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Who's Holding Out? An Experimental Study of the Benefits and Burdens of Eminent Domain

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Who's Holding Out? An Experimental Study of the Benefits and Burdens of Eminent Domain

Who's Holding Out?

An Experimental Study of the Benefits and Burdens of Eminent Domain[*]

By Abel M. Winn *and* Matthew W. McCarter

Abstract

A substantial literature identifies seller holdout as a serious obstacle to land assembly, implying that eminent domain is an appropriate policy response. We conduct a series of laboratory experiments to test this view. We find that when there is no competition and no eminent domain, land assembly suffers from costly delay and failed assembly; participants lose 18.1% of the available surplus. Much of the inefficiency is due to low offers from the buyers ("buyer holdout") rather than strategic holdout among sellers. When buyers can exercise eminent domain the participants lose 18.6% of the surplus. This loss comes from spending money to influence the fair market price and forcing sellers to sell even when the sellers value the property more than the buyer. Introducing weak competition in the form of a less valuable substitute parcel of land reduces delay by 35.7% and virtually eliminates assembly failure, so that only 11.5% of the surplus is lost.

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I. Introduction

A substantial theoretical literature identifies seller holdout as a significant impediment to efficient land assembly (Calabresi and Malamed 1972, Eckart 1985, Bittlingmayer 1988, Cohen 1991, Epstein 1992, 1993, Strange 1995 and Menezes and Pitchford 2004) and a possible justification for eminent domain (Allen 2000, Miceli and Sirmans 2007, Rose 2011). Suppose, for example, that two landowners with adjoining property each value their own parcel at $100,000 and a developer wishes to acquire both parcels. The development is such that both parcels are necessary for its completion. His maximum willingness-to-pay (WTP) is $0 for either one of the parcels but $250,000 for the pair. This may impede efficient assembly because both sellers are in a position to hold out for a large share of the surplus. Strategic holdout can draw out the bargaining process, causing costly delay or assembly failure. This is especially likely if the negotiating parties face uncertainty about one another's valuations for the land (Shupp, et al. 2013).

The holdout problem in land assembly is a special case of the tragedy of the anticommons (Heller 1998, Buchanan and Yoon 2000, Fennell 2004). An anticommons is a property regime in which multiple agents have the unilateral right to prevent the use of a resource. Examples include water rights transfers (Corbin 2011), assembling pharmaceutical patents (Heller and Eisenberg 1998) and assembling contiguous blocks of the broadcast spectrum (Hazlett 2008, 2014). In each case, too many agents with veto power can hinder a resource's use and reduce economic efficiency.

In the case of land assembly, eminent domain allows a developer to reduce delay and ensure assembly by forcing a recalcitrant landowner to sell her property. However, eminent domain may lead to inefficient assembly and invite influence costs. Inefficient assembly occurs where the sum of the fragmented owners' values for their land exceeds the value of the development but they are forced to sell. As Munch (1976) points out, the danger of under-assembly through market mechanisms is mirrored by the danger of over-assembly through eminent domain (see also O'Flaherty, 1994; Miceli and Segerson, 2007; Shavell, 2010).

The threat of inefficient assembly is not idle speculation. In the case of *Kelo v. New London* the Supreme Court upheld the constitutionality of transfering private land to a private developer. The main beneficiary was to be Pfizer, Inc., which would receive a $300 million research center. The case was decided in 2005 and seven families were evicted from their property, their houses demolished or moved offsite. Yet the development group never managed to raise financing and gave up the project in 2008. Pfizer left the city of New London the following year. As of 2015 the land where Ms. Kelo and her six neighbors lived remained an undeveloped field.

Eminent domain also imposes influence costs in determining the "fair market value" of the land; i.e., the price that is to be paid to the owner. This price is determined through a legal process in which both the buyer and seller(s) must, at the very least, obtain counsel and pay for separate and independent appraisals of the property. Both sides improve their chances of a favorable price by expending more resources on the legal process relative to their opponent.

The result of the legal process is that much of the surplus may be spent influencing the final price. In 2013 the city of Modesto, California used eminent domain proceedings to purchase a portion of one resident's property for $120,000. The city spent $180,000 in legal fees (Valine 2013). Moreover, more than two decades of experimental work has shown that participants in contests (like a court battle) frequently overspend relative to their Nash Equilibrium strategies. For a survey of the literature, see Dechenaux, Kovenock and Sheremeta (2015).

A number of experimental studies of land assembly demonstrate that seller holdout does occur and can be costly. (We provide an overview of these results in the following section.) This has led some investigators to suggest that eminent domain may be a necessary tool for efficient land aggregation (Swope, et al. 2011, Cadigan, et al. 2011). However, to date the experimental study of efficiency under a regime of eminent domain versus secure property is limited to a single study (Kitchens and Roomets 2015) that omits several important features of the land assembly problem. Delay in assembly is costless in their experiments, court fees are born only by the buyer and determined exogenously, the court-determined price is known with certainty to all parties and assembly is efficient in all negotiations.

In this paper we provide a comparison between secure property and eminent domain that incorporates inefficient assembly and influence costs. Eminent domain is not efficiency enhancing in our experiments. Participants captured 81.9% of the available surplus when buyers had no alternative to assembly and no recourse to eminent domain. They captured 81.4% of the available surplus when buyers could exercise eminent domain and the fair market price was determined by a contest in which both parties could improve their odds of winning by expending more resources. In a third treatment the developer could buy a less valuable substitute parcel of land instead of assembling parcels from the two primary sellers. Participants captured 88.5% of the available surplus in this treatment.

Interestingly, we find that buyers "hold out" more frequently than sellers. In the baseline treatment with secure property and no competition the sellers rejected a profitable offer in 22.6% of cases, while 60% of buyers' final offers were lower than the profit-maximizing offer. The rate of seller holdout was 6.7% in the treatment with competition and 4.3% in the treatment with eminent domain. These rates do not differ statistically; weak competition was as effective at breaking up seller holdout as eminent domain.

II. Prior Studies of Land Assembly

Two empirical papers use land sale data to estimate a premium for assembled land compared to unassembled land. Cunningham (2013) uses GIS maps of Seattle, Washington to identify assemblies that resulted in new construction between 2005 and 2007. He combines this data with sale prices and property characteristics to estimate a hedonic regression. Cunningham (2013) finds that properties that were assembled for new construction sold at a 17% premium.

Yuming, McMillen and Somerville (2016) study the assembly of small parcels in the urban core of Hong Kong between 1991 and 1998. They find that parcels that were redeveloped as part of a land assembly sold for a premium of 8 – 10% compared to parcels that were redeveloped individually. The final parcel acquired in an assembly sold for a 12% premium.

Brooks and Lutz (2016) study land assembly in Los Angeles, California between 1999 and 2010. They use properties where the existing structure was torn down after sale as a control group against which to compare properties that were assembled. They find that assembly properties sold at a premium of 15% - 40% depending on the modelling specification.

These studies are consistent with seller holdout, but they are not conclusive. As Brooks and Lutz (2016) point out, a premium for assembled land proves that there are frictions in land assembly, but those frictions can come from private sources (e.g., holdout and strategic delay) or public sources (e.g., restrictive zoning and building codes). It is not possible to determine how much of the assembly premium is due to holdout with the data that Cunningham (2013), Yuming, MicMillen and Somerville (2016) and Brooks and Lutz (2016) analyze.

A second difficulty in using field data to study holdout is that sellers who have put their property up for sale (active sellers) likely have lower reservation prices than sellers who have been approached by a developer (passive sellers). A buyer who wishes to redevelop a single property bargains with active sellers and can expect to pay the prevailing market price. But a buyer who needs multiple contiguous properties will almost certainly have to bargain with at least one passive seller, who is in no hurry to sell and values her property above the market price. Thus, assembled properties are likely to have higher reservation prices even in the absence of private frictions.

Laboratory experiments offer a way of observing holdout directly and comparing land assembly under alternative legal frameworks. Several laboratory studies have examined the holdout problem. The most relevant for our research are those by Cadigan, et al. (2009, 2011), Swope, et al. (2011), Collins and Isaac (2012), Parente and Winn (2012), Shupp, et al. (2013), Cadigan, Schmitt and Swope (2014), Zillante, Read and Schwarz (2014), Kitchens and Roomets (2015) and Isaac, Kitchens and Portillo (2016). We summarize these studies in Table 1, listing the treatment variables the authors studied and the primary results.

Strategic holdout occurred in all of the studies, although failure to assemble land tended to be infrequent. Across all of the studies in Table 1 there were 3,036 negotiations in which assembly failure could occur. It occurred in 299 (9.8%) of them. Failure rates were lowest in treatments where there was some competition among the sellers. Cadigan, et al. (2011) conducted experiments in which the assembler negotiated with three landowners but needed only two parcels. Out of 64 groups none failed to assemble the necessary parcels. Parente and Winn (2012) also conducted experiments in which the assembler (represented by the software) needed two parcels and faced three landowners. Out of 768 negotiations where assembly failure was possible, it occurred only 6 times, a failure rate of 0.8%. Isaac, Kitchens and Portillo (2016) created competition in two ways. First, similar to Cadigan, et al. (2011) and Parente and Winn (2012) they had two treatments in which a buyer faced four sellers but needed to assemble only two or three parcels. Out of 64 negotiations across these treatments assembly failure occurred in only five. In a third competitive treatment the buyer could either assemble all four of the primary parcels or purchase a single parcel from an alternative seller.[2] In this treatment one negotiation failed out of 28.

The only experimental study of eminent domain of which we are aware was conducted by Kitchens and Roomets (2015). In their experiments a buyer negotiated sequentially with four sellers who each had a $4 private use value for their properties. If he successfully purchased all four parcels the buyer received $50 minus the sum of negotiated prices. The sellers were paid the prices they had negotiated if they sold voluntarily. The buyer's and sellers' values were common knowledge. Once a seller agreed to a price it became common knowledge as well.

In one treatment the buyer used contingent contracts. Any seller in the sequence could "walk away" from the negotiations, but this voided all prior contracts. In this case the sellers each received a private use value of $4 for their property and the buyer was not paid. In the other treatment all contracts were binding but the buyer could take properties through eminent domain. Each time he invoked eminent domain the buyer paid the seller a predetermined price of $4 and paid court fees of $8.50. The court fees were parameterized such that if the buyer took all four properties the available gains from trade would be completely consumed.

Kitchens and Roomets (2015) found that prices were roughly the same under contingent contracts and eminent domain. They also found that efficiency was statistically indistinguishable

[2] The buyer had the same induced value for assembling the four smaller parcels as for purchasing the larger alternative parcel. This is a key distinction between the design employed by Isaac, Kitchens and Portillo (2016) and our design.

across treatments. Participants captured an average of 91.7% of the available surplus with contingent contracts and 93.2% with eminent domain. Thus, in their experimental environment and institutions eminent domain was not welfare enhancing.

These results are informative and important, but Kitchens' and Roomets' (2015) experimental design omits several features of the land assembly problem. First, they did not incorporate costs of delayed assembly, so assembly failure was the only possible source of inefficiency in their contingent contracts treatment. This is significant because strategic holdout is a dominated strategy in a single-period negotiation with complete information. As noted above, assembly failure does not occur frequently in land assembly experiments, thus the bulk of inefficiency generally comes from costly delay. This omission may positively bias efficiency in Kitchens' and Roomets' (2015) contingent contracts treatment.

Second, the buyer's value for the assembled properties was always considerably greater than the sum of the sellers' private use values. Thus, assembly failure posed the largest threat to efficiency, and this could only occur in the contingent contracts treatment. There was no possibility of inefficient assembly in the eminent domain treatment. This may positively bias efficiency in their eminent domain treatment.

Third, buyers and sellers in these experiments faced a known fair market price that was equal to the sellers' private use values. In actual cases of eminent domain the buyer and seller(s) spend money in the courts because they expect to influence the price in their favor.

Finally, court costs in Kitchens' and Roomets' (2015) experiments were determined exogenously and fell only on the buyer. In the field sellers often expend resources on the legal process as well, and their levels of expenditure are decision variables. Thus the efficiency of eminent domain is dependent to some extent on whether the two parties spend few resources in court or many. Preventing the participants from making this decision on their own could bias efficiency in their eminent domain treatment positively or negatively.

The fact that efficiency may be overstated in the contingent contracts treatment and overstated or understated in the eminent domain treatment makes it difficult to apply Kitchens' and Roomets' (2015) results to policy with high confidence. We introduce an experimental design that incorporates delay costs, inefficient assembly, an uncertain fair market price and endogenous legal expenditures.

[Table 1 Here]

III. Experiment Design

A. Overview of the Negotiation Environment

Our experiment design is inspired by the work of Shupp et al. (2013), who investigated land assembly under conditions of uncertainty regarding the valuations of the buyer and sellers. We model an environment in which one buyer negotiates with two owners (the sellers) through a finitely repeated process of offers and responses.[3] The buyer makes simultaneous independent offers to the sellers, who may accept or reject them.

In our experiments each seller i had a private valuation, v_i, for his own parcel . Valuations were denominated in "points" that were redeemed for cash at the end of the experiment. The v_i were drawn (with replacement) from a discrete uniform distribution with support $[50,100]$ and $E(v_i) = 75$. The buyer's WTP for either of the parcels alone was zero, but his WTP for the pair of them was V, which was drawn from a uniform distribution with support $[100,250]$ and $E(V) = 175$. Note that assembly was efficient in expectation but was

[3] Our experiments required the buyer to assemble both parcels to receive a payoff. See Asami (1988) and Asami and Teraki (1990) for models that allows for assembling subsets of the parcels.

inefficient with non-zero probability. Agents knew their own valuation but only the distributions from which their counterparts' valuations were drawn.

Negotiation lasted up to 5 periods, which was common knowledge. In each period the buyer offered a bid, β_i, to each seller who had not yet agreed to sell her parcel. Sellers could only accept or reject an offer; they could not make a counteroffer. The bids were contingent: if only one seller had accepted a bid by the end of period 5 the buyer did not purchase her parcel.

Prolonged negotiation was costly. Following Cadigan, et al. (2009) we modeled the costs of delay as a penalty assessed against all agents' payoffs. Specifically, if both sellers accepted an offer by period t, then all payoffs were multiplied by $1 - 0.05(t - 1)$. Thus, if both sellers accepted their offers in period 1 there was no cost of delay, while the cost was nonzero and monotonically increasing in all subsequent periods.

We tested land assembly within this general negotiation environment in three treatment conditions. In the first (*Baseline*) the buyer's only profit opportunity was to purchase the parcels from the sellers without recourse to eminent domain. In the second treatment (*Competition*) the buyer could purchase a substitute parcel of land instead of assembling the fragmented parcels. The substitute was not as valuable to the buyer as the fragmented parcels, however, so that the competitive pressure on the sellers was weak. In the third treatment (*Eminent Domain*) the buyer could invoke eminent domain and the parcel's price was determined by a Tullock Contest. A high or low price could result from the contest, and a contestant's probability of achieving his preferred price was proportional to the amount of money he spent in the contest.

B. Baseline Treatment: Secure Property

Participants made their decisions through an electronic computer interface. In the *Baseline* buyers and sellers saw a matrix of two squares labeled (1) and (2), which represented the sellers' parcels. In the first negotiating period the buyer submitted simultaneous private offers to both sellers. Each seller saw her offer in her square of the matrix and indicated her decision by clicking one of two buttons labelled "accept" and "reject." Once a seller had accepted an offer negotiations for her parcel concluded at the price she had agreed to. If at least one seller had rejected her offer the negotiation went on to the next period. Contracts were contingent; the buyer only paid a seller the agreed price if both sellers accepted an offer.

In a single-period negotiation the buyer's optimal strategy is simple to calculate because sellers should accept any offer $\beta_i \geq v_i$. Since the v_i are drawn from the same distribution the buyer has no reason to submit different offers to the two sellers, and so in equilibrium $\beta_1 = \beta_2$. Thus, we omit the subscripts in the following analysis.

The buyer's expected profit, $E(\pi)$, is a function of his value and offers:

$$E(\pi) = (V - 2\beta)\left(\frac{\beta - 50}{50}\right)^2 \qquad (1)$$

The first term in (1) is the profit earned by the buyer if both sellers accept and the second term is the probability that his offers exceed both of the their values. Solving the first order condition of (1) for β yields the equilibrium bid function:

$$\beta^* = \frac{V + 50}{3} \qquad (2)$$

With multiple bargaining periods it becomes difficult to succinctly model buyer behavior after the first period because his best strategy will depend on his beliefs about the sellers. Suppose at least one seller rejects her offer in period one. If the buyer believes that the sellers would only reject an offer that is below their value then in the second period he will incorporate any accepted offer into the first term of equation (1), substitute the first period β^* for 50 in its second term and solve for the new equilibrium offer. But if he believes that the sellers are holding out strategically, then he will not change his offers in the second period. A third possibility is that the buyer places a non-zero probability on the sellers rejecting strategically, in which case he will revise his second period offer(s) upward, but by a smaller amount than if he believed them to be sincere.

In their turn, the sellers' optimal behavior depends on their beliefs about the buyers' beliefs. If they believe him to think they are strategic, then strategic holdout will not be profitable because it will incur the delay cost without increasing the buyer's offers in period two. If they believe him to think they will only reject sincerely – i.e., reject offers below their values – they will hold out in period 1 so long as the difference in equilibrium offers is greater than $0.05v_i$.

The multiplicity of plausible outcomes implies that we cannot predict behavior in the *Baseline* beyond period 1 with any confidence without knowing the beliefs of the agents. However, earlier empirical work by Zillante, Read and Schwarz (2014) and Shupp, et al. (2013) suggests that offers will rise over time. For the current study we will use the equilibrium offer function as a benchmark for buyer offers in the first period.

C. Competition Treatment: Secure Property with a Substitute Parcel

In our Competition treatment the buyer faced the two sellers as in the *Baseline*, but also had the option of buying a substitute parcel of land. The substitute parcel was displayed on participants' screens as a rectangle to the right of the matrix representing the primary parcels. For clarity we will refer to the two fragmented parcels as the "primary parcels" and their owners as the "primary sellers." We will refer to the owner of the substitute parcel as the "alternative seller." The buyer's induced value for the substitute parcel was 80% of his induced value for the two primary parcels. The substitute parcel was of no additional value to the buyer if he purchased both of the primary parcels.

The buyer initially made his offers to the primary sellers as in the *Baseline*. If one or both of them rejected his offer, the buyer then submitted an offer to the alternative seller. The delay cost for the period was only incurred if the alternative seller rejected her offer. Contracts were contingent, as above.

The alternative seller had a valuation for her parcel, v_a, that was drawn from the uniform distribution $[80,160]$ with $E(v_a) = 120$. Notice that the expected surplus from assembling the primary parcels was $E(V) - 2E(v_i) = 175 - 150 = 25$, while the expected surplus from buying the substitute parcel was $0.8E(V) - E(v_a) = 140 - 120 = 20$, so purchasing the substitute parcel was not socially optimal on average.

We again use the one-period model as our benchmark. If the buyer is forced to make an offer to the alternative seller, his expected profit function is:

$$E(\pi_a) = (0.8V - \beta_a)\left(\frac{\beta_a - 80}{80}\right) \tag{3}$$

Solving the first order condition of (3) for β_a yields the equilibrium alternative bid function:

$$\beta_a^* = 0.4V + 40 \tag{4}$$

This implies that in equilibrium the buyer's expected profit from dealing with the alternative seller is:

$$E(\pi_a^*) = \frac{(0.4V - 40)^2}{80} \tag{5}$$

Given that failing to assemble the primary parcels will still generate an expected profit of $E(\pi_a^*)$, the buyer's expected profit when he is making an offer to the primary sellers is now:

$$E(\pi) = (V - 2\beta)\left(\frac{\beta - 50}{50}\right)^2 + E(\pi_a^*)\left(1 - \left(\frac{\beta - 50}{50}\right)^2\right) \qquad (6)$$

We may solve the first order condition of (6) for β to find the equilibrium offer function:

$$\beta^* = \frac{V + 50 - E(\pi_a^*)}{3} \qquad (7)$$

Comparing the equilibrium offer functions (2) and (7) we see that the presence of the alternative seller reduces the buyer's equilibrium offers to the primary sellers by one third of the expected profit from dealing with the alternative seller.

Allowing for multiple periods causes equilibrium behavior to become ambiguous for the reasons discussed in the previous section. However, seller holdout was riskier in the *Competition* treatment due to the risk that the buyer would commit to a contract with the competing party (or parties). Consequently, we expect to see less seller holdout in this environment.

D. Eminent Domain Treatment

In the *Eminent Domain* treatment the buyer was allowed to force a seller who had rejected his offer to sell. This was done by clicking a button labelled "Force Sale" next to a seller's property. If the buyer invoked eminent domain the fair market value was decided through a simulated litigation process. The price the buyer paid was determined by the amount he and the seller spent on litigation. Neither the buyer nor the seller were allowed to spend so much that they could make negative earnings. The most the seller could spend was equal to the low price that could result from the contest. The most that the buyer could spend was calculated based on his value and any price he had already agreed to or other contest he was in. This maximum was set so that even if the buyer had to pay the high price in the contest his total expenditures would not exceed his value. The delay cost was incurred at the end of a period only if at least one seller rejected her offer and the buyer did not force her to sell.

If the buyer and seller spent nothing then the fair market value was 50, the lower bound of the seller's value distribution. This is consistent with a prevailing market price of land less than or equal to all landowner's valuations. If one or both spent an amount greater than zero then the fair market price was assessed to be 40 if the buyer won the contest and 60 if the seller won.[4] The winner was determined probabilistically, with the probability that one contestant wins equal to the amount he spends in the contest divided by the sum of both contestants' spending. Notice that the litigation process effectively offered the buyer and seller a prize equal to 20, the difference between the high and low prices. We may therefore analyze the legal process as a simple Tullock Contest. It is straightforward to show that with two players the Nash Equilibrium in such a contest is for each party to spend one fourth of the prize (Chowdhury and Sheremeta, 2011). Thus, we would expect the buyer and seller to each spend 5 points if the buyer forced a sale.

Of course, the influence costs of a court battle should act as a deterrent to invoking eminent domain in the first place. The buyer knows that if he takes the seller to court the seller's expected profit will be equal to the expected price she will receive minus the amount she spends in court costs. Thus, the buyer's optimal bid offers the sellers an amount that leaves them

[4] This range of prices is conservative. Munch (1976) found that eminent domain prices ranged from 28% below her estimate of market value to more than 100% above it. More recently, Chang (2010) estimated the fair market value of condemned properties in New York City from 1990 – 2002. He found that for many properties compensation was as low as 50% below fair market value and as high as 50% above it.

indifferent between accepting his offer and going to court. Given our parameters this means that $\beta^* = 45$. Notice this implies that theoretically the threat of eminent domain is sufficient for land assembly. We would therefore expect no forced sales in our *Eminent Domain* treatment.

E. Procedures

The parameters of the experiment are summarized in Table 2. Sellers earned their input values even if they did not sell, while buyers only received payment if they assembled both parcels. For this reason we varied the exchange rate between points and dollars by role. Buyers received $1.00 for every 2 points, primary sellers $1.00 for every 4 points and alternative sellers $1.00 for every 7 points due to their higher average input value. These exchange rates ensured that all participants could earn roughly the same cash payment in the experiment. We kept the exchange rates private, but told the participants that their counterparts' exchange rates may be different from their own. The combination of uncertain value draws and private exchange rates made it very difficult for participants to infer their counterpart's earnings. As a result, we would expect other-regarding preferences to be minimized (Cooper and Kagel, 2015).

[Table 2 Here]

We recruited 150 undergraduate and graduate students at a university in the American Southwest. The participants came from a pool of approximately 2,000 who had signed up in advance to participate in economic experiments. Each participant was in only one treatment. We paid them $7 for attending plus earnings that they received from their decisions in the experiment ($16.22 on average). Experimental sessions lasted 30 – 60 minutes, including time for instructions.

Participants sat at desks separated by privacy dividers. Each received a half-page summary of the rules of the experiment as well as important parameter information, such as the distributions from which values would be drawn. An experimenter read the instructions aloud from a script, pausing at predetermined points to elicit questions and answer them. We projected screenshots of the user interface on a screen at the front of the laboratory.

We described the decision space as neutrally as possible to focus the participants' attention on their own profit calculations rather than their personal feelings about eminent domain. We called the parcels of land "inputs" that the buyer wished to purchase and referred to a "forced sale" rather than eminent domain or condemnation, and a "contest" rather than a litigation process.

Negotiations in all treatments were strictly private. Sellers never saw one another's offers, nor were they informed whether another seller had accepted her offer except when the buyer succeeded in assembling the primary inputs or bought the alternative input. In the *Eminent Domain* treatment sellers did not know if the other seller in their group had been forced to sell. When competing in a contest neither contestant was told how much their opponent had spent. Each experiment session consisted of 3 rounds. Each round was a separate negotiation. Participants took the same role in every round, but were matched into different groups. To keep the negotiations independent across rounds we re-matched the participants so that they were never grouped with any of the same counterparts more than once. This prevented participants from rewarding or punishing one another for their decisions in prior rounds. The number of rounds and uniqueness of each round's grouping was common knowledge. After the third round the computer software randomly chose one of the rounds for each participant. The participant was paid according to his earnings in that round's negotiation.

To facilitate unique groups we conducted the *Baseline* and *Eminent Domain* treatments in sessions with nine participants organized into three groups – three buyers and six sellers. This allowed us to obtain nine observations from each session. For the *Competition* treatment every session used twenty participants organized into five groups – five buyers, ten primary sellers and five alternative sellers. This allowed us to obtain fifteen observations per session. We conducted 5 sessions of the *Baseline* and *Eminent Domain* treatments and three sessions of the *Competition* treatment, giving us 45 negotiations for each treatment. (See Table 3.)

[Table 3 Here]

IV. Experiment Results

A. Benchmark simulations and an overview of results

We conducted simulations to find the best-case outcomes that could occur in our experiments if buyers submitted their equilibrium offers and sellers did not hold out. In the *Baseline* and *Competition* simulations the sellers accepted offers greater than or equal to their values and this was known to the buyers. The simulated buyers responded to rejected offers by revising their offers upward optimally in the subsequent period. In the simulated *Eminent Domain* treatment sellers always accepted their offers, so that the buyers never invoked eminent domain. For each treatment we used the same parameter draws as those in the experiments with human participants. We recorded the buyers' opening offers, number of negotiating periods, efficiency and the use of eminent domain. This provides us with a benchmark for comparison to the outcomes from our experiments.

[Table 4 Here]

Table 4 displays the results of our simulations for each treatment alongside the observed results of our experiments. Participants in the *Baseline* performed below the benchmark. The average opening offer was less than the average equilibrium offer. This, combined with some holdout among sellers resulted in more delay in the experiments than in our simulations.

Consequently, on average the participants captured only 81.2% of the available surplus on average, compared to 88.5% in the simulations.

Outcomes in the *Competition* treatment were roughly equal to the benchmark. The average opening offer of 64.4 was only 6% less than the average equilibrium offer of 68.5. The number of negotiating periods was nearly identical in the simulations and the experiments. On average the participants captured 89.9% of the available surplus, slightly more than the benchmark of 89.5%.

In the *Eminent Domain* experiments the buyers' offers were more generous to the sellers than theory would predict. Nevertheless, many sellers did not accept their opening offers, which led to some delay and many instances of forced sales. Across all negotiations 41.1% of sellers were forced to sell their inputs. The high rate of eminent domain lead to considerable spending to determine fair market prices. The average spending was 15.7 for buyers and 15.9 for sellers, more than triple the equilibrium of 5. This resulted in an average efficiency of 80.6%, compared to 95.1% in the benchmark simulations.

Notice that in our simulations the *Eminent Domain* treatment had the highest average efficiency (95.1%), followed by *Competition* (89.5%) and the *Baseline* (88.5%). That is, the experimental environment was the most favorable to achieving high levels of surplus with eminent domain. Yet participants in the *Eminent Domain* treatment of the experiments captured the least of the available surplus. Below we explore the results of our experiments in more detail.

B. Buyer offers

In Figure 1 we present the average deviation of the buyers' first and final offers from our theoretical predictions for each treatment. In the *Baseline* treatment the average first period offer was 58.6, which is 22.7% below the average equilibrium offer of 75.8. This was not due to a small number of outliers. Of the 45 first offers in the *Baseline*, 38 (84.4%) were below the optimal offer given the buyer's value. We compared the first period offers to those in the benchmark simulations with a Wilcoxon sign rank test. The unit of analysis was the average of a buyer's two offers in the first period of the round. We can reject the null hypothesis that first period offers in the *Baseline* treatment were no different from the equilibrium with high confidence ($p < 0.001$).

[Figure 1 Here]

The *Baseline* offers did increase in subsequent periods, but remained overly conservative. The average final offer in the *Baseline* was 69.5. 60% of these final offers were below the Nash Equilibrium. A Mann-Whitney test comparing a buyer's final offer of the round with his first offer indicates that the difference is statistically significant ($p < 0.001$). However, even by the end of negotiations the typical buyer in the *Baseline* offered the sellers 8.3% less than would have been optimal in the first period (Wilcoxon sign rank test, $p = 0.002$).

The pattern was similar in the *Competition* treatment, but not as pronounced as the *Baseline*. The average buyer's value was 168 points, which implied an average first offer of 68.5. Buyers' offers were 64.4 on average, or approximately 6% below equilibrium. The difference between optimal and observed offers is marginally statistically significant (Wilcoxon $p = 0.059$) but rather small in economic significance. The average final offer in the *Competition* treatment was 70.1, which is not statistically different than the equilibrium first-period offer (Wilcoxon, $p = 0.592$). Overall, 42.2% of first offers and 22.2% of final offers were below equilibrium in the *Competition* treatment.

Notice that introducing competition among the sellers was predicted to reduce buyers' average offers by 7.3 points. Instead the buyers increased their offers by an average of almost 10 points. In the *Baseline* treatment buyers may have made low offers in an effort to avoid overpaying one of the sellers and thereby constraining their ability to make an adequate offer to the other. In the buyers' minds this risk may have dominated the risk that making low offers would drag out the negotiations and increase the risk of assembly failure. Overpaying a primary seller was less of a concern in the *Competition* treatment because even if the buyer found himself unable to make a sufficiently high offer to one of the primary sellers he might still negotiate a contract with the alternative seller. Mann-Whitney tests do not find the distributions of first or final offers to be statistically different between the *Baseline* and *Competition* treatments (p = 0.263 and p = 0.765). However, we also compared offers in these treatments by performing chi-squared tests of the frequency of offering less than the equilibrium prediction. Buyers in the *Baseline* were more likely to offer less than the equilibrium in both their first and final offers ($p < 0.001$ in both cases).

While offers under secure property tended to be too low, under eminent domain the buyers did not fully exploit the strength of their bargaining position. The average first offer was 56 in the *Eminent Domain* treatment. This is 24.4% higher than the equilibrium offer of 45, and a Wilcoxon sign rank test indicates that the difference is statistically significant ($p < 0.001$). The buyers may have been motivated by fear that sellers would view the equilibrium offer as unfair and reject it to punish them. This would force both sides to spend money in the Tullock Contest, and could be viewed as a form of costly punishment. Henrich, et al. (2006) have shown that the willingness to engage in costly punishment is a feature of a wide range of human societies.

C. Seller holdout

To analyze seller holdout, we found the highest offer that a seller rejected in a round and subtracted her input value from it. Where this normalized highest rejected offer is greater than zero we consider the seller to have withheld her input strategically. The cumulative distributions of the normalized highest rejected offers are shown in Figure 2. A vertical line at the value of zero separates the shares of each distribution that represent strategic rejections from sincere rejections.

[Figure 2 Here]

Sellers in the *Baseline* strategically rejected the buyer's offer in 22.6% of cases. Notice that this is substantially less than the percentage of buyers in the same treatment who made offers that were lower than the equilibrium. 60% of the buyers' final offers were below equilibrium. If we consider these low offers to be buyer holdout then buyers held out 2.7 times as often as sellers. Moreover, in Section IV d. below we demonstrate that the loss of efficiency from delay was mainly due to buyer holdout. Our findings run counter to the conventional wisdom that sellers are primarily responsible for the difficulties of land assembly.

In the *Competition* treatment the primary sellers strategically rejected far fewer offers. In 6.7% of cases a primary seller's highest rejected offer exceeded her value, a 70.4% reduction compared to the *Baseline*. A chi-square analysis confirms that holdout was statistically less frequent in the *Competition* treatment compared to the *Baseline* (p = 0.013). The effect of competition on strategic holdout is especially impressive when we compare it to eminent domain. Sellers in the *Eminent Domain* treatment rejected profitable offers in 4.3% of cases. A chi-square test cannot reject the null hypothesis that holdout rates were equal in the *Eminent*

Domain and *Competition* treatments (p = 0.609). That is, introducing a weak form of competition was just as effective at discouraging seller holdout as eminent domain.

D. Efficiency

Eminent domain did not increase the gains from trade, but weak competition did. As we noted above average efficiency was highest in the *Competition* treatment (89.9%), followed by the *Baseline* (81.2%) and *Eminent Domain* treatments (80.6%). We compared the outcomes across treatments with pair-wise Mann-Whitney tests. Efficiency was statistically indistinguishable between the *Baseline* and *Eminent Domain* treatment (p = 0.971), but it was statistically significantly higher in the *Competition* treatment than in the *Baseline* (p = 0.012) and *Eminent Domain* treatments (p = 0.045).

[Table 5 Here]

In Table 5 we provide complete information regarding the number of points that could have been earned in each treatment, along with how many points were earned and the number of points that were lost due to the various possible sources of inefficiency. In the *Baseline* participants failed to capture a total of 1,498 points, or 18.1% of the available surplus. Of these, 1,237 points (82.6%) were lost due to delay, and 225 (15%) were lost due to assembly failure. We have already noted that both sellers and buyers held out in the form of rejected offers above seller's values and offers below Nash equilibrium. Which form of holdout cost more in terms of lost gains from trade? We addressed this question by simulating two counterfactuals: a no seller holdout (NSH) counterfactual and a no buyer holdout (NBH) counterfactual. For the NSH counterfactual we simulated buyers whose offers were identical to those submitted by the human buyers and sellers who accepted all offers that were greater than or equal to their value.[5] This allows us to measure how efficient the negotiations would have been without seller holdout, holding observed buyer decisions constant. We conducted 45 simulations for the NSH counterfactual; one for each negotiation in the experiments.

For the NBH counterfactual we simulated buyers who submitted their equilibrium offers and sellers who accepted the offers probabilistically. We constructed an acceptance probability function using the decisions that the human sellers had made in our experiments. For each offer that a human seller had accepted we subtracted the seller's value from the offer to find the normalized accepted offer. The probability that a simulated seller in the NBH counterfactual accepted its offer was equal to the proportion of human sellers who had accepted a normalized offer of equal or lesser value. This allows us to measure how efficient the negotiations would have been without buyer holdout, holding observed seller behavior constant. Due to the probabilistic nature of the simulated sellers' decisions we conducted 1,000 simulations for each negotiation in the experiments, for a total of 45,000.

[Figure 3 Here]

Figure 3 displays the average efficiency in the observed *Baseline* negotiations, as well as those in the NSH and NBH counterfactuals. As the chart makes clear, buyer holdout was more detrimental to efficiency than seller holdout. In the NSH counterfactual the average efficiency was 84%; only 2.8 percentage points higher than the observed *Baseline* efficiency. For the NBH counterfactual the average efficiency was 90.2%; 9 percentage points higher than the human participants achieved. Both of these differences are statistically significant according to Wilcoxon sign rank tests (p = 0.033 for NSH, p < 0.001 for NBH). Notice that average

[5] In some cases sellers in the laboratory experiments accepted offers that were below their values. We replicated these decisions in the NSH counterfactual, so that the simulated sellers never rejected an offer that had been accepted by their human counterparts.

efficiency was higher in the NBH simulations than in our benchmark simulations. This is because the human sellers accepted offers below their values in 52.9% of cases, most likely to avoid delay costs. As a result, negotiations lasted for an average of 2.8 periods in the NBH simulations versus 3.3 periods in the benchmark simulations. In the NSH simulations and laboratory experiments the average negotiation took 3.9 and 4.2 periods respectively.

Participants were able to capture the highest share of the surplus in the *Competition* treatment. Average efficiency was 89.9% in the *Competition* treatment compared to 81.2% in the *Baseline*. This was primarily due to a reduction in delay. The average duration was 2.7 periods for all *Competition* negotiations and 2.1 for those where there was positive surplus available from assembly. Wilcoxon sign rank tests indicate that these were not statistically different than the benchmark simulation averages of 3 and 1.8 ($p > 0.25$ in both cases).

The buyers in the *Competition* treatment made a purchase in 93.8% of negotiations where there were positive gains from trade. They purchased the parcel(s) that generated the higher surplus 65.6% of the time. For each negotiation where the buyer's purchase generated less surplus than if he had negotiated an agreement with the other seller(s), we calculated the difference in surplus between the two possible contracts. This allows us to determine the opportunity cost in efficiency from purchasing the wrong input(s). The total opportunity cost was 304 points, which is only 2.2% of the available surplus in the *Competition* treatment.

Average efficiency was 80.6% in the *Eminent Domain* treatment, which is not statistically different than in the *Baseline*. Delay and failed assembly did not substantially affect efficiency in the *Eminent Domain* treatment. Only two negotiations failed to result in assembly because the buyer could not afford to force both sellers to sell. In both of these negotiations the sellers valued their inputs more than the buyer, so no points were lost from assembly failure.

The average duration was 1.4 periods for all negotiations and 1.2 periods for negotiations with gains from trade. As a result, only 157 points (1.9% of available surplus) were lost due to delay.

However, spending in the Tullock Contest was more than 200% higher than predicted. In theory the buyer and seller should both spend 5 points. In fact, buyers spent an average of 15.7 points and sellers an average of 15.9 points. These high averages were due in part to very high spending by a few participants. However, median spending was 10 points for both buyers and sellers; 100% higher than equilibrium. Wilcoxon sign rank tests confirm that spending was statistically higher than equilibrium for buyers and sellers ($p < 0.01$ for both roles). This is consistent with prior studies on spending in Tullock Contests (see Dechenaux, Kovenock and Sheremeta, 2015)

Since litigation costs were the main cause of efficiency loss in the *Eminent Domain* treatment it is reasonable to consider how sensitive our results are to the variance in prices that could result from the contest. Our parameters required the litigated price to be either 40 or 60; i.e., 20% above or below the true fair market value. The litigated price range determines the size of the contest's prize. Consequently, we would expect a direct relationship between the width of the prices and the level of spending.

To estimate the sensitivity of our results to the litigated price range we recalculated the efficiency in the *Eminent Domain* treatment according to two counterfactuals. In both counterfactuals we assumed that the contestants spent a fixed fraction of the prize. This fraction was calculated for each contestant using the observed spending amounts for the numerator and the observed prize (20 points) in the denominator. In one counterfactual (narrow range) we reduced the litigated price range to be 10% above or below the fair market value. In the second counterfactual (wide range) we followed the estimates of Chang (2010) that litigated range from 50% below fair market value (25 points) to 50% above it (75 points).

In our narrow range counterfactual average efficiency in the *Eminent Domain* treatment increased to 87.6%, statistically significantly greater than the *Baseline* (Mann-Whitney test, p = 0.022). This indicates that where courts face less uncertainty over fair market value eminent domain is likely to be more efficient. However, in the wide range counterfactual the average efficiency is 61.4%, which is statistically significantly less than the *Baseline* (Mann-Whitney test, p = 0.031). Given that the wide range counterfactual is based on empirical estimates, it is reasonable to treat the results of our laboratory experiments as an optimistic comparison of the efficiency of eminent domain versus sovereign property rights. We advise caution in relying on these counterfactual results, however, because they rely on the assumption that spending strategies do not vary with the range of litigated prices.

Theoretically, sellers should accept any offer of 45 points or higher, and the average first offer in the *Eminent Domain* treatment was 56 points. Thus, we would expect litigation to be infrequent, but that was not the case. The buyer invoked eminent domain against at least one seller in 44.4% of negotiations and against both sellers in 11.1% of negotiations. As a result, participants spent 1,149 points to influence the fair market price. This accounts for 73.9% of all points lost in the *Eminent Domain* treatment and 13.7% of the maximum available surplus. Notice that this is almost the same amount that was lost due to delay in the *Baseline*. What eminent domain gave through faster negotiation it took away through influence costs.

V. General Discussion

The results of these experiments push our understanding about eminent domain and collective action in three ways. First, we find that – contrary to the conventional wisdom – a large majority of sellers do not hold out even when the buyer has no alternative to assembly or recourse to eminent domain. Rather, in our experiments it was primarily the buyers who held out for an outsized share of the surplus by making offers that were below the profit maximizing level, and buyer holdout was 3 times as costly as seller holdout. It seems a perverse response under such circumstances to give buyers the right to cut short the bargaining process and force the sale of property. The pattern of buyer holdout across the *Baseline* and *Competition* treatments suggests that buyers held out to avoid overpaying one seller, leaving them with insufficient funds to offer an acceptable price to the other. If this is the case it suggests that competition among sellers is important not only to break up seller holdout, but to give buyers flexibility in how they achieve assembly, resulting in higher offers.

Second, eminent domain did not enhance the efficiency of negotiated outcomes. The surplus that was saved by avoiding delay was spent in litigation costs. One possible policy response would be to curtail or eliminate the degree to which litigants can influence the price of condemned property. Yet such a policy would run directly counter to democratic principles of due process, and it would also open landowners to predatory behavior on the part of government officials. An alternative policy response would be to place a high burden on the party invoking eminent domain to demonstrate that the gains from assembling the properties is very large. Eminent domain ought not to be invoked to achieve modest improvements in land use due to the risk that influence costs will meet or exceed the gains from trade.

Third, we find that even weak competition is sufficient to break down seller holdout and improve economic efficiency. When our buyers had an outside option to assembling the primary sellers' parcels, seller holdout was not statistically higher than when the buyers could force a sale. Having an available substitute also increased the buyers' offers relative to the theoretical equilibrium. The availability of a substitute parcel reduced the duration of negotiations by 35.7% overall (from 4.2 periods to 2.7 periods) and almost no surplus was lost due to assembly failure. Comparing weak competition to eminent domain, participants captured 7.1 percentage points more of the available surplus under competition.

The result that weak competition helps to navigate seller holdout strengthens the findings of Cadigan et al. (2011), Parente and Winn (2012) and Isaac, Kitchens and Portillo (2016) that competition among sellers makes land assembly quite easy. Notice that in their studies the sellers competed with perfect substitutes, while in the present study the buyer incurred a 20% loss in value from buying the alternative parcel. A straightforward implication for policy is that eminent domain should be restricted to cases where the assembling agent has no viable alternative to assembling a single set of properties. An example would be the construction of a road through a mountain range with a single pass. If the land along that pass is owned by multiple parties then eminent domain may be necessary to prevent strategic holdout from thwarting efficient assembly. But suppose there is a second pass that is less suitable for a road, perhaps because it is further from the existing infrastructure or takes a more circuitous route through the mountains. In this case eminent domain is less likely to be justified because an element of competition has been introduced which will break down seller holdout.

More broadly, our findings also contribute to the study of the tragedy of the anticommons, of which the land assembly problem is a special case. Legal research conjectures that, without a superordinate authority the tragedy of the anticommons is inevitable. Indeed, scholars have long endorsed placing a superordinate authority over shared resources to navigate social dilemmas (e.g., Hardin 1968, Kollock 1998). Our findings highlight that a superordinate authority may reduce the negative externalities of seller holdout while imposing externalities of its own.

The anticommons literature – and social dilemma research in general – typically assumes that resource management is a closed system with no outside alternatives; e.g., there is only one configuration of land amenable to development or one set of patents that will permit a suitable pharmaceutical treatment. Relaxing this assumption and taking an open system approach to the tragedy of the anticommons, as we have done with land assembly, introduces an effective substitute for superordinate authority.

Our study does have some important limitations. First, we did not vary the number of sellers, so we cannot measure how the degree of fragmentation interacts with the results reported here. Cadigan, et al. (2011) have demonstrated that delay is exacerbated and assembly failure more common with a larger numbers of sellers. Future research may benefit from examining whether the number of sellers makes land assembly more challenging, especially if sellers are allowed to form coalitions against prospective buyers. Second, we did not vary environmental parameters, such as the magnitude of delay cost or the duration of the eminent domain process. Varying those parameters could affect the relative efficiencies of our *Baseline* and *Eminent Domain* treatments. However, it is worth noting that Kitchens and Roomets (2015) also find that eminent domain does not increase efficiency in experiments that are distinct from our own. Finally, there were no externalities from assembly in our experiments, which may encourage seller holdout (O'Flaherty 1994). Future scholarship may benefit from examining whether the knowledge of positive versus negative externalities to those directly involved in the land assembly impact seller holdouts.

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Table 1. Summary of laboratory experiments of land assembly and holdout.

Study	Treatment Variables	Main Findings
Cadigan, Schmitt, Shupp and Swope (2009)	1. Single period vs. multiperiod bargaining 2. Costly vs. costless delay 3. Buyer proposes vs. sellers propose	1. Single period bargaining and costly delay made offers more generous and holdout less likely, 2. Both buyers and sellers rejected profitable proposals. 3. 19 of 174 negotiations failed (10.9%). Of these, 18 were in single period treatments.
Cadigan, Schmitt, Shupp and Swope (2011)	1. Number of sellers (1 – 4) 2. Costly delay (only with 2 – 4 sellers) 3. Competition (only with 3 sellers) 4. Buyer proposes vs. seller(s) propose(s)	1. Without competition, buyers' surplus fell monotonically with the number of sellers, regardless if buyers or sellers were proposing. 2. Without competition, delay increased with the number of Sellers. 3. Competition reduced delay and increased the buyers' Surplus. 4. 8 of 300 negotiations failed (2.7%). Of these, 6 were in treatments with 4 sellers. None were in treatments with competing sellers.
Swope, Wielgus, Cadigan and Schmitt (2011)	1. Single period vs. multiperiod bargaining 2. Simultaneous vs. sequential bargaining 3. Contingent vs. non-contingent contracts	1. 36 of 175 negotiations failed (20.6%). 2. Single period negotiations failed more frequently than multiperiod negotiations (29.4% vs. 12.2%). 3. Sequential negotiations failed somewhat more frequently than Simultaneous negotiations (16.7% vs. 12.5%). 4. Negotiations failed more frequently with non-contingent contracts than with contingent contracts (32.2% vs. 16.7%).
Collins and Isaac (2012)	1. Contingent contracts vs. non-contingent contracts with a capital constraint 2. Private vs. public information regarding buyers' willingness to pay, capital constraint, offers and acceptances	1. Negotiations failed more frequently with constrained non-contingent contracts than with contingent contracts (6% vs. 54%). 2. Negotiation failure was equally likely with private and public information. 3. Sellers who held out did not earn more on average than sellers who did not.

		4. Buyers' expected earnings were equal with contingent and non-contingent contracts, but variance was lower with non-contingent contracts.
Parente and Winn (2012)	1. Simultaneous vs. sequential offers to sellers 2. Low vs. high vs. uncertain signals of the buyer's maximum willingness to pay (WTP) 3. Strict complementarity (3 of 3 parcels must be assembled) vs. partial complementarity (2 of 3 parcels must be assembled)	1. Final prices were lower with a) simultaneous offers than sequential offers, b) low signals of WTP than uncertain or high signals, and c) partial complementarity than full complementarity. 2. With strict complementarity 14% of negotiations failed. Failure rates were lower with sequential offers than simultaneous offers. 3. With partial complementarity less than 1% of negotiations failed.
Shupp, Cadigan, Schmitt and Swope (2013)	1. Buyer proposes (first) vs. sellers propose (first) 2. Persistent proposer role vs. alternating proposers role 3. Buyer's value and sellers' costs known (certain) vs. value and costs drawn from known distributions (uncertain)	1. 7 of 182 (3.8%) of negotiations failed. 2. 5 of the failed negotiations (71.4%) occurred when values and costs were uncertain. 3. Final prices favored the (first) proposer. 4. Final prices were not significantly different when the buyer and sellers alternated proposals than when one side proposed persistently.
Cadigan, Schmitt and Swope (2014)	1. Buyer proposes vs. sellers propose 2. Costly vs. costless delay 3. Symmetric delay costs to the buyer and sellers vs. delay costs to the buyer only 4. Multi-round negotiation required vs. credible commitment to ultimatum offer	1. Proposing buyers earned more when they had the option to make an ultimatum offer. Proposing sellers earned less when they could make an ultimatum offer. 2. A larger share of surplus went to sellers when delay costs were asymmetric. 3. 16 of 235 negotiations failed (6.8%). Of these, 15 (93.8%) were in treatments with asymmetric delay costs. 10 of the failed negotiations (62.5%) were in treatments where the proposer could make an ultimatum offer.
Zillante, Read and Schwarz (2014)	1. Contingent contracts vs. contracts with a contingent payment and a non-contingent payment (combination) 2. Buyer's value is known to sellers vs. buyer's value is unknown to sellers	1. 16 of 66 negotiations failed (24.2%). Neither of the treatment variables had a statistically significant effect on the rate of aggregation. 2. Sellers rejected the buyers' offers a total of 712 times. 296 of these rejections (41.6%) were strategic, in that the

		offer exceeded the seller's property value. 3. Negotiations were faster with contingent contracts. However, buyers' earnings were higher with contracts that included a non-contingent component.
Kitchens and Roomets (2015)	Contingent contracts vs. eminent domain with fixed court costs	1. Without eminent domain 1 of 12 negotiations (8.3%) failed. 2. With eminent domain 3 of 44 properties (6.8%) were purchased through forced sale. 3. Participants achieved 91.7% of the available gains from trade without eminent domain and 93.2% with it. The difference was not statistically significant. 4. Nash bargaining theory predicted that in the contingent contracts treatment sellers who bargained earlier would receive higher prices than those who bargained later. However, sellers' order in the queue had no effect on the price at which they sold.
Isaac, Kitchens and Portillo (2016)	1. Fraction of properties required for assembly (4 of 4 vs. 3 of 4 vs. 2 of 4) 2. Presence vs. absence of an alternative seller	1. When assembly required 4 of 4 parcels 19 of 32 negotiations (59.4%) failed. 2. With a 3 of 4 assembly requirement 5 of 32 negotiations (15.6%) of negotiations failed. 3. With a 2 of 3 assembly requirement 0 of 32 negotiations failed. 4. When the buyer could assemble 4 of 4 parcels or purchase from an alternative seller 1 of 28 negotiations (3.6%) failed

Table 2. Experimental parameters

Parameter	Value
Buyer exchange rate	\$1.00 = 2 points
Primary seller exchange rate	\$1.00 = 4 points
Alternative seller exchange rate	\$1.00 = 7 points
Distribution of primary sellers' values, v_i	[50,100]
Distribution of buyer's primary value, V	[100,250]
Buyer's alternative value	$0.8V$
Distribution of alternative seller's value, v_a	[80,160]
Negotiating periods	5
Delay cost per round	5%
Set of fair market prices in contest	$\{40,50,60\}$

Table 3. Sessions and observations by treatment

Treatment	Sessions	Groups per Session	Negotiations
Baseline	5	3	45
Competition	3	5	45
Eminent Domain	5	3	45
Total	13	--	135

Table 4. Outcomes from our benchmark simulations of negotiations alongside observed results from our experiments. We tested for differences between the benchmark and observed outcomes using Wilcoxon sign rank tests for continuous variables (opening offers, number of periods, efficiency and contest spending) and a binomial tests for the percent of sellers forced to sell.

Measure	Baseline		Competition		Eminent Domain	
	Optimal	Observed	Optimal	Observed	Optimal	Observed
Average opening offer	75.8	58.6***	68.5	64.4†	45	56***
Average number of periods in all rounds	3.3	4.2*	3.0	2.7	1	1.4***
Average number of periods when assembly produces surplus	2.3	3.75**	1.8	2.1	1	1.2*
Average Efficiency	88.5%	81.2%**	89.5%	89.9%	95.1%	80.6%***
Percent of sellers forced to sell	--	--	--	--	0%	33.3%***
Buyer’s average contest spending	--	--	--	--	5	15.7**
Seller’s average contest spending	--	--	--	--	5	15.9***

† Differs from benchmark at $p \leq 0.10$
* Differs from benchmark at $p \leq 0.05$
** Differs from benchmark at $p \leq 0.01$
*** Differs from benchmark at $p \leq 0.001$

Table 5. The loss from delay in the *Baseline* Treatment is similar to the loss from contest spending in the *Eminent Domain* Treatment

	Baseline	Competition	Eminent Domain
Points Available	8,254	13,816	8,370
Points Achieved	6,756	12,227	6,815
	(81.9%)	(88.5%)	(81.4%)
Loss from delay	**1,237**	1,114	157
	(15.0%)	(8.1%)	(1.9%)
Loss from failed assembly	225	2	0
	(2.7%)	(0.0%)	(0.0%)
Loss from inefficient assembly	36	169	249
	(0.4%)	(1.2%)	(3.0%)
Opportunity cost of inefficient assembly	--	304	--
		(2.2%)	
Loss from contest spending	**--**	**--**	**1,149**
			(13.7%)
Total Loss	1,498	1,589	1,555
	(18.1%)	(11.5%)	(18.6%)

Note: Key findings **bolded.**

Figure 1. Deviation of buyers' average first and final offers from the theoretical prediction

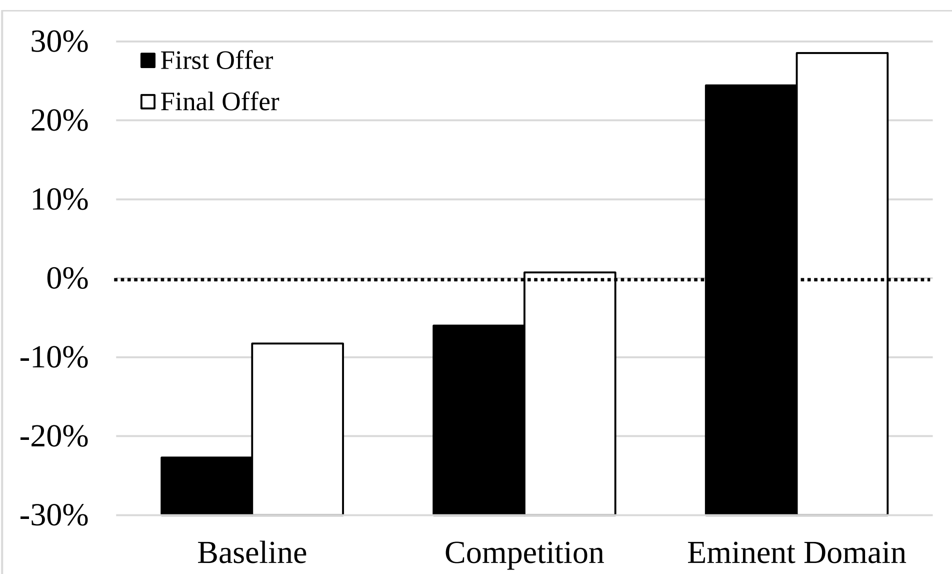

Figure 2. Difference between highest rejected offer and seller's value

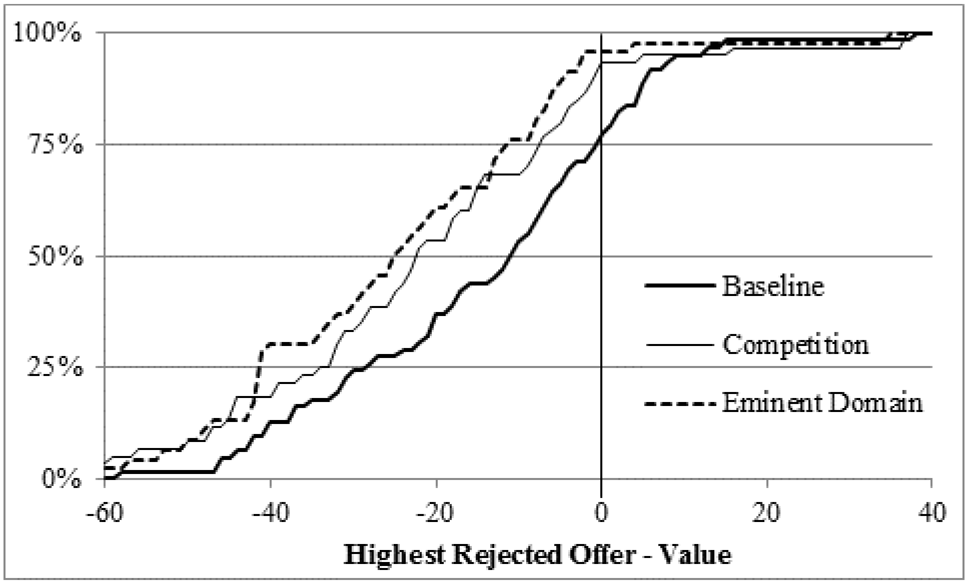

Figure 3. Average efficiency observed in the *Baseline* treatment and the simulated counterfactuals with no seller holdout and no buyer holdout.