Land Assembly without Eminent Domain

Laboratory Experiments of Two Tax Mechanisms

Mark DeSantis, Matthew W. McCarter, and Abel M. Winn

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Abstract

We use laboratory experiments to test the ability of two tax mechanisms to discourage seller holdout and facilitate land assembly. In one mechanism ("revealed assessment"), if a seller rejects a developer's offer, then the property value is reassessed to be equal to the rejected offer and the taxes increase. In the second mechanism ("declared assessment"), a seller must declare a price at which he or she is willing to sell the property. The incentive to overstate the value is mitigated by using the declared price to assess a property tax. The incentive to understate the value is mitigated by allowing developers to buy the property at the declared price. We find that revealed assessment discourages the magnitude of seller holdout but not its frequency. Declared assessment has no effect on the magnitude of seller holdout but increases its frequency. Nevertheless, both tax mechanisms increase the rate of successful land assembly by more than 50 percent. Revealed assessment increases the gains from trade by 22.1 percent relative to the control treatment, but the difference is not statistically significant. Declared assessment increases the gains from trade by 124 percent, and the difference is highly statistically significant.

JEL codes: D9, H2, H7, K4, R0

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ROLAND DAGGET: You're pure evil!

BANE: I'm necessary evil.

—*The Dark Knight Rises*

1. Introduction

Many economists, legal scholars, and citizens of cities like New London, Connecticut, and Brooklyn, New York, would agree (at least in part) that eminent domain is a necessary evil. It is considered necessary to overcome seller holdouts that impede urban developments, such as federal highways, national parks, and airports (Whitman 2006; Kerekes 2011; Becher 2014). Anthony Williams, while mayor of Washington, DC, and president of the National League of Cities, declared eminent domain "essential . . . to spur economic development. That power . . . is the only way cities can keep property owners from holding out and blocking developers from assembling enough land to build the same kinds of grocery stores, shops, and other amenities found in the suburbs" (Savage 2005). Eminent domain is viewed by many to be an evil, because it abrogates property rights and may force some landowners to sell their property below their subjective value for it. In certain cases, the land is more valuable under fragmented ownership than with the new development (see Somin 2004 for a telling case study).

A large body of empirical research demonstrates that the holdout problem does impede efficient land assembly. In laboratory experiments without eminent domain, the frequency of sellers making (non)binding requests of more than the value of their property is high, and sellers earn greater payoffs when they hold out compared to when they do not (see Cadigan et al. 2009, 2011; Swope et al. 2011; Collins and Isaac 2012; Parente and Winn 2012; Shupp et al. 2013; Swope, Cadigan, and Schmitt 2014; Zillante, Schwarz, and Read 2014; Kitchens and Roomets 2015; Isaac, Kitchens, and Portillo 2016). Because holding out is not profitable for sellers unless successful land assembly occurs, negotiations in these experiments often persist for lengthy periods, regardless of whether the delay is costly. The land assembly failure rates in these studies average 9.8 percent, with a minimum of 0 percent and a maximum of 59.4 percent. On the basis of these empirical results, it seems reasonable that eminent domain is necessary to avoid seller holdout and subsequent land assembly failure.

However, two laboratory studies find that while eminent domain does increase the frequency of land assembly, it comes with costs. In the study by Kitchens and Roomets (2015), buyers negotiated sequentially with four sellers. The buyers' and sellers' values—as well as the agreed-upon price for each of the four parcels—were common knowledge. In one treatment, buyers' offers to the sellers were contingent on successful land assembly. A seller could exit the negotiation at any time, but this voided all contracts the buyer had made with prior sellers. When a seller "walked away," the sellers all received a private use value of $4 for their parcels and the buyer received nothing. In a second treatment, the buyers' offers were not contingent on assembly, but, for a fixed "court fee," the buyer could use eminent domain as an alternative to negotiation to acquire a parcel. The court fee for taking a single property was equal to one-fourth of the available gains from trade.

The primary finding in the study by Kitchen and Roomets (2015) is that eminent domain did not improve the party's welfare compared to contingent agreements. When it came to gains from trade, there was little difference (1.5 percent) between contingent contracts and eminent domain.

The second laboratory study of eminent domain is Winn and McCarter (forthcoming). Their experiments differed from Kitchens and Roomets (2015) in that the parties incurred costs for delayed assembly; inefficient assembly could occur through eminent domain, and the court costs of eminent domain were determined endogenously. If the buyer invoked eminent domain, then the buyer and seller spent money in a contest to influence the property's price in their favor. The key finding of Winn and McCarter is that, while eminent domain saved surplus by avoiding costly delay, it wasted an equivalent amount in litigation costs.

Thus, both extant experimental studies of eminent domain find that it fails to increase social welfare. Motivated by this failure, we turn our attention to alternative mechanisms to reduce holdout and facilitate land assembly. Over the years, a host of alternatives and adaptations of eminent domain have been proposed and remained untested. Some propose to navigate eminent domain through introducing seller competition (Kominers and Weyl 2011, 2012), while others propose mechanisms to facilitate fair outcomes when invoking eminent domain (Lehavi and Licht 2007; Heller and Hills 2008). In this paper, we focus on mechanisms to elicit the landowner's true reservation value of his property, which may differ from the assessed or market value, using a property tax. This leads us to two proposals in the law and economics literature: revealed assessment taxation (Miceli, Segerson, and Sirmans 2008) and declared assessment taxation (Plassmann and Tideman 2008).

We use laboratory experiments as a "policy wind tunnel" to test the effectiveness of revealed assessment and declared assessment taxation. We find that both mechanisms increase the rate of land assembly, despite being less effective at discouraging holdout than theory predicts. Both mechanisms also increase the gains from trade compared to our control condition, although the difference is statistically significant only for declared assessment.

2. Review of the Tax Regimes

2.1. Revealed-Assessment Taxation

Current tax law in most jurisdictions reassesses a property's taxable value each time it changes ownership. If an owner sells property to a developer at a price greater than he originally paid for it, then the developer will face a higher tax bill than the original owner did for the same piece of land. Thus, reassessment at the purchase price creates a tax wedge that may impede land assembly. Moreover, if a developer submits an offer in excess of the market value but less than the owner's reservation value, then the owner's incentive is to reject it. This rejection deprives the government of additional tax revenues and provides an incentive to use eminent domain.

The revealed-assessment mechanism proposed by Miceli, Segerson, and Sirmans (hereafter MSS) seeks to avoid such "tax-motivated takings" by taxing the owner's true value. Should an owner refuse a good faith offer, he is signaling that his reservation value is greater than the offer. Under the revealed-assessment mechanism, the government takes the rejected offer as a lower bound for the owner's true value and uses it as the new taxable basis for the property. An owner who rejects an offer greater than the property's assessed value will see property taxes increase. By incorporating this information into the owner's tax burden, the revealed-assessment mechanism eliminates the tax wedge that exists under the status quo.

We first describe the case of a single property owner and then extend the analysis to multiple owners. Note that the revealed-assessment mechanism is theoretically efficient only when there are homogeneous reservation values. This is a key limitation of the mechanism because, in practice, it is unlikely that all owners would value their property identically.

Consider the case of a single property owner with reservation value r and assessed value a. Let t be the tax rate so that ta is the owner's tax burden. Suppose a developer values the property at $V > a$ and seeks to acquire the property. The developer offers (or bids) $\beta > a$ for the property. The developer's maximum bid, accounting for taxes, is given by

$$\beta \leq V - t\beta \text{ or } \beta \leq V/(1+t). \tag{1}$$

The minimum bid the owner is willing to accept is given by

$$\beta \geq r - ta. \tag{2}$$

Per MSS, these equations suggest a consensual sale will occur if and only if

$$V \geq r + t(\beta - a) \tag{3}$$

where $t(\beta - a)$ is the tax wedge created by reassessing the property value when the property changes hands. As $\beta - a > 0$ and $t > 0$ by assumption, there exists a range of values, $(r, r + t(\beta - a))$, for which the owner will reject efficient offers (i.e., offers where the developer's value is greater than the owner's reservation value).

Suppose the owner does, indeed, reject the offer. In this situation, the revealed-assessment mechanism allows the government to reassess the value of the property as the rejected offer, β. The minimum bid the owner should now be willing to accept is given by

$$\beta \geq r - t\beta \text{ or } \beta \geq r/(1+t). \tag{4}$$

Thus, a consensual sale will now occur if $V \geq r$; that is, the sale is efficient.

MSS extend this mechanism to situations in which there are $N > 1$ owners with identical reservation values, r. They assume a fixed amount of government spending, G, which determines the tax rate, $t_0 = G/(Na)$. Suppose a developer seeks to acquire $n \leq N$ properties. MSS show that an owner should accept the developer's offer provided

$$\frac{V}{n} \geq r + G\left[\frac{\beta}{n\beta + (N-n)a} - \frac{1}{N}\right]. \tag{5}$$

As in the single-owner case, equation (5) implies the existence of a range of values for which an efficient assembly of the land may be rejected by one or more owners. However, allowing the government to reassess any property's value equal to the rejected offer implies that owners should accept the developer's offer if and only if $V/n \geq r$, which is the condition for efficiency.

As noted, the primary drawback to this approach is the assumption that all owners have identical reservation values. MSS show that if owners have heterogeneous values, then the condition for efficient sales is that V/n must be greater than or equal to the average of the owners' true values. However, an owner with a reservation value greater than the average may still be inclined not to accept an offer even when faced with an increased tax burden.

2.2. Declared-Assessment Taxation

In situations where owners face a nonzero probability of a developer seeking to acquire their property, Plassmann and Tideman (PT) propose a mechanism in which the owner must declare to the government a price at which he will voluntarily sell his property. That is, if a developer offers the owner this price, he is obligated to sell. PT assume that the probability that a developer will wish to buy an owner's land falls as the declared price increases. They also implicitly assume that the declared prices of one's neighbors do not affect this probability. Each owner is assessed a property tax that is an increasing function of his declared price.

To decrease his probability of selling, the owner may report a value above his true value. However, the owner would then face a higher tax bill. Conversely, suppose the owner reports a value less than his true value to reduce his tax burden. This would make that property more appealing to a developer, increasing the probability that the developer will make an offer on the property, an offer the owner cannot refuse.

PT show that reporting one's true value maximizes one's expected utility under certain assumptions. Let x be the owner's declared price and r be his true reservation value. Let $p(x)$ represent the probability a developer purchases the owner's property. As noted, we assume $p(x)$ is decreasing in x; that is, $dp(x)/dx \leq 0$. Let V denote the developer's value and $T(x)$ denote the owner's tax assessment. Finally, assume the owner is risk averse with an increasing, concave utility function $U(\pi)$, where π represents the owner's profit, given by

$$\pi = \begin{cases} x - T(x), & if\ the\ owner\ sells \\ r - T(x), & otherwise. \end{cases}$$

The owner's expected utility is, therefore,

$$E[U(\pi)] = p(x)U\big(x - T(x)\big) + \big(1 - p(x)\big)U\big(r - T(x)\big). \tag{6}$$

This function is maximized at $x = r$, provided the government sets the marginal tax rate, $dT(x)/dx$, equal to the probability that a developer will buy the property, $p(x)$.[1] To see that this is the case, note that the first-order condition implied by (1) is

$$dE[U(\pi)]/dx = (dp(x)/dx)\big[U\big(x - T(x)\big) - U\big(r - T(x)\big)\big]$$

$$-U'\big(r - T(x)\big)(dT(x)/dx)\big(1 - p(x)\big)$$

$$+U'\big(x - T(x)\big)(1 - dT(x)/dx)p(x) = 0. \tag{7}$$

Setting $dT(x)/dx = p(x)$ simplifies the first-order condition (7) into

$$dE[U(\pi)]/dx = (dp(x)/dx)\big[U\big(x - T(x)\big) - U\big(r - T(x)\big)\big]$$

$$+[p(x) - p(x)^2]\big[U'\big(x - T(x)\big) - U'\big(r - T(x)\big)\big] = 0. \tag{8}$$

[1] As PT note, if owners who declare a value of zero do not pay any tax (i.e., $T(0) = 0$), then the constant of integration would be set to zero when solving for $T(x)$.

Notice that if the owner declares a price $x > r$, then $dE[U(\pi)]/dx < 0$, and if he declares a price $x < r$, then $dE[U(\pi)]/dx > 0$. Therefore, a seller can only maximize his expected utility by declaring his true reservation value.

This mechanism may be applied to environments in which there is one developer and multiple owners with heterogeneous reservation values. Because the owners cannot reject offers, this approach solves the holdout problem. Moreover, all the properties will be acquired only if the developer's value for the entire project, V, is greater than the sum of the owners' declared prices. Assuming the owners maximize their utilities by declaring their true values, then only efficient projects will proceed.

In a practical setting with multiple owners, it is probably not realistic to assume that each owner's probability of selling depends on only his declared price. Rather, it is likely that one owner's probability is dependent on not only his declared price but also the declared prices of nearby landowners. A straightforward extension of the declared-assessment mechanism is possible provided that each owner's probability of selling and tax burden is allowed to be dependent on the declared prices of all owners.[2]

Extending the model in this way does not alter the central result that the owners' dominant strategy is to truthfully declare their reservation values. However, the interdependence of the owners' tax assessments may lead to situations in which cooperative strategies emerge. Suppose there are two owners, i and j, with reservation values r_i and r_j. Further, suppose a developer desires these properties and her value for the assembled parcels, V, is drawn from a uniform distribution over the interval $[A, B]$ similar to our experimental design (see section 3.1).

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[2] Note that a key assumption in this mechanism is that the government is able to accurately gauge (or at least convince the owners that it can accurately gauge) the probability a developer will seek to acquire an owner's property and then set the property tax rate appropriately.

Then the probability that the sum of the owners' declared prices, x_i and x_j, is less than V is given by $(B - x_i - x_j)/(B - A)$. Owner i's tax burden is, therefore, given by

$$T(x_i; x_j) = \int \frac{B - x_i - x_j}{B-A} = \frac{1}{B-A}\left[(B - x_j)\, x_i - \frac{x_i^2}{2}\right], \tag{9}$$

where owner j's declared price is treated as a constant.[3] Note that if owner j increases his declared price, x_j, then not only does owner i's probability of sale decrease, but his tax burden decreases as well. Similarly, if owner i increases his declared price, then owner j's probability of sale and tax burden will both decrease. Thus, cooperative strategies may exist and will depend on the distribution from which the developer's value is drawn.

2.3. Declared Assessment versus Revealed Assessment in Theory and Practice

In theory, declared assessment appears to be the stronger mechanism because it incentivizes truthful value revelation under both homogeneous and heterogeneous value conditions. In practice, however, revealed assessment could be more effective at discouraging holdout for two reasons. First, as noted in section 2.2, in most practical applications of declared assessment, the tax burdens of the landowners will be interdependent, allowing for cooperative strategies in which collective holdout enhances the expected utilities of all neighbors. Truthfully revealing one's reservation value may remain the dominant strategy, but laboratory experiments have repeatedly shown that agents practice higher levels of cooperative play than would be predicted by theory (Dawes and Thaler 1988; McKelvey and Palfrey 1993; Cooper et al. 1996; McCabe and Smith 2000). Cooperation is particularly likely among agents who can

[3] The tax burden for owner j is similarly derived by integrating the probability function with respect to x_j instead of x_i.

communicate with one another. Given the sustained close proximity of landowners, one would expect significant communication in any real-world case of land assembly.

Second, MSS implicitly assume that a developer will offer the same price to all landowners because their reservation values are private and she cannot tell which among them have values above or below the average. In a real-world setting, the developer may attempt to price discriminate during the bargaining process by initially offering low prices to the sellers and raising offers to those who have refused. If the landowners' tax assessments are based on the highest rejected offer, then there is a financial risk to rejecting any offer greater than or equal to their reservation value. This risk may be effective at discouraging holdout during the bargaining process.

3. Experiment Design

The treatment design is shown in table 1 (p. 36). We conducted experiments for one control treatment (*Baseline*) and two experimental treatments corresponding to the revealed-assessment (*Revealed*) and declared-assessment (*Declared*) mechanisms. For each treatment, we ran three sessions of experiments with ten negotiations per session, giving us thirty negotiations per treatment. Below, we describe the design elements common to all three treatments. In subsequent subsections, we describe the design elements unique to each treatment.

3.1. Common Design Elements

In every experiment, participants were partitioned into groups with one developer (called the buyer) and four landowners (called the sellers). The sellers were assigned values for their property denominated in Economic Currency Units (ECU), with an exchange rate of 16,500

ECU to one US dollar. The buyer was assigned a value for the combination of all four properties, with an exchange rate of 60,000 ECU to one US dollar. These exchange rates were private information. The participants saw their exchange rates on their own private computer screens, but we instructed them that different participants could have different exchange rates. For simplicity, on the participants' screens we used "$" to represent ECU, a convention we follow in this paper unless otherwise stated.

The sellers' reservation values were drawn independently with replacement from a discrete uniform distribution with support ($100,000, $150,000) and rounded to the nearest thousand. The buyer's value was a number drawn at random from the discrete uniform distribution ($300,000, $1,250,000) and rounded to the nearest thousand. Both the buyer's and sellers' values were private information, but the distributions from which they were drawn were common knowledge.[4]

The properties all began with an assessed value of $100,000, the lower bound of the seller's value distribution. This simulates a land market in which the equilibrium price is $100,000. All sellers' values were greater than or equal to this equilibrium price because a seller with a property worth less to him than the equilibrium price would have already sold it. The assessed value was the basis for calculating property taxes, which varied in each treatment.

In addition to a value for assembling the four properties, the buyer was endowed with $350,000 cash. The cash was not a limit on the offers she could make to the sellers; it served as an opportunity cost of negotiating. The sellers knew the buyer would have a cash endowment, but they were not told the exact amount. If the buyer chose not to attempt assembly, she received

[4] The lower bound of the buyer's value distribution was less than four times the upper bound of the sellers' value distribution, so that there was a nonzero probability that assembling the land would be inefficient. This design element was necessary to ensure that under declared assessment $dP(x_i)/x_i < 0$ for x_i within the range of the sellers' value distribution.

the cash as her earnings. If the buyer did negotiate with the sellers, then her earnings depended on how many properties she was able to acquire.

A buyer who assembled all four properties earned her value plus the cash endowment minus the sum of the prices she offered the sellers minus property tax. Even if a buyer assembled fewer than four properties, she was still bound to pay the sellers who had accepted her offers, but she was also able to sell the properties back to the experimenter at the equilibrium price of $100,000. Thus a buyer who assembled fewer than four properties earned her cash minus the sum of her accepted offers plus $100,000 times the number of properties she had purchased.

The sellers' earnings depended on whether they sold their property and the tax regime. A seller who sold his property to the buyer earned the price at which he sold (minus tax in the *Declared* treatment). A seller who did not sell his property earned his value for the property (minus tax in all treatments).

We conducted three sessions of experiments for each treatment. In every session, we recruited enough participants for five groups to negotiate simultaneously. After the first negotiation, we partitioned the participants into new groups of five. No participants who had been grouped together in the first negotiation were grouped together in the second. We followed this regrouping procedure to prevent participants from engaging in repeated game strategies. Participants had the same role—buyer or seller—in both negotiations. This design gives us observations from thirty independent negotiations in each treatment.

Before conducting any experiments, we randomly generated buyer and seller values for thirty negotiations. To make comparisons across treatments as accurate as possible, we used the same value draws for every treatment. These value draws are displayed in table 2 (p. 36). Of the thirty negotiations, assembly was efficient in twenty-four (80 percent). We refer to these as the "positive-sum" negotiations. In the remaining six negotiations (20 percent), the properties were more valuable in the hands of the sellers. The available gains from trade in the positive-sum negotiations ranged between $5,000 and $765,000, with an average of $379,000.

3.2. Design Elements of the **Baseline** *Treatment*

In the *Baseline*, property taxes were calculated as 10 percent of a property's assessed value, and the tax was paid by whichever party owned the property at the end of negotiations. All properties began with a $100,000 assessed value, so if a seller refused to sell by the end of the negotiations, he paid $10,000 in taxes. In the case of a sale, the property was reassessed at the contract price. A buyer who assembled all four properties paid a property tax of 10 percent of these prices to the experimenter. A buyer who assembled fewer properties was assumed to have sold the properties to the experimenter before property taxes were due, and so was not liable for tax.

The experiments were facilitated by a graphical software interface. Both the buyer's and sellers' screens displayed a grid of four properties, numbered 1 through 4, with a house icon. On each seller's screen, one of the grid numbers was highlighted yellow to indicate which property belonged to him. All participants' screens also displayed their values, the distributions from which the buyer's and sellers' values were drawn, and their private exchange rate. The buyer's screen also displayed her cash and earnings multiplier (discussed later).

Negotiations proceeded in a series of up to five rounds. Each round consisted of a series of phases. In phase 1, the sellers were given time to communicate with one another via a text chat function on their computer screens. (In the first round, phase 1 lasted for seven minutes; in subsequent rounds, it lasted for two minutes.) Bilateral private messages were not allowed; if a seller sent a text message, all the other sellers could see it. The only restriction placed on the sellers' communication was a warning not to use profanity or threats or to disclose personally identifying information. Once the time for communication had elapsed, the sellers were prompted to send a price request to the buyer. They did so by clicking a button marked "Send Price Request" and entering their request into a pop-up screen. The request was not binding on the buyer or the seller. Rather, it was a means by which the sellers could communicate the offer price they would like to receive from the buyer. Text communication was not allowed between the buyer and sellers, to simulate the asymmetric ease of communicating with one's neighbors compared to communicating with a developer.

In phase 2, the buyer observed the sellers' price requests and decided whether to abandon the negotiations or send offer prices to the sellers. If she chose to abandon the negotiations, a pop-up screen informed the buyer of what her earnings would be and asked her to confirm the decision. If the buyer chose to send offers, a pop-up screen informed her of the maximum and minimum earnings she could receive and asked her to confirm the offers. The buyer's offers were binding and were sent to all sellers simultaneously. The buyer was allowed to offer different amounts to different sellers. To prevent bankruptcy, the software did not allow a buyer to make a set of offers that summed to more than her value for the properties plus her cash.

In phase 3, the sellers observed the offers and decided whether to accept or reject them. They had one minute to communicate with one another, after which the chat function was disabled and they were prompted to click one of two buttons, marked "Accept" and "Reject." Two hyperlinks on the screen displayed the earnings the seller would receive from each decision. Clicking on a hyperlink brought up a pop-up box that displayed how the earnings from that decision were calculated.

If a seller rejected the offer, then negotiations proceeded to phase 1 of the next round. If a seller accepted the offer, the house icon on his property turned green and the price he had accepted appeared below it. (This was shown on all group members' screens.) A seller who accepted an offer was also not allowed to chat with the other sellers or see their text messages in any subsequent rounds. However, he could click a button on his interface that would launch Microsoft Paint. We included this feature so sellers would not prolong negotiations to prevent boredom.

Prolonged negotiations were costly to the buyer but not to the sellers. This cost was represented as a multiplier that would be applied to the buyer's earnings once negotiations concluded. If the negotiations concluded (or were abandoned) in round 1, the multiplier was 100 percent. The multiplier was reduced by five percentage points for each additional round of negotiations: to 95 percent in round 2, 90 percent in round 3, and so forth. If the buyer failed to assemble the properties (and did not abandon the negotiations) in round 5, then the multiplier was 75 percent.

3.3. Design Elements of the **Revealed** *Treatment*

The *Revealed* treatment was identical to the *Baseline* except for a single change to the calculation of taxes. The sellers' properties were initially assessed at $100,000 with a 10 percent property tax. However, if a seller did not sell his property during the experiment, it was reassessed at a value equal to the maximum offer he had rejected. For instance, if a seller rejected offers of $120,000, $135,000, and $130,000 in rounds 1 through 3 and the buyer then abandoned negotiations, the seller's property would be reassessed at $135,000. The seller would then owe $13,500 in property taxes.

Recall from the theoretical model of the revealed-assessment mechanism in section 2.1 that heterogeneous seller values may prevent efficient assembly even without strategic holdout. Given that we induced our sellers with heterogeneous values, these experiments may be considered a stress test of revealed assessment.

3.4. Design Elements of the **Declared** *Treatment*

In the *Declared* treatment, there were no offers from the buyer. The sellers simply declared a price for their property and the buyer bought the properties if the sum of declared prices ("total price") was less than her value. Because there was no role for strategic decision making in the buyer's role, we replaced the human buyers with robot buyers. We informed the sellers that a value had been selected for the buyer in their group and that the computer software would compare their total price to that value to determine whether they would sell their properties.

The sellers' screens in the *Declared* treatment were the same as in the *Baseline*, with two exceptions. First, the "Send Price Request" button was relabeled "Send Price Declaration." Also, we provided a graphing tool to allow sellers to visualize their taxes and expected earnings as a function of their price declaration. A seller could select the tax function or expected earnings function from a drop-down menu and enter the prices he expected the other three members of his group to declare in text boxes, then click a button marked "Generate." The tool generated a line graph with the taxes/expected earnings on the y-axis and the declaration range on the x-axis. The subjects could select regions of the graph to zoom in on. They could also mouse over a point on the curve to see the exact taxes/expected earnings that would occur from a specific declaration.

Following PT, we set a seller's tax function equal to the integral of the probability function that the buyer would choose to purchase his property, based on his declaration and the declarations of his neighbors. The probability that the robot buyer would purchase a seller's property was equal to the probability that its value draw exceeded the four sellers' declarations. We denote the lower and upper bounds of the buyer's value distribution with A and B and the sum of the declarations of the sellers other than i with X_{-i}. The probability that seller i's property will sell is given by

$$p_i(x_i) = \frac{B - x_i - X_{-i}}{B - A}. \tag{10}$$

Taking the integral of (10) and setting the constant term to zero, we have the tax function

$$T_i(x_i) = \frac{Bx_i - \frac{x_i^2}{2} - x_i X_{-i}}{B - A}. \tag{11}$$

Tax function (11) is not wholly satisfactory because for sufficiently high x_i it is decreasing and can even assess a negative tax.[5] We addressed this by defining a maximum declaration that was dependent on the declarations of the other sellers: $x_i^* = B - X_{-i}$. If seller i submitted a declaration $x_i > x_i^*$, then we used x_i^* to calculate his tax (although x_i was still used for the total price).

The sellers received their declarations if the buyer purchased the properties and received their value otherwise. In either case, they paid the assessment tax. In our experiments, each seller's declaration could have a large impact on the probability of assembly, so the equilibrium assessment taxes were very high. For instance, in a group where all four sellers' values were \$125,000 and they truthfully declared these values, the assessment tax would be \$93,750 per seller, so that each would earn \$31,250.

[5] If $X_{-i} < A$, then the probability that the buyer would purchase the properties is equal to 1, and the integral of this region of the probability function would imply a tax equal to x_i. In this case the tax would exactly equal the price the seller was paid for his property, leaving him with a payoff of 0. Thus, the sellers would have no incentive to collusively understate their values. To keep the description of the tax function to the participants as simple as possible, we applied function (10) regardless of the declarations. Function (11) always returns a tax greater than x_i for $X_{-i} < A$, so participants still had no incentive to collusively understate their values.

Notice, however, that tax function (11) is decreasing in X_{-i}, which created room for tax avoidance through collusive holdout. In the extreme, if $X_{-i} \geq B$, then seller i would pay no tax because the probability of assembly would be zero regardless of his declaration. Thus, if each of the four sellers agreed to declare a price equal to one-third of the upper bound of the buyer's value distribution, then the sellers would all keep their property and pay no tax, capturing the full consumption value of their land: $125,000. This would be 300 percent higher than their earnings from truthfully declaring their values. The high potential earnings from collusion make these experiments a formidable stress test of the declared-assessment mechanism.[6]

To allow the subjects to earn the same amount as in the other treatments while keeping their exchange rates the same, we made the tax revenue neutral by redistributing all assessment taxes back to the sellers in the form of a bonus. Specifically, the taxes from one group were divided evenly and paid to the sellers of another group. Communication across groups was not allowed, so the bonus payments were exogenous from the recipient's perspective.

Negotiations in the *Declared* treatment lasted for a single round. For the first twenty minutes of the round, the sellers were allowed to communicate with one another through free-form text, as in the *Baseline*.[7] The content of this communication was saved to a chat log. During this time, the sellers could also explore the tax and expected earnings functions using the graphing tool. After the twenty minutes had elapsed, they were prompted to submit a price declaration. Once all sellers had submitted their price declarations, every seller's declaration was

[6] In our experiments, the cooperation of only four sellers was necessary for successful collusion. It is possible that collusion would be more difficult to achieve for a large number of sellers. Thus, our design stress tests the declared assessment mechanism not only in the incentives to collude but also in the ease of doing so.

[7] Note that in the *Baseline* and *Revealed* treatments the sellers could also communicate for a maximum of twenty minutes if the negotiations took the full five rounds: eight minutes in round 1 (seven minutes in phase 1, one minute in phase 3) and three minutes in each subsequent round (two minutes in phase 1, one minute in phase 3).

publicly displayed below his house. If the buyer's value exceeded the total price, then every house icon turned green as well.

3.5. Procedures

All experiments were conducted at a university in the American Southwest. We randomly recruited a total of 210 participants (75 each for *Baseline* and *Revealed*, 60 for *Declared*) from an online database of approximately 1,500 volunteers. All participants were undergraduate or graduate students, and none participated in more than one session.

Before the experiment, participants were ushered into a computer laboratory and seated at stations separated by privacy dividers. One of the experimenters read the instructions aloud from a script, pausing at predetermined points to answer questions. Screenshots of the user interface were projected on the screen at the front of the room.[8] When the instructions were complete, a one-page summary sheet of the instructions was distributed.

After the first negotiation, an experimenter entered the lab to remind participants that in the second negotiation they would be put in new groups that did not have any of their counterparts from the first negotiation. When the second negotiation was complete, we paid the participants in cash, one by one. They received $7 for attending the experiment in addition to payment based on their decisions. The average participant earnings were $23.89 including the bonus. *Baseline* and *Revealed* sessions typically lasted two and a half hours; *Declared* sessions lasted 90 minutes.

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[8] In the *Declared* sessions, the instructions included a discussion of expected value so participants would understand the purpose of the graphing tool. Participants also viewed two short video clips on how to use the graphing tool.

4. Hypotheses

We test five main hypotheses, each of which is proposed as a null hypothesis, that tax policies proposed by PT (2008) and MSS (2008) are ineffective. The first two hypotheses address whether revealed assessment and declared assessment can achieve their stated purpose of reducing seller holdout. First, there is the question of whether these tax policies discourage sellers from holding out for any amount.

> *Hypothesis 1a* (*H1a*): The frequency of seller holdout will be the same in the *Baseline* and *Revealed* treatments.

> *Hypothesis 1b* (*H1b*): The frequency of seller holdout will be the same in the *Baseline* and *Declared* treatments.

Second, even if the same proportion of sellers hold out in one of the experimental treatments, as in the *Baseline*, it is possible they will hold out for smaller amounts of money. This leads to our second hypothesis:

> *Hypothesis 2a* (*H2a*): The magnitude of seller holdout will be the same in the *Baseline* and *Revealed* treatments.

> *Hypothesis 2b* (*H2b*): The magnitude of seller holdout will be the same in the *Baseline* and *Declared* treatments.

Third, a reduction in seller holdout may also lead to shorter negotiations in the *Revealed* treatment than in the *Baseline*. By design, the negotiations in *Declared* lasted for only one round, so we do not include it in this hypothesis.

> *Hypothesis 3* (*H3*): Negotiations will last for the same number of rounds in the *Baseline* and *Revealed* treatments.

Fourth, reduced seller holdout should make it easier for buyers to assemble the sellers' properties when it is profitable to do so. Thus, our fourth hypothesis:

> *Hypothesis 4a* (*H4a*): The rate of land assembly will be the same in the *Baseline* and *Revealed* treatments.

Hypothesis 4b (*H4b*): The rate of land assembly will be the same in the *Baseline* and *Declared* treatments.

Finally, higher rates of land assembly should be reflected in a higher amount of surplus captured under the experimental tax policies than under the *Baseline*. This gives us our fifth hypothesis:

Hypothesis 5a (*H5a*): Social welfare will be the same in the *Baseline* and *Revealed* treatments.

Hypothesis 5b (*H5b*): Social welfare will be the same in the *Baseline* and *Declared* treatments.

5. Results

5.1. Holdout Frequency and Magnitude

For the *Baseline* and *Revealed* treatments, we measured seller holdout by subtracting each seller's tax-adjusted value from the highest offer the seller rejected. For *Declared*, we subtracted the sellers' values from their price declarations. Positive differences indicate holdout, and the value of the difference is our measure of the magnitude of holdout. In figure 1 (p. 40), we show the empirical cumulative density functions for these differences in each treatment.

Revealed assessment did not reduce the frequency of holdout, but it did reduce the amount for which sellers held out. In the *Baseline*, 48.3 percent of sellers rejected an offer that exceeded their tax-adjusted value; in the *Revealed* treatment, it was 51.7 percent. However, among those sellers who did hold out, the average difference between the highest rejected offer and the seller's value was $53,701.74 in the *Baseline* and $26,453.76 in *Revealed*, a 48.1 percent reduction.

Declared assessment was completely ineffective at eliciting truthful value revelations from the sellers. In fact, compared to the *Baseline*, it was counterproductive. Of the sellers in

Declared, 94.2 percent submitted a declaration that was greater than their value. The average holdout was for \$73,607.08, but this value is skewed by a single group in which every seller asked for \$1 million or more. If that group is omitted, the average holdout was for \$41,538.44, somewhat less than in the *Baseline*.

To test for treatment effects in holdout frequency, we used a logistic regression with a binary dependent variable indicating whether a seller had held out (1 = yes, 0 = no). We tested for treatment effects on holdout magnitude with an ordinary least squares (OLS) regression. We included the same independent variables in both models: dummy variables indicating whether the seller was in the *Revealed* or *Declared* treatment, the negotiation number, and the seller's value (tax adjusted in the *Baseline* and *Revealed* treatments). Sellers likely influenced one another's decisions, so we clustered the standard errors by group in both models. The estimates are displayed in table 3 (p. 37).

The models confirm that revealed assessment had no effect on holdout frequency but did reduce holdout magnitude. In the logistic regression, the estimated coefficient for *Revealed* is not statistically significant ($p = 0.621$), so we are unable to reject H1a. The OLS regression estimates that the magnitude of holdout was \$32,510 less in the *Revealed* treatment than in the *Baseline*. The coefficient is statistically significant at the 5 percent level ($p = 0.025$), so we reject H2a. Relative to the estimated constant of \$96,546, this represents a reduction in holdout of 33.7 percent. Thus, revealed assessment had an economically significant impact on holdout as well as a statistically significant one.

The estimated coefficient for *Declared* is positive and highly statistically significant ($p < 0.001$) in our logistic regression. Thus we reject H1b, but in the opposite direction than predicted by theory. The effect size is quite large. If we assume a seller with a value of \$125,000 in the first negotiation, the model estimates a 32.6 percent probability of holdout in the *Baseline* compared to a 92.9 percent probability in *Declared*, an almost threefold increase.

Why did declared assessment fail to elicit truthful value declarations from the participants? Collusive holdout could increase participants' profits primarily because it allowed them to mutually reduce their tax burdens. However, a review of the chat logs in the *Declared* sessions indicates that very few sellers fully understood this. Participants discussed the tax benefits of collusion in only three of the thirty groups, and only one of these realized they could pay zero taxes by holding out for more than the buyer's maximum value. Instead, most sellers seemed to take it for granted that the purpose of holding out was to sell at a price above their values, and they sought to avoid declaring prices so high the buyer would reject their offers. The most common strategy by far was to coordinate a total price less than the buyer's expected value, so the chances of success exceeded 50 percent. Consequently, sellers in the *Declared* treatment did not hold out for greater amounts than in the *Baseline.* In our OLS model, the estimated coefficient for *Declared* is positive but not statistically significant (p = 0.446). We cannot reject H2b.

5.2. Delay

Revealed assessment had no impact on delay in negotiations. We count a negotiation in which a seller rejected his offer in the fifth round as lasting for six rounds. The average negotiation took 3.8 rounds in the *Baseline* and 3.83 rounds in *Revealed*. If we exclude negotiations where assembly would have been inefficient, the average negotiation length was 4.33 rounds in the *Baseline* and 4.17 in *Revealed*. We fit the data with an OLS regression using the number of rounds as the independent variable. The independent variables were a treatment dummy for *Revealed*, the negotiation number, and a dummy indicating whether assembly would have been efficient. The coefficient for *Revealed* was statistically insignificant ($p = 0.928$). We cannot reject H3, that there was no difference in delay between the *Baseline* and *Revealed* treatments.

5.3. Assembly

Thus far, both tax regimes proved to be disappointing in outperforming the *Baseline*. The only null hypothesis we were able to reject in the intended direction was H2a, that sellers would hold out for equal amounts in the *Baseline* and *Revealed* treatments. Nevertheless, buyers were more successful at assembling properties in *Revealed* and *Declared* than in the *Baseline*.

Figure 2 (p. 40) displays the rate of assembly by treatment in the positive-sum negotiations. In the *Baseline*, buyers were able to acquire all four properties in 50 percent of these negotiations. In the *Revealed* treatment, the assembly rate was 79.2 percent, a 58.4 percent increase relative to the *Baseline*. In the *Declared* treatment, 83.3 percent of the positive-sum negotiations were successful, an increase of 66.7 percent. We tested for statistical significance with a logistic regression. The independent variable was a dummy indicating assembly (1 = success, 0 = failure). The independent variables were dummy variables for the *Revealed* and *Declared* treatments, the negotiation number, and the amount of available surplus (scaled in $10,000s). We limited the dataset to those observations where the available surplus was positive. The results are displayed in table 4 (p. 37).

The estimated coefficients for *Revealed* and *Declared* are both positive and statistically significant at 5 percent ($p = 0.029$ and $p = 0.013$). We therefore reject H4a and H4b. The effects of the experimental tax regimes are also economically significant. If we set the available surplus to the mean observed ($379,000) and assume the participants are in their first negotiation, then the model estimates the *Baseline* probability of assembly at 51.6 percent. The estimated probabilities for *Revealed* and *Declared* are 83.3 percent and 87.2 percent, respectively. These are increases of 61.4 percent and 69 percent relative to the *Baseline*. A Wald test cannot reject the null hypothesis that the estimated coefficients of *Revealed* and *Declared* are equal. In our experiments, the two tax regimes were equally effective at promoting land assembly.

The model also indicates that assembly was more likely in negotiations in which more surplus was available. The estimated coefficient for the available surplus is positive and statistically significant at the 1 percent level ($p = 0.008$). This finding is encouraging because it indicates that holdout is less of a barrier when the development in question is of substantial value. However, the marginal effect is not large. If we parameterize the model, then an increase in available surplus of $10,000 increases the probability of assembly by 1 percentage point in the *Baseline*, 0.5 percentage points in *Revealed*, and 0.4 percentage points in *Declared*.

The fact that assembly rates were so much higher in the *Revealed* and *Declared* treatments than in the *Baseline* is puzzling in light of their mixed performance at reducing holdout. The answer to the puzzle appears to be that buyers were more likely to become discouraged in the *Baseline* and end negotiations early. A buyer could ensure it was in a seller's financial interest to sell by making an offer just above the upper bound of his value distribution minus property taxes. This would be an offer above $140,000 in the *Baseline* and above $136,363 in *Revealed*. Sellers in the *Baseline* rejected 65.5 percent of such offers; sellers in *Revealed* rejected 51.7 percent of the time. Moreover, the average price request from sellers was approximately $199,000 in the *Baseline*, compared to $164,000 in *Revealed*. In the face of more recalcitrant sellers, buyers in the *Baseline* quit negotiating early in 25 percent of the positive-sum negotiations. Buyers in the *Revealed* treatment quit early in only 4.2 percent of these negotiations.

In the *Declared* treatment, buyer discouragement could not affect assembly. The robot buyer simply compared the total price to its value and accepted or rejected it. Despite the fact that more sellers held out in the *Declared* treatment than in the *Baseline*, the amounts for which they held out were typically not enough to make the total price exceed the buyer's value when there were gains from trade available. As described earlier, the average seller in *Declared* held out for about $42,000 (if one outlier group is excluded). This implies that the average group held out for an extra $168,000 of the surplus. In the positive-sum negotiations, 83.3 percent had more than $168,000 of available surplus, so this level of holdout did not thwart many developments.

5.4. Social Welfare

The customary measurement of social welfare in laboratory experiments is efficiency: the earnings participants received divided by the maximum earnings they could have received. However, this measure has two shortcomings for these experiments. First, participants were guaranteed to earn a certain amount regardless of their decisions, which inflates the measure. Second, the negotiations varied in the gains from trade that were available. Capturing 80 percent of $1 million generates more social welfare than capturing 100 percent of $10,000, but dividing the achieved earnings by the maximum earnings treats the latter as a better outcome.

We measure the social welfare as the gains from trade, or surplus, captured in the negotiation. To calculate the surplus, we subtracted the earnings participants would have received if the buyer had never made an offer from earnings at the end of the negotiation. (In the *Declared* treatment, we include the robot buyer's earnings in the calculated surplus so the measure is equivalent across treatments.) Across thirty negotiations, there was a maximum available surplus of $9,096,000. Figure 3 (p. 41) shows the surplus that was actually achieved in each treatment.

In the *Baseline*, the participants gained approximately \$3.7 million through their negotiations, 41.1 percent of the available surplus. Surplus was 22.7 percent higher in the *Revealed* treatment, where participants captured about \$4.6 million, or 50.2 percent of the available surplus. It was in the *Declared* treatment, however, that social welfare was highest. Participants in *Declared* improved their earnings by almost \$8.4 million, or 92.3 percent of the available surplus. This is an increase in social welfare of 124 percent relative to the *Baseline*.

We tested for the statistical significance of these results with an OLS regression. The variance of available surplus was large across negotiations. The minimum available surplus was \$200,000; the maximum was \$765,000. To remove this noise from the data, for each negotiation we normalized the surplus by subtracting out the surplus that had been captured in the *Baseline*. We used these differences as the dependent variable in our regression and included treatment dummies and the negotiation number as the independent variables. The estimates are displayed in table 5 (p. 38).

The model estimates that participants in *Revealed* captured \$27,322 more per negotiation than in the *Baseline*, but the effect is not statistically significant (p = 0.508). This estimate is somewhat skewed by one negotiation in which the difference in surplus was more than three standard deviations below the mean. Even if we omit this datum as an outlier, the effect size is not statistically significant (p = 0.190). Thus we cannot reject H5a.

This null result is surprising given that the rate of assembly was so much higher in *Revealed* than in the *Baseline*. This is explained by the fact that the extra assemblies in the *Revealed* treatment were in negotiations with less available surplus. Table 6 (p. 38) displays the available gains from assembly for each negotiation and indicates whether assembly was achieved in each of the three treatments. The negotiations are rank-ordered from highest available gains from assembly to lowest. Note that assembly never occurred in the six negative-sum negotiations. If we look at the 50 percent of positive-sum negotiations with the highest available surplus (rows 1–12 in table 6), buyers were successful in two-thirds of them in both the *Baseline* and *Revealed*. Among the remaining positive-sum negotiations, the assembly rate was 33.3 percent in the *Baseline* and 91.7 percent in *Revealed*. Thus the extra assemblies did not translate into a statistically significant increase in social welfare.

The model estimates that in the *Declared* treatment, participants generated an extra $155,088 per negotiation relative to the *Baseline*. The estimate is statistically significant at the 0.1 percent level ($p < 0.001$). We can reject H5b. The large gain in surplus relative to the *Baseline* has two major causes. First, the extra assembly in the *Declared* treatment was not limited to negotiations with low available surplus. In the top half of negotiations by available surplus, the assembly rate was 91.7 percent in *Declared*. In the bottom half, the assembly rate was 75 percent.

Second, the fact that all negotiations lasted for a single round in *Declared* meant there was no loss of surplus from delay. In the *Baseline* negotiations that were successful, the cost of delay reduced the achieved surplus by about $1.1 million. In the *Baseline* negotiations that were unsuccessful, the cost of delay resulted in a net loss of slightly more than $1 million. Altogether, the loss of surplus from delay accounts for 46.7 percent of the difference between the *Baseline* and *Declared*.[9]

In addition to total gains from trade, policymakers and stakeholders may be interested in how much surplus was captured by each side of the market. Figure 4 (p. 41) displays the total

[9] Nevertheless, the results are not qualitatively different if we ignore delay costs. We recalculated the achieved surplus as the available surplus if assembly succeeded and zero if it failed and analyzed these figures with the same regression model displayed in table 5. The estimated coefficient was statistically insignificant for the *Revealed* treatment ($p = 0.379$) and positive and statistically significant for the *Declared* treatment ($p = 0.048$).

buyer surplus and seller surplus in each treatment. In the *Baseline*, the buyers captured only about \$208,000, or 5.5 percent of the surplus. This reflects the fact that attempting assembly was quite risky, and buyers often lost money. In fact, the buyer netted a profit in only eight of the negotiations (26.7 percent) and lost money in nineteen (63.3 percent).

Buyers fared much better in the *Revealed* and *Declared* treatments. With revealed assessment, the buyers earned more than \$3 million. They also earned positive profits in fifteen of the negotiations (50 percent) and lost money in twelve (40 percent). In the *Declared* treatment, the buyers earned almost \$5.6 million and never lost money.

The buyers' gains in these treatments came partially at the expense of the sellers. Total seller surplus was approximately \$3.5 million in the *Baseline*, compared to \$1.5 million in the *Revealed* treatment and \$2.8 million in the *Declared*. Thus, landowners may object to the tax reforms. However, there are three reasons our results may provide support for the alternative tax mechanisms. First, the low expected profits to buyers in the *Baseline* treatment may result in reduced gains from trade over time. Our experiments presented the buyers with a static environment in which they were assigned a role and put into negotiations by default. But in a dynamic environment in which developers endogenously decided whether to enter or exit a market and how many resources to invest in pursuing new developments, the low rate of return would likely result in fewer active developers and an atrophy in gains from trade going to both the buyers and the sellers.

Second, the experimental tax treatments resulted in a more equitable distribution of the surplus. The division of surplus in the *Baseline* favored the sellers roughly nineteen to one. In both the *Revealed* and *Declared* treatments, the division favored the buyers approximately two to one. It is also worth noting that in the context of our experiments, the sellers' surplus represents money paid to them not in excess of the market price but in excess of their subjective reservation values.

Finally, regression analyses indicate that the surplus gains to the buyers were statistically significant in the experimental treatments while the reductions in seller surplus were not. We normalized the buyer and seller surplus data as we had the total surplus data, by subtracting the buyer or seller surplus that had been achieved in the equivalent *Baseline* negotiation. We then fit these data to OLS models with treatment dummies and the negotiation number as the independent variables. The models' estimates are displayed in table 7 (p. 39).

The model of buyer surplus estimates that, relative to the *Baseline*, buyers captured \$94,255 more per negotiation in the *Revealed* treatment and \$179,263 more in the *Declared* treatment. These estimates are statistically significant at the 5 percent and 0.1 percent level (p = 0.035 and $p < 0.001$, respectively). The seller surplus model's estimated coefficient for the *Declared* treatment is −\$66,932, but it is not statistically significant (p = 0.158). The estimated coefficient for the *Revealed* treatment is −\$24,174, but it is also statistically insignificant (p = 0.608). Thus, we cannot reject the null hypothesis that sellers were equally well off in the two experimental treatments as they were in the *Baseline*.

6. Discussion

We tested two tax mechanisms for their ability to reduce seller holdout and facilitate land assembly. The results are mixed. Revealed assessment did not reduce the frequency of seller holdout or delay in assembly. However, it cut the average amount of money for which sellers held out almost in half and increased the rate of land assembly by nearly 60 percent. This increased the gains from trade by more than a fifth relative to our *Baseline* condition, but the difference is not statistically significant. Declared assessment almost doubled the frequency of holdout and did not reduce the amount for which sellers held out. Nevertheless, it increased the rate of assembly by two-thirds relative to the *Baseline* condition and increased the gains from trade by 124 percent.

Comparing the performance of the two mechanisms, declared assessment appears to be the clear winner, but implementing it in the field could prove challenging. It would require substantial effort and resources to estimate the sale probability functions for every piece of land in a jurisdiction. Moreover, if landowners in that jurisdiction did not trust that the government had accurately estimated the probability functions, then the mechanism may not be as effective in the field as in our laboratory. There is also the possibility that an assessment tax would be unpopular because it must be paid even if the landowner sells his property during the year.

By comparison, revealed assessment is very simple to implement. It would only require developers to report their offers to the government so that property values could be reassessed. Because the threat of reassessment improves the developer's chance of assembling the properties, it is in their interest to comply with the law. This ease of implementation may make it more attractive to policymakers. Although the gains to social welfare were not statistically significant in our experiments, we did find that revealed assessment increased the profitability of land assembly. This could encourage more investment and competition in development, which may increase social welfare over time. If future research shows this to be true, then revealed assessment would be a simple policy change with a substantial payoff.

In summary, eminent domain is often viewed as a necessary evil to overcome seller holdout when assembling land. In the last decade, scholarship in law and public policy has proposed two tax regimes in place of eminent domain. Our experiments indicate that these alternatives, particularly declared assessment, show promise. The next step for studying these tax mechanisms, in addition to replication, is to go into the field, utilizing empirical data from field and case studies to not only test generalizability but also uncover any nuances these tax mechanisms pose when instituted. In doing so, those interested in eminent domain and its alternatives will benefit from full-cycle modeling of behavioral sciences (Chatman and Flynn 2005).

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Table 1. Treatment Design

Treatment	Sessions	Negotiations per session	Total negotiations
Baseline	3	10	30
Revealed Assessment	3	10	30
Declared Assessment	3	10	30
		Total:	**90**

Table 2. Buyer and Seller Values

			Value in $100,000s						
Session	**Negotiation**	**Group**	**Buyer**	**Seller 1**	**Seller 2**	**Seller 3**	**Seller 4**	**Total seller**	**Gains from assembly**
1	1	1	815	109	147	102	110	468	347
1	1	2	312	135	126	145	106	512	(200)
1	1	3	1,201	125	128	132	101	486	715
1	1	4	363	107	147	132	124	510	(147)
1	1	5	832	146	136	104	109	495	337
1	2	1	1,224	102	119	122	116	459	765
1	2	2	950	114	123	127	147	511	439
1	2	3	816	124	119	108	128	479	337
1	2	4	391	135	100	118	121	474	(83)
1	2	5	663	145	104	144	138	531	132
2	1	1	768	121	102	117	143	483	285
2	1	2	507	142	113	105	142	502	5
2	1	3	828	142	106	123	116	487	341
2	1	4	555	142	136	109	116	503	52
2	1	5	1,051	121	120	102	138	481	570
2	2	1	499	140	129	147	117	533	(34)
2	2	2	524	120	148	124	124	516	8
2	2	3	870	132	147	132	111	522	348
2	2	4	902	103	106	122	126	457	445
2	2	5	1,172	131	112	134	114	491	681
3	1	1	686	100	100	113	128	441	245
3	1	2	870	139	135	145	109	528	342
3	1	3	960	128	125	130	139	522	438
3	1	4	444	106	123	141	122	492	(48)
3	1	5	953	135	125	147	105	512	441
3	2	1	884	144	116	113	109	482	402
3	2	2	417	142	113	150	125	530	(113)
3	2	3	1,144	147	120	135	122	524	620
3	2	4	871	102	148	135	128	513	358
3	2	5	902	139	115	102	103	459	443

Table 3. Estimates on Holdout Frequency and the Amount for Which Sellers Held Out

	Logistic regression on holdout frequency	**OLS regression on holdout magnitude**
Variable	Coefficient (std. err.)	Coefficient (std. err.)
Constant	2.59* (1.19)	96,546* (39,335)
Revealed treatment	0.12 (0.35)	–32,510* (14,224)
Declared treatment	3.01*** (0.71)	32,254 (42,134)
Negotiation number	0.53 (0.34)	41,552 (33,356)
Seller's value (in $10s)	–0.28** (0.10)	–9,466 (7,082)
Observations	336	233
Pseudo R^2	0.187	—
Wald χ^2	23.52	—
R^2	—	0.06
F-Statistic	—	1.65

Source: Authors' analysis.

* Significant at 5 percent.

** Significant at 1 percent.

*** Significant at 0.1 percent.

Table 4. Estimates from Logistic Regression on Successful Assembly of Properties in Negotiations with Positive Available Gains from Trade

Variable	**Coefficient (std. err.)**
Constant	–0.98 (1.08)
Revealed treatment	1.54* (0.71)
Declared treatment	1.85* (0.75)
Negotiation number	–0.47 (0.60)
Available surplus (in $10s)	0.04** (0.02)
Observations	72
Pseudo R^2	0.184
Wald χ^2	15.99

Source: Authors' analysis.

* Significant at 5 percent.

** Significant at 1 percent.

*** Significant at 0.1 percent.

Table 5. Estimates from OLS Regression on Normalized Surplus

Variable	**Coefficient (std. err.)**
Constant	–70,106 (58,177)
Revealed treatment	27,322 (41,138)
Declared treatment	155,089*** (41,138)
Negotiation number	46,738 (33,589)
Observations	90
R^2	0.174
F-Statistic	6.05

Source: Authors' analysis.

* Significant at 5 percent.

** Significant at 1 percent.

*** Significant at 0.1 percent.

Table 6. Successful Assembly in Each Negotiation by Treatment

		Successful assembly		
Rank	**Gains from assembly (in $100,000s)**	***Baseline* treatment**	***Revealed* treatment**	***Declared* treatment**
1	76	X	X	X
2	715	X		X
3	681	X	X	X
4	620	X	X	X
5	570	X	X	X
6	445		X	X
7	443			
8	441	X		X
9	439			X
10	438	X	X	X
11	402	X	X	X
12	358		X	X
13	348	X	X	X
14	347	X	X	X
15	342		X	X
16	341	X	X	X
17	337		X	X
18	337		X	X
19	285		X	X

(continued on next page)

		Successful assembly		
Rank	**Gains from assembly (in $100,000s)**	***Baseline* treatment**	***Revealed* treatment**	***Declared* treatment**
20	245	X	X	
21	132		X	X
22	52			X
23	8		X	
24	5		X	
25	(34)			
26	(48)			
27	(83)			
28	(11)			
29	(147)			
30	(200)			

Note: Negotiations are listed in rank order of the available gains from assembly.

Table 7. Estimates from OLS Regressions on Normalized Surplus Captured by Buyers and Sellers

	Buyers	**Sellers**
Variable	**Coefficient (std. err.)**	**Coefficient (std. err.)**
Constant	–97,442 (62,334)	27,335 (66,422)
Revealed treatment	94,255* (44,077)	–66,932 (46,967)
Declared treatment	179,263*** (44,077)	–24,174 (46,967)
Negotiation number	64,961 (35,989)	–18,223 (38,349)
Observations	90	90
R^2	0.187	0.026
F-Statistic	6.60	0.77

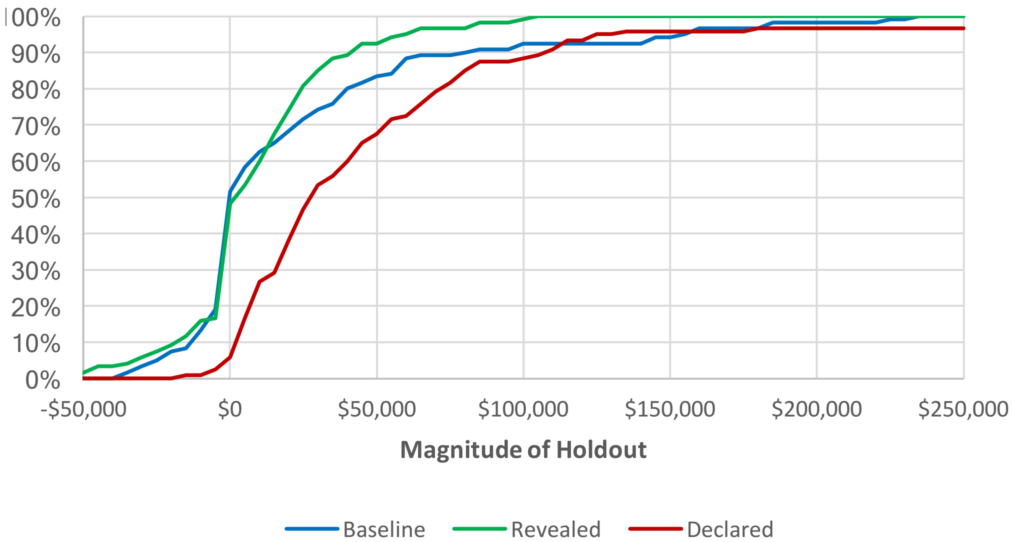
Source: Authors' analysis.

* Significant at 5 percent.

** Significant at 1 percent.

*** Significant at 0.1 percent.

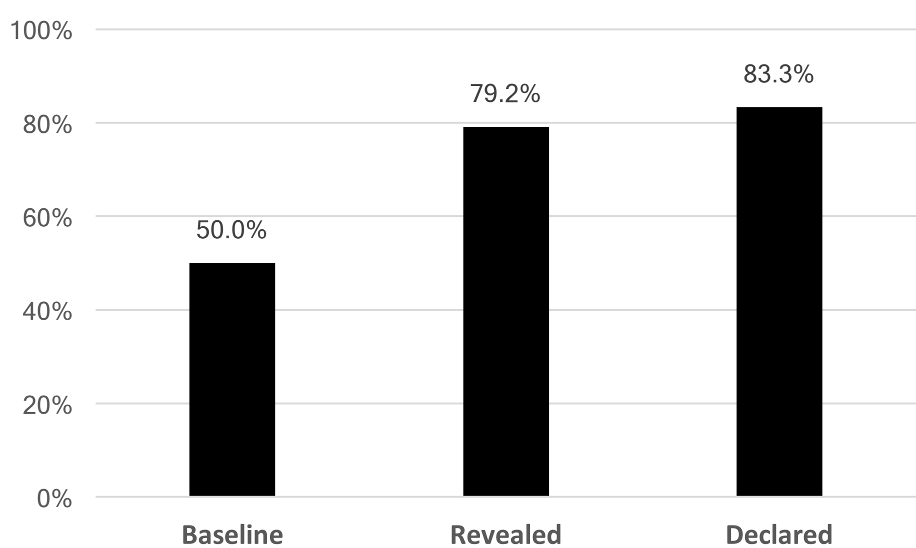
Figure 1. Empirical Cumulative Density Functions of Holdout across Treatments



Note: The magnitude of holdout is calculated as the highest rejected offer minus the seller's tax-adjusted value for the *Baseline* and *Revealed* treatments and the seller's price declaration minus his value in the *Declared* treatment.

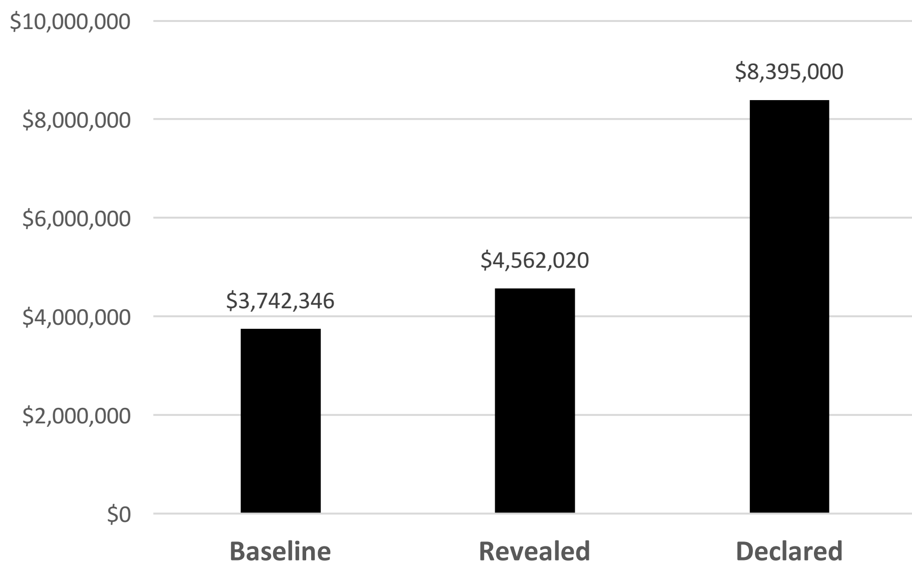
Source: Authors' calculations.

Figure 2. Rate of Successful Assembly in Negotiations Where There Were Positive Gains from Trade



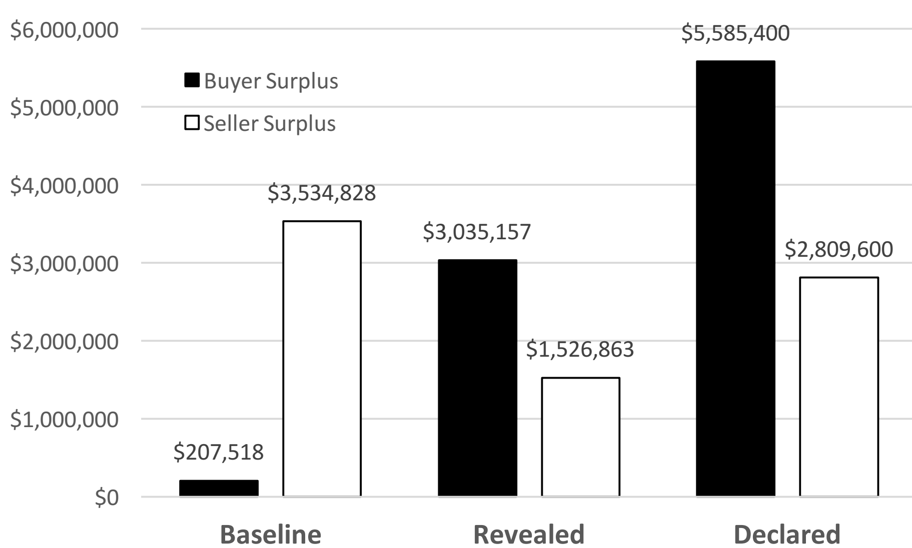
Source: Authors' calculations.

Figure 3. Gains from Trade in Each Treatment. The maximum available gains from trade were $9,096,000



Source: Authors' calculations.

Figure 4. Gains from Trade Captured by Buyers and Sellers in Each Treatment



Source: Authors' calculations.