adaptation has taken place.

Table 4: Ordered probit models for individual permit holdings

	Model 4		***Model 5***	
	Inexperienced	***Experienced***	***Inexperienced***	***Experienced***
Period	- 0.002	0.013	- 0.003	0.016
	(0.04)	(0.14)	(0.04)	(0.14)
Round		0.006		0.005
		(0.09)		(0.09)
Initial	- 0.031	0.034	- 0.032	0.034
	(0.02)	(0.03)	(0.02)	(0.03)
Efficient	0.033	1.120**	- 0.003	1.406**
	(0.14)	(0.19)	(0.23)	(0.31)
Ineffcient	- 0.515***	- 1.870**	- 0.355	- 2.215**
	(0.14)	(0.20)	(0.23)	(0.33)
Tax			0.010	0.062
			(0.23)	(0.30)
Auction			0.166	0.224
			(0.25)	(0.30)
Efficient and Tax			0.475	- 0.082
			(0.33)	(0.43)
Efficient and Auction			- 0.485	- 0.716*
			(0.35)	(0.43)
Inefficient and Tax			- 0.463	0.141
			(0.33)	(0.43)
Inefficient and Auction			- 0.018	0.821*
			(0.35)	(0.44)
LR index (pseudo R^2)	0.02	0.13	0.03	0.13

*) Significant at the 10% level, **) significant at the 5% level, ***) significant at the 1% level.

Production opportunities appear to have a significant effect on the number of production permits in Model 4, especially among experienced participants. In particular, experienced participants that have efficient production opportunities – have low production costs – are likelier to retain more permits (the coefficient at *efficient* is positive) and those that have less efficient production opportunities – high production costs – are likelier to retain fewer permits (the coefficient at *inefficient* is negative) in comparison to those that have average costs. Thus relative efficiency certainly pushes participants in the "right" direction.

Endowment effects appear not to be important (the parameter at the variable *initial* is not significantly different from zero). This agrees with recent studies in the area (List, 2004), which demonstrate that endowment effects are only significant when participants are relatively inexperienced and that indications of such an effect usually disappear when participants have gained experience and competence.

Model 5 in Table 4 has added dummy variables for Treatments B (*tax*) and C (*auction*) and cross-variables between treatments and production possibility dummies. This has the purpose of analysing if the treatment has an effect on the number of permits that participants in different roles choose. The production possibilities are still key explanatory variables and the only ones that turn out to be significant at a 5% significance level. In the case of experienced participants, there also turns out to be a significant difference (at 10% level) in participants' behavior according to the roles in the auction treatment compared to the basic treatment. The effects indicate that participants with efficient production possibilities retain fewer permits in the auction treatment than in other treatments. The behavior of those that have average costs is not significantly different across treatments. Note, however, that the cross effects in the tax treatment have the same sign as those in the auction treatment, but are of a lower magnitude and are far from being significant. This is not in full harmony with the results in Table 3, where the tax treatment had a significantly positive effect on efficiency – based on those results we would have expected reverse signs of the tax-efficiency interactions as compared to the auction treatment. We should, however, not read too much into this result, which may simply be an indication that the ordered probit model is too restrictive for the data.

4 Conclusion

This article has sought to answer the question whether the imposition of fees on previously allocated utilization permits and the way such fees are implemented can have an effect on efficiency. The underlying idea is that fee imposition may accelerate the process of inefficiently run companies reducing production, thus giving more efficient companies room to expand. According to traditional neo-classical principles, fee imposition should not affect either the final allocation of utilization permits or efficiency: companies which have more efficient operation should be prepared to pay more for the utilization permits than ones that are run less efficiently. Inefficient companies could not profit as much with the full utilization of their permits as if they sold them. Thus, there should be a strong tendency to efficiently distribute the utilization permits, whether or not there is any tax imposed on them.

The experiment described here is intended to explore if Montgomery's theorem is upheld in the laboratory. The results show that there is a strong likelihood that fee imposition matters from an efficiency perspective. Furthermore, the method of collecting the fee seems to matter considerably. While taxation on utilization permits seems to be able to increase efficiency, there is a strong indication that temporary allocation and re-allocation by auction reduces efficiency rather than enhancing it.

The experiment is also set up so that this behavior cannot be explained with transaction costs, demand effects or uncertainty.[16] An analysis of individual behavior also demonstrates that the effects of original allocation are not significant. Insufficient experience does not either seem to be a likely explanation, and the above results are even clearer in experiments in which participants took part for the second time.

The explanation which remains and must be considered the most probable one is that participants differentiate between paid cost and opportunity cost, contrary to what neo-classical theories maintain. In addition, the presentation of fee imposition seems to matter in this context. When a tax is imposed on permits, the choices of inefficient firms are quite important, and the decision primarily revolves around the question of whether to retain a particular quantity of permits or reduce their numbers. When permits are withdrawn and resold to the highest bidder, the decision is of a different kind and concerns all the participants, especially those that have the most efficient production possibilities. In addition, the decision relies on different principles, as it mainly revolves around whether or not the company wishes to acquire more permits. Such presentation effects can matter dramatically in experiments as well as in real situations.

It cannot be determined with certainty to what extent these results can be applied to real markets. The possibility cannot be ruled out that participants in the experiment had not gained enough experience in the short time at disposal such that their decisions can be compared to the decisions of real firms. However, the fact that increased experience of participants rather supported the results would seem to weaken the force of this argument and strengthen our conclusions.

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[16] Here, we mean that price developments in markets for utilization permits can have an effect on technical development by reducing the demand for permits, e.g. by better pollution prevention equipment. It has also been argued that uncertainty in property rights is created by withdrawal and that has negative effects, e.g. incentive for innovation and investment and how the resources are treated.

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Table 5: Allocation of permits in the production phase in each period

Treatment A (no fee)

Session	Role	period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	1	2	3	3	4	5	4	5	3	5	4	3	4	4	2	3
	2	3	1	3	2	2	2	2	2	2	2	4	3	3	2	2
	3	2	3	2	3	2	2	1	3	2	3	2	1	0	0	2
	4	5	3	4	4	4	4	4	4	4	4	4	4	4	5	5
	5	1	3	1	1	1	1	1	3	2	2	2	2	3	5	3
	6	2	2	2	1	1	2	2	0	0	0	0	1	1	1	0
2	1	0	2	1	1	0	1	0	1	1	0	0	2	3	4	4
	2	4	3	3	4	3	3	3	3	3	3	4	3	2	2	2
	3	1	1	2	2	2	2	3	3	3	3	3	3	2	2	2
	4	2	1	1	1	3	2	1	1	2	3	2	2	2	1	2
	5	4	3	4	3	3	3	3	3	3	2	2	1	1	1	0
	6	4	5	4	4	4	4	5	4	3	4	4	4	5	5	5
3	1	5	5	5	4	4	4	3	3	2	1	1	0	2	2	2
	2	5	5	4	4	4	4	3	3	3	3	1	0	1	3	4
	3	1	0	1	1	0	2	2	3	4	5	5	5	4	2	0
	4	3	4	3	4	3	3	5	5	5	1	3	3	2	1	3
	5	0	0	0	0	1	1	2	1	1	2	3	3	2	2	2
	6	1	1	2	2	3	1	0	0	0	3	2	4	4	5	4
4	1	4	2	3	3	3	4	4	3	4	3	3	3	5	5	5
	2	3	5	3	3	3	3	3	2	3	3	3	4	2	2	2
	3	1	2	2	2	4	4	4	4	3	4	5	5	5	4	4
	4	5	4	3	3	2	2	2	2	2	2	1	1	1	2	2
	5	0	0	3	3	2	2	2	4	3	3	3	2	1	1	1
	6	2	2	1	1	1	0	0	0	0	0	0	0	1	1	1
5	1	4	4	4	4	4	5	5	5	4	4	4	4	4	4	4
	2	5	4	5	5	5	5	4	5	5	4	4	4	5	5	4
	3	0	1	1	0	1	0	2	2	3	5	5	5	3	5	4
	4	0	0	0	0	0	0	0	0	0	0	0	0	1	0	3
	5	3	3	2	2	2	1	1	1	1	0	0	1	1	0	0
	6	3	3	3	4	3	4	3	2	2	2	2	1	1	1	0
6	1	2	3	2	3	4	4	5	5	5	4	4	3	3	2	5
	2	3	2	3	3	2	2	1	1	1	2	2	1	1	4	3
	3	3	4	3	3	3	3	2	2	2	1	1	0	1	2	3
	4	1	1	1	1	1	1	2	3	3	3	3	5	5	5	3
	5	2	0	2	1	1	2	2	2	2	3	3	4	3	2	1
	6	4	5	4	4	4	3	3	2	2	2	2	2	2	0	0

Treatment B (tax)

Session	Role	period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	1	2	2	2	4	4	5	4	5	5	3	5	5	4	3	3
	2	3	4	3	3	2	2	2	2	1	2	1	3	1	2	2
	3	3	2	4	5	3	4	2	2	5	3	2	1	2	2	2
	4	1	3	2	2	4	2	3	4	1	5	3	4	2	2	2
	5	2	1	1	0	1	1	3	1	2	1	3	1	5	5	5
	6	4	3	3	1	1	1	1	1	1	1	1	1	1	1	1
2	1	4	4	3	4	4	4	3	4	4	3	4	4	3	1	3
	2	4	3	3	4	3	3	3	4	3	3	3	3	5	5	4
	3	0	2	3	1	1	2	3	3	3	3	2	3	2	2	2
	4	5	4	5	4	4	3	2	1	0	3	1	1	0	3	3
	5	0	0	1	0	0	1	2	1	1	0	2	1	1	2	3
	6	2	2	0	2	3	2	2	2	4	3	3	3	4	2	0
3	1	1	2	3	2	2	3	3	3	3	3	3	3	3	3	4
	2	3	4	4	4	5	5	5	5	5	5	5	5	5	5	5
	3	2	2	2	4	3	3	3	3	3	3	3	3	3	3	3
	4	2	2	2	3	3	2	2	2	2	2	2	2	2	2	2
	5	4	2	3	1	2	1	1	1	1	1	1	1	1	1	1
	6	3	3	1	1	0	1	1	1	1	1	1	1	1	1	0
4	1	3	4	4	5	5	5	5	5	5	4	4	4	3	5	3
	2	3	2	1	2	1	0	2	0	1	0	0	2	1	1	4
	3	3	2	4	2	2	5	3	5	3	5	5	3	5	5	2
	4	3	5	4	3	5	4	4	4	4	4	3	4	4	3	5
	5	2	2	2	2	2	1	1	1	2	1	1	1	1	0	0
	6	1	0	0	1	0	0	0	0	0	1	2	1	1	1	1
5	1	0	0	0	2	3	3	5	5	5	5	4	2	3	1	1
	2	4	4	2	1	2	1	1	2	1	2	1	3	2	1	1
	3	1	2	4	5	3	3	3	1	1	0	2	1	4	5	5
	4	4	5	5	4	3	2	2	3	4	5	3	3	2	3	3
	5	1	0	0	0	1	1	0	1	0	0	1	1	1	2	2
	6	5	4	4	3	3	5	4	3	4	3	4	5	3	3	3
6	1	3	3	3	3	4	5	5	5	4	4	3	4	4	3	3
	2	4	4	3	4	4	4	4	4	2	3	3	3	3	3	3
	3	3	3	2	2	2	2	2	2	2	2	2	2	2	1	1
	4	4	3	2	4	4	3	3	3	2	2	3	3	3	3	3
	5	0	1	4	1	1	0	1	0	5	3	3	2	2	5	5
	6	1	1	1	1	0	1	0	1	0	1	1	1	1	0	0

Treatment C (auction)

Session	Role	period 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	1	1	0	0	1	2	3	2	2	1	2	2	1	2	2	2
	2	3	2	3	1	0	0	2	2	3	1	0	2	2	2	3
	3	1	3	3	4	5	2	3	4	5	4	5	4	4	3	2
	4	3	3	2	2	1	2	2	1	1	3	3	3	2	2	2
	5	3	3	3	4	4	4	3	3	2	1	1	2	3	3	5
	6	4	4	4	3	3	4	3	3	3	4	4	3	2	3	1
2	1	2	2	2	4	4	4	4	2	3	2	3	2	3	2	5
	2	4	3	3	3	5	5	5	4	4	4	4	5	4	4	4
	3	1	1	1	0	0	0	0	3	3	2	3	2	3	5	2
	4	4	5	3	4	2	3	3	2	0	3	0	2	2	3	4
	5	4	2	3	3	3	3	3	3	3	1	3	1	0	0	0
	6	0	2	3	1	1	0	0	1	2	3	2	3	3	1	0
3	1	1	0	0	1	3	3	3	1	3	2	2	2	1	2	4
	2	0	0	0	0	0	0	0	0	0	1	0	0	1	1	0
	3	0	3	1	1	1	1	1	3	1	1	2	2	2	3	2
	4	4	3	5	5	3	3	3	3	5	4	3	4	4	4	4
	5	5	5	5	5	5	4	3	4	1	2	3	3	3	3	5
	6	5	4	4	3	3	4	5	4	5	5	5	4	4	2	0
4	1	2	2	3	4	4	3	4	4	4	4	3	2	2	1	5
	2	4	4	3	4	5	4	4	3	3	2	3	3	5	3	5
	3	5	4	4	3	3	2	1	2	1	2	1	1	0	3	2
	4	2	3	1	0	0	3	3	2	2	2	2	2	2	3	3
	5	0	1	3	1	0	2	1	2	2	2	2	4	5	5	0
	6	2	1	1	3	3	1	2	2	3	3	4	3	1	0	0
5	1	3	3	3	1	4	2	3	4	3	3	4	1	3	4	2
	2	1	2	1	4	3	3	3	3	5	3	5	4	1	1	3
	3	2	2	2	3	2	3	3	2	3	4	3	4	3	2	2
	4	4	4	3	2	3	5	3	4	3	4	1	3	3	3	5
	5	4	2	4	1	0	1	1	0	1	1	2	1	0	0	0
	6	1	2	2	4	3	1	2	2	0	0	0	2	5	5	3
6	1	3	4	4	4	3	4	3	3	3	2	2	3	3	3	2
	2	2	0	0	0	0	0	0	1	4	4	3	3	4	3	3
	3	2	5	5	5	4	3	5	5	4	4	4	3	3	4	5
	4	2	0	0	1	0	0	0	0	0	0	0	0	0	0	1
	5	4	4	4	3	4	5	4	4	3	4	4	4	4	3	4
	6	2	2	2	2	4	3	3	2	1	1	2	2	1	2	0

Treatment A (no fee) — round / period

Role	Round 1				Round 2				Round 3			
	1	2	3	4	1	2	3	4	1	2	3	4
1	3	2	3	4	3	4	3	4	1	4	4	4
2	3	3	4	4	4	4	4	4	4	3	3	3
3	2	3	2	2	2	2	3	2	2	1	2	1
4	2	2	1	2	3	1	0	2	3	3	2	3
5	1	0	1	0	3	3	4	3	2	1	0	0
6	4	5	4	3	0	1	1	0	3	3	4	4
1	5	4	3	4	2	3	4	4	4	2	5	4
2	4	4	4	4	3	5	4	5	4	4	4	5
3	3	3	3	3	1	1	2	2	3	4	3	4
4	2	2	2	3	5	3	3	3	2	2	2	2
5	0	2	2	1	0	0	0	0	2	3	1	0
6	1	0	1	0	4	3	2	1	0	0	0	0

Treatment B (tax) — round / period

Role	Round 1				Round 2				Round 3			
	1	2	3	4	1	2	3	4	1	2	3	4
1	2	3	3	3	3	4	5	4	4	4	3	4
2	3	3	3	5	2	4	5	5	1	1	2	2
3	3	3	2	1	1	1	1	2	1	3	5	4
4	1	1	2	4	5	2	1	2	3	2	2	3
5	3	3	3	0	2	1	1	1	3	3	2	1
6	3	2	2	2	2	3	2	1	3	2	1	1
1	3	3	4	5	2	4	4	5	4	4	4	4
2	4	3	3	4	5	4	4	3	4	4	4	4
3	1	1	1	2	3	2	3	2	2	2	2	2
4	2	2	1	2	3	3	2	2	4	4	4	3
5	2	3	3	1	1	1	1	0	0	0	0	1
6	3	3	3	1	1	1	1	3	1	1	1	1

Treatment C (auction) — round / period

Role	Round 1				Round 2				Round 3			
	1	2	3	4	1	2	3	4	1	2	3	4
1	3	4	3	4	0	0	0	4	2	3	4	5
2	5	4	4	4	5	5	4	3	5	5	5	4
3	2	4	4	5	2	2	2	1	3	3	3	3
4	3	2	3	2	4	4	4	5	2	2	0	2
5	1	1	0	0	1	2	3	0	0	0	1	1
6	1	0	1	0	3	2	2	2	3	2	2	0
1	3	3	2	1	2	3	2	2	4	3	4	3
2	2	4	3	5	2	2	3	4	3	3	4	3
3	2	2	3	1	2	2	2	2	0	2	0	2
4	4	2	3	4	4	3	3	2	3	2	2	2
5	3	3	4	4	3	3	4	5	1	1	0	0
6	1	1	0	0	2	2	1	0	4	4	5	5

Green color indicates efficient holding of permits, yellow indicates a deviation of one or two units from the efficient level and red indicates a greater deviation from effic