TI 2023-046/VII
Tinbergen Institute Discussion Paper

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A Wind Tunnel Test of Wind Farm Auctions*

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July 19, 2023

Abstract

Globally, governments increasingly rely on auctions to advance renewable energy. This paper studies the design of wind farm auctions and evaluates the impact of price guarantees and subsidies on auction efficiency, government revenue, and renewable-energy production. While the theoretical analysis suggests that the price guarantee has no effect, our laboratory experiment suggests that the price guarantee improves efficiency and that it often increases production and revenue. An important explanation for these results is that less risk averse subjects tend to bid less aggressively and produce less. Without the price guarantee, and hence with more uncertainty in the auction, this increases the chances that risk-loving bidders win the auction, thus compromising auction efficiency. The subsidy is less effective than suggested by theory. Bidders with a higher valuation tend to bid more conservatively than the equilibrium prediction, thus neutralizing the efficiency-enhancing effect of the subsidy.

*The authors thank Adriaan Soetevent, seminar participants at the University of Amsterdam, the INFORMS virtual meeting 2020, the ESA global around-the-clock meeting 2021, and the EARIE conference 2022 for useful comments on earlier drafts. Funding from GrEELab at the University of Groningen and the ASF initiative of the University of Amsterdam is also gratefully acknowledged. Xinyu Li gratefully received funding as part of the NWO-Dinalog project 'Sustainable Service Logistics for Offshore Wind Farms' (grant number 438-13-216).

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1 Introduction

Around the world, governments have been actively working to encourage the adoption of renewable energy through straightforward, transparent, and effective policies. One commonly employed strategy is the use of auctions, for example, to allocate permits to build wind farms. In practice, auctions to award wind farm permits greatly vary in format as well as effectiveness, which raises the question as to what is the best way to design them. In this paper, we address this question. In particular, we study the role of price guarantees and subsidies, in a theoretical analysis as well as a laboratory experiment.

Wind farm auctions have become increasingly important in the past few years. Jansen et al. (2022) report the following telling statistics. Until 2017, 26 auctions were held for 41 wind farms with 16.8 GigaWatt (GW) of capacity, mostly in Europe. Between 2018 and 2021, 32 auctions for 76 wind farms and 36.6 GW of capacity took place. As of 2021, only 24% of all installed capacity was auctioned, but this share is expected to rise to 97% by 2030. At least 52 auctions were announced for the post-2021 period. There were 37 auctions planned for 2022 and beyond with 63.6 GW of capacity. Policy targets indicate that a total of 224 GW of offshore wind will be realised by 2030.

Many formats for wind farm auctions have been used. In the US, the Bureau of Ocean Energy Management routinely auctions wind farms to the highest bidder, while the UK has widely used the Contracts for Difference (CfD) auction which determines the unit price the winning renewable-energy producers receive per unit of energy.[1] In Germany, bidding takes place on price floors for the electricity generated.[2] In 2021, the UK introduced a new auction format where owners of offshore wind farms pay an upfront fee and a share of future revenue for the right to develop offshore wind. Jansen et al. (2022) also report that the auction designs differ in terms of the contract the auction winner signs with the government, which may or may not include feed-

[1] https://www.gov.uk/government/publications/contracts-for-difference/contract-for-difference

[2] See the German law "Renewable Energy Act | EEG (2014)".

in tariffs, one-sided and two-sided CfDs, mandated power purchase agreements, and mandated renewable energy certificates.

The design of wind farm auctions, including the corresponding contracts, matters as witnessed by suboptimal auction performance in the past. For example, in a recent German auction, multiple zero-subsidy bids were submitted, so that winners were randomly chosen, shedding doubt on the efficiency of this auction.[3] Moreover, there are concerns about whether wind farms can be installed and operated profitably at the bidding price, shedding doubts on whether they will be realized (see, e.g., Matthäus, 2020).

Indeed, properly designing wind farm auctions is challenging. Contrary to what is commonly assumed in auction theory, bidders face an environment that has both common-value elements (e.g. bidders are uncertain about the costs of building a wind farm) as well as private-value elements (e.g. production efficiency may differ between firms). On top of that, the future price of electricity is unknown, adding further uncertainty to the revenue of the winner. Moreover, the winner has to invest before this uncertainty is resolved. Also, given the huge sums that are at stake, risk aversion of bidders may be an issue. An additional complication is that governments organizing wind farm auctions typically aim at balancing multiple objectives including auction efficiency, the amount of renewable energy produced, and revenue. It may also want to avoid the winner falling prey to the winner's curse, i.e., making losses after winning the auction – which may result in the wind farm not being built in the first place.

In this paper, we study an auction setting that captures the most important elements of real-world wind farm auctions: a combination of common and private value elements, additional uncertainty about future electricity prices, and investment decisions that have to be made before the auction. In that setting, we study the effect of two instruments that can be part of the auction design: a price guarantee and a subsidy. We explore the extent to which these instruments enhance policy desiderata such as efficiency, renewable energy production, revenue, and avoiding the winner's curse.

[3] https://www.windpowermonthly.com/article/1726902/rwe-edf-win-zero-subsidy-bids-german-offshore-wind-te

We model the price guarantee as a CfD: the auction winner receives a predetermined fixed price per unit of electricity produced. If the actual price is lower than the fixed price, the producer receives the difference from the government. If it is higher, it pays the difference to the government. The subsidy is provided per unit of electricity produced. The subsidy incentivizes the winning producer to build the wind farm without delay and to operate and maintain it properly over its expected lifetime (15-25 years). This set-up is relevant even if wind energy is profitable without government subsidy, as is likely in the near future (Kreiss et al., 2017; Jansen et al., 2020).[4]

We study the effects of these instruments using a laboratory experiment. Participants compete in the first-price sealed-bid auction. The winner builds the wind farm and sets the quantity of renewable energy. Firms differ in their productivity, which is private information. The costs of building a wind farm are unknown but equal for all firms. Before the auction, each firm receives a signal of the true costs. The auction thus has a private value element (determined by each firm's productivity) and a common value element (the common costs of building the wind farm). Moreover, there is uncertainty about the future price of electricity.

In the experiment, we use a 2x2 between-subjects design. We vary whether the winner obtains a price guarantee, and whether the winner receives a subsidy per unit of quantity produced. This allows us to test two hypotheses that follow from our theoretical analysis. Assuming risk neutrality, our first hypothesis is that the price guarantee does not affect auction efficiency, production, net auction revenue, or government surplus (the weighted sum of production and net auction revenue). Our second hypothesis is that the subsidy has a positive impact on auction efficiency, production, as well as net auction revenue, and, for a sufficiently large shadow price of wind energy, on government surplus. The subsidy benefits the most productive firms most, and hence makes the private value element more important relative to the common value element. This positively affects the variables of interest.

[4]Indeed, bidders in recent auctions have revealed that they expect to be able to build and run wind farms without a subsidy. In 2018, the US generated $400mln by auctioning off the rights to develop offshore wind. And, as noted, in the recent German auctions, bidders submitted a zero bid.

Our experimental results are as follows. Contrary to our predictions, we find that the price guarantee increases auction efficiency. It also positively affects the production of renewable energy and auction revenue — though these effects are only significant if the subsidy is also in place. For a sufficiently high shadow price of wind energy, the price guarantee has a positive effect on government surplus. Inconsistent with our second hypothesis, the subsidy does not affect auction efficiency, nor does it boost auction revenue. It does increase the production of renewable energy and positively affects government surplus for a sufficiently large shadow price of wind energy. The winner's curse is not prominent in any of the treatments.

Zooming in on individual behavior allows us to explain why we have to partly reject our hypotheses. We find that decisions are more noisy if there is no price guarantee in place. Also, subjects bid less aggressively and produce less the more risk averse they are. This increases the probability that the least risk averse bidders wins the auction rather than the most productive, thus compromising auction efficiency, production, and revenue. Moreover, the theoretical prediction of the subsidy increasing auction revenue is voided by the fact that more productive bidders tend to bid more conservatively relative to the equilibrium prediction.

Our study speaks to the experimental literatures on auctions and incentive contracts. Most auction experiments explore pure private-values or pure common-value settings. In pure private-value auctions, bidding in first-price auctions is typically consistent with risk-averse preferences rather than risk-neutral preferences (e.g., Cox et al., 1982). A common finding in the common-value literature is that in the first-price sealed-bid auction, a substantial fraction of the bidders falls prey to the winner's curse (e.g., Kagel and Levin, 1986). Our experimental setting is based on Goeree and Offerman's (2003) model of auctions where the object possesses both private and common value elements. In an experimental test of equilibrium bidding in this model, Goeree and Offerman (2002) find that the observed efficiency levels in the first-price sealed-bid auction are close to the theoretical predictions, despite the fact that some participants fall prey to the winner's curse.

In a laboratory experiment about piece-rate incentives, Bull et al. (1987) observe that, after some learning, participants choose effort levels close to the theoretically optimal levels. In the context of procurement auctions, Bichler (2000) and Chen-Ritzo et al. (2005) point to the risk of the winner underperforming after the auction. Our set-up attenuates the risk of underperformance as the winner's earnings depend on her effort. Our paper also adds to the experimental literature on 'wind tunnel' testing of theoretically promising auction designs. The experiments run in preparation for FCC spectrum auctions in the 1990s and 2000s are prominent examples of such wind tunnel tests (McMillan, 1994; Goeree and Holt, 2010).

The remainder of this paper is structured as follows. Section 2 presents the experimental design and hypotheses. We describe the game that we study, derive equilibrium strategies, and use that to formulate the hypotheses for our experiments. Section 3 reports on the main results of our experiment, in terms of the performance criteria we are interested in. In Section 4 we disentangle the difference between theory and experiment into three main effects. In section 5 we take a closer look at individual behavior, to try to understand the results we found in Section 3 – especially how these differ from our theoretical predictions. Section 6 looks at the impact of risk aversion, Section 7 provides further discussion, and Section 8 concludes.

2 Experimental design and hypotheses

In this section, we present the experimental design and our hypotheses. Trying to capture the essential features of wind farm auctions in practice, our model contains a private-value element (subjects differ in their productivity) as well as a common-value element (each subject receives a private signal on the common fixed costs). Also, there is additional uncertainty (future electricity prices are unknown) and winners have to make an investment decision before the price uncertainty is resolved.[5] Our treatments

[5] In real world wind farm auction governments typically publish comprehensive information packages before the auction, including, for instance, environmental impact assessments and meteorological studies thus providing some information on the common value. In addition, the minimum installed capacity is specified. Developers place bids based on various technical and organizational decisions such as turbine capacity and technology, installation methods and operations and maintenance strategies (e.g., Johnston et al., 2020; van der Wal et al., 2017). Such decisions may require significant investments, such as the use of a service operation vessel (SOV), which may cost hundreds of millions of euros, and may vary significantly among developers, hence adding a private-value element to these

differ in whether government provides a price guarantee, and whether it provides a subsidy per unit of output produced.

2.1 Procedures and parameters

The experiment was run online in September and October 2020 using LIONESS Lab (Giamattei et al., 2020).[6] We organized 12 sessions recruiting 192 subjects from the undergraduate population of the University of Amsterdam using the CREED subject pool. At the start of each session, groups of three subjects were formed, who interacted in 25 rounds (no rematching). Earnings are expressed in Experimental Currency Units (ECUs) with an exchange rate of €1 = 50 ECU. Subjects' earnings over five random rounds were added to their initial endowment of €12. If the earnings at the end of the experiment was below €4, we paid €4.[7] Subjects earned €18.26 on average in about 90 minutes.

In each round, each group member plays the role of a firm bidding in the first-price sealed-bid auction for a single license to produce some good (e.g. electricity using a windfarm). Each bidder also submits the quantity q_i it will produce should it win the auction. The highest bidder wins the license and pays her own bid.[8] The winner faces fixed costs F to enter the market (e.g. to build the wind farm) and sells its quantity q_i at the market price p. If firm i wins, its profits gross of the payment at the auction are given by

$$\pi_i = p \cdot q_i - F - \frac{q_i^2}{r_i},$$

where r_i is firm i's productivity. Productivities r_i are private information, and independent draws from a uniform distribution on $[12, 20]$. Before the auction, each firm obtains a private signal k_i about F, where the k_i's are independent draws from a

auctions.

[6] The experimental instructions are in Appendix A.

[7] This was the case for 16 subjects.

[8] In case of a tie, the winner is chosen randomly among the bidders submitting the highest bid.

uniform distribution on $[0, 300]$. The actual fixed costs F are given by

$$F = \frac{1}{3} \sum_i k_i,$$

and hence are identical but unknown for each firm. This is a common way to model common values, see Bikhchandani and Riley (1991) and Goeree and Offerman (2003) and the references therein. The price p is drawn from a uniform distribution on $[10, 20]$ and is only revealed after the auction.

The parameters p, r_i, and k_i are independently drawn in each round. To aid comparability, draws are identical between treatments. An on-screen profit calculator is provided to help subjects make their decisions. After each round, subjects learn all bids that were submitted, whether they won the auction, the fixed cost, market price, their earnings, and, for the losers of the auction, the amount they would have earned if they had won the auction. In a post-experiment survey, we ask subjects to self-assess their risk attitude, using the question introduced in Dohmen et al. (2011), where the willingness to take risks is measured on a Likert-scale from 0 to 10, where 10 indicates a high willingness to take risks.[9]

Summarizing, the auction has a private-value element (the productivity parameter r_i), a common value-element (the value F that is common to all bidders, and for which they all receive a private signal), and additional uncertainty (the price p for which all bidders know the prior distribution but not the realization).

We use a 2x2 experimental design. Treatment conditions are whether the subsidy is paid, and whether the price guarantee is given. With the subsidy, the winner receives $s = 3$ ECU per unit of production. Effectively, this shifts the range of possible prices from $[10, 20]$ to $[13, 23]$. With the price guarantee, price uncertainty is eliminated and the winner receives a price that equals its a priori expected value. Thus, without the subsidy, the winner faces a price of $\bar{p} = 15$; with the subsidy, one of $\bar{p} + s = 18$. Treatments are denoted NSNG (No Subsidy, No Guarantee),

[9] *Specifically, we ask the question: "How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks? Please tick a box on the scale, where the value 0 means: "not at all willing to take risks" and the value 10 means: "very willing to take risks"."*

NSYG (No Subsidy, Guarantee), YSNG (Subsidy, No Guarantee), and YSYG (Subsidy, Guarantee). The 192 subjects are randomly allocated to a treatment, yielding 16 independent groups of three participants per treatment. Table 1 summarizes the experimental design.

Table 1: Experimental design.

	No price guarantee	Price guarantee
No subsidy	$p \sim U[10, 20]$	$\bar{p} = 15$
Subsidy	$p + s \sim U[13, 23]$	$\bar{p} + s = 18$

2.2 Analysis

We are interested in how the subsidy and the price guarantee affect auction efficiency, production, net auction revenue, and government surplus. Auction efficiency is defined as the percentage of cases in which the most productive firm (the firm having the highest r_i) wins the auction. Production is the output produced by the auction winner. Net auction revenue A equals the winning bid minus any subsidy that is paid to the winner:

$$A \equiv b^{(1)} - sq^{(1)} - \mathbf{1}_G \cdot (E\{p\} - p)q^{(1)}.$$

Here, $b^{(1)}$ represents the highest bid, s the subsidy per unit produced, and $q^{(1)}$ the production of the winner. The indicator variable $\mathbf{1}_G$ equals unity if and only if the price guarantee is in place: if so, revenue also includes a term $(E\{p\} - p)q^{(1)}$, as the government pays the (potentially negative) difference between the guaranteed price $E\{p\}$ and the actual price p.

As noted in the introduction, we allow for the possibility that the government is not only interested in auction revenue, but also in the output of renewable energy. We therefore aggregate production and net auction revenue to a single measure S of expected government surplus:

$$S \equiv b^{(1)} + (\lambda - s)\, q^{(1)},$$

where $\lambda \geq 0$ is the shadow price of wind energy.

We base our theoretical predictions on the perfect Bayesian Nash equilibrium (equilibrium henceforth), assuming that bidders are risk neutral. When winning the auction, firm i's ex ante expected profits are given by

$$E\{\pi_i\} = E\{p+s\} \cdot q_i - E\{F|k_i\} - \frac{q_i^2}{r_i}.$$

Maximizing with respect to q_i yields the optimal production level

$$q_i^* = \frac{1}{2}E\{p+s\}r_i. \tag{1}$$

Crucially, bidder i's optimal production does not depend on F or the r_j's of the other bidders, and hence it is not affected by variables about which she is incompletely informed. Her optimal production is also not affected by the price guarantee because she is risk neutral. Plugging the optimal production level back into expected profits, we get

$$E\{\pi_i\} = \frac{1}{4}E\{p+s\}^2 r_i - E\{F|k_i\}. \tag{2}$$

The first term is the private-value component of the auction, the second term the common-value component.

To derive equilibrium bidding behavior, we follow Goeree and Offerman (2003) by mapping a bidder's two-dimensional private information into a one-dimensional summary statistic m_i which is defined as[10]

$$m_i \equiv R_i - \frac{k_i}{3}, \tag{3}$$

with $R_i \equiv \frac{1}{4}E\{p+s\}^2 r_i$ the expected revenue of firm i if it wins the auction. Based on its productivity r_i and fixed cost estimate k_i, each bidder can thus derive its summary statistic m_i. Its equilibrium bid is a function of m_i.

[10] In Goeree and Offerman (2003) this is denoted as s_i. We prefer m_i to avoid confusion with the subsidy s.

Let $Y_1 \equiv \max_{j=1,2,3}\{m_j\}$ and $y_i \equiv \max_{j \neq i}\{m_j\}$. Following Goeree and Offerman (2003), the equilibrium bidding curve in our setting is given by

$$\begin{aligned} B(x) &= E\{R_i - F|m_i = x, Y_1 = x\} - E\{Y_1 - y_i|m_i = x, Y_1 = x\} \\ &= E\{y_i|y_i \leq x\} - \frac{2}{3}E\{k_j|m_j \leq x\}. \end{aligned} \tag{4}$$

In other words, each bidder bids the expected value of the highest statistic m_j among all other bidders conditional on her own summary statistic m_i being the highest, and further shades by subtracting an additional term reflecting the fixed costs of entering the market. In Appendix B, we give the exact equilibrium bids in all four scenarios that we consider. The expressions are complicated, involving three intervals and ratios of fifth- and fourth-degree polynomials.

Note that prices only enter this expression via $E\{p+s\}$; the distribution of prices has no impact on bidding behavior. That implies that the price guarantee has no effect on bidding behavior. A full theoretical comparison of all scenarios is not feasible, so we ran 1 billion simulations to derive our theoretical predictions, given in Table 2.

Table 2: Theoretical predictions

	No subsidy NG YG	Subsidy NG YG
Efficiency	89.7%	92.7%
Winning bid	745.6	1143.5
Net revenue	745.6	657.4
Subsidy paid	0	486.1
Production	134.9	162.0
Productivity winner	17.98	18.00
Government surplus		
$\lambda = 0$	745.6	657.4
$\lambda = 3$	1150.2	1143.5
$\lambda = 5$	1420.0	1467.6

We base our hypotheses on the following observations. First, auction efficiency is higher with the subsidy. Second, while the average subsidy paid is 486, it only decreases net revenue by 112. The rest is competed away in the auction: the winning bid is on average almost 400 higher. Third, production is much higher with the subsidy. This is partly because of an increase in efficiency (which implies average productivity of the winner is higher), but mostly since a higher subsidy directly increases production, see (1). This suggests that, as long as the shadow price of wind energy is high enough, it is welfare improving to provide a subsidy. From Table 2, that is indeed the case. More precisely, for any $\lambda > 3.2$, the subsidy that we consider yields higher expected government surplus.

To understand the effect on auction efficiency, note the following. From (2), the private-value element of profits is given by $R_i = \frac{1}{4}E\{p+s\}^2 r_i$. As $r_i \sim U[12, 20]$, without subsidies we have $E\{p\} = 15$ so $R_i \sim U[675, 1125]$. With subsidies $E\{p+s\} = 18$ so $R_i \sim U[972, 1620]$. Hence, with a subsidy, *the range of possible values of R_i is wider*, which implies that uncertainty about the common value becomes relatively less important, so the bidder with the highest r_i is now more likely to win.

2.3 Naive bidding

An oft-cited concern in auctions with common values is that winners fail to take into account that the highest bidder is the most likely to have overestimated the common value, a phenomenon leading to the winner's curse, see e.g. Kagel and Levin (1986) and Eyster and Rabin (2005). It is easy to allow for such 'naive bidding' in the analysis.[11] As noted, rational bidders will bid according to (4), where the first term is the equilibrium bid in an auction where the summary statistic y_i is a private value, whereas the second term corrects for the fact that the highest bidder is likely to have overestimated the common value.

Naive bidders will bid as if they participate in a private value auction and hence will not take the second term into account. Hence

$$B_{\text{naive}}(x) \quad = \quad E\{y_i | y_i \leq x\}. \tag{5}$$

Running simulations with this bid function, we find that the average winning bid increases, from 745.6 to 850.8 in the cases with no subsidies, and from 1143.5 to

[11] See also Goeree and Offerman (2003).

1247.1 with subsidies. Of course, this also increases net revenue. However, the other outcomes in the top panel of Table 2 remain unaffected. Both with naive and with rational bidding, equilibrium bids strictly increase in the summary statistic m_i so that auction efficiency is the same in both cases.[12] Production decisions are unaffected, hence so is the productivity of the winner and the subsidy paid.

2.4 Hypotheses

From our theoretical analysis, we derive the following hypotheses that we will test in our experiment.

H1 The price guarantee (a) does not affect auction efficiency; (b) does not affect production; (c) does not affect net auction revenue; (d) does not affect government surplus.

H2 The subsidy (a) increases auction efficiency; (b) increases production; (c) increases net auction revenue; (d) yields higher government surplus if and only if $\lambda > 3.2$.[13]

Hypothesis H1 indicates that the price guarantee has no impact on the outcome criteria. As noted, it is based on the assumption that bidders are risk neutral; if bidders differ in their risk attitude, the effects of the price guarantee will depend on the extent to which risk attitude affects bidding strategies. We will come back to the effect of risk attitude on bidding behavior and production decisions in the discussion. According to hypothesis H2, the subsidy does have effects: it positively affects auction efficiency, production, and net auction revenue, and it has a positive effect on government surplus for sufficiently high λ.

[12] In both cases, the bidder with the highest summary statistic wins, resulting in the same allocation of the good, and, in turn, the same likelihood of it ending up in the hands of the most efficient bidder.

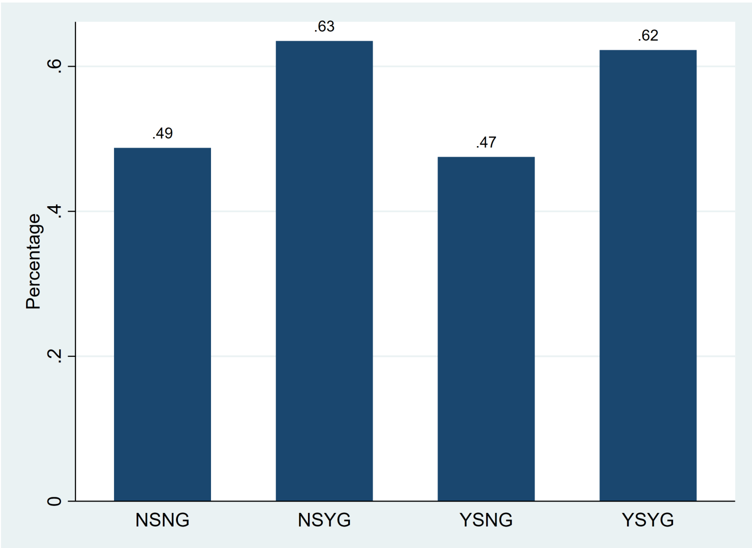
[13] The lower bound 3.2 applies to rational bidders; in the case of naive bidders, the corresponding lower bound is 3.3.

3 Results: overview

In this section, we report the treatment effect in terms of main indicators for auction performance: auction efficiency, production, net auction revenue, and government surplus. We base our data analysis on group averages over all periods, yielding 16 independent observations for each treatment.

3.1 Auction efficiency

Figure 1: Efficiency across treatments.



Note: Percentage of auctions won by the most efficient producer, for each treatment.

We mark an auction outcome as efficient if and only if the bidder having the highest production level wins the auction. Efficiency differences across treatments are illustrated in Figure 1. Without the price guarantee, permits are allocated to the most productive producer roughly 50% of the time. The price guarantee improves efficiency by more than 10 %-points, which is statistically significant.[14] The subsidy itself does not have a significant impact on efficiency.[15],[16]

[14]Mann-Whitney, two-sided: NSYG vs. NSNG, $p = 0.0044$; YSYG vs. YSNG, $p = 0.0054$

[15]NSNG vs YSNG, $p = 0.556$; NSYG vs YSYG, $p = 0.820$.

[16]Goeree and Offerman (2002, 2003) use a slightly different definition of efficiency, looking at the difference between the private value of the winner and the lowest private value in the auction, as a

Result 1 *The price guarantee increases auction efficiency. The subsidy does not affect auction efficiency.*

This is a surprising result, as it is the exact opposite of our theoretical predictions. Overall efficiency levels are also much lower than the theoretical results in Table 2. That comes as less of a surprise as bidding behavior in experiments tends to be more noisy than what is assumed theoretically (see e.g. Kagel and Levin, 1986).

3.2 Production

Production differences across treatments are given in Figure 2. All differences across treatments are significant, apart from the difference between NSNG and NSYG.[17] Our experiment thus confirms that the subsidy increases production (hypothesis H2(b)). However, the hypothesis that the price guarantee does not affect production (hypothesis H1(b)) is rejected if the subsidy is in place, in favor of the alternative hypothesis that the price guarantee boosts production.

Result 2 *The price guarantee increases production only if the subsidy is in place. The subsidy increases production.*

3.3 Net auction revenue

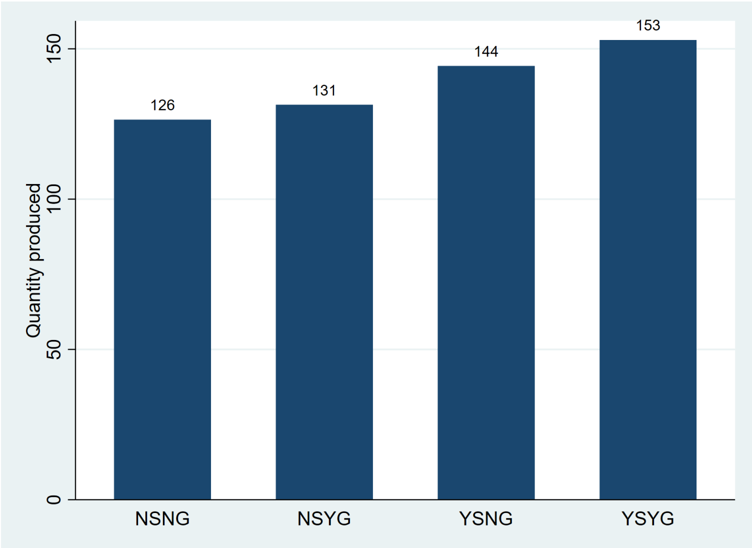
Figure 3 displays auction revenue net of any subsidy paid to the winner. Treatments NSNG, NSYG and YSYG do not differ significantly from each other in pairwise comparisons, but revenue in YSNG is significantly lower than in each of the other treatments.[18] Note that theory predicts that the subsidy lowers net auction revenue, but by far less than the subsidy amount. In our experiment, that is true in YSNG,

percentage of the difference between the highest and lowest private value. That measure is somewhat problematic in our auction, as it partly depends on the production choices that subjects make. Still, using that measure gives the exact same qualitative outcome as the one we are using; efficiency levels in the three treatments then are .71, .80, .72, and .82 respectively, with the differences between NSNG and NSYG and between YSNG and YSYG being significant, and the differences between NSNG and YSNG and between NSYG and YSYG not being significant.

[17] NSNG-YSNG, $p = 0.010$; NSNG-YSNG, $p < 0.001$; NSYG-YSNG, $p = 0.029$; NSYG-YSYG, $p < 0.001$; YSNG-YSYG, $p = 0.035$.

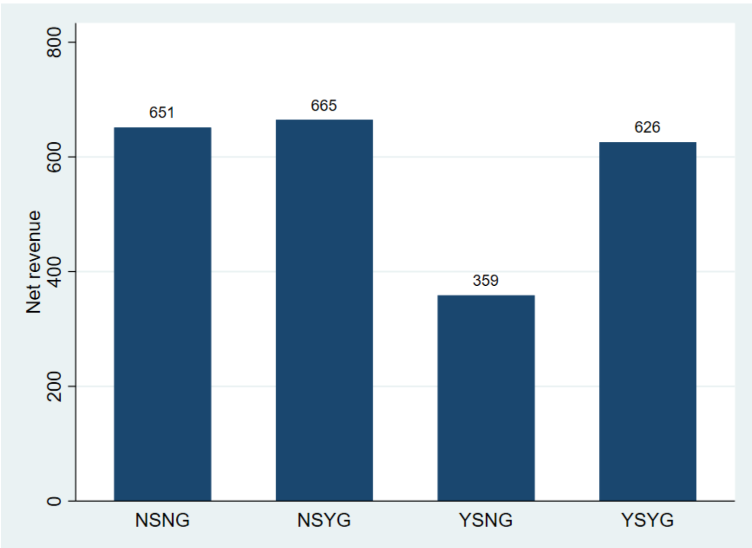
[18] $p = 0.024$ when compared with NSNG, $p = 0.008$ with NSYG, $p = 0.022$ with YSYG.

Figure 2: Production.



Note: Average production across treatments.

Figure 3: Net auction revenue.



Note: Auction revenue net of subsidies.

but there is no significant shortfall in YSYG (the average subsidies paid amount to 433 in YSNG and 459 in YSYG).

Result 3 *The price guarantee increases net auction revenue only if the subsidy is in place. The subsidy lowers net auction revenue only without the price guarantee.*

Again, we see that the price guarantee affects behavior, contrary to hypothesis H1. The effect of the subsidy is weaker than what hypothesis H2(c) predicts.

3.4 Government surplus

Table 3 is the empirical equivalent of Table 2, summarizing the above results and also showing government surplus across treatments for different shadow prices λ. As government surplus is the weighted average of production and net auction revenue, our results here merely reflect what we found for those two variables. From the table it is clear that for all λ, government surplus is greater with the price guarantee than without. For high enough λ, treatment YSYG yields unambiguously higher government surplus than any other treatment. In our theoretical analysis the subsidy increases government surplus for $\lambda > 3.2$. In our experiment this is already the case for $\lambda > 1.8$ provided that the price guarantee is offered. Without price guarantee, the subsidy improves government surplus only if $\lambda > 16.4$.

Result 4 *The price guarantee always increases government surplus. With the price guarantee in place, the subsidy increases government surplus if* $\lambda > 1.8$*; this cut-off is substantially lower than what theory predicts.*

Table 3: Experimental outcomes

	No subsidy		Subsidy	
	NG	YG	NG	YG
Efficiency	49%	63%	47%	62%
Winning bid	651	665	792	1085
Net revenue	651	665	359	626
Subsidy paid	0	0	433	459
Production	126	131	144	153
Productivity winner	16.64	17.15	16.63	17.12
Government surplus				
$\lambda = 0$	651	665	359	626
$\lambda = 3$	1030	1059	792	1085
$\lambda = 5$	1283	1322	1081	1391

4 Disentangling deviations from equilibrium behavior

In this section, we try to shed light on why we failed to find support for some of our hypotheses by disentangling the differences between actual behavior and equilibrium behavior. We first do so for bidding behavior and then for production decisions.

4.1 Factors affecting revenue

We explore three avenues through which auction revenue in the experiment may be lower than in theory: inefficient allocation, suboptimal production, and excessive bid shading. Starting with inefficient allocation, note that in all treatments auction efficiency is lower in the experiment than in equilibrium (compare Tables 3 and 2). This implies that gross profits of the auction winner will be lower on average than the equilibrium profits, which in turn adversely affects auction revenue. We call this the **misallocation effect**: the decrease in average gross profits of the auction winner because the most productive bidder fails to win the auction. Second, the auction winner does not always choose the optimal production level, negatively affecting her gross profits. We call this the **misproduction effect**: the decrease in average gross profits due to a suboptimal production level. Third, bidders may submit a bid below their gross profits. The difference is the extent of bid shading. Whenever bid shading is higher than in the theoretical analysis, We call this the **bidding effect**: the difference between the actual bid shading and the bid shading in equilibrium. Note therefore that the bidding effect is negative if subjects bid higher than their theoretical counterparts.

In Table 4, we distill these effects for the four treatments. For example, for treatment NSNG, we have the following. From the second column, auction revenues in the equilibrium are 740 on average for the parameter values used in the experiment. In equilibrium, the winner shades her bid by 109 on average, so her gross profits are 849 on average. In the experiment they are 784 – conditional on the winner choosing the optimal production level. This implies a misallocation effect of 65. Moreover, the winner not always chose the optimal production level: average gross profits of

Table 4: Decomposing auction revenue

	NSNG	NSYG	YSNG	YSYG	
Auction revenue	**740**	**741**	**1135**	**1135**	THEORY
+Equilibrium shading	*109*	*114*	*156*	*162*	
Gross profits winner	**849**	**855**	**1291**	**1297**	
−Misallocation effect	*65*	*39*	*95*	*59*	EXPERIMENT
Profits optimal production	**784**	**816**	**1196**	**1238**	
−Misproduction effect	*87*	*50*	*98*	*22*	
Gross profits winner	**697**	**766**	**1098**	**1216**	
−Equilibrium shading	*109*	*114*	*156*	*162*	
−Bidding effect	*–63*	*–13*	*150*	*–31*	
Auction revenue	**651**	**665**	**792**	**1085**	

Note: Rows give, for each treatment, averages for: auction revenue in the theoretical analysis; equilibrium shading in the theoretical analysis; gross profits of the winner in the theoretical analysis; the misallocation effect (loss in auction revenue due to a less efficient bidder winning the auction); the misproduction effect (loss in auction revenue due to production mistakes by the auction winner); the bidding effect (loss in auction revenue due to more conservative bidding than in equilibrium); the resulting average auction revenue in the experiment.

the winner were only 697, implying a misproduction effect of 87. Third, the winning bidders shaded her bid 63 units less than the equilibrium prediction. Taken together, these effects imply average revenues in the experiment of 651.

Overall, the Table shows that revenue shortfall in the experiment is caused for roughly half by lower auction efficiency, and for roughly half by suboptimal production decisions. It also suggests that the misallocation effect is stronger in treatments without the price guarantee, consistent with Figure 1. Overall, bidding is slightly more aggressive than in equilibrium, yielding a negative bidding effect in most treatments. Treatment YSNG is the odd one out in that the bidding effect is strongly positive, and hence subjects bid less than their theoretical counterparts. The misproduction effect is stronger in the no-guarantee treatments.

While Table 4 looks at average effects, Table 5 reports on a linear regression where for each winning bid, we evaluate the three effects for the particular parameters this bidder was facing. We then regress these individual effects on the summary statistic

Table 5: Explaining the effects on revenue

	(1) Efficiency	(2) Misproduction	(3) Bidding
Summary statistic	-0.46^{***}	-0.10	0.41^{***}
	(0.000)	(0.116)	(0.000)
Willingness to take risks	1.31	6.45	-19.63^{*}
	(0.359)	(0.146)	(0.033)
Subsidy	218.38^{***}	37.66	-78.91
	(0.000)	(0.166)	(0.251)
Guarantee	-15.77^{**}	-57.06^{*}	-67.04
	(0.006)	(0.015)	(0.293)
Constant	470.55^{***}	153.07^{*}	-254.63^{*}
	(0.000)	(0.035)	(0.031)
Observations	1600	1600	1600

Note: Columns explain the three effects identified in the main text. Summary statistic is the summary statistic on the basis of which equilibrium bids are determined; Risk attitude is and individual's self-assessed level of risk seeking; Subsidy is a dummy for subsidy treatments; Guarantee a dummy for price guarantee treatments. p-values in parentheses. Standard errors clustered at the group level. * significant at 5%; ** at 1%; *** at 0.1%.

that determines equilibrium bids, and on dummies indicating whether the treatment involved the subsidy and/or the price guarantee. We also include the self-reported willingness to take risks.

From the second column, we see that the efficiency effect is negatively affected by the level of the summary statistic. The higher the auction winner's summary statistic, the more likely the winner was also the bidder with the highest productivity, hence the less likely that lack of efficiency is a problem in this auction. More interestingly, we see that the subsidy has a strong positive effect, while the effect of the price guarantee is strongly negative. In other words, with the subsidy, the misallocation effect becomes more of a problem, with the price guarantee it becomes less so. Hence, while the subsidy is supposed to increase efficiency, it fails to do so. The price guarantee, however, makes bidding less noisy, which implies that it is also more likely that the most productive bidder wins the auction.

The misproduction effect is not affected by the summary statistic. Hence, production mistakes are made to the same extent by all subjects, regardless of their productivity. Yet, the price guarantee lowers the misproduction effect.

For the bidding effect, we see that bidders with a higher summary statistic tend to bid increasingly less aggressive relative to the equilibrium bid. Also, the effect decreases in self-reported risk attitude. Hence, subjects that identify themselves as being more prone to take risks, tend to bid more aggressively. Guarantees and subsidies have no significant effect.

Summing up, we thus find the following:

Result 5 *(a) Overall, revenue shortfall in the experiment is caused for roughly half by lower auction efficiency, and for roughly half by suboptimal production decisions. The treatments have little impact on the bidding effect, except for treatment YSNG, in which we observe substantial underbidding relative to the equilibrium.*

(b) The price guarantee leads to less of a misallocation effect and less of a misproduction effect. The subsidy leads to more of a misallocation effect.

(c) The greater a subject's self-reported taste for risk, the more aggressively they bid. Subjects having received more favorable signals tend to bid more conservatively relative to the equilibrium.

4.2 Factors affecting production

As noted, government surplus is also affected by the quantity produced. We can do a similar exercise as for revenue, disentangling how production is affected relative to the equilibrium outcome. Table 6 provides the results.

There are two avenues through which production in the experiment may be lower than in theory. First note that auction efficiency is lower in the experiment than in equilibrium. That implies that, on average, productivity of the auction winner will also be lower. This is another **misallocation effect**. It equals the decrease in the auction winner's production if that winner chooses its optimal production level.

Table 6: Decomposing the total experimental effects on production

	NSNG	NSYG	YSNG	YSYG
Production (theory)	**133.7**	**133.7**	**160.6**	**160.6**
−*Misallocation effect*	9.0	5.1	11.0	6.5
Optimal production (experiment)	**124.7**	**128.6**	**149.6**	**154.1**
−*Misproduction effect*	-1.7	-2.8	5.3	1.2
Production (experiment)	**126.4**	**131.4**	**144.3**	**152.9**

Rows give, for each treatment, averages for: optimal production for the bidder who wins according to theory; the misallocation effect (loss in production due to a less efficient bidder winning the auction); optimal production for the bidder who actually wins; the misproduction effect (loss in production due to production mistakes of the auction winner) and the resulting production in the experiment.

Production may further increase if the winner does not choose the optimal production level. This is a **misproduction effect**. It equals the decrease in production due to a suboptimal production decision. Note that here, the misproduction effect can also be negative if there is overproduction.[19]

From Table 6, production shortfall in the experiment is caused largely by lower auction efficiency, much less so by suboptimal production decisions. We again have that the misallocation effect is especially pronounced in treatments with the price guarantee. Without the subsidy, the misproduction is negative on average, while it is positive with the subsidy. This is especially true in treatment YSNG.

In Table 7, we regress these two effects on the summary statistic, self-reported risk attitudes and dummies for subsidy and guarantee treatments. Again, the misallocation effect is obviously decreasing in the summary statistic: the higher a winner's productivity, the higher the summary statistic, and the less of a misallocation effect there is. Also, the misallocation effect is increasing with the subsidy, but decreasing with the price guarantee. This is consistent with the misallocation effect for revenue in Table 5. Interestingly, the misproduction effect is decreasing in self-reported risk attitude. This seems to contradict Table 5, but note that the misproduction effect here measures the extent to which production is below the optimal level, whereas

[19]In the context of revenue, the misproduction effect is always positive because, by definition, suboptimal production always leads to lower gross profits.

Table 7: Explaining the effects on production

	(1) Efficiency	(2) Misproduction
Summary statistic	-0.06^{***}	0.01
	(0.000)	(0.162)
Willingness to take risks	-0.00	-2.48^{***}
	(0.985)	(0.001)
Subsidy	25.09^{***}	0.21
	(0.000)	(0.958)
Guarantee	-2.14^{***}	-1.42
	(0.001)	(0.649)
Constant	59.03^{***}	4.48
	(0.000)	(0.562)
Observations	1600	1600

Columns explain the two effects identified in the main text. Summary statistic is the summary statistic on the basis of which equilibrium bids are determined; Risk attitude is the self-assessed level of risk seeking of the individual; Subsidy is a dummy for subsidy treatments; Guarantee a dummy for price subsidy treatments. p-values in parentheses. Standard errors clustered at the group level. * significant at 5%; ** at 1%; *** at 0.1%.

that for revenue depends on the absolute value of the difference. Hence, these results suggest that the more risk-seeking subjects tend to produce more.

Finally, the fact that the guarantee dummy is significant in explaining the misproduction effect in Table 5 but not that in Table 6 is consistent with the interpretation that the price guarantee leads to less noise in the production decision: from Table 6 the price guarantee does not affect the level of production, but from Table 5 it does affect the difference between production and its optimal level.

Result 6 *(a) Overall, production shortfall in the experiment is caused largely by lower auction efficiency, much less so by suboptimal production decisions.*

(b) The price guarantee leads to less of a misallocation effect on production. The subsidy leads to more of a misallocation effect.

(c) The greater a subject's taste for risk the more they produce.

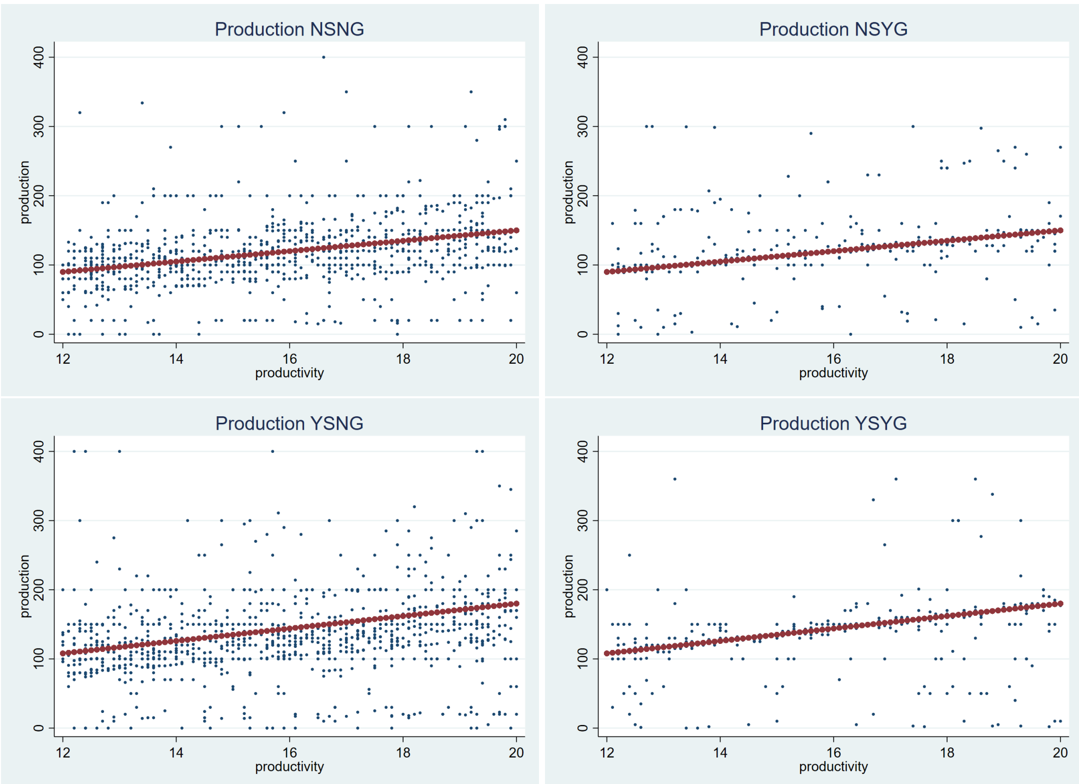
5 Individual behavior

Several of our main results go against our hypotheses. In our experiment, the price guarantee positively affects auction performance, despite our theoretical prediction that it has no effect. In the previous section, we documented that this is due to less misallocation as well as less misproduction. Similarly, the subsidy does not improve efficiency, while theoretically, it should. In the previous section, we documented that this is due to more misallocation.

In this section we analyze the individual behavior of our experimental subjects to try to further understand what drives these differences. In the following subsections, we study production decisions and bidding behavior respectively.

5.1 Production decisions

Figure 4: Observed production decisions.

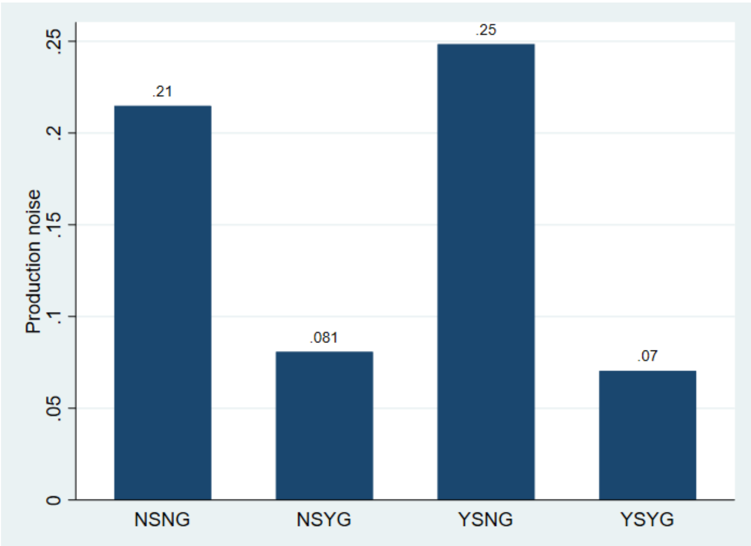


Note: Observed production levels (blue) and optimal levels (red), as a function of productivity for each bidder. Some outlying observations higher than 400 were capped at 400 to restrict the scale of the graphs.

Optimal production decisions are relatively straightforward (see Eq. (1)). Figure

4 plots all individual production decisions as a function of a bidder's productivity in that round (the blue dots). The figure also includes optimal production levels, given by (1) (the red dots). In the treatments with the price guarantee (those at the right-hand side) the picture is far less noisy than without the price guarantee, which suggests that many of the individuals' choices coincide with the optimal production level. The figures also suggests that in treatment YSNG there are relatively many cases of underproduction, while in treatment NSNG the instances of over- and underproduction are roughly equal.[20]

Figure 5: Noise in production decisions.



Variation coefficient of production mistakes over all auctions in a treatment.

To substantiate the claim that production decisions with the price guarantee are less 'noisy', we define noisiness as follows. For each observation, we take the absolute value of the difference between the production mistake in that particular decision, and the average production mistake in that treatment. We then normalize by dividing that number by the sum of the correct production decision for that case, and the average production mistake in that treatment. We then take the average of these numbers for each treatment. In Figure 5 displays the resulting production noisiness.

[20]Further analysis reveals that in treatments NSYG and YSYS, the fraction of production decisions that is within 5% of the optimal value is 85% and 86%, respectively. In NSNG and YSNG, this is 35% and 25%. In YSNG 51% of production decisions are more than 5% below the optimum and 25% is above. In NSNG these fractions are 35% and 30%.

We see that production decisions in treatments with the guarantee are indeed noisier. The subsidy has no effect on noise.[21]

Summing up, we thus have the following:

Result 7 *On average, subjects make the correct production decisions and average production mistakes do not differ significantly between treatments. Yet, production decisions are less noisy with the price guarantee.*

This result offers a potential explanation why the price guarantee increases auction efficiency. The noise in the production levels in the no-guarantee treatments implies noise in the bidders' values, which, in turn, may imply that it is less likely that the most efficient bidder wins the auction. Also, when production decisions are less noisy, it is less likely that a production mistake will still allow one to win the auction. This is consistent with what we found in Table 5, where the price guarantee indeed lowers the misproduction effect.

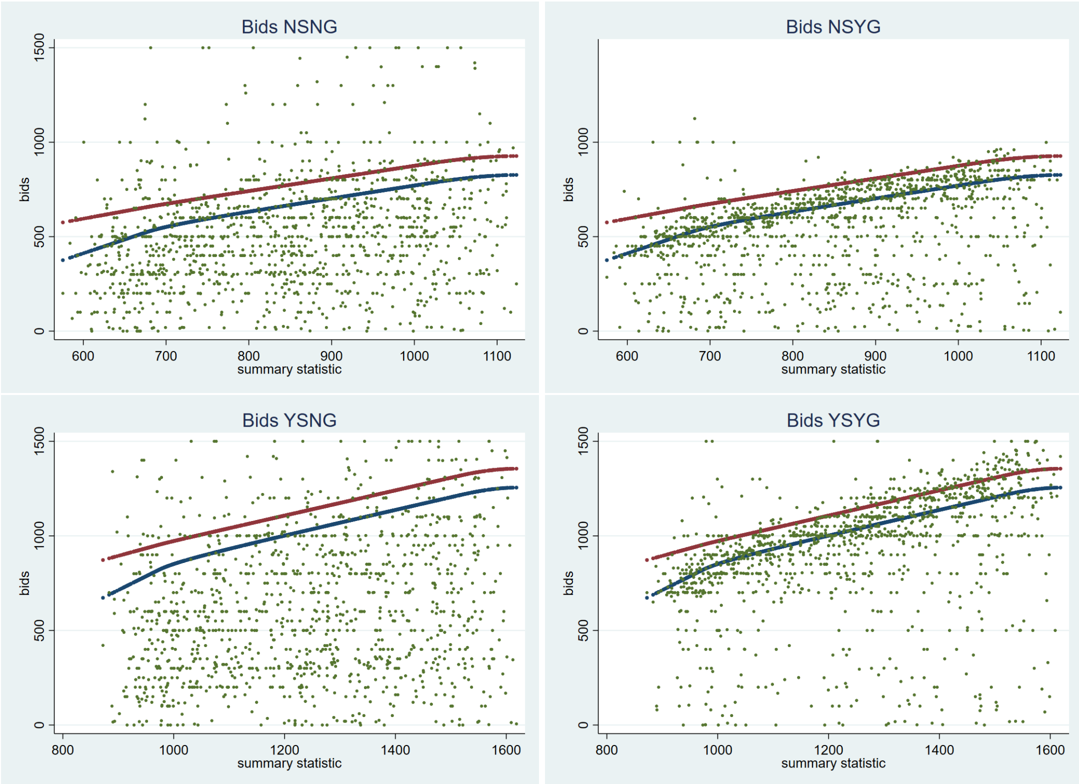
5.2 Bidding behavior

To evaluate subjects' bidding behavior, we compare each bid that is submitted in the experiment to both the equilibrium bid and the naive bid that we derived in Section 2.2. More precisely, when a bidder has drawn a productivity r_i and a fixed cost signal k_i, we calculate her value of the summary statistic m_i, and compare the bid that she submitted in the auction to the equilibrium bid given by (4), and the naive equilibrium bid given by (5).

Figure 6 plots all individual bids as a function of a bidder's summary statistic in that round (in green: the individual dots). The figure also includes equilibrium bids (in blue: the lower curve) and naive equilibrium bids (in red: the higher curve). In treatments with the price guarantee (those at the right-hand side), most bids are remarkably close to the equilibrium: the bulk are somewhere between the equilibrium and the naive equilibrium. In treatments without the price guarantee (those at the left-hand side) most bids are substantially lower than in the equilibrium.

[21] Pairwise differences between NSNG and YSNG, and between NSYG and YSYG fail to be significant at 10%. All other pairwise comparisons are significant at 0.1%.

Figure 6: Observed bids.



Observed bids (green dots), theoretical bids (blue curve) and naive bids (red curve), as a function of the value of summary statistic $m_i = R_i - k_i/3$ of each bidder. Some outlying observations higher than 1500 were capped at 1500 to restrict the scale of the graphs.

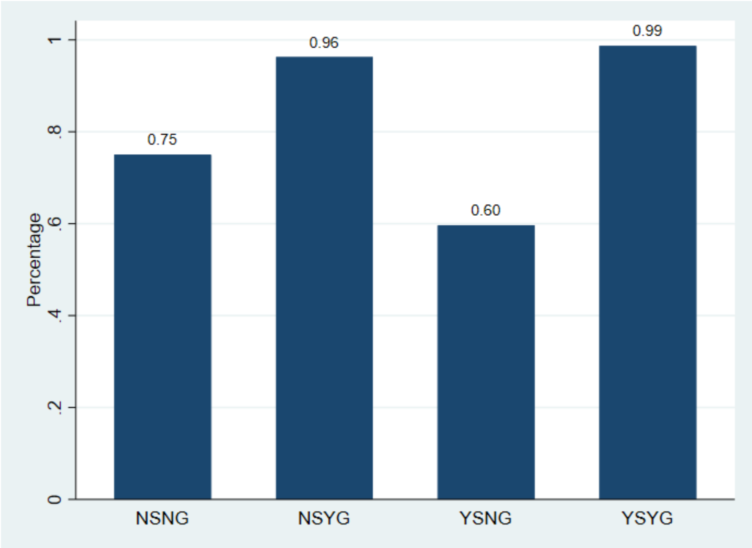
To summarize the information given in Figure 6, we proceed as follows. For each individual bidding decision, we take the ratio of the submitted bid and the equilibrium bid for the exact same parameters. Figure 7 reports on those ratios for all bidders in each treatment. To avoid the possible effect of outliers, we look at median values rather than averages.

First, note that in the Guarantee treatments, median bids are remarkably close to their theoretical values. This is all the more surprising since, as noted, equilibrium bids are a function on three intervals that involves ratios of fifth- and fourth-order polynomials. But medians in no-guarantee treatments are markedly lower, especially in YSNG. Hence, without the price guarantee, subjects bid more conservatively.[22]

We also look at the noisiness of bidding behavior, along the same lines as we did

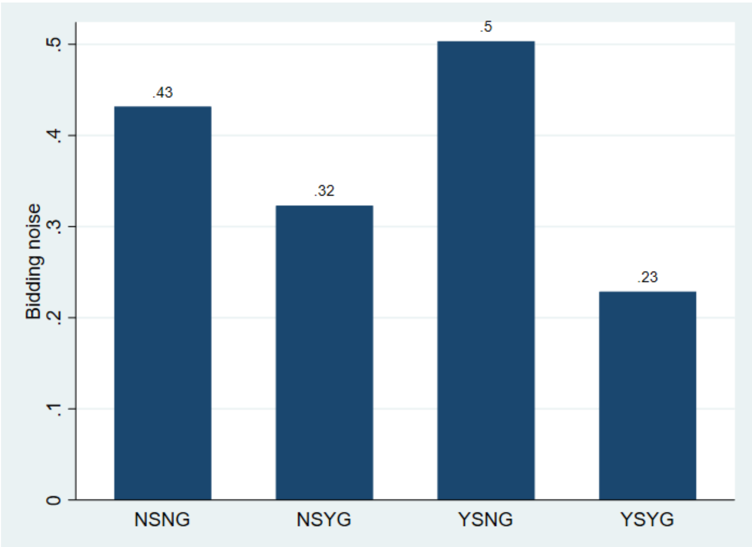
[22]Differences between YSNG and NSYG, and between YSNG and YSYG are significant at 5%; all others are not.

Figure 7: Median ratio bids to theoretical bids, all bidders



For each bid, we take the ratio of that bid and the theoretical equilibrium bid for that particular bidder. The graph gives the median of that ratio for all treatments.

Figure 8: Noise in bidding decisions.



Variation coefficient of bidding mistakes, over all auctions in a treatment.

for production mistakes. Variation coefficients are reported in Figure 8.[23] We find

[23]More precisely, for each observation, we take the absolute value of the difference between the bidding mistake (i,e, the extent of over- or underbidding) in that decision, and the average bidding mistake in that treatment. We normalize by dividing the result by the sum of the theoretical bid for that case, and the average bidding mistake in that treatment. We then take the average of these numbers in a treatment.

that the price guarantee also makes bidding decisions less noisy. The subsidy makes bidding less noisy, but only if no price guarantee is in place. With a guarantee, its effect is not significant.[24]

As noted, in Figure 7 we take the ratio of actual bids and equilibrium bids derived in our theoretical model. But that equilibrium bid is derived under the assumption that subjects set optimal production decisions. Hence, whenever a subject does not submit the equilibrium bid, that may partly reflect the fact that she did not make the optimal production decision. Ideally, we want to derive the equilibrium bids for the actual rather than the optimal production decisions.

To try to correct for this, we proceed as follows. For each productivity level r_i and production level q_i that is chosen, we calculate the value of $E\{p+s\}q_i - q_i^2/r_i$, which is the private value of winning for this bidder, before subtracting the (common) value of fixed costs. We then determine the "virtual r", i.e. the $\hat{r}_i$ that, if q were chosen optimally, would yield the true private value of winning as calculated above.[25] From (1) and (2), we have $\hat{r} = q_i^2/(E\{p+s\}q_i - E\{p+s\}^2/4)$. For this virtual r, we determine the optimal bid. We then compare that to the actual bid submitted. Hence, for bidders that do not choose the optimal production level, $\hat{r}$ is the r that would yield the same private value they now face. In this way, we thus fully eliminate the production decision, allowing us to focus on bidding behavior.[26]

Results are given in Figure 9. Unsurprisingly, ratios in guarantee treatments are unaffected: from Figure 4, most of these bidders already made optimal production decisions. The numbers for the no-guarantee treatments are only marginally lower than those in Figure 7.[27] Hence, non-optimal production decisions are only a minor explanation for the underbidding we observed in no-guarantee treatments.

It is also interesting to note that when we redo the analysis on the noisiness of bids in Figure 8 on the basis of the virtual productivity, the difference between NSYG and YSYG is no longer significant, while other significance levels do not change. Hence,

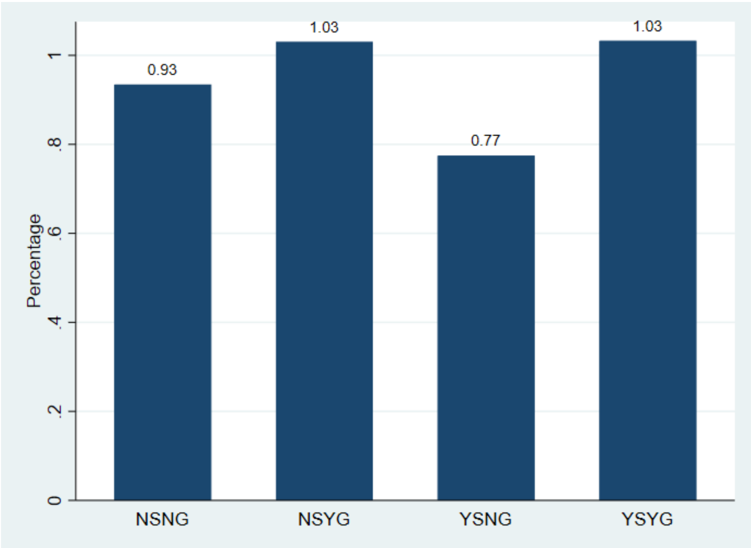
[24]Pairwise comparisons are significant at 5%, except the difference between NSNG and YSNG.

[25]However, when $\hat{r} < 12$, we set $\hat{r} = 12$. Otherwise we cannot plug it in the bid function.

[26]Note that this procedure does assume that each bidder assumes all others make optimal production decisions, as they still determine their bid on the basis of the original distribution of r.

[27]In this analysis, only the difference between YSNG and YSYG is significant at 5%.

Figure 9: Median ratio winning to theoretical bids, adjusted for production.



For each winning bid, we calculate the true value of winning, and the virtual productivity r that would have yielded the same value of winning if this bidder had made an optimal production decision. We then take the ratio of her bid and the equilibrium bid for that virtual r for this bidder. The graph gives the median of that ratio for all treatments.

allowing for production mistakes, noise is never significantly affected by the subsidy.

To measure the prominence of the winner's curse, we take all winning bids and measure to what extent these exceed the true value to the winner. Of course, whenever there is a loss, this could just be due to bad luck, i.e. a low realization of price. For the purpose of this exercise, we therefore evaluate the value at the expected price.[28]

The results are given in Figure 10. Clearly, the bulk of the winners in the experiment do not make a loss. For most treatments, the fraction that makes a loss stands at 22%, though the fraction is substantially higher[29] in treatment NSNG.

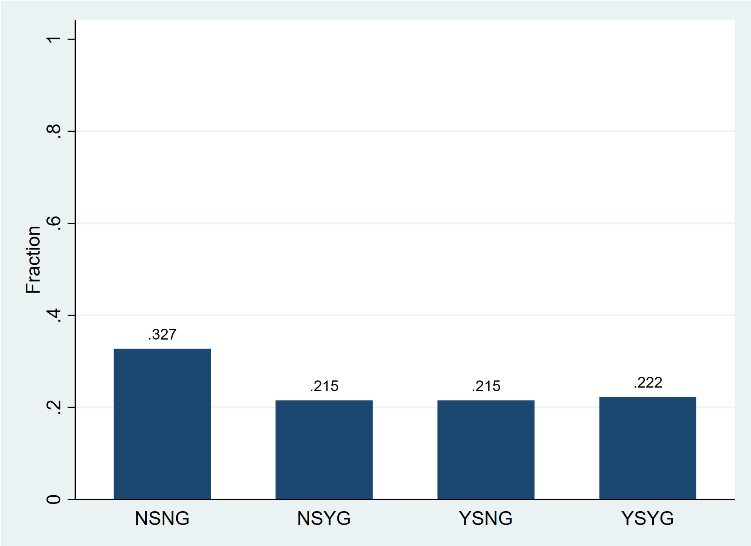
Summing up, we thus find:

Result 8 *On average, bidding decisions are close to the equilibrium bids, especially in treatments with a price guarantee. The price guarantee makes bidding less noisy. The subsidy also does so, but only with the price guarantee in place. When correcting for*

[28]Failing to do so would also yield a substantial amount of losses for the theoretical equilibrium bids; it clearly makes no sense to qualify this as a winner's curse.

[29]And significantly so, when compared to each of the other three treatments.

Figure 10: Incidence of winner's curse.



Fraction of winners that end up paying more than the value, evaluated at the expected price.

production mistakes, the subsidy has no effect. The winner's curse is not prominent: in all treatments, the majority of the winners do not suffer from the winner's curse.

6 The Role of Heterogeneity in Risk Aversion

As noted, the outcome of our experimental auctions is different from the theoretical predictions. We find that the subsidy does not affect efficiency. It does increase revenue and production, but not to the extent predicted by theory. Different from the prediction of no effect, the price guarantee does increase efficiency, and increases revenue and production with the subsidy in place. Our data analysis so far suggests that these anomalies are not caused by a failure of subjects to understand the experiment: overall, equilibrium bids are remarkably close to the equilibrium (especially with the price guarantee), and subjects do not fall prey to a winner's curse massively.

Our analyses in Section 5 suggest the price guarantee reducing noise in the bids might offer an explanation as to why it has desirable effects not anticipated by the theory. The price guarantee makes decisions less noisy, which implies it less likely that the 'wrong' bidder (i.e. one that does not have the highest productivity) wins the auction, enhancing the auction's efficiency. As a result, the price guarantee also lowers the impact of production mistakes, as a bidder making such mistakes is less likely to win the auction.

Why is behavior more noisy without the price guarantee? One potential explanation is that decisions are harder, hence bidders are more prone to make mistakes. Bidder heterogeneity in terms of risk aversion is another potential explanation. Suppose bidders differ in their risk attitude. Moreover, suppose that without the price guarantee, bidding strategies are more sensitive to risk attitude. As risk attitude is uncorrelated with productivity, without the price guarantee it is then less likely that the most productive firm wins. As a result, the price guarantee will enhance auction efficiency as well as production.

Table 8: Assessing the interplay between risk attitude and the price guarantee

	No Guarantee		Guarantee	
	(1) Bidding	(2) Misproduction	(3) Bidding	(4) Misproduction
Statistic	0.42***	0.01	0.41*	0.00
	(0.000)	(0.147)	(0.020)	(0.689)
Willingness to take risks	−35.68*	−3.75***	2.80	−0.74
	(0.013)	(0.001)	(0.739)	(0.332)
Subsidy	28.69	−0.57	−187.72*	1.84
	(0.790)	(0.925)	(0.022)	(0.708)
Constant	−229.13	7.26	−404.26	−2.06
	(0.114)	(0.497)	(0.060)	(0.874)
Observations	800	800	800	800

Columns explain the bidding effect for revenue, and the misproduction effect for production, as explained in the main text. Statistic is the summary statistic on the basis of which bids should be determined; Risk attitude the self-assessed level of risk seeking of the individual; Subsidy a dummy for subsidy treatments. Columns (1) and (2) consider treatments NSNG and YSNG; columns (3) and (4) consider NSYG and YSYG. p-values in parentheses. Standard errors clustered at the group level. * significant at 5%; ** at 1%; *** at 0.1%.

Table 8 suggests that the risk aversion explanation has some bite. We again consider the misallocation effect on revenue and the misproduction effect on production;

in Tables 5 and 6, we found that these were significantly affected by risk attitude. But we now run these regressions for the No Guarantee and for the Guarantee treatments separately. From Table 8, without the price guarantee, both the bidding effect in the auction and the misproduction effect in the production decision are significantly affected by the self-reported risk attitude. That is no longer the case in the Guarantee treatments. This suggests indeed that the price guarantee mitigates the noise in the auction that is due to differences in risk aversion and that ultimately yields lower auction efficiency.

Contrary to the theoretical prediction, we find that subsidies do not affect auction efficiency. In theory, subsidies lead to a stronger weight on the private value, making it more likely that the bidder with the highest private value wins the auction. In the experiment, there is no such effect. Overall, we do find that bidders with a higher private value bid more conservatively; this adversely affects efficiency as it makes it less likely that bidders with a high private value win the auction. Apparently this adverse effect cancels out the positive effect of the weight of private values relative to the common value.

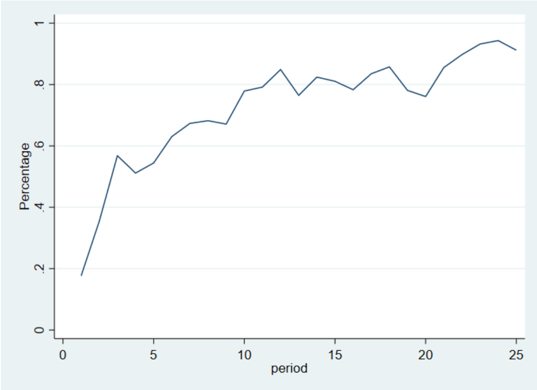
7 Further Discussion

In this section, we discuss two final issues. In Section 7.1, we study the extent to which treatment differences may be explained by behavior evolving differently over time across treatents. In Section 7.2, we examine the variances of revenue and production, in which governments may also be interested in, next to their averages.

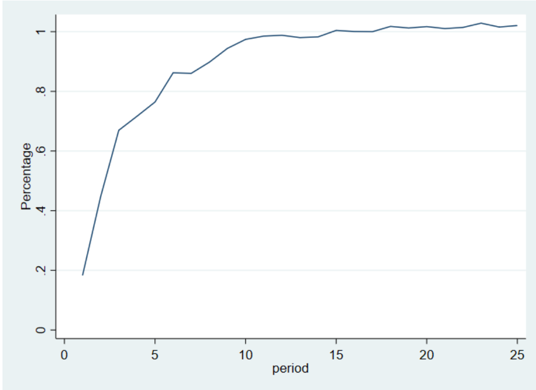
7.1 Behavior over Time

Figure 11 gives the extent of underbidding (bids expressed as a fraction of the equilibrium bid) per period, for each treatment. From the figure, subjects bid very conservatively in the first few rounds, but that extent of underbidding quickly decreases. Bidding becomes more aggressive over time, although that increase is no longer very pronounced after period 10. There is no statistical difference in the speed of learning

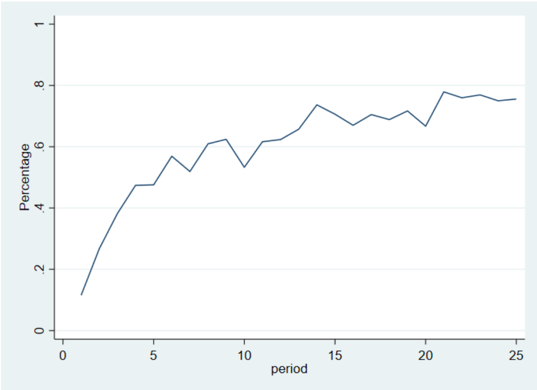
Figure 11: Median ratio bids to theoretical bids, per period.



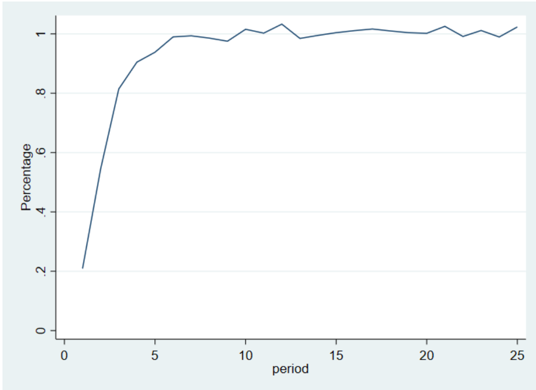
(a) NSNG



(b) NSYG



(c) YSNG



(d) YSYG

For each bid, we take the ratio of that bid and the theoretical equilibrium bid for that particular bidder. The graph gives the median of that ratio per period, for each treatment.

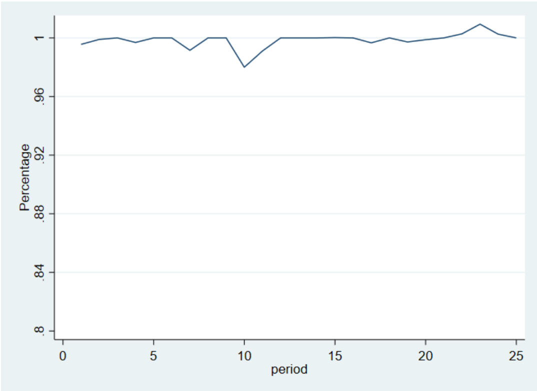
between the treatments.[30]

If we look at the extent of overproduction over time, we see a remarkable pattern in YSNG. Figure 12 gives the median overproduction per period. Remarkably, subjects are still close to the optimal production in the earlier periods but lower their production towards the end of the experiment. This pattern does not appear in the other treatments.

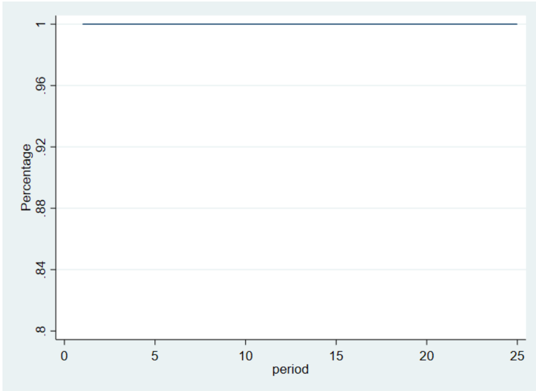
Still, all the analyses we carried out in the main text do not change qualitatively if we drop the first 10 rounds from our analyses.

[30] To check this, we compared the median overbid (on subject level) in the first 10 periods to that in the last 5 and, as a robustness check, also in the first 5 periods vs. the last 5. The difference between the last 5 and the first 10 periods is around 0.12 in each treatment, the difference between the first 5 and the last 5 roughly 0.23. In either case, any pairwise Mann-Whitney test is insignificant.

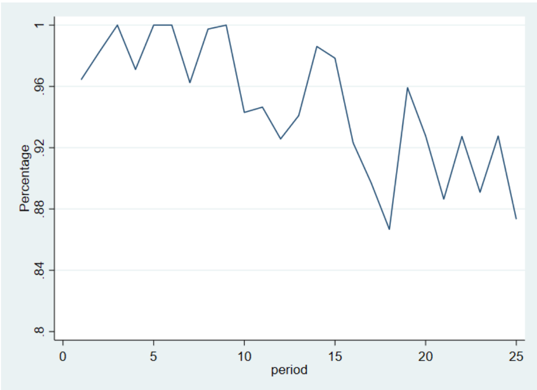
Figure 12: Median ratio production levels to optimal levels, per period.



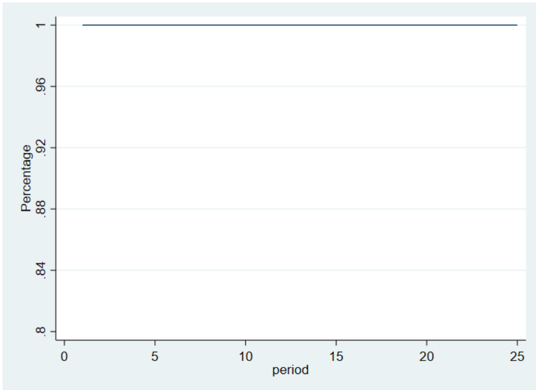
(a) NSNG



(b) NSYG



(c) YSNG



(d) YSYG

For each production decision, we take the ratio of that production decision and the optimal production level for that particular bidder. The graph gives the median of that ratio per period, for each treatment.

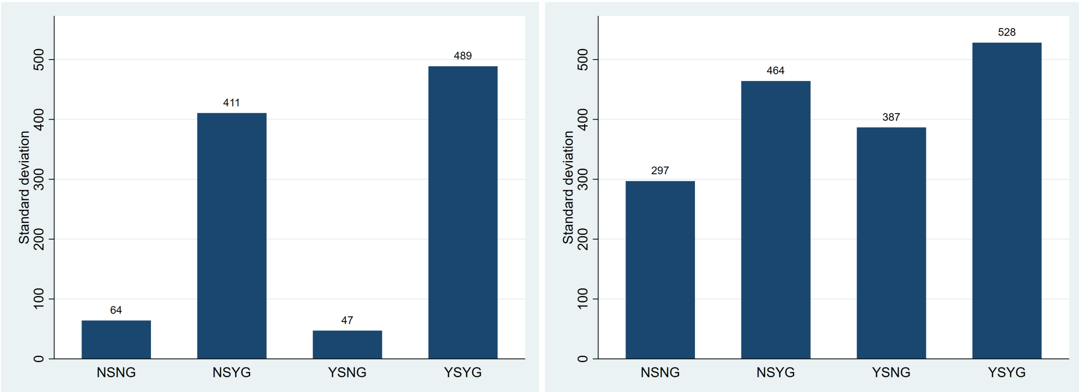
7.2 The Variance of Revenue and Production

How do the various mechanisms perform in terms of variance of the government revenue and production? We first consider government revenue, which equals the winning bid minus the production subsidy and the costs of the price guarantee.[31] In the left-hand side of Figure 13, we show the standard deviation of government revenue in our theoretical analysis. As can be seen, this is fairly low without a price guarantee, and much higher with one. The right-hand side shows the outcome of our experiment. The results are comparable for treatments with the guarantee, but much higher for those without one. Overall, both the subsidy and the guarantee increase

[31] The latter can of course also be negative if the true price turns out to be lower than the guaranteed price.

the uncertainty surrounding government revenue.[32]

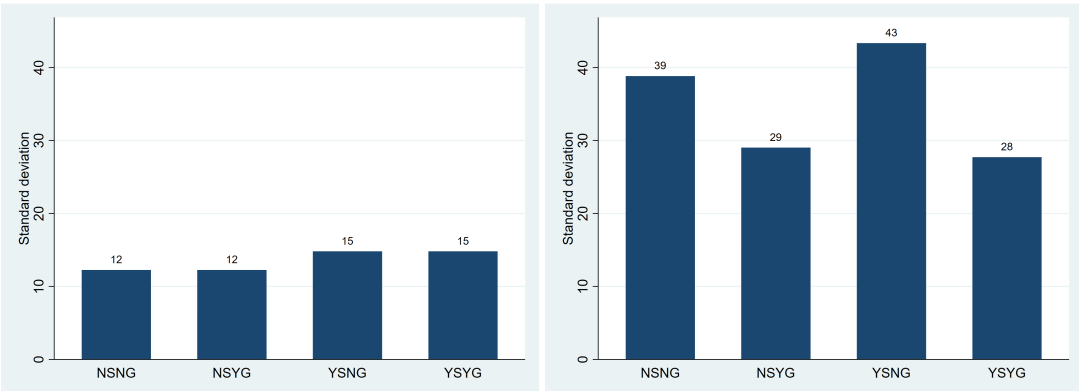
Figure 13: Standard deviation of government revenue.



(a) Theory (b) Experiment

Standard deviation of government revenue over all auctions in a treatment, in the theoretical analysis (left) and the experiment (right).

Figure 14: Standard deviation of the production of the winner.



(a) Theory (b) Experiment

Standard deviation of the production of the winner over all auctions in a treatment, in the theoretical analysis (left) and the experiment (right).

In Figure 14, we do the same analysis for the standard deviation of the production of the winner. The left-hand side gives the theoretical prediction. Again, the subsidy increases the standard deviation, while the price guarantee has no effect. The right-hand side shows the outcome of our experiment. The standard deviation increases with the subsidy, but only if there is no guarantee in place. It decreases with the price

[32] All pairwise differences are significant at 5% using Levene's robust test statistic for the equality of variances.

guarantee.[33] Again, this is consistent with the observation that behavior is less noisy with the price guarantee. When deciding whether or not to offer the price guarantee, a government that cares about the volatility of the auction should carefully trade off the increased volatility in government revenue and the decreased volatility in the production.

8 Conclusion

In this paper, we have studied auctions for the provision of renewable energy, focusing especially on wind farm auctions. In a setting characterized by private-value and common-value elements, uncertainty about future energy prices, and investment decisions that have to be made before the auction, we have examined the impact of price guarantees (that lower uncertainty for the bidders) and price subsidies (that raise the stakes in the auction and also have the effect of making the private value element more prominent).

In theory, removing price uncertainty for the bidders by giving them the price guarantee should not have an effect on production or bidding behavior. Yet, the outcome of our experiment is very different. With the price guarantee, production and bidding decisions are less noisy, leading to higher auction efficiency and hence higher auction revenue and higher output of renewable energy. Moreover, the price guarantee dampens the volatility in the production levels, although this should be weighted against the increased standard deviation in government revenue. In theory, subsidies should increase auction efficiency, as they make the private value of the auction more prominent. We fail to see such an effect in the experiment.

An important explanation for these results is that less risk averse subjects tend to bid less aggressively and produce less. Without the price guarantee, and hence with more uncertainty in the auction, this increases the chances that risk-loving bidders win the auction, thus compromising auction efficiency. Indeed, we find a significant effect of risk attitude on bidding and production in treatments without the price

[33] All pairwise differences are significant at 5% using Levene's robust test statistic for the equality of variances, except for the difference between NSYG and YSYG.

guarantee, but not in those with one.

We hypothesized that the subsidy increases efficiency. Yet, we find that bidders with a higher valuation tend to bid more conservatively than the equilibrium prediction, thus neutralizing the efficiency-enhancing effect of the subsidy. We also hypothesized that the subsidy increases production. That is also the case in our experiment, though the effect is not as large as hypothesized, largely due to conservative production choices by the auction winners.

Our experimental results also suggest that the winner's curse may be less of a concern than is sometimes argued. In none of our treatment does the auction winner systematically pay too much. If anything we find underbidding, especially if there is no price guarantee in place.

The policy implications of our analysis are straightforward. Our experimental results suggest that it pays for the auctioneer to decrease the uncertainty in the auction, for example by offering a price guarantee. Doing so reduces the risk that a bidder wins the auction because she is the least risk averse, rather than because she is the most efficient. In contrast, providing subsidies is much less effective than what theory predicts. Clearly, these insights are also relevant for other auctions in which rights are allocated to enter markets characterized by heavy upfront investments and uncertainty about future payoffs. Examples include mobile-telecom markets (e.g., McMillan, 1994; Goeree and Holt, 2010), markets for welfare-to-work services (e.g., Onderstal, 2009), and regional railway markets (e.g., Lalive and Schmutzler, 2008).

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Appendices

A Experiment instructions

Welcome

Thank you for participating in today's experiment on interactive decision-making. The session will last about 90 minutes. We will start with a brief instruction. Please read this instruction carefully. Instructions are identical for all participants. If you have any question about the instruction or at any other time during the experiment, please send a message via Zoom to the experimenter. The experimenter will answer your question privately. Please don't close the window/tab during the experiment, otherwise you will be automatically excluded from the experiment and have no way to rejoin it.

Your payment

Your starting balance in this experiment is 12 euros. During the experiment you can obtain more money or lose some money, so your final payment can be higher or lower than this. You will get paid at least 4 euros for this experiment, so if your final balance goes below 4 euros, we will set your final payment to 4 euros. If your final balance is higher than 4 euros, we will pay you your final balance.

You will play 25 rounds. In each round, you will have some earnings, which can be positive or negative. In the instructions and all decisions tasks that follow, earnings are reported in Experimental Currency Units (ECUs). At the end of the experiment, the central computer will pick 5 rounds at random, where each round is equally likely to be picked. Your earnings from those 5 rounds will be converted to euros such that each 50 ECU that you have earned is worth 1 euro. If your total earnings from the five rounds are positive, then your gains will be added to your 12 euro starting balance. If your total earnings from the five rounds are negative, then your loss will be subtracted from your 12 euro starting balance. As noted, you will never receive less than 4 euros, even if your final balance is below that amount.

You will be paid by bank transfer after the experiment. We use your IBAN to make the payments. Your decisions in the experiment will remain anonymous; the computer program will not ask for your name or student id.

The experiment

Introduction

You will play the role of a firm. You will play a total of 25 rounds. You will be matched with two other participants in the experiment for the entire game. In each round, the three people in your group participate in an auction. Each has to submit a bid. The highest bidder has to pay his/her bid, and is allowed to produce on a market. (S)he will be the only one allowed to do so and hence becomes the only producer of a good in that round. All other bidders have zero earnings in that round. If there is more than one highest bidder, one of them will be randomly selected.

Decisions

You have to make two choices in each round.

- You have to decide how much you want to produce if you are the only producer,
- and you have to decide how much you want to bid to be the producer in the auction.

Profits

If you are the producer, you will receive profits. Those profits will be the difference between total revenues and total costs on the market. Put differently:

Profits = p×q – F – v(q)

Here, p is the price, q is the output the producer chooses, F is fixed costs, and v(q) is variable costs that depend on q.

Uncertainties

However, you face a lot of uncertainty. The other bidders face the same problem and face the same uncertainty. Here is more information about the components that make up profits:

- The price p of the product is between 10 and 20, where each number is equally likely. The producer has no influence on the price. It is determined by chance. [This is for treatment NSNG.]

  The price p is always equal to 15. [This is for treatment NSYG.]

  The price p of the product is between 13 and 23, where each number is equally likely. The producer has no influence on the price. It is determined by chance. [This is for treatment YSNG.]

  The price p is always equal to 18. [This is for treatment YSYG.]

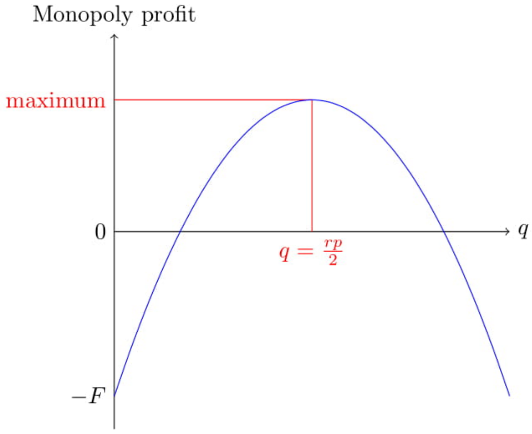
- The output q can be chosen by the producer.

- The fixed cost F is also uncertain. However, you will receive an estimate of F. The other two bidders will also receive an estimate (Remark: The computer randomly draws three estimates, one for each bidder. Each estimate is drawn independently from the other estimates.). Each estimate is a number between 0 and 300, where each number is equally likely. Hence, each estimate is a separate number, and these numbers are unrelated to each other. The true value of fixed cost F is the average of the three estimates, so this is what the producer has to pay. You will only learn your own estimate, not that of the other bidders.

- Variable costs will depend on output and on the productivity of the producer. Variable costs v(q) are equal to q×q/r, where again q equals the output the producer chooses, and r equals its productivity. The higher your productivity r, the lower your costs. For each bidder, its productivity is determined by chance and will equal some number between 12 and 20, where all numbers are equally likely. A bidder's productivity is drawn independently from the other bidders' productivity. Before an auction, you will only learn your productivity level, not that of the other bidders.

Optimal output

The graph below shows the relationship between profits and output in general. Output is on the horizontal axis. On the vertical axis, you see profit as a function of output q. Profit reaches its maximum at q=r×p/2.



Example

Suppose the price p is 15, your productivity r is 16, the estimate you received about the fixed cost F is 150, and the estimates the other two bidders in your group received are 0 and 300. Since the fixed cost F is the average of the three estimates in your group, you know the fixed cost F=(150+0+300)/3=150. According to the description above, your profit = 15q - 150 - q×q/16. If you use the formula about the optimal quantity, then you have, when you produce output q=r×p/2=16×15/2=120, your profit reaches its maximum 750. [This is for treatment NSNG and NSYG.]

Suppose the price p is 18, your productivity r is 16, the estimate you received about the fixed cost F is 150, and the estimates the other two bidders in your group received are 0 and 300. Since the fixed cost F is the average of the three estimates in your group, you know the fixed cost F=(150+0+300)/3=150. According to the description above, your profit = 18q - 150 - q×q/16. If you use the formula about the optimal quantity, then you have, when you produce output q=r×p/2=16×18/2=144, your profit reaches its maximum 1146. [This is for treatment YSNG and YSYG.]

Summary

Each auction proceeds as follows

1. You learn your estimate of the fixed costs F, and your productivity r. The other bidders will learn their estimate of fixed costs, and their productivity.
2. We ask you to determine what output q you will choose if you become the producer.
3. We ask you how much you want to bid to be the producer in the auction.
4. The highest bidder becomes the producer. If there is more than one highest bidder, one of them will be randomly selected to be the producer. The producer pays its bid.
5. The price p and fixed costs F are determined. All bidders will learn these values, and will learn all bids that were submitted.
6. The producer receives its profits.
7. If you are the producer, your earnings in this round equal your profits minus your bid. If you are not the producer, your earnings in this round equal zero.

Please note that in each auction, new values for prices[only for treatment NSNG and YSNG], fixed costs estimates and productivity levels will be determined. These are in no way influenced by what happened in previous rounds.

B Equilibrium bidding strategies

Note that, given the distribution of r_i, we have that $R_i \sim U[3E\{p+s\}^2, 5E\{p+s\}^2]$. Moreover, we have that $k_i \sim U[0, 300]$. This implies

$$E\{k_i|m_i \leq x\} = \int_{3E\{p+s\}^2-100}^{x} E\{k_i|m_i = m\} \frac{f(m)}{F(m)} dm \tag{6}$$

where

$$E\{k_i|m_i = m\} = \begin{cases} 300 - \frac{3}{2}\left(m - 3E\{p+s\}^2 + 100\right) & \text{if } m \in [3E\{p+s\}^2 - 100, 3E\{p+s\}^2] \\ 150 & \text{if } m \in [3E\{p+s\}^2, 5E\{p+s\}^2 - 100] \\ \frac{3}{2}\left(5E\{p+s\}^2 - m\right) & \text{if } m \in [5E\{p+s\}^2 - 100, 5E\{p+s\}^2]. \end{cases} \tag{7}$$

The density f and corresponding distribution F of m_i is given by

$$f(m) = \begin{cases} \frac{1}{2E\{p+s\}^2}\left(\frac{m-3E\{p+s\}^2+100}{100}\right) & \text{if } m \in [3E\{p+s\}^2 - 100, 3E\{p+s\}^2] \\ \frac{1}{2E\{p+s\}^2} & \text{if } m \in [3E\{p+s\}^2, 5E\{p+s\}^2 - 100] \\ \frac{1}{2E\{p+s\}^2}\left(\frac{5E\{p+s\}^2-m}{100}\right) & \text{if } m \in [5E\{p+s\}^2 - 100, 5E\{p+s\}^2] \end{cases} \tag{8}$$

and

$$F(m) = \begin{cases} \frac{1}{400E\{p+s\}^2}\left(m - 3E\{p+s\}^2 + 100\right)^2 & \text{if } m \in [3E\{p+s\}^2 - 100, 3E\{p+s\}^2] \\ \frac{m+50}{2E\{p+s\}^2} - \frac{3}{2} & \text{if } m \in [3E\{p+s\}^2, 5E\left\{p\right\}^2 - 100] \\ 1 - \frac{E\{p+s\}^2}{16} + \frac{m}{40} - \frac{m^2}{400E\{p+s\}^2} & \text{if } m \in [5E\{p+s\}^2 - 100, 5E\{p+s\}^2] \end{cases} \tag{9}$$

respectively. Moreover, we have

$$E\{y_i|y_i \leq x\} = x - \int_{3E\{p+s\}^2-100}^{x} \left(\frac{F(\xi)}{F(x)}\right)^2 d\xi. \tag{10}$$

Combining the above terms allows us to write an explicit expression for $B(x)$.

B.1 No-subsidy treatments

From (10) and (9), we have

$$E\left\{y_i | y_i \leq x\right\} =$$
$$\begin{cases} \frac{4}{5}x + 115 & \text{if } x \in [575, 675] \\ x + \frac{1}{(x-625)^2}\left(-\frac{1}{3}x^3 + 625x^2 - 390\,625x + 81\,371\,875\right) & \text{if } x \in [675, 1025] \\ x + \frac{-\frac{1}{5}x^5 + 1125x^4 - 2471\,250x^3 + 2645\,156\,250x^2 - 1382\,094\,140\,625x + \frac{847\,450\,919\,921\,875}{3}}{(x^2 - 2250x + 1175\,625)^2} & \text{if } x \in [1025, 1125] \end{cases}$$

From (6), (7), and (8),

$$E\{k_i | m_i \leq x\} = \begin{cases} 875 - x & \text{if } x \in [575, 675] \\ \frac{150x - 91\,250}{x - 625} & \text{if } x \in [675, 1025] \\ \frac{-x^3 + 3375x^2 - 3796\,875x + 1410\,328\,125}{x^2 - 2250x + 1175\,625} & \text{if } x \in [1025, 1125] \end{cases}$$

From (4), we then have

$$B(x) =$$
$$\begin{cases} \frac{22}{15}x - \frac{1405}{3} & \text{if } x \in [575, 675] \\ x - \frac{x^3 - 1575x^2 + 801\,875x - 130\,053\,125}{3x^2 - 3750x + 1171\,875} & \text{if } x \in [675, 1025] \\ x + \frac{7x^5 - 39375x^4 + 88593750x^3 - 99532968750x^2 + 55637982421875x - 12342915419921875}{15(x^2 - 2250x + 1175\,625)^2} & \text{if } x \in [1025, 1125] \end{cases}$$

B.2 Subsidy treatments

From (10) and (9), we have

$$E\left\{y_i | y_i \leq x\right\} =$$
$$\begin{cases} \frac{4}{5}x + \frac{872}{5} & \text{if } x \in [872, 972] \\ \frac{2\left(x^3 - 1383x^2 + 391\,876\,224\right)}{3(x-922)^2} & \text{if } x \in [972, 1520] \\ \frac{12x^5 - 72\,900x^4 + 154\,872\,000x^3 - 121\,247\,280\,000x^2 + 28\,304\,593\,971\,200\,000}{15(x^2 - 3240x + 2494\,800)^2} & \text{if } x \in [1520, 1620] \end{cases}$$

From (6), (7), and (8),

$$E\{k_i|m_i \leq x\} = \begin{cases} 1172 - x & \text{if } x \in [872, 972] \\ \frac{150x-135\,800}{x-922} & \text{if } x \in [972, 1520] \\ \frac{\left(-x^3+4860x^2-7873\,200x+4232\,088\,000\right)}{x^2-3240x+2494\,800} & \text{if } x \in [1520, 1620] \end{cases}$$

From (4), we then have

$$B(x) =$$

$$\begin{cases} \frac{22}{15}x - \frac{9104}{15} & \text{if } x \in [872, 972] \\ \frac{2\left(x^3-1533x^2+274\,100x+266\,668\,624\right)}{3(x-922)^2} & \text{if } x \in [972, 1520] \\ \frac{2\left(11x^5-76950x^4+208008000x^3-269953560000x^2+166770122400000x-38638768726400000\right)}{15(x^2-3240x+2494\,800)^2} & \text{if } x \in [1520, 1620] \end{cases}$$