Subsidy Incidence in Factor Markets: An Experimental Approach

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Laboratory market experiments are used to estimate the incidence of a stylized subsidy in factor market negotiations with university student and agricultural professional subjects. In separate sessions with both groups, prices converged approximately four and a half tokens higher when a 20-token per-unit subsidy was paid buyers; this equates to 44% of the predicted 10-token split. A proportional market incentive treatment clarifies this subsidy effect. Discrepancies between predicted and observed incidence are similar to previous empirical estimates of subsidy incidence in agricultural land rental markets. A behavioral anomaly as well as buyer–buyer market competition may contribute to experimental results.

Key Words: agricultural policy, *ex-ante* policy analysis, factor market, laboratory market experiments, land rental market, professional vs. student subject pools, subsidy incidence

JEL Classifications: Q1, Q15, Q18, C92

Since the Agricultural Act of 1933, a succession of U.S. farm bills have sought to enhance farmer income. The U.S. government paid out $277 billion to agricultural producers between 1995 and 2011 (Environmental Working Group,

2012). These subsidies largely define American agricultural markets (Sumner, 2007). As subsidized producers purchase inputs, the way in which these payments pass through and are capitalized in factor markets raises important questions about intended beneficiaries, transfer efficiency, and the potential for market distortions associated with different agricultural policies.

Subsidy policies may have compound, unintended economic impacts in factor markets. An important example can be seen in the agricultural land rental market. In the United States, a significant portion—38%—of farmland is rented with higher rates in Midwest states (USDA National Agricultural Statistics Service, 2012). Several policy issues stem from potential impacts of agricultural subsidies in land rental contract negotiations. If tenant producers are able to supplement their incomes with government payments tied to farmland, they may be willing to pay a higher price to rent that

land. In such circumstances, a portion of government payments intended for the farmer may be "passed through" to landowners through higher rental rates. If this is the case, income transfers to tenant producers are not an efficient means to boost incomes for these producers. Additionally, subsidies paid or passed to landowners or input sellers may be capitalized into land or other input values resulting in inflated asset prices. This could inhibit new farmers' entry into the industry and decrease the mobility of assets, obscuring efficient market signals and resource use. Future support reform becomes challenging as policy expectations impact asset values (Tweeten and Zulauf, 2008) and programs become embedded in all aspects of the industry (Sumner, 2007). Government-supported crop insurance and disaster payments also have been found to have a significant unintended impact on land use, notably on increasing conversion rates of native grassland to cropland (Claassen, Cooper, and Carriazo, 2011). Evidence of subsidy impacts on agricultural wage rates and illegal immigration (Luckstead, Devadoss, and Rodriguez, 2012) further illustrates the complex interactions and influence of government payments in agricultural factor markets.

The agricultural land market is emphasized in the literature with regard to the incidence and impacts of income transfers from taxpayers to producers through production subsidies. Bhaskar and Beghin (2009) review empirical studies that indicate distortions occur through land markets as payments are passed onto landowners through higher rents and capitalized into land values. Neoclassical theory on the capitalization of payments in factor markets focuses on price elasticity and predicts that government payments will be capitalized into the factor with the most inelastic supply—land (Floyd, 1965). Net present value models commonly used to identify and quantify variables contributing to land values often rely on this prediction and assume full incidence, that is, the entire subsidy payment is assumed to be capitalized into the value of land (Feichtinger and Salhofer, 2011; Kirwan and Roberts, 2010). Kirwan (2009) contrasts this elasticity-effect prediction with results from an analysis of farm-level data, observing only approximately 25% of agricultural subsidies paid to tenant producers are captured by landlords. To explain this deviation from predictions of full incidence in neoclassical models, Kirwan cites several potential market behavior influences including imperfect competition in agricultural land rental markets as well as social norms, trust, and fairness embedded in tenant–landlord relationships. In a more refined field-level analysis, Kirwan and Roberts (2010) address farm-level aggregation issues, allowing better control for land quality and a more direct link between subsidies and cash rent. Incidence estimates in this study indicate previous results may have been upwardly biased with revised estimates of 14–24% pass-through to landlords. Additionally, they find that this incidence rate falls with each additional year contracted. Again, the authors highlight the need for further investigation into this notable disparity between observed and theorized incidence.

Observations in agricultural land rental markets indicate both customary practices (Young and Burke, 2001) and personal relationships (Kostov, Patton, and McErlean, 2008) may affect contract negotiations. The experimental economics literature as well as related work in psychology and sociology provides a wide body of evidence documenting how many factors other than material self-interest motivate behavior in market and contract negotiations. Fehr and Schmidt (2006) present an extensive review of literature investigating the impact of generosity and fairness in motivating surplus distribution.

As our review indicates, payment incidence observed in agricultural land markets may not reflect the standard economic theoretical prediction based on inelastic supply—full incidence of subsidy values. Moreover, fundamental econometric issues remain in estimating the level of incidence. Continuing with land rental markets as an example, rental rates are highly connected with subsidy payments, expected returns to land from market vs. subsidy income are unobservable (Goodwin, Mishra, and Ortalo-Magné, 2003), and privately negotiated contracts in factor markets are often not public.

Experimental markets are well suited to isolate and explore the impacts of market behaviors on subsidy incidence. Complementing empirical work, the experimental laboratory

provides control of extraneous factors to systematically examine particular aspects of market behavior. Our objective is to gain a better understanding of bargaining behavior in subsidized markets as well as provide a measure of payment incidence in this controlled experimental setting across different subject pools. For our experimental design, specified supply and demand relationships and market competition between buyers and sellers define the predicted equilibrium that can be used as a benchmark from which to compare market outcomes under different treatment conditions. From this competitive equilibrium, we expect subsidy incidence equivalence, that is, buyers and sellers are expected to split a subsidy payment equally through price negotiations.

We present results from an experimental private negotiation, forward market to assess the effect of subsidies on prices and trading decisions in a factor market. A market consists of a trading institution and method of delivery. The trading institution defines the rules by which buyers and sellers interact and arrive at trades. Private negotiation is the relevant trading institution in many agricultural factor markets, especially agricultural land rental markets. In private negotiation, two agents, a buyer and a seller, make offers and counteroffers until there is agreement on price and other contractual arrangements. Two methods may be used in delivery of goods traded: advance production or forward delivery. In advance production, sellers enter a market with inventory in stock, incurring sunk costs before sales; in a forward market transaction, price and quantity are agreed on before production (Menkhaus et al., 2000). Generally, in land as well as labor and capital markets, sunk costs associated with advance production (and their resulting risks and incentives) are not relevant. Forward contracting also occurs in agricultural input supply markets (Haydu, Myers, and Thompson, 1992; Mishra and Perry, 1999). Additionally, forward delivery is a standard "simplest" market design in experimental markets (Phillips, Menkhaus, and Krogmeier, 2001). A forward delivery market therefore is used in this research.

The experimental design described subsequently controls for market power and established customs and personal relationships between buyers and sellers. Treatments are designed to measure the incidence of subsidy payments and an equivalent market incentive in a simulated factor market. We use both a convenience sample of university students and compare their data with data collected in the field from sessions with agricultural professionals. This allows us to explore how robust incidence phenomena may be between economic agent types who may bring different customs or norms as well as experience to the negotiation or bargaining process.

Interestingly, we find similar incidence rates but differing negotiation patterns among the two subject groups. Furthermore, results indicate, in accordance with econometric studies cited previously, that rates of subsidy incidence are not well predicted by theory. This disparity occurs even in our experimental markets that control for a number of factors that may affect data traditionally used in econometric studies. In an additional treatment conducted with student subjects only, we find that market behavior differs depending on whether incentives are framed as a subsidy payment or incorporated into the underlying terms of trade.

Methods

In this study we propose a laboratory market as a method to observe subsidy incidence resulting from competitive market negotiations. Our experimental design defines aspects of the trading environment that may influence actual trade contracts to focus on bargaining behavior in response to a specific, simplified subsidy.

Experimental Design

Each experimental session consisted of one replication of one of three treatments, two support policy treatments—no-subsidy or a per-unit buyer subsidy—as well as a corresponding market incentive or revised buyer schedule treatment. Replications of the two support policy treatments were conducted with both student and agricultural professionals, each with a new set of participants. Replications of the third market incentive treatment were conducted only

Table 1. Summary of Experimental Treatments Conducted

Subject Group Treatment	Abbreviation	Number of Replications Completed	Number of Participants
Student participants			
No subsidy	NS	3	24
Per-unit subsidy	PU	3	24
Revised buyer schedule	RBS	3	24
Professional participants			
No subsidy	NS	4	28[a]
Per-unit subsidy	PU	3[b]	24

[a] Two of the four replications were conducted with six, rather than eight, participants.
[b] Data from one of four sessions conducted was dropped as a result of a system crash.

in sessions with student participants. In total, there were five subject group/treatment combinations as summarized in Table 1. A detailed description of the laboratory market and experimental design procedures followed for each session, descriptions of experimental treatments and subject pools, payment procedures as well as methods used to collect and analyze data follow.

Laboratory Market Design and Procedures. Trades in the laboratory market were negotiated between buyer–seller pairs by submitting bids and offers over a computer network. Computers were equipped with privacy screens and trading pairs were identified only by an assigned number. Privacy and anonymity minimize subject bias in the market, controlling for behavioral issues of reputation building and personal relationships—both of which may affect market transactions. Experimental sessions using student participants were conducted in a campus experimental economics laboratory. Sessions with agricultural professionals were conducted using a mobile laboratory of laptop computers linked through a wireless network at recruitment area locations.

Each experimental session followed a standard procedure (Davis and Holt, 1993, pp. 22–23).[1] At the beginning of each session, eight participants were randomly designated as four buyers and four sellers. Instructions were presented outlining payment procedures, the basic market design, trading mechanics and rules, profit and earnings calculations for buyers and sellers, and a description of the per-unit buyer subsidy for sessions including this payment. To encourage anonymity and independence in the market, interaction between participants in the room was discouraged by requesting participants to refrain from talking and to turn off all electronic devices before and during the session. Participants were encouraged to ask any clarifying questions of the experimenter and then traded in one or more practice periods. Practice periods used similar treatment procedures but different costs and values than the primary experiment. Actual trading did not begin until all participants reported they were comfortable with the mechanics of trading and understood how to submit bids and offers, make a trade, and how their unit profits (market as well as subsidy earnings, if applicable) and total earnings were calculated. Following instructions and practice periods, 20 or more trading periods, each consisting of three 1-minute bargaining rounds, were conducted during each experimental session. Participants did not know when the session would end to avoid strategic behavior in the final period.

Unit redemption value and cost schedules provided to each player—which were linked through trading performance to actual cash payments at the end of the experimental session—motivated participants to maximize trade profits.

[1] Experiment instructions and presentation slides are available from the authors on request.

Table 2. Unit Buyer Redemption Value (RV), Buyer Per-Unit Subsidy, and Seller Cost (Cost) Amounts (tokens) by Treatment

|	Treatment								
	No Subsidy			Per-Unit Subsidy			Revised Buyer Schedule		
	Buyer		Seller	Buyer		Seller	Buyer		Seller
Unit	RV	Subsidy	Cost	RV	Subsidy	Cost	RV	Subsidy	Cost
1	130	0	30	130	20	30	150	0	30
2	120	0	40	120	20	40	140	0	40
3	110	0	50	110	20	50	130	0	50
4	100	0	60	100	20	60	120	0	60
5	90	0	70	90	20	70	110	0	70
6	80	0	80	80	20	80	100	0	80
7	70	0	90	70	20	90	90	0	90
8	60	0	100	60	20	100	80	0	100

The importance of induced value performance to motivate economic decision-making in an experimental setting is emphasized by Davis and Holt (1993, pp. 24–26) and also by Smith (1976). Before each trading period and during each bargaining round, each buyer was shown unit redemption values for eight units they could purchase in that period. Each seller was shown a table of unit costs for eight units available to sell in that period. Following standard practice for an induced value experiment (Davis and Holt, 1993, p. 127; Smith, 1976), each buyer/seller only saw their own value/cost schedule for the units available to them for trading in each period. For all buyers in both no-subsidy and per-unit subsidy treatments, redemption values started at 130 tokens for the first unit and decreased by 10 to 60 tokens for the eighth unit; all sellers' costs began at 30 tokens increasing by 10 to 100 tokens for the eighth unit. Buyers' redemption values for each unit in the revised buyer schedule treatment were 20 tokens higher, starting at 150 tokens for the first unit and decreasing by 10–80 tokens for the eighth unit. Complete value and cost schedules used for each treatment are listed in Table 2.

Individual and aggregate unit cost and unit redemption value schedules are step functions modified from a simple market design presented by Davis and Holt (1993, pp. 9–14). Summing the aggregate supply (cost) and demand (redemption value) relationships results in induced supply and demand from which equilibrium market outcomes are predicted. For the base unit values and unit costs, with four buyers and four sellers, the expected equilibrium price and number of trades are, respectively, 80 tokens and a quantity tunnel of 20–24 units (Figure 1).[2] At this competitive equilibrium, expected earnings are 150 tokens per buyer and 150 tokens per seller for each trading period resulting in a relative earnings measure (calculated as seller earnings minus buyer earnings) of zero. Expected total earnings at the predicted equilibrium for four buyers and four sellers is 1200 tokens.

Units were traded sequentially, starting with the first unit. As trades were made, buyers earned the difference between the redemption value for the unit traded and the agreed price; sellers earned the agreed price minus their unit cost. Following this experimental design, market power is controlled through uniform cost and redemption schedules and symmetrical numbers of buyers and sellers in the market. Aside from specific treatment incentives, all individuals are on equal footing as they enter the market.

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[2] Discrete step functions, which result from aggregating multiple players using the same cost and value schedules, necessarily result in either a price tunnel or a quantity tunnel (like in our design) (Davis and Holt, 1993, p. 130).
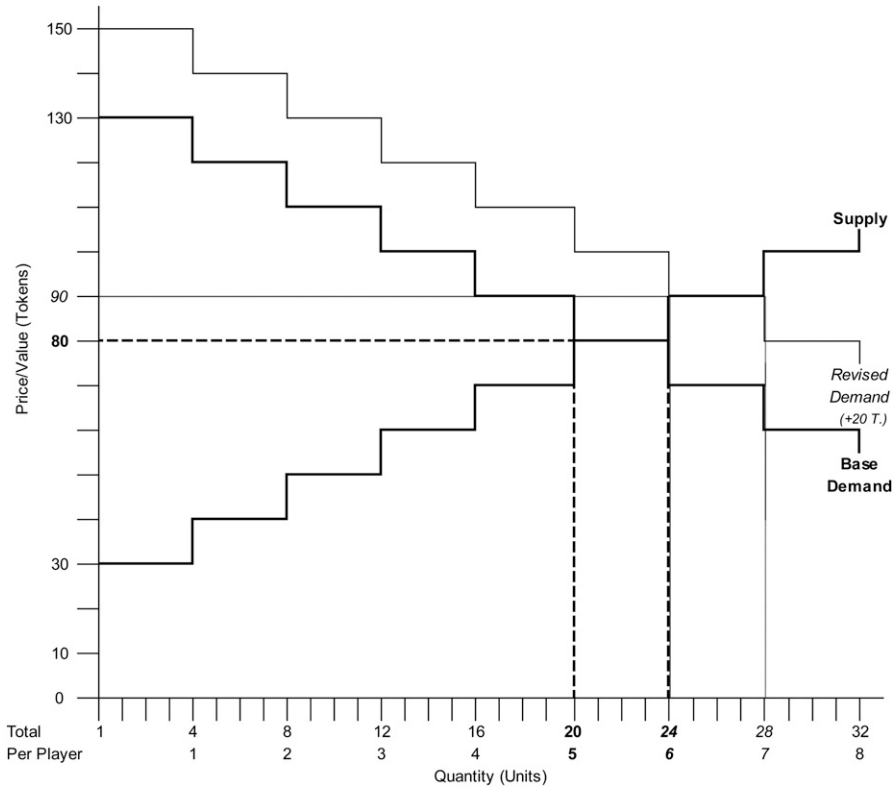
Figure 1. Induced Aggregate Market Supply and Demand (and revised demand) for Four Buyers and Four Sellers

Each trading period consisted of three 1-minute bargaining rounds.[3] Each buyer and each seller could trade up to eight units during each three-round trading period. Three times per period, at the beginning of each new bargaining round, buyers and sellers were randomly paired to negotiate prices and trade. Random matching is a base design and, along

with anonymity, controls for the confounding effects reputation can have on outcomes. Moreover, anonymous pairing controls for influences such as personal relationships. The seconds remaining in each round ticked down from the 1-minute mark on each trader's screen until all available units were sold or the round timed out. During a bargaining round, trading pairs were free to enter offers and counteroffers between themselves, similar to the rules of a double auction. A buyer could raise a bid without waiting for a response from the seller, and a seller could lower an offer without waiting on response from the buyer. Players could, if they wished, break off negotiations by simply no longer entering or responding to bids or offers and waiting for the next bargaining round. Again, following common practice (Davis and Holt, 1993, p. 41), an improvement rule was implemented in which buyers were bound to make progressively higher bids and sellers to make progressively lower offers. In

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[3] In previous work, both five and three bargaining rounds or matches per trading period have been used. Bargaining opportunities in factor markets are often geographically constrained, limiting the overall number of potential landlord–tenant matches, for example. A market design with three bargaining rounds in a trading period addresses risk associated with limited matches in factor markets. Results from related experiments suggest that this matching risk is important. Menkhaus et al. (2007) includes a more complete discussion of matching risk in experimental and real-world markets where transactions are conducted through private negotiation. Specifically, Menkhaus et al. find that as matching risk increases, traders are more likely to trade units in earlier rounds.

addition to a schedule of unit redemption values or costs, during trading, each player was provided with private trading information including their current bid or offer, their trading partner's current bid or offer, and a calculation of profits they earned as each unit was traded. At the end of each trading period, a summary of each participant's period and total earnings was privately displayed. Each participant was supplied with a pad of paper and pen to make notes on trading or profits if they wished.

Subsidy Treatments and Subject Pools. Two support policy treatments are investigated. The first is a market in which no support is paid out. This no-subsidy treatment allows for comparison of how the market might be impacted under a subsidy policy. Base unit redemption values available to buyers (descending from 130 tokens for unit one by 10s to 60 tokens for unit 8) and unit costs for sellers (ascending from 30 tokens for unit one by 10s to 100 tokens for unit 8) were used, as described previously and presented in Table 2.

The second treatment is a per-unit subsidy treatment in which factor buyers (representing agricultural producers) were paid an additional 20 tokens on each unit they purchased.[4] Again, base unit redemption values and costs presented to buyers and sellers on period information, trading, and recap screens were used (see previously, Table 2). Reflecting the public nature of agricultural policy, all buyers and sellers were made aware of subsidy payments through session instructions for this treatment. Instructions for per-unit subsidy treatment sessions included a visual and verbal statement to all participants that: "In this experiment an additional per-unit subsidy of 20 tokens will be paid to the buyer on each unit traded at the end of each trading period." Participants were reminded of this payment and any questions were openly answered regarding payment and profit calculations during practice periods preceding actual trading.

A third treatment consisted of a revised buyer schedule. In this treatment, each buyer's redemption value schedule was increased by an amount equal to the per-unit subsidy payment, that is, each unit redemption value was increased by 20 tokens (as indicated on Table 2). Buyers viewed their revised value schedule before each trading period and during each bargaining round. Like in the two support policy treatments, sellers viewed only their private cost schedule.

In the base, no-subsidy treatment, given the market design as discussed, buyer and seller earnings at the predicted equilibrium are equal. Buyers and sellers enter this base market with equal prospects given their uniform value and cost schedules. A 20-token per-unit buyer subsidy implies an upward shift in buyers' demand. Thus, at the new subsidized competitive equilibrium, we would expect subsidy incidence equivalence with 10 tokens—50% of the 20-token subsidy—passed from the buyer to the seller through price negotiations, that is, through buyer–seller competition.[5] An even split of the subsidy amount would push the equilibrium price up from 80–90 tokens and quantities traded to a new predicted tunnel of 24–28 units (from Figure 1).

In the revised buyer schedule treatment, the upward shift in demand implied by a per-unit subsidy payment is explicitly incorporated into the buyers' demand schedule, that is, redemption values are increased by 20 tokens per unit. Whether buyers receive an additional payment for each unit sold or are paid the same amount as part of their redemption value when they, in effect, sell this unit back to the experimenter, at the revised equilibrium, we expect negotiated prices reflecting an even split of the additional surplus. This shift in demand results in the new competitive predicted equilibrium depicted in Figure 1. At this revised competitive-level, price is 90 tokens, quantity 24–28 units, expected earnings are 210 tokens per buyer and 210

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[4] The subsidy of 20 tokens is comparable, on a per-unit basis, with that used by Bastian et al. (2008) and Phillips et al. (2010).

[5] Changing the elasticity of supply could result in predicting a subsidy incidence anywhere from zero to the full 20 tokens (100%) passed to the seller depending on if the supply schedule is perfectly elastic (flat) or perfectly inelastic (vertical), respectively. In our design, the predicted payment incidence is an equal share to buyers and sellers.

tokens per seller for each trading period (expected relative earnings, or difference between buyer and seller earnings, are again zero), and expected total earnings for four buyers and four sellers are 1680 tokens.

In the two support policy treatments, the market behavior of 48 university students was compared with that of 52 agricultural professionals. An additional 24 students participated in the revised buyer schedule treatment. University students were recruited on campus, mainly from business and economics classes; professionals working in agriculture were recruited by telephone and e-mail using lists provided by county cooperative extension offices. Following a protocol suggested in Davis and Holt (1993, pp. 58–60), all participant recruitment was conducted using a standard procedure. Invitations used language that was careful not to suggest any expected behavior or provide detail about the market or experimental treatments beyond a general outline. Recruitment of individuals discouraged participation of groups with previous social connections. A number of alternates recruited for each session were paid the show-up fee and asked to sign up for a following session.

Payment Procedures. Participants in both subject pools were paid based on their earnings in the experimental market. Earnings were denoted in a monetarily convertible currency referred to as tokens (one token equaled 1 cent). Market earnings accumulated during the sequence of trading periods and token earnings were cashed in at the end of the experiment.

Students were paid a $7 show-up fee in addition to their market earnings. Unlike student participants, who had lower opportunity costs and attended sessions easily accessible to them on campus, professional recruits often had to travel significant distances and take time away from their jobs or responsibilities. To compensate them for their time and travel costs, each professional participant received a $50 show-up fee in addition to their market earnings.

Analysis

Data are described both graphically and with parameters estimated using a convergence model.

The following general convergence model, based on those developed by Ashenfelter et al. (1992) and Noussair, Plott, and Riezman (1995), was used to describe the convergence process of market outcomes for each subject group and to conduct tests for statistical differences in variables of interest across treatments:

(1)
$$Z_{it} = B_0 \left[\frac{(t-1)}{t} \right] + B_1 \left(\frac{1}{t} \right)$$
$$+ \sum_{j=1}^{i-1} \alpha_j D_j \left[\frac{(t-1)}{t} \right]$$
$$+ \sum_{j=1}^{i-1} \beta_j D_j \left(\frac{1}{t} \right) + u_{it}$$

where Z_{it} is the variable of interest such as average sale price, number of units traded, or earnings outcome across replications for each of t trading periods $(1, \ldots, 20)$ in the treatment cross-section i; B_0 is the predicted asymptote and B_1 is the starting level of the dependent variable (price, trades, or earnings outcome) for the base (no-subsidy) treatment; α and β are, respectively, adjustments to the asymptote and starting level for each treatment's relation to the base; D is a dummy variable representing the jth treatment (equal to zero for the base treatment and one for the compared treatment); and u_{it} is an error term. The Parks method (Parks, 1967) was used to estimate the model because it accounts for unique statistical properties resulting from the panel data sets. Analyses were conducted in SAS using the PANEL Procedure (SAS, 1999).

Because we are interested in describing converged market outcome levels over time, estimated asymptotes (B_0) are reported for the base convergence asymptote and asymptote adjustment terms (α_j) are reported for each test treatment (j) (Table 3). Respective treatment adjustments are added to base parameter estimates to calculate convergence levels for each treatment.

Parameter estimates and treatment adjustments for estimated asymptotes from the convergence model allow for statistical comparisons between treatments within each subject pool. To conduct statistical tests for differences in converged market outcome levels between test treatments, data were assessed for a normal distribution of residuals.

Table 3. Estimated Base, No Subsidy Asymptotes (B_0), and Treatment Adjustment Coefficients (α_j) (standard errors) for Market Outcomes by Subject Group

	Market Outcome			
Subject Group Treatment	Price (tokens)	Number of Trades	Relative Earnings (SE-BE)	Total Earnings (tokens)
Student participants				
No subsidy (B_0)	81.65 (0.55)	16.53 (0.14)	14.74 (5.28)	1064.51 (4.96)
Per-unit subsidy (α_{PU})	4.31[a,c] (1.04)	0.91[a,c] (0.31)	49.75[a,c] (8.38)	16.73[a] (12.22)
Revised buyer schedule (α_{RBS})	8.70[bc] (0.67)	4.30[b,c] (0.55)	−8.45[a] (5.92)	462.57[b,c] (23.53)
Professional participants				
No subsidy (B_0)	76.03 (0.95)	18.97 (0.25)	−39.40 (9.67)	1177.73 (4.67)
Per-unit subsidy (α_{PU})	4.42[c] (0.96)	0.20 (0.32)	48.73[c] (9.35)	−34.86[c] (7.04)

[a,b] Different letters indicate a significant difference between pairwise adjusted convergence levels, 95% confidence level.
[c] The adjusted convergence level is significantly different from no-subsidy base, 95% confidence level.

Market Results

Three replications were conducted for each of the three treatments—no-subsidy, per-unit subsidy, and revised buyer schedule—in nine separate sessions with student participants. Four replications were planned—for a total of eight sessions—with professional subjects, each session consisting of one replication of one of the two support policy treatments—no-subsidy and per-unit subsidy. The additional fourth replication was added for each treatment with professional subjects as a precaution after initial problems with attendance and technical issues related to the remote laboratory facilities. Data from one of these sessions were eliminated from analysis as a result of a system crash early in trading. Additionally, two professional sessions were conducted with six, rather than eight, participants (as indicated in Table 1).[6]

Average market earnings, paid to participants in addition to their $7 (student) or $50 (professional) show-up fee, were $34.53. Sessions, including instructions and practice rounds, generally lasted 90 minutes.

The data represent average prices, trading levels, relative earnings, and total earnings for each trading period across the three or four replications by policy treatment of each subject pool. Experimental results include estimated convergence levels as well as a graphic illustration of each market outcome per period by treatment of each subject pool.[7] Table 3 includes parameter estimates (base asymptote and treatment adjustments) as well as statistical tests for differences between treatments from the convergence analysis. Figures 2 through 5 graphically illustrate market outcomes for prices, trades, relative earnings, and total earnings over 20 trading periods by treatment, comparing student and professional participant groups. Additional outcomes including model statistics, starting levels from the convergence model, and predicted equilibria by subject group and

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[6] Trade data for these sessions were corrected to account for six rather than eight players by multiplying period trades outcomes by 1.333. A statistical comparison was conducted using convergence model estimates, which indicate no difference between two professional sessions with eight and the two sessions with six players (no difference in converged price, t-value = 0.34, p value = 0.74; no difference in converged trades, t-value = −1.29, p value = 0.21).

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[7] Primary interest of this study is the difference between subsidy treatments within each subject group. The convergence model analysis is therefore conducted separately for each subject pool. Given the nature of variability across subject pools, normality is violated. Statistical tests across the student and professional participants thus require nonparametric (Wilcoxon) tests, which do not show statistical differences across subject pools for price and relative earnings. We do, however, see significant differences between no-subsidy and subsidy treatments for each group for trades and total earnings.
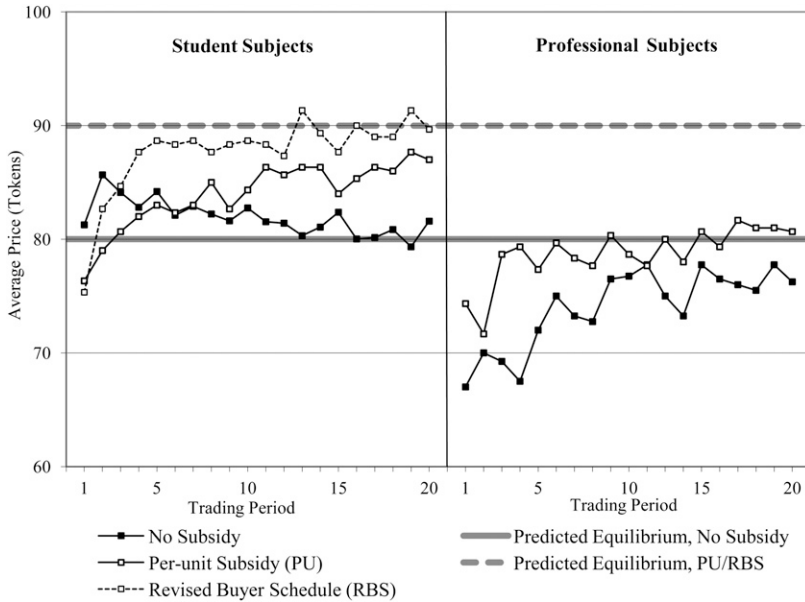
Figure 2. Predicted Equilibria and Observed Average Market Prices (tokens) per Trading Period by Treatment for Student and Professional Subjects

treatment are available from the authors on request.

Price

In student results, negotiated prices in the base market with no-subsidy converged to an estimated value of 81.65 tokens, near the base predicted equilibrium of 80 tokens. When buyers in the market were paid a 20-token subsidy on each unit they purchased, prices rose by 4.31 tokens, converging to an estimated 85.96 tokens in the student sessions (Table 3; Figure 2).

Professional subjects negotiated comparatively lower prices than their student counterparts across both support policy treatments. With no subsidy paid out, estimated average prices in markets with professional subjects converged at 76.03 tokens. However, in keeping with the policy treatment effect seen in student results, professional participants negotiated significantly higher prices, converging 4.42 higher at 80.45 tokens, when buyers in the market were paid the 20-token per-unit subsidy (Table 3; Figure 2). Within each subject group, estimated average prices increased by approximately four and a half tokens when a 20-token

payment was made to the buyer on each unit traded, resulting in 43% of the predicted 10 tokens passed from subsidized buyers to sellers for students and 44% for professionals relative to the respective no-subsidy base price estimate for each group.

Prices observed with market incentive proportional to the unit subsidy conducted with student participants are much closer to the predicted equilibrium of 90 tokens. Prices in this revised buyer schedule treatment increase significantly, 8.70 tokens higher than the no-subsidy base to an estimated asymptote of 90.35 tokens. Relative to the base, 87% of the predicted 10-token share is passed from buyers trading with redemption values increased by 20 tokens per unit to sellers through price negotiations. Results from the proportional market incentive highlight the interesting deviation from theoretical outcomes that we observe when incentives are specifically framed as subsidy payments because the predicted equilibrium would be expected whether buyers received a subsidy or an equivalent market incentive.

Consistent with early experiments of the kind conducted in this study, average prices (Figure 2) exhibit wide variances, particularly
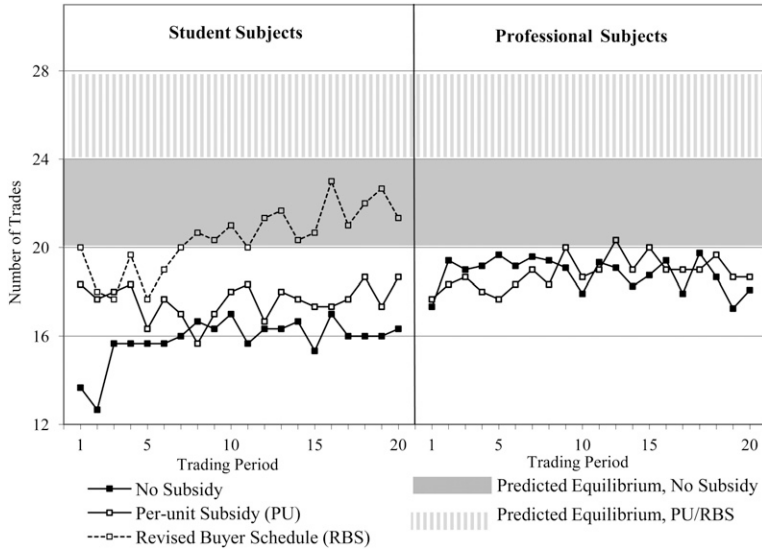
Figure 3. Predicted Equilibria and Observed Number of Trades per Period by Treatment for Student and Professional Subjects

in early trading periods, before approaching respective treatment convergence levels (Grether and Plott, 1984; Hong and Plott, 1982). The convergence patterns are similar for four of the five treatment series—converging to their respective equilibrium levels from below, perhaps as a result of learning, indicating a surplus advantage to buyers. The no-subsidy treatment

with the student group is the exception, although more consistent with the two previously mentioned articles. Still, price in this treatment, given time, converges to a level near the predicted competitive equilibrium. Why the difference in these patterns across treatments and subject pools? The explanation may be as simple as that recognized by Plott (1982, p. 1496) for
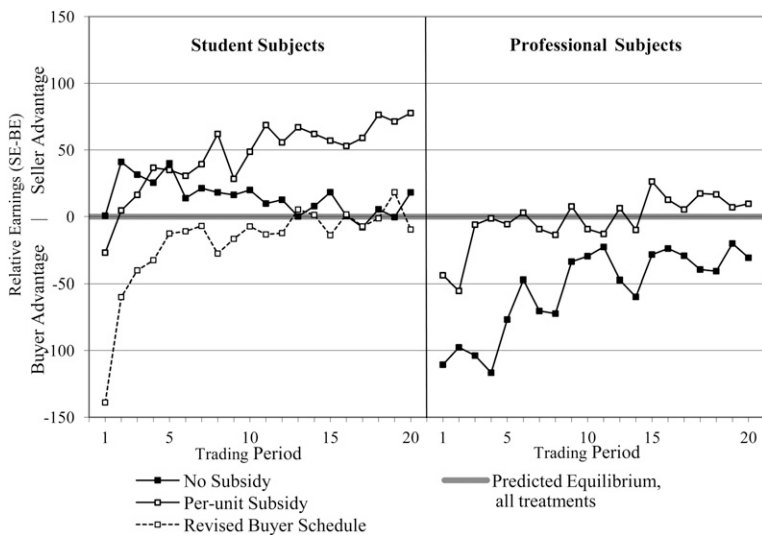
Figure 4. Predicted Equilibrium and Observed Relative Earnings (seller earnings less buyer earnings) per Period by Treatment for Student and Professional Subjects
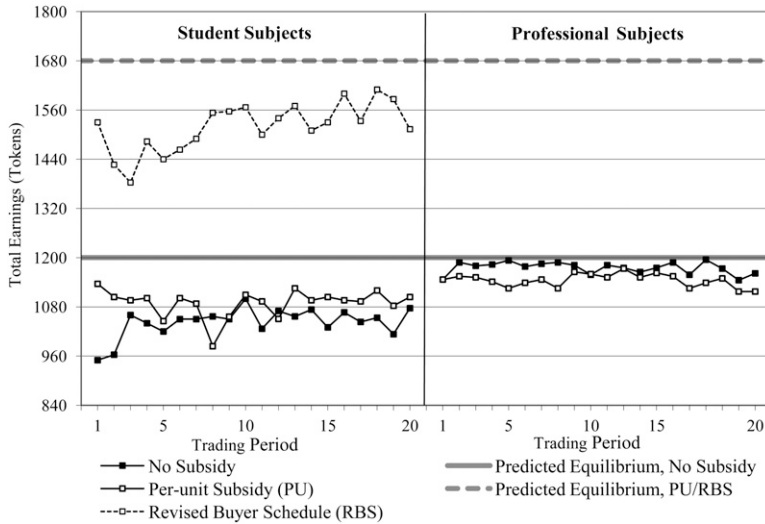
Figure 5.   Predicted Equilibria and Observed Total Earnings (tokens) per Period by Treatment for Student and Professional Subjects

private negotiation experiments—some agents are just better negotiators than others, but the source of this (dis)advantage is unknown. Nevertheless, given time, there is convergence to equilibrium levels in all treatments from which treatment effects can be estimated (Table 3).

Trades

The number of trades per period in student markets converged at an estimated level of 16.53 in the no-subsidy treatment. A subsidy paid to the buyer on each unit traded resulted in a significant increase of just under one trade (0.91) or an estimated convergence level of 17.44 trades per period (Table 3; Figure 3).

The number of trades was higher overall in markets with professional subjects. Trades converged at an estimated level of 18.97 in the no-subsidy treatment. Although the incentive to trade increased (by 0.20 to an estimated 19.17 units) when professional buyers were paid a subsidy, the increase was not statistically significant (Table 3). Higher trade levels observed from the professional group, as compared with the student pool, contributed to relatively lower prices in the professional pool. The number of trades negotiated by professional participants with both no-subsidy and per-unit subsidy policy treatments was closer to the predicted no-subsidy

market equilibrium of 20–24 units than the generally lower trading levels observed in student outcomes (Figure 3).

Trading outcomes from the revised buyer schedule treatment were significantly higher than either the no-subsidy or per-unit policy treatments conducted with student participants. Relative to the no-subsidy base, 4.30 more trades are estimated resulting in an adjusted asymptote for the revised buyer schedule treatment of 20.83 (Table 3; Figure 3). This higher number of trades is expected given the design used in the revised buyer schedule treatment.

Overall, trading results are consistent with previous work that reports trading levels significantly below the predicted equilibrium can be expected in the private negotiation trading institution compared with open market auctions. Menkhaus, Phillips, and Bastian (2003) report, with supply and demand conditions identical to those reported here, trading levels significantly lower than the lower bound of the competitive model in both advance-production and forward-delivery private negotiation markets. In a double auction trading institution, in contrast, these authors report convergence levels for trades are within the equilibrium tunnel regardless of the method of delivery. Matching risk pushes trades down, especially with

additional risks incurred in private negotiation trading. These risks are inherent in the private negotiation trading institution, both in real-world institutions, which may be geographically constrained, and in experimental markets with a limited number of trading partners and matching opportunities.

Relative Earnings

Relative earnings, calculated as seller minus buyer market earnings, is a measure comparing earnings advantages: a negative level indicates a buyer advantage and a positive level a seller advantage in the market. At the predicted competitive equilibrium, relative earnings are zero, indicating an even split in market earnings or profits. Convergence estimates for relative earnings outcomes are reported in Table 3 and relative earnings per trading period for each treatment and subject group are illustrated in Figure 4.

With student participants in the base, no-subsidy treatment-estimated relative market earnings indicate a seller advantage of 14.74 tokens over buyers (Table 3). In the graph we see this earnings advantage decrease to nearly an even split by period 13 (Figure 4). Within the student subject group, a 20-token per-unit subsidy paid to buyers increased the base sellers' advantage significantly by an estimated 49.75 tokens: relative to buyers' unsubsidized market earnings, student sellers earned an estimated 64.49 tokens more than buyers each period in the per-unit subsidy treatment (Table 3). This correlates to higher negotiated per-unit subsidy treatment prices reported previously.

Within the professional subject pool, relative earnings indicate a buyers' advantage in the no-subsidy base. Estimated relative market earnings (not including subsidy payments) increased significantly by 48.73 tokens with a per-unit subsidy paid to buyers in the professionals market, resulting in a slight sellers' advantage (9.33 tokens) (Table 3). Despite overall lower prices giving buyers in the professional sessions an advantage, the magnitude in the shift in earnings in response to the per-unit subsidy treatment is commensurate to that observed with student participants.

Relative earnings for student participants in the revised buyer schedule treatment show an initial five periods with a large buyer advantage. However, earnings by the last five periods are nearly even (Figure 4). Relative earnings convergence model estimates for this treatment are not significantly different from the base, no-subsidy asymptote, which suggests seller and buyer earnings are statistically equal. In Figure 4 we see similar, fairly even relative earnings in both the no-subsidy and revised buyer schedule treatment after approximately period 10.

Total Earnings

Total earnings is a measure of market efficiency based on trading levels. The predicted competitive equilibrium generates the maximum possible surplus, which, given our base cost and redemption schedules with four buyers and four sellers, is 1200 tokens. Total earnings convergence outcomes are reported in Table 3 and total earnings observed each period for each treatment and subject group are illustrated in Figure 5.

With student participants, total earnings in the no-subsidy base treatment and per-unit subsidy treatment are similar, seen converging in Figure 5 at approximately 1070 tokens or approximately 90% of the maximum market surplus. The convergence model estimates total earnings for the base, no-subsidy treatment at 1064.51 tokens with no significant difference between the no-subsidy and per-unit subsidy treatments (Table 3).[8]

Professional subjects' total earnings in the base, no-subsidy treatment has an estimated asymptote of 1177.73 tokens. Estimated total earnings in the per-unit subsidy treatment is 34.86 tokens lower than the base, generating an asymptote of 1142.90 (Table 3). In Figure 5 we

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[8] Total earnings data from the student subject pool data did not meet our standard for normal distribution of residuals (H_O: normal, $\alpha = 0.01$) based on the Shapiro-Wilk test statistic (W 0.9446, p value = 0.0087). However, visual and descriptive tests indicated that a single low outlier in the first trading period of one session contributed to nonnormality. Based on this, we consider student total earnings data to be normally distributed for descriptive purposes in reporting statistical differences between converged treatment levels.

see total earnings for the no-subsidy treatment generally staying around 1175 tokens, or approximately 98% of the maximum surplus; per-unit subsidy total earnings is at approximately 1145 tokens, indicating an efficiency rate of approximately 95% of market surplus.

Comparing overall total earnings results for student and professional subject groups, we see that higher numbers of trades negotiated by professional participants, as expected, resulted in higher total earnings in both the no-subsidy and per-unit subsidy treatments. Within each subject group, the impact of a subsidy payment is negligible with a slightly lower efficiency rate observed in the professional group and no significant impact seen in student sessions.

Total earnings at the predicted equilibrium (that is, maximum surplus) in the revised buyer schedule treatment is 1680 tokens. With student subjects, estimated total earnings in this treatment were 462.57 tokens higher than the no-subsidy base, converging at 1527.07 tokens or approximately 91% of the revised maximum surplus (Table 3). This rate is similar to 90% efficiency observed in student no-subsidy and per-unit subsidy treatments.

Summary and Discussion

Laboratory results indicate the incidence of a per-unit buyer subsidy was very similar for students and agricultural professionals with identical market structures and institutions. Prices converged approximately four and a half tokens higher within each group with a 20-token per-unit subsidy paid to buyers. This equates to approximately 44% of the predicted 10 tokens being passed on to sellers in the market through higher negotiated prices in subsidized markets relative to the no-subsidy base. At the competitive equilibrium in our laboratory market design, we expect subsidy incidence equivalence, generating a price that splits subsidy surplus evenly between buyers and sellers. Compared with this benchmark, we observe approximately half of the predicted incidence or price pass-through. In contrast, 14–25% incidence rates reported in recent empirical studies on land rental markets (Kirwan, 2009; Kirwan and Roberts, 2010) are only approximately

one-fourth compared with full incidence expected from theory given inelastic supply. It should be noted that these studies examined only subsidy impacts on land rental prices and that further incidence could have occurred in other input markets.

It is notable that in empirical land rental market studies as well as in our experimental factor market, we observe a similar phenomenon, that is, a discrepancy between predicted and observed incidence. That only a portion of the predicted incidence is observed from the two methodologies is important. An additional market incentive treatment clarifies this subsidy effect.

To differentiate between market incentives influenced by experimental framing effects and payments in the subsidy treatment, an additional revised buyer schedule treatment was conducted with student participants. Interestingly, when buyers traded using revised redemption values, which were increased by 20 tokens per unit (rather than being given this amount as an additional payment of 20 tokens on each unit traded), negotiated prices increased by nearly the entire predicted amount of 10 tokens: relative to the base, 87% of predicted share passed from buyers to sellers. The outcome from this treatment highlights that the interesting deviation from theoretical outcomes that we observe in our experimental factor market is specifically a subsidy effect. Whether additional available surplus is presented as a subsidy or a change in buyers' redemption values matters during negotiation or bargaining.

We pose two possible explanations for the particular incidence rates observed in the experimental setting, one a behavioral anomaly and one stemming from market competition. It may be that—similar to surplus splits observed in ultimatum games and other behavioral experiments examining human tendencies to act based on feelings of generosity and reciprocity—motivations outside of economic competitive self-interest are at play. That is, subsidized buyers may feel more generous in price negotiations; likewise sellers' expectations of a "fair share" may influence their bargaining behavior when they are aware of a subsidy paid to buyers. In an extensive review of a wide range of

literature in behavioral economics and psychology, Fehr and Schmidt (2006) conclude "the evidence indicates that other-regarding preferences are important for bilateral negotiations [and] for understanding the functioning of markets and economic incentives" (p. 77). Comparatively higher prices negotiated when traders bargained over units with higher redemption values than for units tied to an equivalent, public per-unit buyer subsidy may suggest a tendency for traders to assume buyers were more entitled to keep market surplus presented as a subsidy payment.

Although other-regarding behavior may explain a portion of the price shifts we observe, competition between buyers may also influence bargaining for subsidized units. In addition to buyer–seller competition, buyer–buyer and seller–seller competitions influence bargaining. Here, the buyer–buyer competition is more relevant than seller–seller competition. When additional surplus is made available to buyers in our laboratory market, there is an increase in market competition among the four buyers for a limited supply of more valuable units. If buyers could coordinate or had monopsony power, subsidy incidence resulting from buyer–buyer competition could be zero; if there were many buyers (in the absence of motivations such as generosity or fairness), this incidence might approach 100% of the subsidy amount. Buyer–buyer competition appears to be mitigated when surplus is made available to buyers in a market as a public subsidy rather than as a private increase in unit redemption values. Further treatments varying buyers' market power would provide insight into the presence and magnitude of buyer–buyer competition.

Although the treatment effect measuring subsidy incidence is similar between the two subject pools, there is a behavioral difference in price levels and the number of trades made by students and professionals. The number of trades conducted per period by professional participants was notably higher than for students. This higher number of trades negotiated by professionals contributes to an explanation of lower price levels negotiated by this subject pool. The associated earnings advantage enjoyed by professional-subject buyers relative to student

buyers can be attributed to generally lower prices negotiated by professional group traders. Differences in market efficiency observed between subject pools follow trading levels with professional participants extracting more of the total available market surplus than their student counterparts.

An explanation for the difference in the trading levels between students and professionals goes beyond the experiment design used in this study. What can be said about the differences between these two subject pools? Professionals likely were more experienced traders than students, particularly in private negotiation. This could contribute to more efficient negotiations for price and a corresponding increase in the number of trades completed during a trading period in our laboratory market. Despite these subject pool differences, the consistent subsidy treatment effect between students and professionals is encouraging, particularly in the use of experimental laboratory methods for *ex-ante* analyses of agricultural policies. We are aware of only one other study (Herberich and List, 2012) comparing experimental results from students and U.S. agricultural professionals. This study concluded that "there is suggestive evidence that farmers are slightly more risk averse than students" (p. 463).

Results suggest that even in a controlled, simplified laboratory trading environment, a payment incidence phenomenon that deviates from predicted incidence is observed. This suggests payment incidence phenomena exist absent of such things as quality differences or norms and expectations specific to certain factors and in an environment where buyers and sellers have equal resource bases. This result has implications for any future income transfer policy recommendation or design in which resulting allocative inefficiencies are a concern.

Although these results are an important first step, they indicate that economic experiments offer a way to isolate and study potential behaviors contributing to payment incidence and thus may provide important information regarding potential impacts of policies designed to either control for payment incidence phenomena or address other policy goals such as decoupling. For example, experimental treatments extending

the research presented here might tease out potential specific impacts of behavioral anomalies and market competition. Preliminary work has been conducted examining the impact of asymmetric knowledge of subsidy payments to control for sellers' expectations vs. buyers' generosity in splitting a portion of the subsidy (Bastian et al., 2011). With such results, policies that mitigate potential issues found previously could be tested in the laboratory before implementation.

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