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This is an Accepted Manuscript of an article published in *Applied Economics Letters*, volume 26, issue 7, in 2018, available online at DOI: [10.1080/13504851.2018.1488047](https://doi.org/10.1080/13504851.2018.1488047). It may differ slightly from the final version of record.

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Land Assembly with Taxes, Not Takings

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Forthcoming in Applied Economics Letters.

Abstract: We use a novel tax mechanism – “rejected offer reassessment” – in laboratory experiments to discourage seller holdout and facilitate land assembly. Under this mechanism if a landowner rejects a developer’s offer his taxable property value is reassessed to be equal to the rejected offer, increasing his taxes. We find that, relative to a control treatment, rejected offer reassessment discourages the magnitude of seller holdout (but not its frequency) and increases the rate of successful land assembly by almost 60%. It also increases the gains from trade by 22.1% relative to the control treatment, but the difference is not statistically significant.

Keywords: Land assembly, holdout, taxation, real estate

JEL Codes: H20, K34, R52

Funding: This research was supported by the Mercatus Center

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Data Availability

The data that support the findings of this study are available from the corresponding author, AW, upon reasonable request.

Disclosure

The authors of this paper received a research grant from the Mercatus Center. The grant was for a total of \$7,500, of which each author received \$2,500 as a stipend. 50% of this stipend was paid upon submitting an initial draft of the research findings. The remaining 50% was paid once the final draft was accepted after a round of peer review.

The Mercatus Center had no financial or professional interest in the results of the experiments presented in this paper. Moreover, the authors pre-registered their hypotheses and statistical design with the Center in advance. We therefore declare no conflict of interest with the Mercatus Center.

Doug & Marion Lee gave one of the authors, AW, a gift of \$250 for his professional use. This money was used to defray the expenses of participant payments. Mr. & Mrs. Lee are retired and not interested parties. AW and MD paid for the remaining participant earnings from their research budgets at Chapman University.

1. Introduction

DeSantis, McCarter & Winn (2017) tested two self-assessment tax mechanisms to facilitate land assembly when properties are complementary inputs to a development project.² The mechanisms achieved successful assembly in 83.3% of negotiations, capturing more than 90% of the available surplus. However, self-assessment taxation may be politically unpopular because it binds a landowner to sell at his self-assessed price.

We consider an alternative tax mechanism, “rejected offer reassessment” (ROR). Under ROR – proposed by Miceli, Segerson & Sirmans (2008) – if a landowner rejects a developer’s offer the government takes the offer as a lower bound on his subjective value for the property and reassesses the taxable value of the property at the rejected offer. Thus, strategic holdout is costly for landowners and successful assembly is promoted.

We conduct laboratory experiments to test the efficacy of ROR at discouraging holdout and facilitating land assembly. We find that, compared to a control treatment, ROR increases the assembly rate by 58.4% and the gains from trade by 22.1%. The effect on assembly is statistically significant, but the effect on gains from trade is not.

2. Experiment Design

We conducted experiments for one control (*Baseline*) and one experimental treatment (*ROR*). Our experimental design for *Baseline* follows DeSantis, McCarter & Winn (2017), and we refer the reader to that paper for a detailed description. Each treatment consisted of 30 negotiations conducted in three sessions with 10 negotiations each.

² Holdout has been observed in laboratory experiments by, e.g., Cadigan, Schmitt, Shupp, & Swope (2009, 2011), Swope, Wielgus, Schmitt & Cadigan (2011), Parente & Winn (2012), Collins & Isaac (2012), Zillante, Schwarz, & Read (2014), Swope, Cadigan & Schmitt (2014) and Winn & McCarter (2017). Brooks & Lutz (2016) and Portillo (2017) report field data consistent with seller holdout.

Negotiation proceeded in groups of five participants: one developer (buyer) and four landowners (sellers). The sellers were assigned values for their property, while the buyer was assigned a value for the combination of all four properties. Values were denominated in Economic Currency Units (ECU, denoted “\$”). Sellers (Buyers) received \$1 US for \$16,500 (\$60,000) ECU. The exchange rates were private information.

All values were drawn from uniform distributions and rounded to the nearest thousand. The sellers’ (buyer’s) distribution had support [\$100,000, \$150,000] ([\$300,000, \$1,250,000]). These distributions were common knowledge, but the participants’ value draws were private information. Sellers earned their values for the property minus tax if they did not sell, or the price at which they sold if they did.

The buyer was endowed with \$350,000, which served as an opportunity cost to negotiating. The amount of cash was private information, but its existence was common knowledge. If the buyer refused to negotiate she kept the cash. If a buyer assembled four properties she earned her value plus the cash endowment minus the sum of the prices she paid minus property tax. If a buyer assembled fewer than four properties she paid the sellers who accepted her offers, but sold the properties to the experimenter for \$100,000 each.

We randomly generated buyer and seller values for 30 negotiations and used the same value draws in both treatments. Assembly was efficient in 24 (80%) of the negotiations, which we call the “positive-sum” negotiations. The minimum, maximum and average available gains from trade in these negotiations were \$5,000, \$765,000 and \$379,000.

Negotiations proceeded for up to five rounds, with three phases each:

- Phase one: Sellers communicated via text chat, after which they each sent a nonbinding price request to the buyer. Phase one lasted for seven minutes in the first round and two minutes in subsequent rounds.
- Phase two: The buyer observed the price requests and chose to either abandon the negotiation or simultaneously send binding offers to the sellers. The buyer could offer different amounts to different sellers, but the sum of the offers could not exceed her value plus her cash.
- Phase three: Sellers observed the offers and chose to accept or reject them. They had one minute to communicate with one another before submitting their decision.

If a seller rejected his offer negotiations continued to phase one of the next round. If a seller accepted his offer the price at which he sold was disclosed to all sellers. A seller who accepted an offer was not allowed to chat with the other sellers or see their text messages in subsequent rounds. He could click a button on his user interface to launch Microsoft Paint. We included this function to prevent sellers from extending negotiations to stave off boredom.

All properties had a \$100,000 assessed value. 10% of the assessed value was collected from the owner of the property at the end of negotiations. (A buyer who purchased fewer than four properties sold them to the experimenter and so was not liable for tax.) Reassessment on sale is the status quo in most US jurisdictions. In *Baseline* properties were reassessed upon sale, so sellers who kept their property paid \$10,000 in tax. In *ROR* unsold properties were reassessed at the end of a negotiation according to the highest rejected offer. For instance, if a seller rejected offers of \$120,000, \$135,000 and \$130,000 in rounds one through three and the buyer then abandoned negotiations, the seller's property would be reassessed at \$135,000, and the seller would owe \$13,500 in tax.

Prolonged negotiations were costly to the buyer. The cost took the form of a multiplier applied to the buyer's earnings after the negotiation. The multiplier was 100% if the negotiation ended in round one. It decreased to 95% in round 2, 90% in round 3, etc. The multiplier was 75% if the buyer had not assembled all four properties (or abandoned negotiations) after round five.

4. Results

Finding 1: ROR did not reduce the frequency of holdout, but did reduce its magnitude.

Support: We measured holdout by subtracting each seller's tax-adjusted value from the highest offer they rejected. Positive differences indicate holdout, and the value of the difference measures the magnitude of holdout. In *Baseline* 48.3% of sellers rejected an offer exceeding their tax-adjusted value; in *ROR* it was 51.7%. However, among sellers who did hold out, the average difference between the highest rejected offer and the seller's value was \$53,701.74 in *Baseline* and \$26,453.76 in *ROR*; a 48.1% reduction.

We test for treatment effects in holdout frequency and magnitude using a logistic regression for holdout (1 = yes, 0 = no) and an OLS regression for holdout magnitude (Table 1).³ The logistic regression only includes observations from sellers who received an offer and the OLS regression only includes observations from sellers who held out. Both models control for treatment, negotiation number and the seller's tax-adjusted value. Standard errors are clustered by group. In the logistic regression the estimated coefficient for ROR is not significant ($p = 0.872$). The OLS regression estimates the magnitude of holdout was \$28,024 less in *ROR* than in *Baseline*, and the

³ A Heckman selection model found no significant correlation between the error terms of the frequency and magnitude models ($\rho = -0.05$, S.E. = 0.07, 95% C.I. = -0.18 to 0.08). Thus, we estimate the models separately for ease of interpretation.

Table 1. Estimates on holdout frequency and magnitude.

Variable	Logistic Regression on Holdout Frequency Coefficient (Std. Err.)	OLS Regression on Holdout Magnitude Coefficient (Std. Err.)
Constant	0.88 (1.41)	59,037 (40,767)
<i>ROR</i>	0.05 (0.34)	-28,024* (13,042)
Negotiation Number	0.61 (0.35)	14,048 (11,686)
Seller's Value (in \$10,000s)	-0.13 (0.10)	-2,187 (2,582)
Observations	220	120
Pseudo R ²	0.023	--
Wald χ^2	4.02	--
R ²	--	0.113
F-Statistic	--	2.11

* Significant at 5%

effect is significant ($p = 0.037$). Relative to the estimated constant of \$59,037 this represents a reduction in holdout magnitude of 47.5%.

Finding 2: *ROR* increased the rate of assembly.

Support: In *Baseline* buyers assembled all four properties in 50% of the positive-sum negotiations. In *ROR* the assembly rate was 79.2%, a 58.4% increase. We test for statistical significance with a logistic regression on a binary variable indicating assembly (1 = success, 0 = failure), controlling for treatment, negotiation number and amount of available surplus (in \$10,000s) (See Table 2). We limit the dataset to observations from the positive-sum negotiations.

The estimated coefficient for *ROR* is positive and significant ($p = 0.033$). Assuming the available surplus is the mean observed (\$379,000) and participants are in their first negotiation,

Table 2. Estimates from logistic regression on successful assembly in positive-sum negotiations.

Variable	Coefficient (Std. Err.)
Constant	-0.61 (1.18)
<i>ROR</i>	1.46* (0.69)
Negotiation Number	-0.46 (0.68)
Available Surplus (in \$10,000s)	0.03 (0.02)
Observations	48
Pseudo R ²	0.138
Wald χ^2	8.63

* Significant at 5%

the model estimates the *Baseline* probability of assembly at 55.4% compared to 84.3% in *ROR* - an increase of 52.2%.

Buyers could ensure that it was in a seller's financial interest to sell by making an offer greater than his maximum tax-adjusted value: \$140,000 in *Baseline* and \$136,363 in *ROR*. Sellers in *Baseline* rejected 65.5% of such offers; sellers in *ROR* rejected 51.7%. Moreover, sellers requested approximately \$199,000 on average in *Baseline*, compared to \$164,000 in *ROR*. Seller recalcitrance led buyers in *Baseline* to quit negotiating early in 25% of the positive-sum negotiations compared to 4.2% in *ROR*.

Finding 3: *ROR* did not increase the gains from trade.

Support: We measure the social welfare as the surplus captured through negotiation. The surplus is defined as the participants' actual earnings minus their earnings had the buyer abandoned negotiations in round one. There was \$9,096,000 in available surplus in each treatment.

Table 3. Estimates from OLS regression on normalized surplus.

Variable	Coefficient (Std. Err.)
Constant	-58,950 (63,485)
<i>ROR</i>	27,322 (38,282)
Negotiation Number	39,300 (33,589)
Observations	60
R ²	0.027
F-Statistic	0.78

* Significant at 5%

Participants in *Baseline* captured approximately \$3,700,000 (41.1%) of this. In *ROR* they captured about \$4,600,000 (50.2%).

For each negotiation we subtracted the surplus that had been captured in *Baseline* to minimize the variance. We fit these differences as the dependent variable in an OLS regression with a treatment dummy and the negotiation number as regressors. (See Table 3.) The model estimates that participants in *ROR* captured \$27,322 more per negotiation than in *Baseline*, but the effect is not significant ($p = 0.478$).

This null result has two causes. First, *ROR* did not reduce delay. The average negotiation took 3.8 rounds in *Baseline* and 3.83 rounds in *ROR*. Second, the extra assemblies in *ROR* were in negotiations with less available surplus. Of the 50% of positive-sum negotiations with the highest available surplus, buyers were successful in two thirds in both *Baseline* and *ROR*. Among the remaining positive-sum negotiations the assembly rate was 33.3% in *Baseline* and 91.7% in *ROR*.

5. Conclusion

We test Miceli, Segerson & Sirmans' (2008) proposal to facilitate land assembly by re-assessing property taxes after a refused offer from a developer. In our laboratory experiments this policy is effective in reducing the amounts for which sellers hold out, and increases the rate of assembly by 58.4% relative to the status quo. However, the resulting improvement to social welfare is not statistically significant. The self-assessment mechanisms tested by DeSantis, Winn & McCarter (2017) provide a similar improvement in successful assembly along with large, statistically significant increases in social welfare. Subject to political constraints, these self-assessment mechanisms should be preferred to the re-assessment mechanism evaluated here.

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