Abstract: A basic tenet in microeconomics is tax incidence equivalence, which holds that the burden of a unit tax on buyers and sellers is independent of who actually pays the tax. By contrast, policymakers and the public often mistake statutory incidence for economic incidence. Using competitive laboratory markets, I test both tax incidence equivalence and an analogous theorem for subsidies. For sufficiently large markets, the results show strong support for both theories; there is little to no evidence, even in the short run, of the popular misperception that statutory incidence equals economic incidence. In smaller markets in which competitive forces are weaker and relative bargaining strengths may play a role, the evidence for tax incidence equivalence is weaker as minor price discrepancies may persist between markets.

keywords: tax incidence equivalence, subsidy, competitive markets, experimental economics, framing effects.

JEL classification codes: C92, D41, H22.

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1 Introduction

1.1 Motivation

A basic tenet in elementary microeconomics is tax incidence equivalence, alternatively referred to as tax liability-side equivalence. This tenet holds that the burden of a unit tax on buyers and sellers is independent of who actually pays the tax. In other words, the price paid by the buyers will be the same no matter whether the buyers or sellers pay the tax; similarly, the price that the sellers receive (which equals the price that the buyers pay minus the amount of the tax) will be the same no matter who pays the tax. Thus, the economic incidence of the tax is independent of the statutory incidence. Instead, the division of the tax burden between buyers and sellers depends solely on the relative elasticities of supply and demand.

We teach this fundamental proposition in introductory economics (see, for instance, Mankiw 1997, pp. 121-124) as well as public finance classes (e.g. Rosen 1999, pp. 260-265; Stiglitz 1988, chapter 17). Tax incidence equivalence theory is robust to market structure and to the type of good or service upon which the tax is imposed. It also extends naturally to subsidies: the relative benefit of a subsidy is independent of who actually receives the subsidy. For want of a better phrase, I refer to this theorem as “subsidy incidence equivalence”.

Despite its central place in elementary microeconomics, tax incidence equivalence and the subject of tax incidence more generally remain poorly understood outside of academic economics. The media and the public often mistake statutory incidence (who pays the tax) for economic incidence (who bears its burden). The much publicized debate over the cigarette tax represents a case in point. Those who want to discourage smoking advocate taxing the consumer with the expectation that the price of cigarettes will increase. Those who believe the tobacco companies should be punished focus on taxing the producer, expecting the price received to decrease (Rosen 1999, p. 265). While both sides are correct in their assessment of the impact on price of a tax, they both ignore that a tax, no matter upon whom it is levied, will simultaneously increase the price paid by the consumer and decrease the price...
received by the producer (assuming neither supply nor demand is perfectly inelastic). Who pays the tax is inconsequential to the relative amounts by which these prices will change.

Another display of public and media naiveté on the subject of tax incidence concerns the public outcry and political backlash from the Canadian Conservative Party’s replacement of a manufacturers’ sales tax with a consumer sales tax on January 1, 1991. From 1924 to 1990, goods and services made in Canada were subject to a tax at every stage of the manufacturing process.¹ This form of multiple taxation placed Canadian exporters at a severe disadvantage in countries like the U.S. (Canada’s largest trading partner) in which the sales tax has always fallen on consumers. By the same reasoning, goods imported from the U.S. were at an advantage compared to domestically produced goods whose prices were inflated by this anticompetitive and distortionary sales tax. Michael Wilson, the Finance Minister of Canada responsible for replacing Canada’s manufacturers’ sales tax with the current consumption sales tax in 1991, estimated that “[a]pproximately one-half of the total sales tax collected [under the former manufacturers’ sales tax was] derived from tax on business inputs such as transportation equipment, office equipment, and building materials. The tax on these inputs can then result in cascading, as these inputs may be used to produce goods that are subsequently taxed again” (Wilson 1987, pp. 46-47). Sound economics notwithstanding, the shift in statutory incidence was perceived by the media and the public as another government tax grab. The already unpopular Conservative Party leader and Prime Minister, Brian Mulroney, was forced to resign within a year after introducing the consumer sales tax.

Policymakers do not always appear better informed than the public on the subject of tax incidence. Krugman (2000) castigates President Bush Jr.’s (then a presidential candidate) proposal to cut consumer gasoline taxes as a means to combat rising gasoline prices, arguing that, given the inelastic demand for gasoline, OPEC and U.S. oil refiners would respond by raising oil prices.²

The economic importance of tax incidence equivalence is clear. If the theory holds, then governments should simply levy taxes to minimize collection and compliance costs and not

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¹ I thank Tom McGirr, Chief Economist of the Sales Tax Division, Department of Finance, Canada, for informative e-mail exchanges on the history of Canada’s sales tax.

² Borck et al. (2001) provide numerous additional examples of publicized controversies reflecting European policymakers’ lack of understanding of tax incidence equivalence.
to place local producers at a disadvantage. However, if the division of statutory incidence has real distributional consequences, then sense can be made of the enormous resources expended by employers, labor unions, manufacturers and consumer advocate groups to lobby governments to tax the other side of the market and of the policy in many countries to split the payroll tax between employer and employee.

1.2 Tests of Tax Incidence Equivalence

There are no empirical tests of tax incidence equivalence using naturally-occurring data. The difficulty lies in the absence of data: to test tax incidence equivalence requires price data on a commodity or service upon which the statutory incidence of the tax fell upon the producers at some point in time and, at another point in time, the statutory incidence shifted entirely to consumers. Policymakers don’t often accommodate economic research with such stark turnabouts in policy.

Perhaps the closest such shifts in policy occurred in Japan, New Zealand and Canada in the late 1980s and early 1990s. Faced with growing budget debts and increased foreign competition, New Zealand and Canada replaced a narrowly based manufacturers’ sales tax with a broader based consumption sales tax. Still, these instances of shifts in statutory incidence policy do not easily afford opportunities to test tax incidence equivalence. For one, changes in other economic variables confound the measurement of tax incidence attributable to the shift in policy. Moreover, the sales tax is an ad valorem tax, a fixed percentage of the product’s value rather than a fixed amount per unit. For tax incidence equivalence to hold for ad valorem taxes, the underlying supply and demand conditions need to be known so that the precise tax rates needed for the theory to hold can be set.

Controlled laboratory experiments do offer the opportunity to observe the division of economic tax incidence both when a tax is levied on the producer and when it is levied on the consumer. Laboratory evidence on the validity of tax incidence equivalence is mixed. Kachelmeier et al. (1994) were the first to examine experimentally tax incidence equivalence. They compare the tax incidence of three different ad valorem consumption taxes in multilevel double-auction experiments involving consumers, retailers and wholesalers. Their results
from all three tax regimes offer some support for the theory; deviations from the theory, however, indicate that those that pay the tax bear the least tax burden. One feature of their experimental procedure that may contribute to this finding is the avoidance of the word “tax” in their experiments. Instead, the taxpaying side of the market is told that a percentage charge would be added to each transaction. For this reason, Kachelmeier et al.’s paper is unable to deal with the framing issues surrounding tax incidence and dealt with in this paper.

A paper closer in spirit to my paper is the work of Kerschbamer and Kirchsteiger (2000) (hereafter K&K). To test tax incidence equivalence, they use the ultimatum game. Player 1, the proposer, is given 70 Austrian shillings to divide between herself and player 2, the responder. If the responder rejects the proposer’s split, then both players earn zero and the game is over. In the standard ultimatum game, acceptance entails payoffs determined by the split of the available sum. K&K’s variation consists of a 20 shilling tax levied in the case of acceptance only. Proposers pay the tax in one treatment, responders in another. K&K provocative title, “Theoretically robust but empirically invalid? An experimental investigation into tax equivalence”, reflects their central finding: proposers’ net offers were significantly higher if the tax is levied on the proposer. Stated simply, in accordance with popular perceptions, the side of the market that pays the tax shoulders a disproportionate share of the tax burden.

I suggest that K&K’s test of tax incidence equivalence is a very special case. For one, the ultimatum game is a very particular representation of bilateral monopoly with sequential timing, a restricted strategy space and an all-or-nothing payoff structure. Second, the peculiarities of ultimatum game behavior are well known: in addition to not converging to the unique subgame-perfect-equilibrium outcome, even with repetition and large monetary incentives, ultimatum-game offers and rejections have been shown to be sensitive to a plethora of contextual variables, framing and other perceptual effects, issues of fairness, intentions and property rights.3

3 In fact, K&K recognize the particularity of their results. They conjecture that if “trade takes place on competitive markets . . . prices and quantities converge to the market clearing level rather quickly” (p. 733). Pursuing K&K’s conjecture, this paper offers a more general test of the theory by exploring markets of varying size and by extending the test to subsidies.
Intrigued by K&K’s findings, Borck et al. (2001) conduct their own laboratory test of the theory. Using a posted-offer market (a multi-player extension of the ultimatum game in which buyers face take-it-or-leave-it posted prices), Borck et al. find that sellers are able to maintain prices above the competitive equilibrium range in both of their “SellerTax” and “BuyerTax” treatments. However, the amount by which prices differ across the two treatments is precisely the amount of the tax wedge. Thus, tax incidence equivalence is supported by their experiments. All of their posted-offer markets involve, nonetheless, exactly three buyers and three sellers.

Riedl and Tyran (2003) also find support for tax incidence equivalence in experimental gift-exchange labor markets in which firms post anonymous, noncontingent wage offers. Observing these offers, each worker in turn decides whether to accept a wage offer and how much costly effort to exert for the firm. Even though net wages and effort levels are above equilibrium levels and effort levels respond positively to wage levels (as predicted by the efficiency wage hypothesis), the magnitudes of these equilibrium deviations and the relationship between wages and effort are the same whether the tax is imposed on firms or on workers.

All four of these previous tests of tax incidence equivalence involve particular numbers of traders: either bilateral monopoly (K&K), precisely three buyers versus three sellers (Borck et al. and Kachelmeier et al.), or four firms versus six workers or five firms versus seven workers (Riedl and Tyran). In this paper, I test both the tax and subsidy equivalence theorems for markets varying in size between 16 and 30 traders. Traders participate for 19 rounds to provide favorable conditions for convergence to the outcomes predicted by the incidence equivalence theorems. Even when convergence occurs, short-run, out-of-equilibrium behavior may be significant and in the direction predicted by the popular misperception that statutory incidence equals economic incidence. I devise a test for this possibility.
2 Experimental Design, Procedures and Subjects

2.1 Experimental Design

All experiments take place in a pit market. Price negotiations in the pit market are decentralized: buyers and sellers meet in an open space and freely negotiate with one another.\[^4\] At least eight pairs of buyers and sellers participate in all experiments.

Each buyer in these experiments possesses a single unit of demand while each seller has available one unit of production for sale. At the beginning of each period, each buyer receives a randomly drawn card from a set of cards; likewise, each seller receives a randomly drawn card from a different set of cards. The value (cost) of a buyer’s (seller’s) unit is given by the number written on his card. Buyers earn their consumer surplus, namely, their valuation minus the negotiated price in each period that they fulfill their unit demand. Sellers earn their producer surplus, namely, the negotiated price minus their cost in each period that they sell their unit of production. A subject’s profit is zero for periods in which he does not trade.

All sessions consisted of 19 three-minute trading periods. During the first 8 periods, subjects participated in an ordinary pit market, with no tax or subsidy. Beginning in period 9, a single change was introduced, the payment of a 10-unit tax by either the buyers (to be referred to as “taxb” sessions) or the sellers (“taxs”), or the receipt of a 10-unit subsidy by either the buyers (“sudb”) or the sellers (“suds”). Subjects remained in this same treatment for 11 periods, until the end of the experiment.

All subjects were inexperienced in this trading institution. Many market experiments suggest that eight periods may be insufficient to permit prices to stabilize. For this reason

\[^4\] The pit market closely resembles the double-auction market. The two institutions differ in the way bids and asks are organized. In the pit market, traders negotiate directly with whomever they choose from the other side of the market, with participants freely exchanging bids and asks between them until a transaction price is agreed upon. In the double-auction market, bids and asks are publicly recorded on the blackboard or computer screen. Only improvements (i.e., increases in the standing bid or decreases in the standing offer) are recorded. A transaction occurs when a market participant accepts the standing bid or offer of the other side of the market. Like the double-auction market, pit markets are known to be highly competitive (see, e.g., Holt 1996, Ruffle 2003). The pit market was preferred for these experiments because it permits more trading periods in a given amount of time.
and since none of the research hypotheses relate to the baseline (i.e. pre-tax or pre-subsidy) treatment, the data from it will not be analyzed. Instead, the main purpose of the baseline treatment is to allow subjects to acquire experience in this environment and to familiarize them with the trading rules, negotiation process and profit calculations, before complicating the design with the introduction of a tax or subsidy.

One source of potential variation between experiments is the size of the market. I vary the number of traders in the market from 16 to 30 (i.e. between 8 and 15 pairs of buyers and sellers) in order to evaluate the robustness of the incidence equivalence theories to market size.

To compare results across experiments it is important to hold constant the equilibrium price for all market sizes. Table 1 displays the distributions of valuations and costs as a function of the number of pairs of buyers and sellers in the market.\footnote{In approximately half of the sessions, six units were added to all valuations and costs. This shift in parameters provides a further robustness test of the results. In order to compare the results of the different sessions, I report the normalized prices, i.e. subtract six units from the prices in the shifted sessions.} Constructing the supply and demand curves from these parameters, one observes that a two-unit competitive price tunnel exists between 31 and 33, where the supply and demand curves overlap. This competitive price tunnel is held constant across all sessions by maintaining two units at the marginally profitable (or second-to-last) demand step of 33 and two units at the marginally profitable (or second-to-last) supply step of 31. For sessions with 12 pairs of traders, two units of demand and supply exist at each step on the respective demand and supply curves. When the number of pairs of traders differs from 12, units are added to, or subtracted from, interior (inframarginal) steps on the supply and demand curves, leaving the competitive price range and the number of traders outside of the competitive equilibrium unaffected.

The symmetry of the design implies that the addition of a tax increases the equilibrium price paid by the buyers by five units (half the amount of the tax) to between 36 and 38, whereas the post-tax equilibrium price received by the sellers falls by five units to between 26 and 28. This holds for both the $\text{tax}_b$ and $\text{tax}_s$ treatments and for all market sizes explored herein. Furthermore, the post-tax competitive equilibrium quantity falls by two units for both tax treatments and all market sizes.
The addition of the subsidy to the baseline treatment has a similar effect: it increases the equilibrium price received by the sellers to between 36 and 38, and decreases the price paid by the buyers to between 26 to 28. The payment of the subsidy permits the trading of two additional units in both the sodb and suds treatments compared to the baseline treatment for all market sizes.

[insert Table 1 here]

2.2 Experimental Procedures

Upon arrival subjects were seated and divided into two equal groups of buyers and sellers. In the event that an odd number of subjects showed up, I solicited a helper from among the subjects, offering to pay him the average subject payment in the session. The instructions were distributed along with a personal record sheet allowing the subjects to keep track of their transactions and profits.\(^6\) The subjects read the instructions and then I read them aloud answering any questions about the experiment. The buyers’ valuation and sellers’ cost cards were distributed for the first three-minute period.\(^7\) Subjects were told the period had begun at which point they rose from their seats to begin negotiating in the center of the classroom. Whenever a buyer and a seller agreed upon a price, together they approached one of the transactions booths. They submitted their cards, face down, to one of the experimenters and reported the transaction price.\(^8\) They then returned to their seats, filled in their record sheets and awaited the commencement of the next period. Meanwhile, one experimenter recorded the transaction while another wrote the transaction price on the blackboard for all subjects to observe.

At the end of the eighth period, subjects were instructed verbally of the introduction of a tax or a subsidy to begin in period 9 through the final period 19. They were informed of

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\(^6\) A much more detailed discussion of the experimental design, procedures, including instructions, and features of the data typical in pit markets can be found in Ruffle (2003).

\(^7\) The card each subject received was his private information. The distributions of cards were unknown, although buyers (sellers) learned about the distribution of buyers’ valuations (sellers’ costs) through receiving a new card each period.

\(^8\) I did not restrict negotiated prices to be below buyers’ valuations or above sellers’ costs. While there were a few isolated instances of negative period profits, subjects quickly learned to trade at a profit. All subjects made positive experimental earnings.
the meaning of the change for their profit calculations. For example, in the $tax_b$ treatment, they were told:

Now we begin the second part of the experiment which involves a single change: on each transaction, the buyer must pay a tax in the amount of 10 units. This amount, 10 units, is simply subtracted from the buyer’s profit on each transaction that occurs. If the buyer does not purchase a unit in a given period, he does not pay the tax. The sellers’ profits are unaffected by the tax.

Note that buyers in this treatment, as in all other treatments, were not explicitly told that the addition of the tax is equivalent to subtracting 10 units from the values written on their cards or that the tax amounts to a downward shift in their unit demand by 10 units. The intentional omission of this equivalence and the framing of the downward shift in demand as a tax provides full scope for the observance of psychological effects possibly associated with statutory tax or subsidy incidence such as the sense of duty or moral obligation to bear the burden of the tax or the sense of right or moral entitlement to reap the benefit of the subsidy. A second possible effect following the introduction of the tax (subsidy) suggests that subjects perceive the bargaining position of the side of the market that pays the tax (receives the subsidy) as weakened (strengthened) compared to the baseline treatment. No matter the reasoning, both of these perceptions push prices apart in the same direction in the $tax_b$ and $tax_s$ sessions as well as in the $sub_b$ and $sub_s$ sessions. Moreover, anchoring on the prices experienced in the baseline treatment inhibits convergence and pushes prices apart in the same directions hypothesized by the framing effects. Convergence to the competitive equilibrium in spite of the latitude yielded to such framing and anchoring effects would attest to the strength of the competitive outcome and the tax and subsidy incidence equivalence theorems.

During each period, subjects were free to negotiate with whomever they chose from the other side of the market. Also, I did not prevent subjects from discussing negotiating strategies or colluding. Collusion would thus seem much more likely than in more common computerized market experiments in which subjects are anonymous and verbal communication is forbidden. Yet, previous double-auction experiments attest to the strength of the

\[9\text{In the extreme, a subject who equates statutory with economic incidence will believe that the imposition of a 10-unit tax on, say, the buyers means that the price buyers pay will rise by 10 units.}\]
competitive outcome, even when sellers are permitted to collude and fix price between periods (Isaac and Plott 1981). My belief was that competitive forces would also foil attempts to collude in these pit-market experiments. This belief turned out to be correct, with one important exception discussed in detail in section 3.3.

2.3 Subjects and Payments

An effort was made to vary the number of traders in different sessions while, at the same time, balancing the composition of sessions across treatments in terms of the numbers of traders.\(^{10}\) Thus, for instance, the six sessions in which the 10-unit tax was imposed on the buyers (\textit{taxb}) consisted of 8, 10, 11, 12, 13, and 14 subject pairs compared to 8, 9, 10, 12, 13 and 15 subject pairs in the \textit{taxs} sessions. Similarly, the six \textit{sudb} sessions consisted of 9, 10, 11, 13, 13, 15 subject pairs compared to 9, 10, 11, 12, 13, 15 subject pairs in the six \textit{suds} sessions.

Subjects’ fields of study were also balanced across treatments. For instance, three \textit{taxb} sessions and three \textit{taxs} sessions were conducted entirely on first-year economics majors during a two-hour class tutorial. Similarly, one \textit{sudb} and one \textit{suds} session consisted entirely of first-year economists. For the remaining sessions, we recruited from the overall student body, targeting in particular economics, business and engineering majors, again with an eye to balancing the subject pool composition across treatments.

In total 552 subjects participated in one of 24 tax or subsidy sessions at Ben-Gurion University. The seemingly large number of sessions per treatment reflects an effort to allow a better chance at rejecting the theory through increased statistical power.

Subjects’ earnings were the sole function of their realized profits from trading. No showup fee was paid: my belief is that the absence of a security profit encourages subjects to trade even for a single unit of profit. Profit maximization is a necessary condition for convergence to the competitive equilibrium quantity.

The new Israeli shekel (NIS) to experimental currency exchange rate was set at 1:3 and

\(^{10}\) Minor differences in the composition of the treatments remain due to the usual unpredictability of no-shows, despite telephone confirmation the day before each experiment.
1:4 for the tax and subsidy experiments, respectively, for the eight sessions conducted on first-year economics majors during their two-hour tutorials. When I turned to general recruiting, the respective exchange rates were increased to 1:2 and 1:3.5 (to compensate students for showing up specifically to participate in the experiment).

The experiments took approximately one hour and 40 minutes. The average payment for the eight sessions conducted during the compulsory classroom tutorial was 44.75 NIS (s.d. 12.7), compared to 63.81 NIS (s.d. 13.4) for other sessions in which subjects showed up especially for the experiment.\textsuperscript{11}

\section*{3 Results}

\subsection*{3.1 Convergence and Tax and Subsidy Incidence Equivalence}

Figures 1 and 2 display the median transaction prices by period for each of the twelve tax and twelve subsidy sessions, respectively. Recall that the 10-unit tax wedge results in competitive equilibrium price ranges of 36-38 for the \textit{taxs} sessions and 26-28 for the \textit{taxb} sessions since the latter range does not include the payment of the tax; while the 10-unit subsidy produces a competitive equilibrium price range of 36-38 for \textit{sudb} and 26-28 for \textit{suds}. Visual inspection of the two figures points to the paper’s first main result.

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figures/figure1.png}
\caption{Median transaction prices for tax sessions.}
\end{figure}

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figures/figure2.png}
\caption{Median transaction prices for subsidy sessions.}
\end{figure}

\textbf{Result 1} \textit{Prices converge to the post-tax competitive equilibrium in all 12 tax sessions and to the post-subsidy competitive equilibrium in 11 of the 12 subsidy sessions.}

Beginning with the tax treatments, the median transaction price converges to the competitive price range within the 11 designated periods in all 12 tax sessions. Price convergence is particularly uniform in the \textit{taxb} sessions. All six sessions show the same median price of 27, the midpoint of the competitive tunnel, in the final period. Moreover, convergence to

\begin{footnotesize}
\textsuperscript{11} At the time these experiments were conducted $1 \text{ US} \equiv \text{4 NIS. Almost all subjects earned considerably more than their outside opportunity cost of 18 NIS per hour, the minimum wage in Israel.}
\end{footnotesize}
the competitive price range is notably rapid: in ascending order according to the size of the market, the six \textit{taxb} sessions required 6, 1, 3, 2, 3 and 1 period to converge to, and remain within, the competitive price range.

The transactions graph for session \textit{tax13b10}, typical in its pricing dynamics, is displayed in Figure 3.\textsuperscript{12} Each dot represents a transaction, with the height of the dot indicating the negotiated price. Vertical lines separate between the periods. The dots in each period are arranged according to the order in which transactions occurred. The average price and quantity exchanged are displayed beneath the x-axis for all 19 periods.

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure3.png}
\caption{Transactions graph for session \textit{tax13b10}.}
\end{figure}

Session \textit{tax13b10} begins with moderate price dispersion in the first few periods of the baseline treatment. (Many sessions display considerably higher price variance in the initial periods.) Price variance generally decreases over time. By period 5, both the average price and quantity have converged to the competitive outcome. The introduction of the tax in period 9 lowers prices by less than the full 5 units initially. However, competitive forces produce a gradual downward drift in transaction prices until they enter the competitive range for good by period 3 of the treatment by both the median and mean price measures. Notice that the price variance at the beginning of the tax treatment in this session (as in every other tax and subsidy session) is considerably lower than at the outset of the baseline treatment. The probable explanation is that by period 9 traders have accumulated both experience in this market environment and knowledge about their own valuation or cost distribution. As a result, uncertainty and price experimentation are reduced, and “mistakes” (in the form of buying a unit at a price considerably higher than the other publicly displayed transaction prices or selling a unit at a price substantially lower than other displayed prices) are eliminated.

Price convergence to the competitive range occurs in all six \textit{taxs} sessions as well, albeit more slowly than the \textit{taxb} sessions. Sorted by ascending order according to the size of the

\textsuperscript{12} To illustrate my system for labelling sessions, “tax13b10” indicates that this session was a tax (“tax”) experiment with thirteen (“13”) pairs of buyers and sellers. The tax was imposed on the buyers (“b”) and this was the tenth (“10”) tax experiment conducted in the series of 12.
market, the six *taxs* sessions required 5, 5, 11, 4, 6, and 5 periods before settling into the competitive price range for the duration.

Examining the two tax treatments together, we see that 11 of the 12 sessions converge to, and remain within, the competitive range by period 6 of the tax treatment (period 15 overall). Session *tax10s11* required all 11 periods before the median price reached 36.

Turning now to the two subsidy treatments, these experiments display considerably higher price variance immediately following the introduction of the subsidy than the comparable initial periods of the tax experiments. One explanation is that subjects are even less familiar with the concept of a subsidy than they are with a tax and therefore respond to the subsidy more erratically and less uniformly. In spite of initial price variance, convergence prevails in the subsidy experiments as well.

According to the median transaction price, all sessions (with the exception of session *sud9b7*, to be discussed below) converge to the competitive price range no later than the fifth period of the subsidy treatment (see Figure 2). What is more, five of the sessions fall and remain within the competitive price tunnel from the first period of the treatment, while three additional sessions do so from the second period. Figure 4 displays the transactions graph for a session, *sud10b3*, with high price variance at the beginning of both the baseline and subsidy treatments. Nonetheless, already by the fourth period of the subsidy treatment, all but at most one trade occurs within the competitive price range in every period.

That prices converge to the competitive outcome in all 12 tax sessions and 11 of 12 subsidy sessions does not rule out the possibility that prices nonetheless differ between the *taxb* and *taxs* as well as between the *sudb* and *suds* treatments. Given the competitive price range, prices in the paired treatments may differ by as much as two units and still converge to the competitive equilibrium. Result 2 suggests some variation in transaction prices as a function of the size of the market.

**Result 2**  When the competitive quantity exceeds six units (alternatively, when more than six pairs of market participants can trade profitably at the competitive price), tax incidence
equivalence is upheld in these experiments. However, for smaller markets (when the competitive equilibrium quantity is six units or less) relative negotiating strengths may influence transaction prices, resulting in minor price discrepancies from the midpoint of the competitive range. Because the post-subsidy competitive equilibrium quantity exceeds six units in all markets examined, subsidy incidence equivalence finds unequivocal support in these experiments.

We previously noted the rapid and uniform price convergence of all six taxb sessions to 27, the midpoint of the competitive tunnel in this treatment. Prices in the six taxis sessions, however, display greater heterogeneity. Differences in prices across these six sessions can be neatly characterized as a function of the market size. Close inspection of terminal period prices from the taxis sessions reveals that both the median and mean prices are lowest in the three smallest sessions, test8s7, test9s1 and test10s11. The period 19 median price in these three sessions is 36, one and one-and-a-half units lower than the median price in each of the three larger sessions in this treatment. The first two rows of Table 2 report summary statistics for the period 19 prices in the six taxb sessions, and the taxis sessions divided according to market size.

[insert Table 2 here]

The data reveal that when the tax falls on the buyers, they are able to bring down the price by the full 5 units prescribed by the midpoint of the competitive price range. When the sellers pay the tax and the market is relatively thin (a volume of six units or less at the competitive equilibrium according to the results), buyers are able to resist the full five-unit price increase. This resistance may be seen in the form of slower convergence to the competitive range from below, as previously discussed, and convergence to the lower bound of the competitive range.  

13 The observation that buyers tend to be stronger in experimental market negotiations is not new. Smith and Williams (1982, p. 115) provide a plausible explanation for disproportionate buyer negotiating strength: students have more lifetime experience as buyers and therefore are more adept at negotiating in the role of buyers than as sellers.
On the other hand, all but one session with a competitive quantity of seven or more units (11 or more pairs of traders) converge precisely to the midpoint of competitive tunnel.\textsuperscript{14} These findings suggest that the degree of support for tax incidence equivalence depends on the extent to which the market is characterized by competitive conditions.

Taking into account the nature of the observed price discrepancies in the \textit{taxs} sessions, we may first separate the six \textit{taxs} sessions into the three large, “\textit{taxs} (3 large)”, and the three small, “\textit{taxs} (3 small)”, markets and compare their prices with each other and with the prices from the six \textit{taxb} sessions.\textsuperscript{15} The Kruskal-Wallis test checks for differences in the central tendencies of the price distributions. We are able to reject the null hypothesis that the period 19 average price distributions of the \textit{taxb}, \textit{taxs} (3 large) and \textit{taxs} (3 small) are the same ($\chi^2(2) = 6.29$, $p=.043$). The result is even stronger if we compare the period 19 median price distributions ($\chi^2(2) = 9.50$, $p=.009$).

The source of the difference in prices between these three groups is clear. When the period 19 average transaction prices from all 12 tax sessions are ranked from lowest to highest, the mean rank (upon which the Kruskal-Wallis test is based) from the \textit{taxs} (3 small) sessions is significantly lower than that of the other two groups (see the third row of Table 2). Put differently, despite price convergence in the \textit{taxs} (3 small) sessions, prices in these three sessions remain, on average, one unit below prices from \textit{taxs} (3 large) and \textit{taxb} (inclusive of the tax) sessions. Comparing period 19 average prices in the \textit{taxs} (3 large) sessions with those in the \textit{taxb} sessions, we find no difference between them (Wilcoxon-Mann-Whitney $Z=−.259$, $p=.905$). The same result holds if instead we compare the period 19 median prices (Wilcoxon-Mann-Whitney $Z=−1.41$, $p=.548$).

For the subsidy treatments, the period 19 transaction prices in the \textit{suds} and \textit{sudb} treatments (excluding the single outlier session, \textit{sud9b7}) look very similar: for instance, the period 19 median price is 37.0 for both the \textit{sudb} (n=60) and \textit{suds} (n=67) treatments. The period 19 mean prices are also nearly identical, 36.9 for \textit{sudb} and 36.7 for \textit{suds}. Comparing the period 19 mean (median) prices at the session level, we cannot reject the null hypothesis

\textsuperscript{14} Session \textit{tax13s3} converges to a median price of 37.5, a mere half a unit above the midpoint of the competitive tunnel, and a mean price of 37.13.

\textsuperscript{15} Ten units (the transfer of the tax) are added to all prices in the \textit{taxb} sessions in order to compare them with prices from the \textit{taxs} sessions.
that the sample distributions are drawn from the same population distribution (Wilcoxon-
Mann-Whitney $Z=−.730$, $p=.537$ for mean prices and $Z=−1.927$, $p=.126$ for median prices).

In light of the findings of the tax experiments, the absence of a difference between the
two subsidy treatments should come as no surprise. We saw in the taxes sessions that for
competitive quantities less than or equal to six units, prices do not necessarily converge
to the same price as they do in larger sessions. However, the competitive quantity in the
subsidy treatments is four units larger than the competitive quantity of the tax treatment
with an equivalent number of traders, and two units larger than the comparable baseline
treatment. Consequently, the smallest competitive quantity in the subsidy sessions is eight
units. These markets are thus large enough so that competitive forces bring about strong
convergence to the same price.

3.2 Direction of Convergence and Initial Framing Effects

The above analysis is based entirely on terminal-period prices. Except for relatively small
markets in which the tax is imposed on the seller, I found no evidence for a tax or subsidy
framing effect in which individuals associate statutory incidence with economic incidence.
In the tax experiments, this could take the form of either the perception that the bargaining
power of the side of the market upon which the tax was levied was weakened or a sense of
moral obligation to bear a disproportionate share of the tax burden on the part of this side
of the market. Similarly, according to the subsidy framing effect, the side of the market that
receives the subsidy either perceives its bargaining power to have been strengthened or it
treats the receipt of the subsidy as a right or moral entitlement to be the primary beneficiary
of the subsidy. These hypothesized framing effects notwithstanding, prices converged to the
competitive range regardless of who paid the tax or who received the subsidy.

It may nonetheless be that this sense of altered bargaining power or moral obligation or
moral entitlement is present immediately after the introduction of the tax or the subsidy,
and is “squeezed out” over time by competitive pressures. In fact, the protests and charges
of unfairness at the beginning of period 9 heard by those subjects required to pay the tax
and those who didn’t receive the subsidy would suggest that such initial reactions exist.
Short-term, temporary framing effects may have substantial economic consequences. For example, the short term for infrequently renegotiated wage contracts may be several years. If management and labor unions behave as if statutory and economic incidence are related, then out-of-equilibrium wages will persist for years in countries in which the payroll tax is levied upon only one side of the market. Even in the absence of long-term labor contracts, wages may be endogenously rigid. Blinder (1988, p. 12) makes the point that, “economists insist on thinking long-run equilibrium while everyone else lives in short-run disequilibrium. The truth of the matter is that the incidence of the payroll tax may differ dramatically in the short and long runs.” \(^{16}\)

The following example illustrates a possible psychological mechanism underlying initial framing effects in the \textit{taxb} treatment. Associating statutory incidence with economic incidence, the buyers feel either obliged to bear most of the tax burden or unable to avoid it. Thus, they are willing to accept relatively high prices, prices above the competitive equilibrium. This leads to the testable hypothesis that prices in the \textit{taxb} treatment converge to the competitive equilibrium from above. Similarly, if the sellers are prepared to shoulder a disproportionate share of the tax burden when they pay the tax, prices in the \textit{taxs} treatment should initially be below the post-tax competitive equilibrium and converge from below. \(^{17}\)

Initial framing effects in the subsidy experiments imply that the direction of convergence should also differ by treatment. Prices in the \textit{sudb} experiments should start out below the equilibrium price range since buyers feel empowered or entitled to benefit disproportionately from the subsidy paid to them, and sellers acquiesce with low prices. Accordingly, prices should converge from below in this treatment. By the same logic, prices in the \textit{suds} experiments can be expected to converge from above.

To test the convergence properties of the two tax and two subsidy treatments, I estimate the following linear regression developed in Noussair et al. (1995) and referred to as the

\(^{16}\) Riedl and Tyran (2003) discuss the possible relevance of short-run rigidities for tax incidence equivalence in gift-exchange markets.

\(^{17}\) Anchoring on the prices experienced in the pre-tax or pre-subsidy treatment yields exactly the same hypothesized directions of convergence as the framing effects. While the discussion focuses on framing explanations for possible out-of-equilibrium prices, anchoring may be used interchangeably.
Ashenfelter-El-Gamal model.

\[ P_{it} = B_{11}D_1(1/t) + \ldots + B_{1i}D_i(1/t) + \ldots + B_{1k}D_k(1/t) + B_2\left(\frac{t-1}{t}\right) + u_{it} \quad (1) \]

The dependent variable \( P_{it} \) is the transaction price in period \( t \) in experiment \( i \). The term \( D_i \) is a dummy variable that assumes a value of 1 for experiment \( i \), \( i = 1, \ldots, k \), and 0 otherwise. Notice that when \( t = 1 \), the price in experiment \( i \) equals \( B_{1i} \). The coefficient \( B_{1i} \) can thus be thought of as the initial price origin for experiment \( i \). Its impact decays over time, as indicated by the term \( 1/t \). By contrast, the impact of \( B_2 \) increases over time, as indicated by the expression \( \frac{t-1}{t} \). The coefficient \( B_2 \) can thus be thought of as the common price asymptote. The random error term \( u_{it} \) is distributed normally with mean zero.

As Noussair et al. point out, this dynamic statistical model of pricing behavior has several desirable properties. First of all, the weights attached to the initial price estimate and the price convergence estimate sum to 1. In period \( t = 1 \), the price in experiment \( i \) equals \( B_{1i} \). As the experiment progresses, the weight accorded to the initial price \( B_{1i} \) decreases and shifts to the convergence asymptote, \( B_2 \). In the limit, as \( t \) becomes large, the price converges to \( B_2 \) since the weight attached to the initial price decreases to zero. Note that the subscript \( i \) on the \( B_{1i} \) term indicates that the model allows for different initial prices for each experiment. However, in accordance with competitive equilibrium theory, all experiments are assumed to converge to a common price asymptote, \( B_2 \).

Noussair et al. employ the Ashenfelter-El-Gamal regression model to test whether various price and quantity time series converge to one theoretical benchmark or another in more complex trading environments than the one examined here. Price convergence in our comparatively simple environment has already been firmly established. Instead, my interest concerns the direction of convergence, given by the relationship between \( B_{1i} \) and \( B_2 \). If the fitted regression line reveals that \( B_{1i} > B_2 \), then the regression line is negatively sloped, indicating that experiment \( i \) converges to \( B_2 \) from above. On the other hand, if \( B_{1i} < B_2 \), then experiment \( i \) converges from below. Table 3a and the first row of Table 3b report the results from the separate \( taxb \) and \( taxes \) regressions, while the first row of Table 4a and Table 4b report the regression results from the \( sudb \) and \( sudsc \) experiments, respectively.\(^{18}\)

\(^{18}\) I exclude the outlier session from the \( sudb \) regression since it does not converge and therefore follows
Result 3 summarizes the convergence tendencies of the tax and subsidy treatments.

**Result 3** None of the tax or subsidy treatments displays any systematic direction of convergence. The implication is that from the initial period of the tax and subsidy treatments, competitive market forces dominate any perception of altered bargaining power and any sense of duty, right or moral obligation or entitlement associated with the payment of the tax or receipt of the subsidy.

[insert Tables 3a and 3b here]

Table 3a reveals that the initial price of three of the six taxb experiments (tax10b6, tax12b8 and tax13b10) does not differ significantly from the price asymptote; these experiments converge from within the competitive price range. Two experiments converge from above (each significant at the 5% level), as predicted by the initial framing effect hypothesis. And, contrary to the framing effect, one experiment converges from below (significant at the 10% level).

The taxa experiments reveal even less support for the initial framing effect. Whether we examine the overall taxa regression (row 1 of Table 3b) or the separate regressions for the three large taxa experiments (row 2) and the three small taxa experiments (row 3), at most one of the six experiments, tax15s12, converges from above, the direction predicted by the framing hypothesis. Three or even four experiments show no direction of convergence and two experiments converge from above.\(^{19}\)

On a different note, the separate estimates of \(B_2\) for the taxb and taxa treatments reinforce two previous results. That both \(B_2\) estimates (27.25 (not including the tax) and 36.23) fall within the competitive price range confirms Result 1. Nonetheless, if we compare the tax-inclusive price asymptotes from the taxb and taxa regressions a one-unit price discrepancy a different data generating process. In the next subsection, a separate regression will be estimated for this session to examine its convergence properties.\(^{19}\)

\(^{19}\) Repeated observations for the same subject raise the possibility that observations for the same subject are correlated, usually positively. Such correlations would mean that the estimated standard errors are less than the true ones. If so, the finding that neither the taxb nor taxa treatment displays a significant direction of convergence would be even stronger with the true standard errors.
exists. Result 2 and the discussion that follows it suggest that the smaller market sizes in the taxa treatment may account for the comparatively low convergence price of 36.23. To verify this earlier result, I report separate regressions for the three large taxa experiments (row 2 of Table 3b) and the three small ones (row 3 of Table 3b). These separate $B_2$ estimates reveal a 1.22 unit price gap between the large and the small sessions; whereas the taxb and taxa (3 large) sessions converge to prices that differ by a mere 0.59 units.

As for the subsidy treatments, the first row of Table 4a reveals that two of the five subd experiments converge from within the competitive tunnel and show no significant direction of convergence. Only one experiment converges from below, as predicted by the framing effect hypothesis; whereas, counter to this hypothesis, two experiments converge from above. The framing effect hypothesis performs slightly more favorably in the subd experiments. Although one subd experiment converges from below (contrary to the hypothesis), and two experiments display no direction of convergence, three of the six experiments converge from above in accordance with the hypothesis (see Table 4b).

3.3 Collusion

As noted in section 2.2, subjects were permitted to coordinate negotiating and pricing strategies between periods. In fact, non-binding, verbal, collusive agreements were reached in numerous sessions. Typically, between periods, a trader, say a buyer, proposed aloud to other buyers to remain in their seats and not to buy above some stated price. After some discussion, all of the buyers agreed. At the opening of the next period, buyers indeed remained seated. Not surprisingly, sellers did not easily succumb to the buyers’ price demands. Seconds ticked away. The experimenter announced that one minute remained in the period. At this point, those buyers with the highest valuations became nervous. Unwilling to forego a large profit, these high-valuation buyers were almost inevitably the first to rise from their seats in search of a seller willing to trade at a price above the collusive agreement. The collusive agreement subsequently unravelled: other, perhaps medium-valuation, buyers observed the defector and decided that they too preferred to cash in a positive profit before
the period terminated. By the next period, there was usually little, if any, discussion of a collusive price. The following observation summarizes these findings and notes the exception to the rule.

**Observation 4**  *Efforts to collude are numerous. However, in the face of the profit maximization motive and competition, collusion typically breaks down in these markets soon after it is attempted – often in the same period. In the single instance in which collusion succeeded in maintaining out-of-equilibrium prices, the lost sales made it no more profitable.*

In the baseline, tax and subsidy treatments, numerous attempts at price collusion were observed. All such attempts were soon abandoned, often within the same period in which they were initiated. Session *sud9b7* provides a striking exception to this rule. Figure 5 displays the transactions graph for this session.

[insert Figure 5 here]

Note how well-behaved prices are in the baseline treatment of this session: already in period 1 the median price falls within the competitive range where it remains for all eight periods. However, precisely between periods 8 and 9, after the announcement of the 10-unit subsidy to the buyers, the sellers confer and agree not sell below 40 (two units above the top of the competitive range). While some transactions do occur below 40 in period 9, a redoubled effort in period 10 succeeds in maintaining the prices of 6/7 trades above 40. This pattern in which all but one or two trades occur at a price of 40 or more continues through period 15. Beginning in period 16 there are signs that the collusion is beginning to break down as prices drift downwards. However, as the regression reported in row 2 of Table 4a points out, this downward drift in prices is not significant. Namely, the initial price term of 40.24 differs by a mere 0.73 units from the convergence asymptote of 39.51 ($z=1.066$, p-value=.145). According to Noussair et al. (1995, p. 474), data converge weakly if the convergence asymptote, $B_2$, is closer to the model’s prediction than to the initial price term, $B_{1i}$. By this definition, I reject weak convergence for session *sud9b7* since 39.51 is closer to 40.24 than to 38 (the upper bound of the competitive tunnel).
In summary, this session does not converge, even weakly, to the competitive price. What is more, prices show no significant downward trend over time. Finally, that prices are above the competitive equilibrium contradicts the hypothesized framing effect in this subsidy session.

What distinguishes this session from all of the others in which failed attempts at collusion were soon aborted? The answer lies largely in the familiarity of subjects in this experiment with one another. All nine of the colluding sellers (and 17/18 subjects in the experiment) were engineering students, six of them classmates in the second year. The fact that all of these sellers knew one another enabled them to trust each other enough to permit cooperation. Furthermore, the threat of continued interaction with one another after the experiment imposes a cost to those who defect from the collusive agreement. The relatively small size of the market further facilitated successful collusion.20

A natural question to ask is, did the sellers profit from their collusion? There is no clear evidence that they did. The average seller profit from the ten subsidy sessions in which subjects were recruited ranges from 58.4 NIS to 64.4 NIS. At 61.4 NIS, the profits of sellers in this session rank fifth out of ten. Thus, the higher prices achieved from the collusion appears roughly to compensate the sellers for the foregone sales. Buyers’ profits, however, are severely hurt. With average earnings of 51.0 NIS, buyers in this session earned substantially less than buyers in the other nine recruited, subsidy sessions who earned on average between 56.0 NIS and 69.3 NIS.

4 Conclusions

The results from these experiments can be seen as an extension of the bounds of applicability of the competitive equilibrium in market institutions. Prices and quantities converge to the competitive outcome following a one-time, announced shift in either the supply or demand curve, framed as either the collection of a tax on the buyers or the sellers or the disbursement 20

20 This set of conditions that proved conducive to collusion corresponds to the results in Price and List (2004) who conducted experiments in a decentralized multilateral market with professional sportscard dealers as the sellers. Among the five treatments they examined, prices were highest and above the competitive equilibrium in the “insiders” treatment in which all four sellers were local dealers bound to interact with one another at future sportscard shows and seller communication between experimental rounds was permitted.
of a subsidy to buyers or sellers. The precise shifts of the supply and demand curves are
designed to test the tax and subsidy incidence equivalence theorems. The framing of the
shift as a tax or a subsidy permits subjects to interpret the imposition of a tax on them as a
moral obligation or duty or as having weakened their bargaining power and the payment of
a subsidy to them as a moral entitlement or right or as having strengthened their bargaining
power. In spite of the potential scope for such framing effects, these markets show surprisingly
little evidence of them, even in the short run. However, for smaller markets (in which six
or less pairs of market participants can trade profitably at the competitive equilibrium), the
relative bargaining strengths of traders may play a residual role, possibly resulting in minor
price discrepancies between different markets. The direction of these price discrepancies is
consistent with the framing effect hypothesis. On the other hand, when the market size is
sufficiently large (namely, seven or more pairs of market participants able to trade profitably
at the competitive price), I find strong support for both the tax and subsidy incidence
equivalence theorems.

These findings, along with the results of Kerschbamer and Kirchsteiger, suggest that the
domain of policy debate concerning who should pay the tax or receive the subsidy should first
be focused on determining to what extent competitive forces characterize the market under
consideration. If it is determined that the industry in question is a competitive one, then
the tax (subsidy) should be administered in a way that minimizes collection and compliance
(disbursement) costs and accords with other local or national priorities. For instance, so
as not to impede exports of a particular domestically produced good, a consumption rather
than a production tax should be preferred; alternatively, the production rather than the
consumption of the same good should be subsidized. Insofar as the market is not perfectly
competitive, discussions about who should pay or who should receive take on increasing
relevance.
References


## Distributions of Costs and Valuations

<table>
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<tr>
<th>Cards</th>
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<th>Buyers' Valuations</th>
<th>Number of pairs of sellers and buyers</th>
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<td></td>
<td>11</td>
<td>53</td>
<td>8</td>
</tr>
<tr>
<td></td>
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</tr>
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<td></td>
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<td></td>
<td>36</td>
<td>28</td>
<td>2</td>
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Table 1: The distributions of cost and valuation parameters used in the baseline (that is, pre-tax or pre-subsidy) treatment as a function of the number of pairs of sellers and buyers in the session.
Figure 1: Median transaction prices by period for each of the 12 tax experiments. Periods 1-8 involved no tax and a competitive price range of 31-33. Beginning in period 9, a 10-unit tax on each transaction was paid either by the buyers ("taxb"), in which case the competitive price range (not inclusive of the tax) was 26-28, or by the sellers ("taxs"), in which case the competitive price range was 36-38.
Figure 2: Median transaction prices by period for each of the 12 subsidy experiments. Periods 1-8 involved no subsidy and a competitive price range of 31-33. Beginning in period 9, a 10-unit subsidy on each transaction was paid either to the buyers ("sudb"), in which case the competitive price range was 36-38, or to the sellers ("suds"), in which case the competitive price range (not inclusive of the subsidy) was 26-28.
Figure 3: Transactions graph for session tax13b10. Each dot represents a transaction with the height of the dot indicating the negotiated price. The vertical lines separate the periods. The dots within each period are arranged in chronological order. The average price and quantity traded are displayed beneath the x-axis for each of the 19 periods. In periods 1-8, there was no tax, a competitive price range of 31-33 and a competitive quantity of 11. Beginning in period 9, a 10-unit tax was imposed on the buyers, lowering the competitive price range (not inclusive of the tax) to 26-28 and the competitive quantity to 9 units.
Figure 4: Transactions graph for session sud10b3. Periods 1-8 involved no tax, a competitive price range of 31-33 and a competitive quantity of 8. In period 9, a 10-unit subsidy was paid to the sellers on each transaction, with a resulting competitive price range of 36-38 and a competitive quantity of 10.
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<td>median price</td>
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<td>ave p mean session rank</td>
<td>8.17</td>
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Table 2: Summary statistics for the period 19 prices for the taxb sessions, the three relatively large taxes sessions (taxs (3 large)) and the three relatively small taxes sessions (taxs (3 small)). All prices are inclusive of the 10-unit tax for the purpose of comparison.
Table 3a: Results from the Ashenfelter-El-Gamal dynamic linear regression model for the \textit{taxb} experiments. I estimate the initial price, \(B_{1i}\), for each experiment \(i\) and a common convergence price asymptote, \(B_2\). (White heteroskedasticity-consistent standard errors in parentheses.) The difference between the initial price term and the convergence asymptote indicates the direction of convergence. Namely, if \(B_2 > B_{1i}\), experiment \(i\) converges from below; whereas, if \(B_2 < B_{1i}\), experiment \(i\) converges from above. “none” indicates no direction of convergence; that is, the difference between \(B_{1i}\) and \(B_2\) is not significant. Anchoring and framing effects predict convergence from above.

<table>
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<tr>
<th>(B_{1,\text{taxb}5})</th>
<th>(B_{1,\text{taxb}6})</th>
<th>(B_{1,\text{taxb}7})</th>
<th>(B_{1,\text{taxb}8})</th>
<th>(B_{1,\text{taxb}9})</th>
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<td>(.674)</td>
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<td>(.961)</td>
<td>(.104)</td>
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* The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 10% level (t-test).
** The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 5% level (t-test).
*** The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 1% level (t-test).

Table 3b: Three separate regressions for the \textit{taxs} experiments using the Ashenfelter-El-Gamal model. The first row reports the results using all six \textit{taxs} sessions. The second row uses the three large \textit{taxs} sessions, while the third row uses the three small \textit{taxs} sessions. Anchoring and framing effects predict convergence from below.

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<tr>
<th>(B_{1,\text{taxs}7})</th>
<th>(B_{1,\text{taxs}1})</th>
<th>(B_{1,\text{taxs}11})</th>
<th>(B_{1,\text{taxs}2})</th>
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* The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 10% level (t-test).
** The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 5% level (t-test).
*** The difference between \(B_{1i}\) and \(B_2\) estimates is significant at the 1% level (t-test).
### sudb regressions: dependent variable P

<table>
<thead>
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<th>B1, sud11b6</th>
<th>B1, sud13b1</th>
<th>B1, sud13b12</th>
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* The difference between B1i and B2 estimates is significant at the 10% level (t-test).
** The difference between B1i and B2 estimates is significant at the 5% level (t-test).
*** The difference between B1i and B2 estimates is significant at the 1% level (t-test).

Table 4a: Results from the Ashenfelter-El-Gamal dynamic linear regression model for 5/6 sudb experiments in row 1 (White heteroskedasticity-consistent standard errors in parentheses) and a separate regression for the outlier experiment sud9b7 in row 2. I estimate the initial price, B1, for each experiment i and a common convergence price asymptote, B2. The difference between the initial price term and the convergence asymptote indicates the direction of convergence. Namely, if B2 > B1i, experiment i converges from below; B2 < B1i implies experiment i converges from above, while “none” indicates no significant direction of convergence. Anchoring and framing effects predict convergence from below.

### suds regression: dependent variable P

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<td>(.322)</td>
<td>(.356)</td>
<td>(.434)</td>
<td>(.313)</td>
<td>(.370)</td>
<td>(.060)</td>
<td></td>
</tr>
<tr>
<td>above</td>
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<td>none</td>
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</tbody>
</table>

* The difference between B1i and B2 estimates is significant at the 10% level (t-test).
** The difference between B1i and B2 estimates is significant at the 5% level (t-test).
*** The difference between B1i and B2 estimates is significant at the 1% level (t-test).

Table 4b: Regression results from the Ashenfelter-El-Gamal model for the suds experiments. Anchoring and framing effects predict convergence from above.
Figure 5: Transactions graph for session sud9s7, a case of sustained seller collusion. Sellers began to collude in the first period of the subsidy treatment (period 9) and maintained prices above the competitive price range of 36-38 and below the competitive quantity of 10 throughout the remainder of the session.