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Competitive behavior in market games: Evidence and theory ☆

John Duffy [a,*], Alexander Matros [b], Ted Temzelides [c]

[a] *Department of Economics, University of Pittsburgh, Pittsburgh, PA 15260, United States*
[b] *Department of Economics, Moore School of Business, University of South Carolina, Columbia, SC 29208, United States*
[c] *Department of Economics, Rice University, Houston, TX 77251, United States*

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Abstract

We explore whether competitive outcomes arise in an experimental implementation of a *market game*, introduced by Shubik (1973) [21]. Market games obtain Pareto inferior (strict) Nash equilibria, in which some or possibly all markets are closed. We find that subjects do not coordinate on autarkic Nash equilibria, but favor more efficient Nash equilibria in which all markets are open. As the number of subjects participating in the market game increases, the Nash equilibrium they achieve approximates the associated competitive equilibrium of the underlying economy. Motivated by these findings, we provide a theoretical argument for why evolutionary forces can lead to competitive outcomes in market games.

JEL classification: C72; C73; C92; D51

Keywords: Market games; Full Nash equilibrium; Market power; Competition; Experimental economics; Evolutionary stability

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* Corresponding author.
E-mail address: jduffy@pitt.edu (J. Duffy).

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1. Introduction

This paper explores whether agents exhibit behavior consistent with competitive equilibrium in a laboratory implementation of a *market game*, introduced by Shubik [21].[1] Market games give rise to competitive outcomes when agents lack market power. Thus, they have served as a non-cooperative foundation for competitive outcomes. Even in large economies, however, in addition to approximately competitive outcomes, market games obtain Pareto inferior (strict) Nash equilibria, in which some, and possibly all, markets are closed due to a coordination failure. While these games offer an interesting benchmark, to our knowledge there are no attempts to experimentally implement a market game setup. Our work is an attempt to fill this gap and is in the spirit of much other experimental work exploring properties of different market structures in the laboratory.

Our experimental findings reveal that subjects placed in a two-good pure exchange market game coordinate away from equilibria where markets are closed (even though such equilibria are strict). Instead, subjects' play concentrates around the "full" Nash equilibrium in which markets are open and there is a large volume of trade. We perform comparative statics on the size of the economy and find that, as the number of agents participating in the game increases, the full Nash equilibrium they achieve comes closer to approximating the associated competitive equilibrium of the underlying economy. Indeed, our findings suggest that if the economy is sufficiently large, the market game mechanism will reliably lead agents to competitive equilibrium allocations.

Motivated by these observations, we build a theoretical model of a market game played by boundedly rational agents. Instead of imposing Nash-behavior, we study this game from an evolutionary point of view.[2] We introduce a *strong* version of evolutionary stable strategies (*SESS*) for asymmetric, finite games and demonstrate that (partial) autarky outcomes are *not SESS*. Roughly speaking, *SESS* requires stability against all coalitions consisting of at most one agent per population. In a market game context, a suitable small-size coalition can generate trade and open a market. Thus, evolutionary forces provide an avenue through which the economy can avoid situations where some markets are closed due to a coordination failure.[3] We demonstrate that, if the game is sufficiently large so that agents' market power is insignificant, the full Nash equilibrium is an approximate *SESS*.

The remainder of the paper is organized as follows. In the next section we present the simple, 2×2 market game that will be used in our experiment. Section 3 details our experimental design and findings. Section 4 contains our theoretical results and Section 5 offers a concluding discussion.

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[1] There is a large literature on market games. Key references include Shapley [19], Shapley and Shubik [20], Dubey and Shubik [3], and Mas-Colell [12]. More recent related references include Peck and Shell [13], Peck, Shell, and Spear [14], and Ghosal and Morelli [8]. While the models used in these studies differ in many respects, our analysis can be modified in order to be applied to most versions of market games that are found in the literature. For a related study of market behavior that compares theory and experiments see Friedman and Ostroy [6].

[2] After all, competitive outcomes are often justified by appealing to the natural selection of behavior that is more "fit." See Alchian [1] for one of the first attempts to formalize this argument. Weibull [26], Vega-Redondo [24], and Samuelson [16] provide reviews of evolutionary models.

[3] Our results are related to Dubey and Shubik [4], who introduce an outside agency that ensures that arbitrarily small amounts of bids and asks are present in all markets (see also Dubey [2]). Our argument, however, does not rely on the existence of such an agency. In addition, we impose minimal rationality requirements, and we explicitly consider non-Nash outcomes.

2. 2 × 2 market games

We begin by presenting the main setup on which our experimental design will be based: symmetric market games built around an Edgeworth box economy with two types of agents and two consumption goods.

There are two populations of equal size, each consisting of n agents. We will refer to agents in the first population as *type I* and those in the second population as *type II*. There are two consumption goods, x and y. Populations are distinguished by preferences and endowments. Agents of type t, where $t \in \{\mathrm{I}, \mathrm{II}\}$, have continuous, strictly increasing, strictly convex, smooth preferences denoted by $\succcurlyeq^t$ and an endowment of the two goods, $w^t = (w_x^t, w_y^t) \in \mathbb{R}^2_{++}$. We shall restrict ourselves to economies with a unique, interior competitive equilibrium.

Agents participate in a market game similar to the one introduced by Shapley and Shubik [20]. Each agent can bid a positive amount of one good in exchange for an amount of the other good. The exchange rates, or prices, are determined as the ratios of the aggregate bids for the two commodities.

More precisely, let b_j^{ti} be the amount of good j bid by agent i of type t, and let $B_j^t = \sum_{i=1}^{2n} b_j^{ti}$ be the total amount of good j bid by all agents of type t. Bidding takes place according to the following rules. For any agent i of type t, $t \in \{\mathrm{I}, \mathrm{II}\}$, we require that:

$$b_x^{ti} = 0 \quad \text{or} \quad b_y^{ti} = 0, \quad \text{or both,} \tag{1}$$

$$0 \leqslant b_x^{ti} \leqslant w_x \quad \text{and} \quad 0 \leqslant b_y^{ti} \leqslant w_y. \tag{2}$$

The first condition restricts agents to bid a positive amount for at most *one* of the two goods. The second condition requires that individual bids are feasible. The consumption baskets of type I agents in the resulting allocation are determined by market game mechanism:

$$x^{Ii} = w_x^{Ii} - b_x^{Ii} + b_y^{Ii} \frac{B_x^I + B_x^{II}}{B_y^I + B_y^{II}}, \tag{3}$$

$$y^{Ii} = w_y^{Ii} - b_y^{Ii} + b_x^{Ii} \frac{B_y^I + B_y^{II}}{B_x^I + B_x^{II}}, \tag{4}$$

and similarly for type II agents. In order to "visualize" this mechanism, one could imagine the existence of a trading post where the total bids of the two goods are aggregated and then the total amount bid of good $x(y)$ is distributed across the good $y(x)$ bidders in proportions equal to the relative size of their individual bids. The relative prices of goods x and y, p_x and p_y, are given by the ratios $\frac{B_x^I + B_x^{II}}{B_y^I + B_y^{II}}$ and $\frac{B_y^I + B_y^{II}}{B_x^I + B_x^{II}}$, respectively. While these implicit prices are not determined until bids are submitted, in equilibrium agents' expectations of prices will be correct. Note that if only one side of the market submits a positive bid, these bids are wasted as the market does not effectively open.

A standard approach is to consider the Nash problem that each individual faces in the above game. Assuming that agent i of type t is a bidder of good x, this problem can be characterized as follows: given the total bids by the other agents, choose b_x^{ti} so as to maximize $\succcurlyeq^i$ subject to constraints (1)–(2), and similarly for bidders of good y. Since agents of the same type are identical, it makes sense to consider outcomes where agents of the same type make the same bids and achieve the same allocation, $b_x^{ti} = b_x^{tj}$ for any $i \neq j$. It is straightforward to verify that $b_x^{ti} = 0$, $b_y^{ti} = 0$ for all $i = 1, \ldots, n$ and $t \in \{\mathrm{I}, \mathrm{II}\}$, is a strict, "autarkic" Nash equilibrium. Unless the endowment point itself is the competitive equilibrium allocation, this equilibrium results in a Pareto inferior outcome. It is also straightforward to verify that there exists another Pareto superior strict Nash equilibrium in which trade occurs. However, this equilibrium is not Pareto efficient as agents in a finite game have some market power. This inefficiency diminishes as the number of agents increases. Indeed, the Nash equilibrium with trade approximates the competitive allocation of the underlying economy as each agent's bid becomes insignificant relative to the corresponding aggregate bid.[4]

We now focus on a specific economy with $n > 1$ of each of the two player types, I and II. The two types have the following endowments and preferences over x and y:

$$w_x^I = w_y^{II} = w, \qquad w_y^I = w_x^{II} = W \tag{5}$$

and

$$u^I(x, y) = x^2 y, \qquad u^{II}(x, y) = xy^2. \tag{6}$$

Under the assumption of perfect competition, the demand functions for this economy are given by:

$$\begin{aligned}
x^{Ii} &= x^I = \frac{2(p_x w_x^I + p_y w_y^I)}{3p_x} = \frac{2(p_x w + p_y W)}{3p_x},\\
y^{Ii} &= y^I = \frac{(p_x w_x^I + p_y w_y^I)}{3p_y} = \frac{(p_x w + p_y W)}{3p_y},\\
x^{IIi} &= x^{II} = \frac{(p_x w_x^{II} + p_y w_y^{II})}{3p_x} = \frac{(p_x W + p_y w)}{3p_x},\\
y^{IIi} &= y^{II} = \frac{2(p_x w_x^{II} + p_y w_y^{II})}{3p_y} = \frac{2(p_x W + p_y w)}{3p_y},
\end{aligned}$$

for any $i = 1, \dots, n$. Using market clearing and the endowments given in (5), the (unique) Walrasian competitive equilibrium for this economy is given by:

$$p_y = p_x,$$

$$x^I = \frac{2(w + W)}{3}, \tag{7}$$

$$y^I = \frac{(w + W)}{3}, \tag{8}$$

$$x^{II} = \frac{(W + w)}{3}, \tag{9}$$

$$y^{II} = \frac{2(W + w)}{3}. \tag{10}$$

By contrast, consider next the Nash problem faced by agent i of type I (and similarly for a type II agent) in the market game version of this same economy. Agent i takes into account the finite population of n agents of each type and the market game allocation mechanism and solves:

$$\max_{b_y^{Ii} \in [0, w_y^I]} \left(w + \frac{b_y^{Ii}}{B_y^I} B_x^{II} \right)^2 \left(W - b_y^{Ii} \right) = \max_{b_y^{Ii} \in [0, w_y^I]} \left(w + \frac{b_y^{Ii}}{(b_y^{Ii} + \sum_{j \neq i} b_y^{Ij})} B_x^{II} \right)^2 \left(W - b_y^{Ii} \right).$$

[4] See Postlewaite and Schmeidler [15].

The FOC for this problem gives:

$$2\left[w + \frac{b_y^{Ii}}{(b_y^{Ii} + \sum_{j\neq i} b_y^{Ij})} B_x^{II}\right]\left(W - b_y^{Ii}\right) B_x^{II}\left[\frac{\sum_{j\neq i} b_y^{Ij}}{(b_y^{Ii} + \sum_{j\neq i} b_y^{Ij})^2}\right]$$
$$- \left[w + \frac{b_y^{Ii}}{(b_y^{Ii} + \sum_{j\neq i} b_y^{Ij})} B_x^{II}\right]^2 = 0,$$

and similarly for the type II agent. These two equations can be solved for the symmetric Nash equilibrium bids. These are given by

$$2\left(W - b_y^{Ii}\right) B_x^{II}\left[\frac{\sum_{j\neq i} b_y^{Ij}}{(b_y^{Ii} + \sum_{j\neq i} b_y^{Ij})^2}\right] = w + \frac{b_y^{Ii}}{(b_y^{Ii} + \sum_{j\neq i} b_y^{Ij})} B_x^{II}$$

and

$$2\left(W - b_x^{IIi}\right) B_y^{I}\left[\frac{\sum_{j\neq i} b_x^{IIj}}{(b_x^{IIi} + \sum_{j\neq i} b_x^{IIj})^2}\right] = w + \frac{b_x^{IIi}}{(b_x^{IIi} + \sum_{j\neq i} b_x^{IIj})} B_y^{I}.$$

In the symmetric Nash equilibrium,

$$b_x^{IIi} = b_x, \qquad b_y^{Ii} = b_y, \quad \text{for } i = 1, \ldots, n,$$

and

$$B_x^{II} = nb_x, \qquad B_y^{I} = nb_y.$$

It is easy to show that all Nash equilibria are such that $b_x = b_y$. It follows that there are precisely two solutions. The first is the strict "autarkic" equilibrium where $b_x = b_y = 0$, and allocations are equal to initial endowments. The second solution is the following symmetric, "full" Nash equilibrium allocation as a function of $n > 1$:

$$b_x = b_y = b = \frac{(2W - w)n - 2W}{3n - 2}. \tag{11}$$

Respecting (1)–(2), b is the amount that all n type $i = \text{I}, \text{II}$ players bid of the good for which they have endowment W for the other good with the higher marginal payoff for their type. Specifically, in the symmetric full Nash equilibrium, $b_y^{Ii} = b_x^{IIi} = b$ and $b_x^{Ii} = b_y^{IIi} = 0$. We shall further restrict attention to the case where agents make non-negative bids so that:

$$(2W - w)n - 2W \geqslant 0. \tag{12}$$

In that case, using (11) and (3)–(4), the symmetric full Nash equilibrium allocation is given by:

$$\widehat{x}^{I} = w + b = 2\frac{n-1}{3n-2}(W + w), \tag{13}$$

$$\widehat{y}^{I} = W - b = \frac{n}{3n-2}(W + w), \tag{14}$$

$$\widehat{x}^{II} = W - b = \frac{n}{3n-2}(W + w), \tag{15}$$

$$\widehat{y}^{II} = w + b = 2\frac{n-1}{3n-2}(W + w). \tag{16}$$

Table 1
Equilibrium predictions: NE = Nash equilibrium, WE = Walrasian equilibrium.

Treatment	No. each type, n	Group size = $2n$	Endowments type I; type II	Full NE bid $b_x = b_y$	Full NE allocation type I; type II	WE allocation type I; type II
1	2	4	(10, 200); (200, 10)	95	(105, 105); (105, 105)	(140, 70); (70, 140)
2	10	20	(10, 200); (200, 10)	125	(135, 75); (75, 135)	(140, 70); (70, 140)
3	10	20	(135, 75); (75, 135)	0	(135, 75); (75, 135)	(140, 70); (70, 140)

Given the 2×2 economy, the symmetry of endowments (5), and our choice of preferences (6) for given n, the full Nash equilibrium allocation described by (13)–(16) depends only on the sum of endowments $W + w$ and not on the allocation of that sum. An implication of the latter result is that if the endowment is set equal to (13)–(16) (as we consider in one of our experimental treatments below) then $b_x = b_y = b = 0$, that is, in this special case the "full" and the "autarky" Nash equilibria coincide.[5]

Notice further that as $n \to \infty$ the full Nash equilibrium allocation (13)–(16) approaches the Walrasian competitive equilibrium as given in (7)–(10).

3. The experiment

This section presents our experimental implementation of the symmetric 2×2 market game presented in the previous section. We report a number of experimental findings that motivate our theoretical model. We begin by describing the experimental design and then we discuss the main findings.

3.1. Experimental design

Our experiment implements versions of the 2×2 market game described in the previous section where the treatment variables are: (1) the number of players of each type (I and II), $n = 2$ or $n = 10$ (so that groups/economies consist of 4 or 20 players), and (2) the initial endowment for each player type, which was either given by $\{w = 10, W = 200\}$ or $\{w = 135, W = 75\}$; the latter allocation equals the full Nash equilibrium allocation in the case where $n = 10$ and agents start with endowments $\{w = 10, W = 200\}$.

Table 1 provides the full Nash equilibrium bid amounts and allocations for the treatments of our experiment. The final column gives the Walrasian competitive equilibrium allocation which remains constant across all three treatments. The numbers in Table 1 were calculated using equations (11)–(16) and (7)–(10). Notice that, consistent with our earlier discussion, as the number of players of each type increases from $n = 2$ in Treatment 1 to $n = 10$ in Treatment 2 (all else, e.g., preferences and endowments, held constant), the full Nash equilibrium allocation comes closer to the Walrasian competitive equilibrium allocation, a main focus of our experiment.

The experimental design is summarized in Table 2. The choice of endowments $\{w^I = (10, 200), w^{II} = (200, 10)\}$ was the same for both Treatments 1–2. This choice of endowments

[5] We emphasize that this no trade prediction is *not* a general result for any market game, but it does hold for the symmetric, 2×2 market game we study, given our choice of preferences and endowments. In other market game environments, e.g., under different preferences, this no-trade result may not hold. For example, in the slightly different variant of the market game considered by Peck et al. [14], our full autarky NE is not "interior" in the sense of positive aggregate bids and offers so their Proposition 2.9 does not apply.

Table 2
Experimental design.

	Endowment	
	$w^I = (10, 200),\ w^{II} = (200, 10)$	$w^I = (135, 75),\ w^{II} = (75, 135)$
$n = 2$	Treatment 1: 3 sessions (16 Sbj/Sess., 4 groups)	Not studied
$n = 10$	Treatment 2: 3 sessions (20 Sbj/Sess., 1 group)	Treatment 3: 3 sessions (20 Sbj/Sess., 1 group)

was far away from the full Nash equilibrium prediction, enabling us to ascertain whether subjects of both types would adhere to the strict, autarkic no-trade Nash equilibrium ($b = 0$) and remain at their endowment allocation or bid so as to achieve the full Nash equilibrium allocation. In an effort to comprehend whether subjects bid for rational, payoff-maximizing reasons, we also studied a third treatment (Treatment 3) in which $n = 10$ subjects of each type began with endowments that were already equal to the full Nash equilibrium allocation in the case where $n = 10$ and agents start with endowments $\{w = 10, W = 200\}$: this allocation is $\{w^I = (135, 75), w^{II} = (75, 135)\}$. As indicated in Table 1, the prediction for this third treatment is that both player types will choose to bid 0; in this case the unique NE is autarky; full and autarky Nash equilibrium allocations coincide. As Table 2 makes clear, for each of the three treatments of our experimental design, we have conducted three sessions for a total of nine sessions involving 168 subjects.

At the start of each session, subjects were randomly divided up into two equal-sized groups and assigned a role as either a type I or type II player. Subjects were given instructions on the objectives of both player types, but were informed that they would remain in the *same* role (type I or type II) for all 25 rounds of the experiment. Notice from Table 2 that in the $n = 2$ treatment, we used 16 subjects per session. In these $n = 2$ sessions, we used a "strangers" (random) matching protocol; at the start of each of the 25 rounds, the 16 subjects were randomly divided up into four groups of 4 subjects, with each group consisting of exactly two type I and two type II players. In the $n = 10$ treatment, we used 20 subjects per session, ten of type I and ten of type II players and a "partners" matching protocol.[6]

The experiment was computerized and implemented using Fischbacher's [5] z-Tree software. Subjects were seated at individual computer workstations separated by dividers. They were given written instructions which were read aloud in an effort to make the instructions "common knowledge." A copy of the instructions used in Treatments 1 ($n = 2$) and 2 ($n = 10$) is provided in online Appendix B. Subjects were informed that they would participate in 25 rounds of decision-making, with each round consisting of a repetition of the exact same decision. They were instructed that there were two goods, x and y. They were told their initial endowments of these goods at the start of each round, and that these initial endowments would be the same at the start of every round. They were further instructed that their payoff for the round would depend on their *final* allocation of the two goods. They were also informed of their payoff function over final allocations: $\pi^I = x^2 y$ or $\pi^{II} = x y^2$.[7]

[6] Ideally, we would also have liked to use a "strangers" random matching protocol for the $n = 10$ treatment, but our laboratory did not allow for the population size of 40 subjects that would have been necessary to implement such a design.

[7] To aid subjects in calculating these payoffs, they were given tables displaying payoffs for type I and type II players as a function of the final allocations of x and y for a large set of final allocations; this payoff table included the payoffs from not trading and from a wide variety of other final allocations including the trade Nash equilibrium allocation (the instructions in online Appendix B include the payoff tables shown to subjects).

The sequence of events in each round of the experiment was as follows. Subjects were reminded of their initial endowment of goods x and y and of their payoff (utility) function, all of which remained the same in every round. They were then asked to choose one of three trading decisions: (1) trade good x for good y, (2) trade good y for good x, or, (3) no trade. Subjects who chose option (1) or (2) (trade) were then asked how many units of good x (b_x^i), or good y (b_y^i), they wanted to trade for the other good (y or x, respectively). Trade amounts were restricted to be integers between one unit and the subject's total endowment of the good.[8] Once all subjects had made their trading decisions, the computer program calculated $B_x = \sum_i b_x^i$ and $B_y = \sum_i b_y^i$ for each group and then determined each player's end-of-round allocation according the allocation rules (3)–(4). The manner in which the final allocations were chosen by the computer program—the market game mechanism—was carefully explained to subjects.[9]

At the end of each round, all subjects were told (i) the amount they chose to trade (bid) of one good for the other (if any), (ii) the total amounts bid of goods x and y, B_x and B_y, by all members of their group (size 4 or 20) including themselves; the latter information allowed subjects to construct the implicit prices, $p_x = B_x/B_y$ and $p_y = B_y/B_x$, (iii) the fraction of B_x or B_y they individually acquired from their bid (if any), (iv) their individual final allocation of goods x and y, and finally, (v) their individual payoff in points for the round and the dollar value of that point total (the conversion rate of 100,000 points = \$1 was public information). To make this information as salient as possible, we not only showed it to subjects on their computer screens but we also asked them to record all these pieces of information on individual record sheets. Once this information was provided and recorded the round was over. If the 25th round had not yet been played, a new round would then begin.

Subjects were instructed that, at the end of the session, one round would be chosen randomly from all 25 rounds played and their dollar earnings for that round would comprise part of their earnings for the session. In addition, subjects were awarded a \$5 show-up payment. Total earnings averaged \$17.28 per subject in the three sessions where $n = 2$, and \$18.42 per subject in the six sessions where $n = 10$. Each session lasted about 90 minutes. Subjects were recruited from the undergraduate population at the University of Pittsburgh. No subject participated in more than one session. Next, we turn to a description of our experimental results.

3.2. Experimental findings

We begin by examining bidding behavior over the *entire* 25 rounds of our three experimental treatments. Figs. 1–3 show time series on average bids of good x for good y and of good y for good x by both player types I and II using pooled data from all three sessions of each treatment. Please note the use of a *different vertical scale* in all three figures: The smaller vertical scale on the left is for type I (II) bids of good $x(y)$ for good $y(x)$—which are predicted to be zero—while the $10\times$ larger vertical scale on the right is for type I (II) bids of good $y(x)$ for good $x(y)$, which, in treatments where subjects' initial endowments were *not equal* to the full NE (Figs. 1–2 only), are predicted to be strictly positive.

[8] Notice that while choices were limited to just one type of trade (or no trade), we did *not* restrict the choice of good that either player type was allowed to trade, and *no trade* of either good was always a choice option.

[9] Before playing, subjects had to answer a number of quiz questions that tested their understanding of the experimental design, the trading rules, the final-allocation market mechanism and their understanding of the payoff table. The reader is referred to the experimental instructions, provided in online Appendix B, for further details.

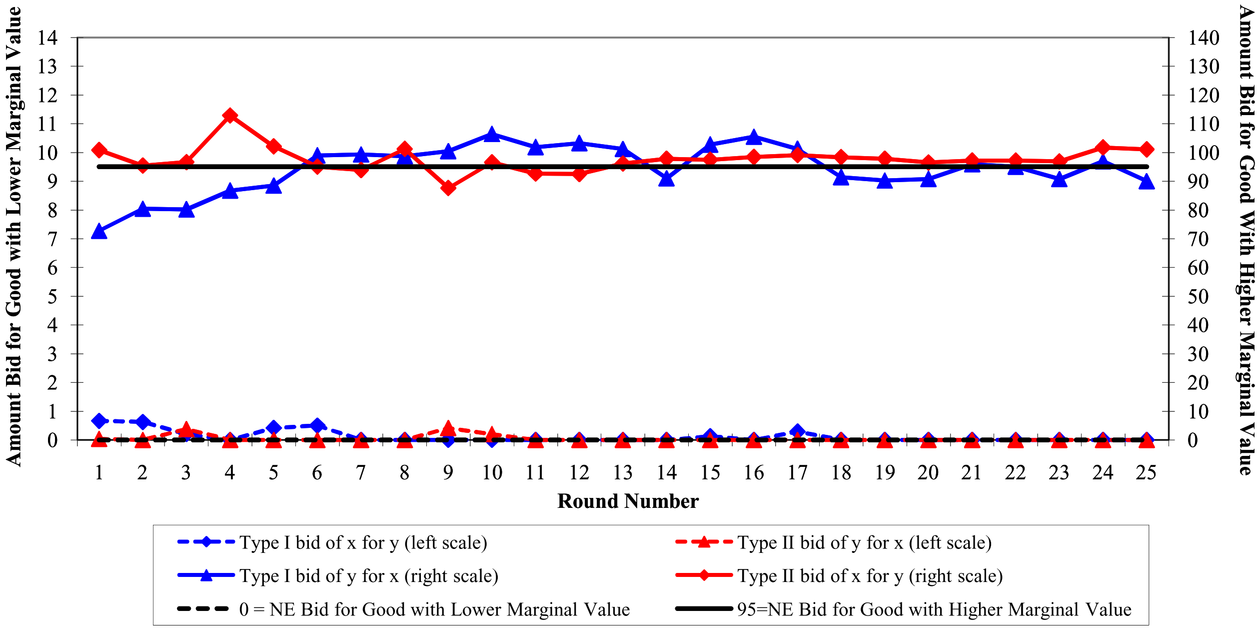

Fig. 1. Average bids over all rounds of all sessions of the treatment where $n = 2$ and initial endowments for types I, II are: $(10, 200)$, $(200, 10)$.

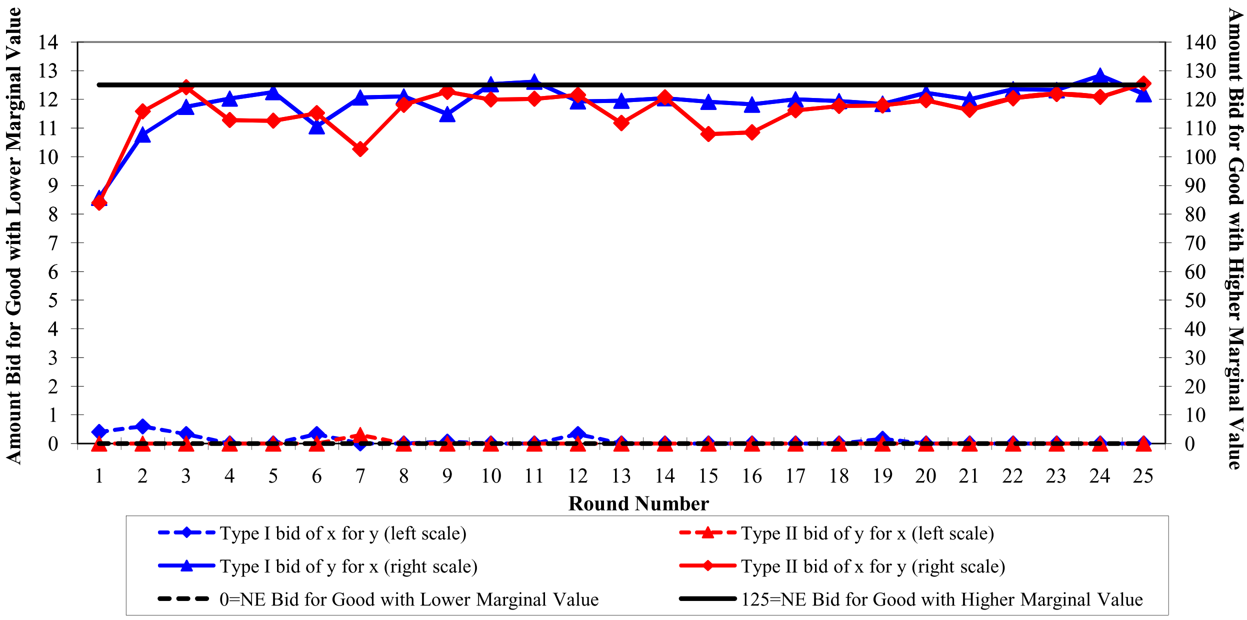

Fig. 2. Average bids over all rounds of all sessions of the treatment where $n = 10$ and initial endowments for types I, II are: $(10, 200)$, $(200, 10)$.

Consistent with theoretical predictions we see that in Treatments 1–2 (Figs. 1–2), type I players quickly learn to bid only good y for good x, and type II players quickly learn to bid only good x for good y; the alternative types of bids (shown as dashed lines in these figures), while available to subjects, are generally small (use the left vertical scale for these bids) and converge to zero over time in all treatments. Second, notice that the *amounts* of good $y(x)$ bid for good $x(y)$ by types I and II are close to the full NE predictions after only a few rounds of play (use the right vertical scale for these bids). Specifically, in Fig. 1 which reports data from all sessions of Treatment 1, the full NE prediction is that type I (II) bids 95 units of good $y(x)$ for good

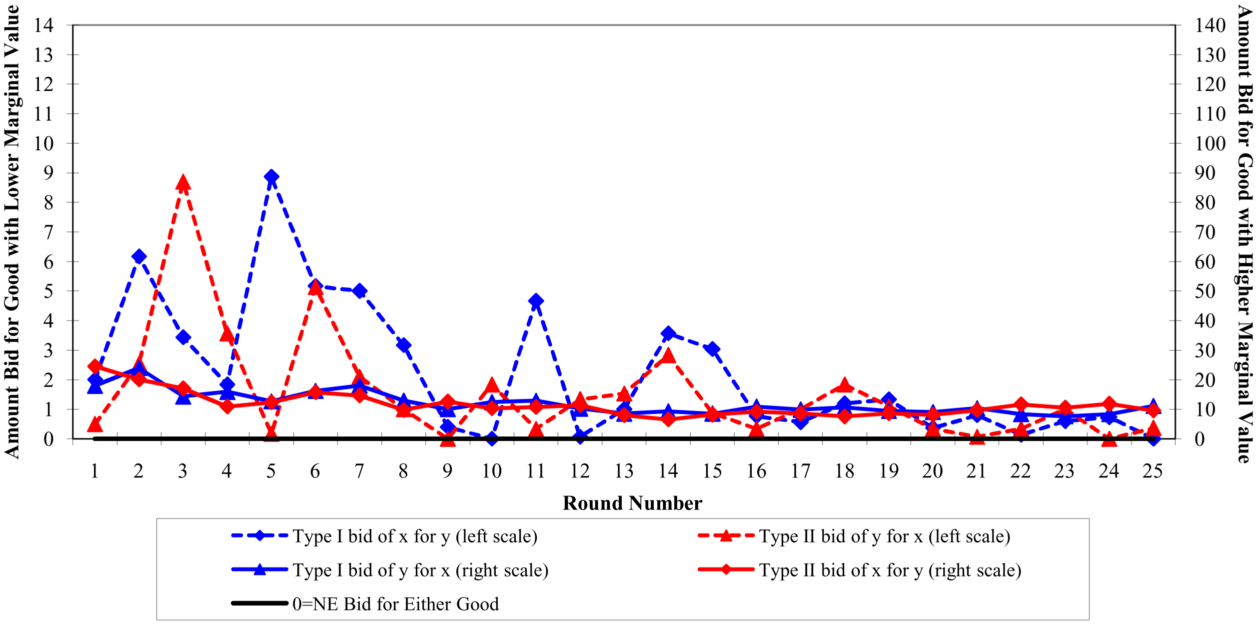

Fig. 3. Average bids over all rounds of all sessions of the treatment where $n = 10$ and initial endowments for types I, II are: $(135, 75)$, $(75, 135)$.

$x(y)$, and subjects are on average close to this prediction after 5 rounds of play. Similarly, in Fig. 2 which reports data from all sessions of Treatment 2, the full NE prediction is that type I (II) bids 125 units of good $y(x)$ for good $x(y)$, and this prediction is, on average, close to being met after around 10 rounds of play. Finally, in Fig. 3, which reports data from all sessions of Treatment 3, the full NE prediction is that all bid amounts should be zero. Notice that for this treatment, type I (II) bids of good $y(x)$ for good $x(y)$ (solid lines) average 17.0 (across both player types) in the first 5 rounds of Treatment 3 but *decrease* to an average of 9.9 in the last 5 rounds of this treatment. This difference is statistically significant using a Wilcoxon signed rank test for paired observations of session-level bid averages ($p = 0.10$). Thus, the data indicate that subjects in Treatment 3 *are* learning to decrease their bids over time. We speculate that learning *not* to bid, as is called for in the unique Nash equilibrium of Treatment 3, is more difficult for subjects who have a desire to "do something" than is learning *to* bid positive amounts as is called for in our Treatments 1–2, an observation that may help to explain why the strict autarky NE is not stable.

Figs. 4–6 show time series on the implicit price of good x, $p_x = B_x/B_y$ (the implicit price of good y, being the reciprocal, is not shown) for each of the three sessions of the three different treatments. Given the symmetric nature of our parameterization of the economy, these implicit prices should equal 1 in the full NE of all treatments. In Fig. 4, which shows (average) prices for Treatment 1 where $n = 2$ we see that, initially there are large departures from the equilibrium price of 1, but this variance in prices becomes more tightly centered around the equilibrium prediction of 1 as subjects gain experience. By contrast, Fig. 5 shows that prices in Treatment 2 where $n = 10$ (and therefore, subjects had considerably less market power), are very close to the equilibrium prediction of 1 in nearly every round, beginning with the first. Finally, Fig. 6 shows prices for Treatment 3 where $n = 10$ but subjects' initial endowments were set equal to the full Nash equilibrium. While prices are quite volatile in this case, they remain centered around 1. The persistent volatility in the latter case arises because the Nash equilibrium prediction calls for *no trade*, a situation in which prices are not well-defined.

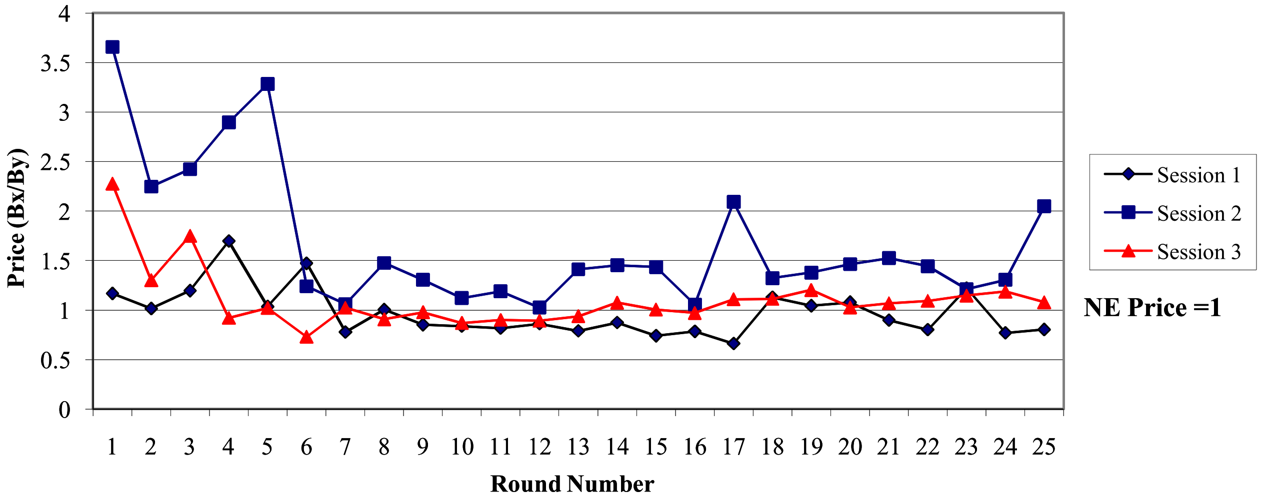

Fig. 4. Implicit price of good X, 3 sessions where $n = 2$ and initial endowments for types I, II are: (10, 200), (200, 10) (average of 4 groups).

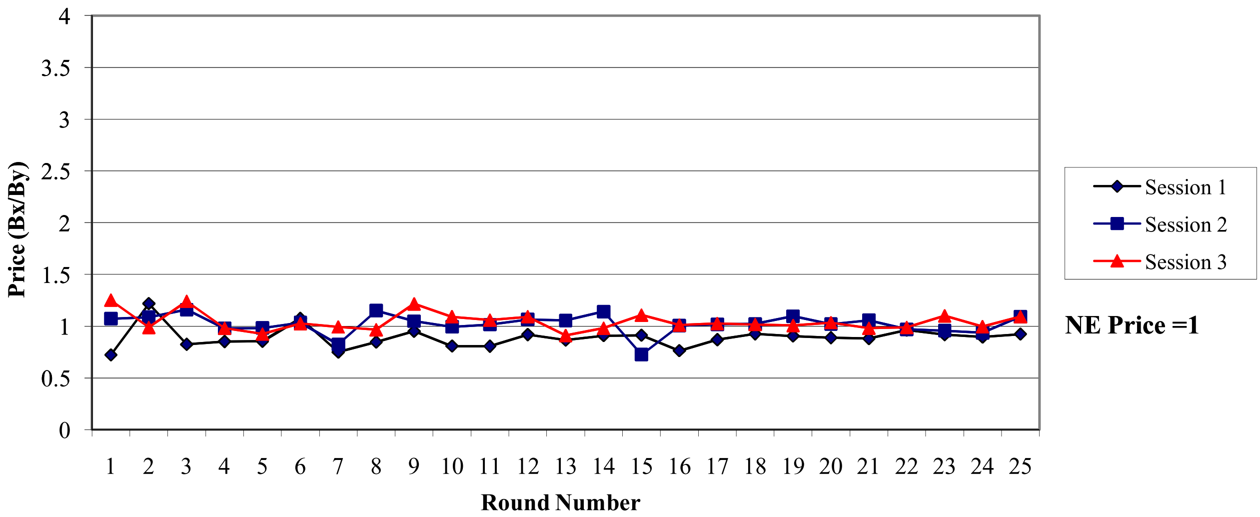

Fig. 5. Implicit price of good X, 3 sessions with $n = 10$ and initial endowments for types I, II are: (10, 200), (200, 10).

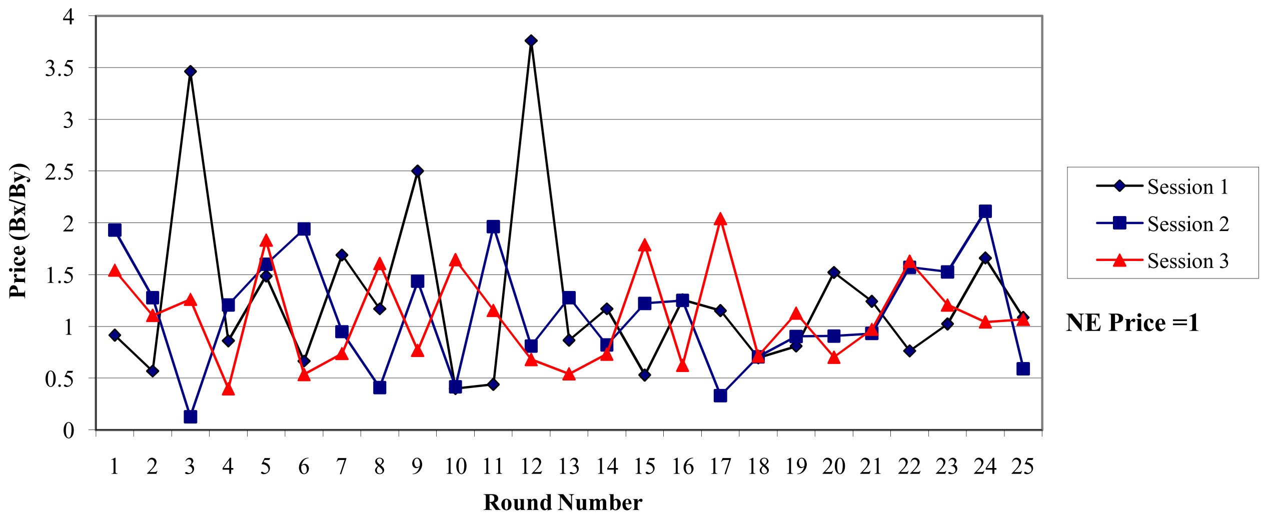

Fig. 6. Implicit price of good X, 3 sessions with $n = 10$ and initial endowments for types I, II are: (135, 75), (75, 135).

Table 3
Averages by player types I, II, from the last 5 rounds of Treatment 1 and 2 sessions where $w^I = (10, 200)$, $w^{II} = (200, 10)$.

Session	n	Type I, $w^I = (10, 200)$				Type II, $w^{II} = (200, 10)$			
		% No trade	Avg. bid	Final alloc.	Pay. eff. % full NE	% No trade	Avg. bid	Final alloc.	Pay. eff. % full NE
1	2	0.00	(0, 95)	(91, 105)	0.727	0.00	(81, 0)	(119, 105)	1.152
2	2	0.00	(0, 83)	(115, 117)	1.264	0.00	(105, 0)	(95, 93)	0.737
3	2	0.00	(0, 103)	(121, 97)	1.194	0.00	(111, 0)	(89, 113)	0.924
Avg. 1–3	2	0.00	(0, 94)	(109, 106)	1.062	0.00	(99, 0)	(101, 104)	0.937
Full NE	2	0.00	(0, 95)	(105, 105)	1.000	0.00	(95, 0)	(105, 105)	1.000
WE	–	–	(0, 130)	(140, 70)	1.185	–	(130, 0)	(70, 140)	1.185
1	10	0.00	(0, 130)	(129, 70)	0.833	0.00	(119, 0)	(81, 140)	1.111
2	10	0.00	(0, 113)	(124, 87)	0.933	0.00	(114, 0)	(86, 123)	0.948
3	10	0.00	(0, 126)	(140, 74)	1.013	0.00	(130, 0)	(70, 136)	0.902
Avg. 1–3	10	0.00	(0, 123)	(131, 77)	0.926	0.00	(121, 0)	(79, 133)	0.987
Full NE	10	0.00	(0, 125)	(135, 75)	1.000	0.00	(125, 0)	(75, 135)	1.000
WE	–	–	(0, 130)	(140, 70)	1.004	–	(130, 0)	(70, 140)	1.004

To abstract from possible learning effects, we will now focus on player types I and II behavior using all data from the last 5 rounds of each session. In particular, we use data from the final 5 rounds to calculate (1) the average frequency with which each player type chose "No Trade," (2) the average amount of each good bid by each type (among those players who chose to trade), (3) the average final allocation for each type, and finally, as a measure of efficiency, (4) the average payoff earned by each type as a percentage of the full Nash equilibrium payoff. These session-level averages are reported in Tables 3–4, where average amounts bid ("Avg. bid"), and average final allocations ("Final alloc."), are represented as *pairs* in the format (amount of good x, amount of good y). We focus primarily on *bids* as that is what subjects were asked to choose. Final allocations follow from application of the market game mechanism, (3)–(4), to subjects' bids. Individual final allocations from the last 5 rounds of our three treatments are shown in Figs. 1–3.

Consider first the experimental results for Treatment 1 ($n = 2$) and Treatment 2 ($n = 10$) where subjects start out with endowments $w^I = (10, 200)$ and $w^{II} = (200, 10)$ as reported in Table 3. The results in both treatments are striking: in the last 5 rounds, *all* subjects are choosing to engage in trade, i.e., the percent choosing No trade is always 0, so subjects have clearly chosen to move away from the strict, autarkic Nash equilibrium. Further, as a simple test of rationality, we observe that both player types are offering to bid for the good with the higher marginal payoff for their type even though they were free to bid for either type of good, or not to bid at all. Specifically, type I subjects are choosing to bid some of their endowment of good y for good x, as evidenced by the 0 in the first element of their bid pairs, and likewise, type II subjects are choosing to bid some of their endowment of good x for good y, as evidenced by the 0 in the second element of their bid pairs. More importantly we observe that for both treatments ($n = 2$, $n = 10$) the average amounts bid by each player type for the good with the higher marginal payoff is not significantly different from the full Nash equilibrium prediction. Specifically, using the three, session-level average bids by players of type I or II as reported in Table 3 for the $n = 2$ and $n = 10$ treatments, a (non-parametric) Wilcoxon signed rank test indicates that we cannot reject the null hypothesis that bids do not differ from the relevant Nash equilibrium bid prediction, i.e., $b = 95$ when $n = 2$ and $b = 125$ when $n = 10$ ($0.285 \leqslant p \leqslant 1.0$ in four separate tests, 3 session-level observations for each test). Further, in Treatment 1 ($n = 2$ players per type), subjects are *not* bidding in a manner consistent with the Walrasian competitive equilibrium. To achieve the latter outcome would require that each type bid 130 units of their endowment of 200 units of good x or y for the other good with the higher marginal payoff for their type (see Table 1). But using the Wilcoxon signed ranks test we can reject the null hypothesis that $b = 130$ using the 3 session-level average bids per player type for the $n = 2$ treatment reported in Table 3 in favor of the alternative that bids are less than 130 ($p = 0.10$ for both tests). However, for Treatment 2 ($n = 10$ players per type), the Wilcoxon signed ranks test on session-level bid averages does not allow us to reject the null hypothesis that players of both types were bidding in accordance with the competitive equilibrium prediction of $b = 130$, though the p-value for both tests, $p = 0.16$, remains low. The latter finding is not so surprising given the closeness of the full NE bid prediction, $b = 125$, to the competitive equilibrium bid prediction of $b = 130$. Indeed, it suggests that with $n = 10$ the economy is already large enough to be considered approximately Walrasian, though bidding behavior is also consistent with the full NE prediction, an observation that provides motivation for the theoretical model we develop in the next section.

We further observe in Table 3 that subjects are achieving approximately 100% of the payoffs they could have earned had they played according to the full Nash equilibrium; in some instances certain player types are doing slightly better due to less than complete coordination on the NE and the inefficiency of that equilibrium relative to the competitive equilibrium. Notice further that, consistent with theoretical predictions, most of these averages lie below the competitive equilibrium allocation (WE) predictions.

Perhaps most importantly, there is strong evidence for the comparative statics prediction of greater trade volume (bid amounts) as the number of each player type increases from 2 to 10. Using the three session-level averages for the bids (of good y for good x) for type I players or the bids (of good x for good y) for type II players, a non-parametric, Mann–Whitney test confirms that we can reject the null hypothesis of no difference in bid amounts between the $n = 2$ and $n = 10$ treatments in favor of the alternative hypothesis that bids by both player types are *greater* when $n = 10$ than when $n = 2$ ($p = 0.05$ for both tests).

For further evidence that group size matters, we report below the results of a simple regression analysis where the dependent variable, "AvgBid," is the average amount bid by each subject for the good with the higher marginal payoff for their type in the last 5 rounds of all six sessions of Treatments 1 and 2 where individuals began with endowments of $w^I = (10, 200)$ and $w^{II} = (200, 10)$. Unlike the non-parametric test results described above which used three session-level average observations per treatment, in this regression analysis we are using a total of 108 *individual*-level average bid observations.[10] The independent variables in this regression are a constant, a treatment dummy, *DTreat*, that is equal to 0 if $n = 2$ and 1 if $n = 10$ and a type dummy, *DType*, that is equal to 0 if the player was a type I player and 1 if he was a type II player. The regression result is as follows (robust standard errors, clustered on sessions, are shown in

[10] While the session-level observations may be regarded as independent of one another, the individual bid observations within a session are not independent of one another. For this reason, our regression analysis involves clustering of standard errors for observations within the same session.

Table 4
Averages by player types I, II, from the last 5 rounds of Treatment 3 sessions where $n = 10$ and $w^I = (135, 75)$, $w^{II} = (75, 135)$, i.e., the full NE allocation.

Session	n	Type I, $w^I = (135, 75)$				Type II, $w^{II} = (75, 135)$			
		% No trade	Avg. bid	Final alloc.	Pay. eff. % full NE	% No trade	Avg. bid	Final alloc.	Pay. eff. % full NE
1	10	0.48	(0, 6)	(141, 69)	1.004	0.30	(6, 0)	(69, 141)	0.988
2	10	0.32	(0, 11)	(147, 65)	1.007	0.26	(13, 0)	(63, 145)	0.928
3	10	0.34	(1, 11)	(147, 65)	0.996	0.18	(13, 1)	(63, 145)	0.953
Avg. 1–3	10	0.38	(0, 9)	(145, 66)	1.002	0.25	(11, 0)	(65, 144)	0.956
Full NE	10	1.00	(0, 0)	(135, 75)	1.000	1.00	(0, 0)	(75, 135)	1.000
WE	–	–	(0, 5)	(140, 70)	1.004	–	(5, 0)	(70, 140)	1.004

parentheses):

$$\underset{}{\text{AvgBid}} = \underset{(4.79)}{95.81} + \underset{(6.57)}{25.94 DTreat} + \underset{(5.01)}{0.90 DType} \quad R^2 = 0.26.$$

Notice first that, consistent with the symmetric NE prediction, the coefficient on *DType* is not significantly different from zero. The coefficient on the constant term is significantly different from zero, again supporting the claim that subjects have avoided the strict, autarkic Nash equilibrium. Further, a Wald test indicates that the coefficient on the constant term is not significantly different from the NE prediction of 95 for the $n = 2$ treatment ($p = 0.87$). The coefficient on the treatment dummy is significantly positive indicating that as n rises from 2 to 10 the average amount bid increases from 95.81 to 121.75; the latter amount is not significantly different from the NE prediction of 125 for the $n = 10$ treatment according to a Wald test ($p = 0.53$).

These findings suggest that as the number of players of both type in the economy increases—as market power subsides—strategic considerations diminish in importance as the full NE allocation gradually approximates that of the WE.

We next consider Treatment 3 where groups of 20 subjects ($n = 10$ of each type) start out with endowments equal to the relevant full NE allocation (for $n = 10$) as reported in Table 4. Recall that in this particular case, the NE is unique and calls for both types to bid 0, i.e., there should be *no* trade. Table 4 reveals that, contrary to this prediction, a majority of subjects *do* choose to engage in some trade—the percentage clicking on the "no trade" button (% No trade) over the last 5 rounds of each session averages just 38% for type I and 25% for type II players (31.5% for both types). While the frequency of subjects choosing no trade in Treatment 3 is significantly greater than in Treatment 2 where $n = 10$ subjects of both types did not start out at the full NE allocation ($p = 0.05$, Mann–Whitney test using session-level observations), the frequency of subjects choosing no trade is still far less than the theoretical prediction of 100%. Notice, however, that the amounts bid are rather *small*. Type I agents bid, on average, just 9 units of good y for good x while type II agents bid, on average just 11 units of good x for good y. The alternative type of trade available to each player type averages 0, as in the other treatments. A Mann–Whitney test using session-level data confirms that trade volume (bids of good y for x by type I players and bids of good x for y by type II players) is significantly less than in Treatment 3 where $n = 10$ subjects of each type start out at the full NE allocation than in Treatment 2 where they do not ($p = 0.05$). However, a Wilcoxon signed rank test on session-level bid averages by both player types indicates that we can reject the null hypothesis that $b = 0$ in Treatment 3 ($p = 0.10$ for both tests).

Further evidence in support of these findings is found in a regression analysis similar to the one reported earlier, again using individual-level data on the average amounts bid for the good with the higher marginal payoff, "AvgBid," in the final 5 rounds of all 6 sessions of Treatments 2 and 3 where $n = 10$ (120 individual observations from the 6 sessions with 20 subjects). The independent variables are again *DType* and *DTreat*. In this regression, *DType*, as before, is a player type dummy variable. But *DTreat* is now equal to 0 in the case where $w^I = (135, 75)$, $w^{II} = (75, 135)$ (Treatment 3) and (as before) is equal to 1 in the case where $w^I = (10, 200)$, $w^{II} = (200, 10)$ (Treatment 2). The regression result is as follows (robust standard errors, clustered on sessions, are shown in parentheses):

$$\text{AvgBid} = \underset{(3.55)}{10.60} + \underset{(4.72)}{111.82}\,\textit{DTreat} - \underset{(2.20)}{0.46}\,\textit{DType} \qquad R^2 = 0.96.$$

Again, consistent with the symmetric NE prediction, the coefficient on *DType* is not significantly different from zero. The coefficient on the constant term, 10.60, is significantly different from zero ($p = 0.03$) contrary to the Nash equilibrium prediction of 0 in the third treatment where subjects start out with endowments equal to the full Nash equilibrium allocation. Finally, the coefficient on the *DTreat* dummy is positive and significant: a change in the initial endowment from $\{w^I = (135, 75),\ w^{II} = (75, 135)\}$ to $\{w^I = (10, 200),\ w^{II} = (200, 10)\}$ holding n fixed at 10 leads to an increase in the average amount bid from 10.6 to 122.42. The latter amount is not significantly different from the NE prediction of 125 for the $n = 10$ when subjects start far from the full NE according to a Wald test, $p = 0.57$.

The small amount of trade that does occur in Treatment 3 (where subjects start out with allocations equal to the full NE) has the effect of moving subjects closer in the direction of the competitive equilibrium (WE) allocation. Achievement of the WE allocation in Treatment 3 would require that subjects bid just 5 units of the good with the lower marginal payoff for their type for 5 more units of the good with the higher marginal payoff for their type. On average, it appears that subjects overshoot this WE outcome by bidding approximately 10 more (rather than just 5 more) units of the good with the lower marginal payoff for the good with the higher marginal payoff. Indeed, we can reject the null hypothesis that the amounts subjects bid in Treatment 3 for the good with the higher marginal payoff are significantly different from 5 using a Wilcoxon signed ranks test on session-level bid averages for each player type ($p = 0.10$ for both tests). Nevertheless, we also find that we cannot reject the null hypothesis of no difference in payoff efficiency relative to the full NE benchmark for Treatment 3 as compared with the other two Treatments 1 and 2 involving $n = 2$ or 10 subjects of each type where initial endowments were far away from the full NE ($p \geqslant 0.35$, for all pairwise comparisons using the Mann–Whitney test on session-level efficiency averages).

In summary, while subjects in Treatment 3 violate the no-trade theoretical prediction, the violation is small. One rationalization for the puzzling behavior in Treatment 3 is that the full NE endowment from which subjects start is not Pareto optimal; subjects may be trying to find their way to a more efficient outcome, even though the WE allocation is *not* an NE under the market game mechanism given the number of agents considered here. Alternatively, we note that the payoff surface is not very steep in a small neighborhood of the full NE so that subjects may not perceive departures from the full NE to be too costly. A final observation is that the persistently small amounts bid in the last 5 rounds of Treatment 3 by the 68.5% of subjects choosing to engage in trade indicates that these subject are primed by their environment

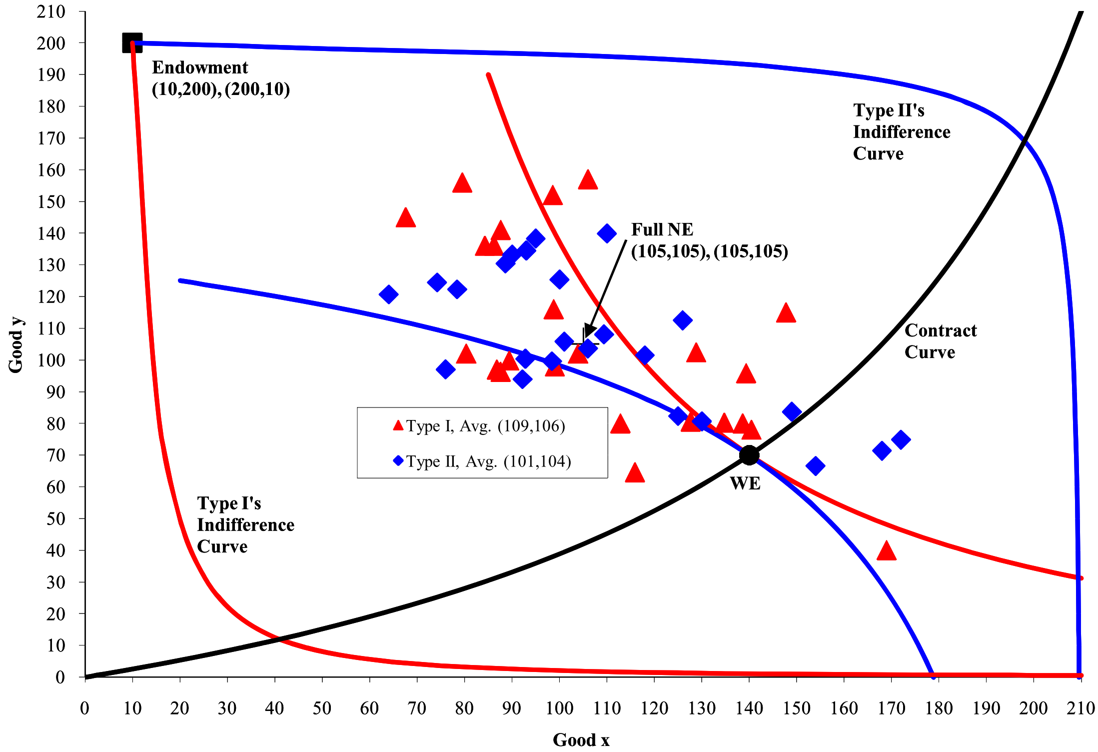

Fig. 7. Average final allocations, last 5 rounds, treatment where $n = 2$ and initial endowments for type I, II are: (10, 200), (200, 10).

to try to "do something"—an observation that serves as motivation for the boundedly rational, evolutionary approach to equilibrium selection that we develop in the next section. Indeed, in Section 4.3, we explain how our evolutionary solution concept can rationalize such behavior.

Thus far, with the exception of our regression analysis, we have primarily considered *aggregate* outcomes. Figs. 7–9 show all *individual final allocations* (averaged over the final 5 rounds) using data from all three sessions of each of the three treatments. These individual allocations are situated within the Edgeworth box representing the pure exchange economy that we implemented experimentally. For Treatment 1 where $n = 2$ and initial endowments are $w^I = (10, 200)$ and $w^{II} = (200, 10)$ Fig. 1 reveals that there remains considerable dispersion in individual final allocations in the final 5 rounds, though averages for the two player types are very close to the full NE prediction. This dispersion can be attributed to the relatively greater market power subjects have in this treatment. By contrast, in Treatment 2, when n increases form 2 to 10 so that market power is greatly reduced, the dispersion in individual, average final allocations is also greatly reduced as revealed in Fig. 2, with the averages for each player type remaining very close to the full NE prediction. Finally, as Fig. 3 shows, in Treatment 3 where $n = 10$ and subject's endowment is at the full NE allocation, $w^I = (135, 75)$, $w^{II} = (75, 135)$, the dispersion in individual, average final allocations is even further reduced. We will have more to say about the variance in subjects' payoffs later in Section 4.3 when we assess the empirical relevance of our evolutionary model.

Having presented strong evidence that human subjects eschew the strict, autarkic NE in favor of the full NE and that, as predicted, the full NE allocation approximates the competitive WE allocation as the size of the economy (number of each player type) grows, we next turn toward providing a theoretical explanation of these findings.

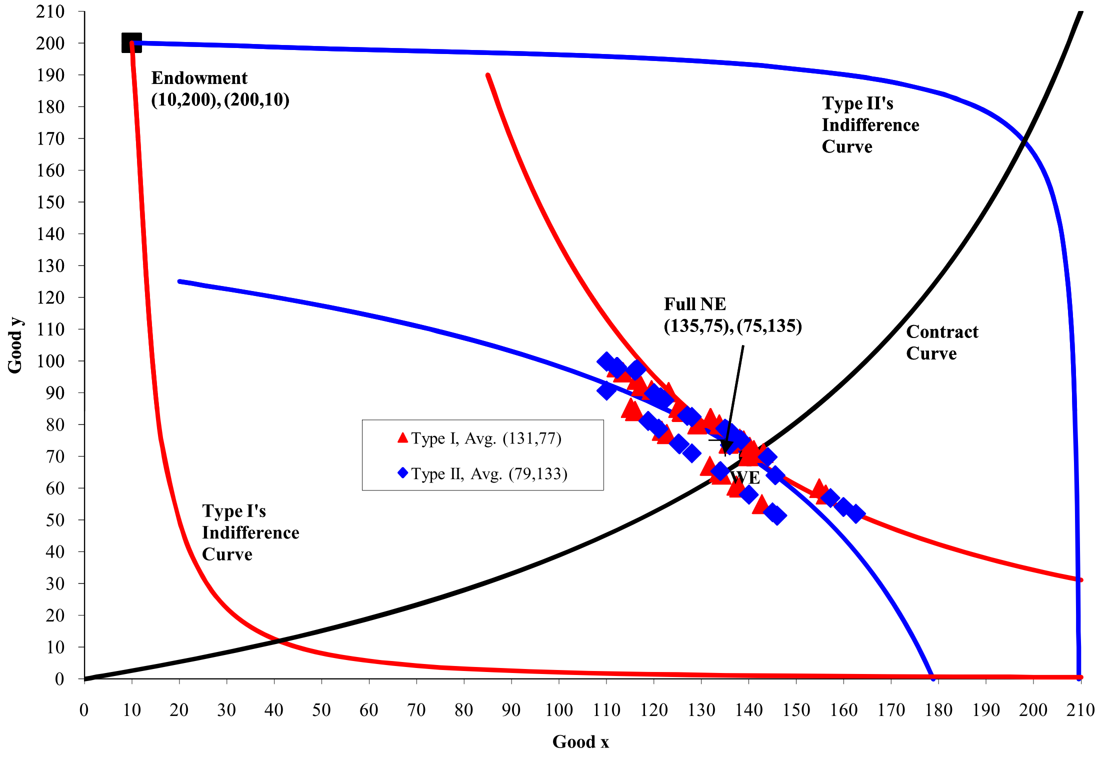

Fig. 8. Average final allocations, last 5 rounds, treatment where $n = 10$ and initial endowments for types I, II are: $(10, 200)$, $(200, 10)$.

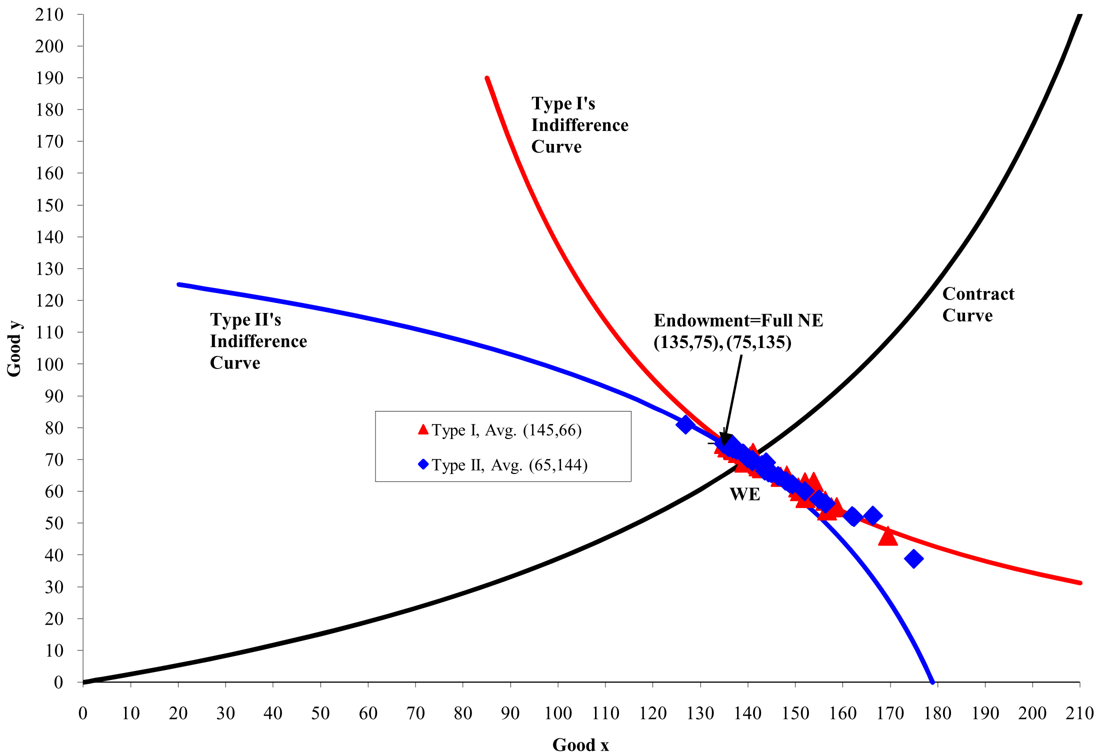

Fig. 9. Average final allocations, last 5 rounds, treatment where $n = 10$ and initial endowments for types I, II are: $(135, 75)$, $(75, 135)$.

4. The theoretical model

In order to characterize the main experimental findings, our theoretical model must have two important features. First, it must be able to assist in equilibrium selection by distinguishing among strict Nash equilibria. Second, the model must be able to distinguish between NE and competitive allocations and generate the right comparative statics. In particular, consistent with our experimental findings, Nash behavior must approximate competitive behavior only as agents' market power declines. In what follows, we build a model that has these two features.

4.1. The solution concept

We begin by stating an existing definition of evolutionary stability in the context of an abstract normal form game. First, consider a single population consisting of a continuum of identical agents, and assume that N agents are selected to play a normal-form game $\Gamma = (N, S, U)$, where S is the set of available (pure) strategies, and U represents payoffs. The definition of *ESS* for N-player symmetric games is as follows (see Schaffer [17,18]):

Definition 1. A strategy $s \in S$ is an **ESS** if, for any strategy $t \in S$, $t \neq s$,

$$U\big(s, (t, \bar{s})\big) \geqslant U\big(t, (s, \bar{s})\big),$$

where $(t, \bar{s})$ and $(s, \bar{s})$ denote the strategies of the other $(n-1)$ players. In particular, $(s, \bar{s})$ indicates that all other players play strategy s, while $(t, \bar{s})$ indicates that one player plays strategy t, while all other players play s.

We will amend Schaffer's [17] definition in two ways. First, we extend the definition of an *ESS* from one to multiple, distinct, finite populations. Second, we will require a strong version of evolutionary stability: one that requires stability against simultaneous deviations by multiple agents from different populations.

We first present the concept in the context of an example. In the next section, we will apply it to a market game. Assume that there are 2 finite populations. Each population contains $n \geqslant 2$ agents. Agents play an N-player game, Γ, where $N = 2n$. The game is assumed to have the following symmetry property. All players from population i have the same set of strategies, X^i, and the same payoff function, U^i. In other words, if two players (from the same population) play the same strategy, they will obtain the same payoffs. Hence, we can indicate the normal form game as

$$\Gamma = \big(2n; \underbrace{S^1 \times \cdots \times S^1}_{n\,\text{times}} \times \underbrace{S^2 \times \cdots \times S^2}_{n\,\text{times}}; \big(U^1; U^2\big)\big).$$

In what follows, we will need to consider the situation where one agent from population i plays strategy t^i, while every other agent from that population plays strategy s^i. More generally, in the case where at most one agent in each population plays a strategy, t, which is different from the one chosen by every other agent in his population, the payoff of the agent from population i who plays a different strategy than his peers can be written as:

$$U^i\big(t^i; \big(t^1, \overline{s^1}\big); \big(t^2, \overline{s^2}\big)\big), \tag{17}$$

where, as before, $(t^i, \overline{s^i})$ denotes that one agent from population i plays t^i, while all other agents from population i play s^i.

We are now ready to define our main concept.

Definition 2. A symmetric strategy profile

$$\bar{s} = (\underbrace{s^1, \ldots, s^1}_{n}; \underbrace{s^2, \ldots, s^2}_{n}) \in \underbrace{S^1 \times \cdots \times S^1}_{n} \times \underbrace{S^2 \times \cdots \times S^2}_{n}$$

is a **Strong ESS** (**SESS**) if, for all i,

$$U^i\left(s^i; \gamma^1, \gamma^2\right) \geqslant U^i\left(t^i; \gamma^1, \gamma^2\right), \tag{18}$$

for any strategy $t^i \in S^i$, $t^i \neq s^i$, and for all γ^j, such that $\gamma^j = (s^j, \overline{s^j})$, or $\gamma^j = (t^j, \overline{s^j})$.

In other words, a notable feature of the *SESS* is that it requires stability against up to 2 simultaneous deviations (one per population). Clearly, this is a stronger concept than Schaffer's *ESS*. Thus, *SESS* will not exist in general. In the next section, we motivate and use *SESS* in the context of 2×2 market games. However, it is worth noting that *SESS* might have applications in other contexts in which two or more asymmetric populations coevolve.[11]

4.2. *Evolutionary stability in* 2×2 *market games*

We now apply the SESS concept to the 2×2 market game presented in Section 2 as related to the pure exchange economy described in (5) and (6). We begin by presenting a definition of ϵ-*SESS* in the context of a market game.

Definition 3. A symmetric strategy profile

$$\bar{s} = (\underbrace{s^I, \ldots, s^I}_{n}; \underbrace{s^{II}, \ldots, s^{II}}_{n}) \in \underbrace{S^1 \times \cdots \times S^1}_{n} \times \underbrace{S^2 \times \cdots \times S^2}_{n}$$

is an ϵ-**SESS** if

$$U^i\left(s^i; \gamma^1, \gamma^2\right) \geqslant U^i\left(t^i; \gamma^1, \gamma^2\right) - \epsilon,$$

for all i, for any $t^i \neq s^i$, and for all γ^j, such that $\gamma^j = (s^j, \overline{s^j})$, or $\gamma^j = (t^j, \overline{s^j})$.

The above conditions require that a deviating agent be better off by at most ϵ relative to the other agents of his type when at most one agent per population deviates.

Define an exchange economy with two consumption goods by

$$\mathcal{E}(n) = \left\langle 2n, (X, Y), (w, W), \left(U^1; U^2\right)\right\rangle,$$

where $2n$ agents belong to 2 different populations (or types); (X, Y) denote the respective consumption possibility sets; (w, W) denote the respective endowment vectors; and $U^i : \mathbb{R}^2_+ \to \mathbb{R}$ are the Cobb–Douglas utility functions. Agents belonging to the same type have identical preferences and endowments.

[11] One current example concerns investing in renewable energy. The Texas Energy Conservation Office states that "Wind power developers are reluctant to build where transmission lines do not yet exist; and utilities are equally reluctant to install transmission in areas that do not yet have power generators." Thus, for such investments to be successful, two kinds of investments, by two different types of investors, are often necessary. Other examples might include the coevolution of species in biology.

The proof of our first result proceeds by deriving explicit bounds for the effects of a deviating coalition on the terms of trade. Such effects are small provided that the economy is sufficiently large. Given this fact, the proof establishes that the resulting change in consumption baskets and, thus, in utility, for the non-deviating agents is also small.

Theorem 1. *Consider an economy described in expressions* (5), (6). *For any positive number* $\epsilon > 0$*, there exists* $n_\epsilon > 1$ *such that the competitive equilibrium profile*

$$\big(\underbrace{s^I, \ldots, s^I}_{n_\epsilon}; \underbrace{s^{II}, \ldots, s^{II}}_{n_\epsilon}\big)$$

of the market game $\mathcal{E}(n_\epsilon)$ *is an* ϵ*-SESS.*

Proof. Let $\epsilon > 0$ be given. Consider an economy $\mathcal{E}(n)$ and the (unique) Walrasian competitive equilibrium allocation for this economy is given by

$$x^I = \frac{2(w+W)}{3}, \qquad y^I = \frac{(w+W)}{3},$$
$$x^{II} = \frac{(W+w)}{3}, \qquad y^{II} = \frac{2(W+w)}{3},$$

or

$$b_x^I = 0, \qquad b_y^I = W - \frac{(w+W)}{3} = \frac{2W-w}{3} > 0$$

and

$$b_x^{II} = \frac{2W-w}{3} > 0, \qquad b_y^{II} = 0.$$

Fix a positive number $\epsilon > 0$. We want to find an $n_\epsilon > 1$ such that

$$\max_{d_y^I, d_x^{II}} \big[U^i\big(d^i; \gamma^1, \gamma^2\big) - U^i\big(s^i; \gamma^1, \gamma^2\big)\big] \leqslant \epsilon$$

in the market game $\mathcal{E}(n_\epsilon)$, where d^i denotes the deviation from the competitive equilibrium by agent type i. Consider

$$\begin{aligned}
&\big[U^i\big(d^i; \gamma^1, \gamma^2\big) - U^i\big(s^i; \gamma^1, \gamma^2\big)\big] \\
&\quad = \big[U^I\big(d_y^I; b_y^I, \ldots, b_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\big) - U^I\big(b_y^I; b_y^I, \ldots, b_y^I; d_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\big)\big] \\
&\quad \leqslant \big|U^I\big(d_y^I; b_y^I, \ldots, b_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\big) - U^W\big| \\
&\qquad + \big|U^I\big(b_y^I; b_y^I, \ldots, b_y^I; d_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\big) - U^W\big|,
\end{aligned}$$

where $U^W = (x^I)^2 y^I$ is the agent's utility in the competitive equilibrium and, for simplicity, we have replaced strategy s with the positive component of the strategy profile, b_j^i.

Note that

$$\widetilde{x}^I = w_x^I + b_y^I \frac{(n-1)b_x^I + d_x^{II}}{(n-1)b_y^I + d_y^I}, \tag{19}$$

$$t_x^I = w_x^I + d_y^I \frac{(n-1)b_x^I + d_x^{II}}{(n-1)b_y^I + d_y^I}, \tag{20}$$

$$\widetilde{y}^I = w_y^I - b_y^I, \tag{21}$$

$$t_y^I = w_y^I - d_y^I. \tag{22}$$

Therefore,

$$\begin{aligned}&\left|U^I\left(d_y^I; \widehat{b}_y^I, \ldots, \widehat{b}_y^I; \widehat{b}_x^{II}, \ldots, \widehat{b}_x^{II}; d_x^{II}\right) - U^W\right| \\ &\quad = \left|\left(w + d_y^I \frac{(n-1)\frac{2W-w}{3} + d_x^{II}}{(n-1)\frac{2W-w}{3} + d_y^I}\right)^2 \left(W - d_y^I\right) - \left(\frac{2(w+W)}{3}\right)^2 \frac{(w+W)}{3}\right|.\end{aligned} \tag{23}$$

From (23) and our assumption that the endowment (w, W) differs from the competitive equilibrium, it follows that there exists $n_1 > 1$ such that

$$\left|U^I\left(b_y^I; b_y^I, \ldots, b_y^I; d_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\right) - U^W\right| < \frac{\epsilon}{2},$$

for any market game $\mathcal{E}(n)$ such that $n \geqslant n_1$. Analogously, it follows that there exists $n_2 > 1$ such that

$$\left|U^I\left(b_y^I; b_y^I, \ldots, b_y^I; d_y^I; b_x^{II}, \ldots, b_x^{II}; d_x^{II}\right) - U^W\right| < \frac{\epsilon}{2},$$

for any market game $\mathcal{E}(n)$ such that $n \geqslant n_2$.

The theorem follows for n_ϵ defined by

$$n_\epsilon = \max\{n_1, n_2\}.$$

We conclude that the competitive equilibrium profile is an ϵ-*SESS*. □

A couple of remarks are in order. First, the proof of the above theorem uses the "large economy" assumption. This turns out to be a necessary condition for the result.[12] This feature is consistent with our experimental findings and we consider it to be a central feature of our model as it suggests that evolutionary arguments can be used as a foundation for competitive equilibria only when agents lack market power. Further as we showed in Section 2 for the 2×2 market game studied in the experiment, as $n \to \infty$, the full Nash equilibrium approaches the unique Walrasian competitive equilibrium for the economy.[13] This observation and Theorem 1 lead to the following result.

Theorem 2. *Suppose the economy* (5), (6) *has a unique symmetric full Nash equilibrium. For any positive number* $\mu > 0$, *there exists* $n_\mu > 1$ *such that the symmetric full Nash equilibrium profile*

$$\big(\underbrace{\widehat{s}^I, \ldots, \widehat{s}^I}_{n_\mu}; \underbrace{\widehat{s}^{II}, \ldots, \widehat{s}^{II}}_{n_\mu}\big)$$

of the market game $\mathcal{E}(n_\mu)$ *is a* μ-*SESS*.

The proof is similar to the proof of Theorem 1 and therefore omitted.[14]

Next, we demonstrate that the symmetric autarky Nash equilibrium profile is not a μ-SESS.

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[12] This is in contrast to Vega-Redondo [25], where a competitive (zero profit) outcome is shown to be evolutionary stable in an oligopoly game independent of the number of competing firms.

[13] This is a standard result in the literature. See, for example, Postlewaite and Schmeidler [15].

[14] In what follows, we use μ-SESS when we talk about full Nash equilibria and ϵ-SESS when we talk about competitive equilibrium.

Theorem 3. *Suppose the endowment (w, W) differs from the competitive equilibrium allocation. There exists a positive number $\mu_0 > 0$ such that for any $n > 1$ the symmetric autarky Nash equilibrium profile*

$$(\underbrace{\widetilde{s}^I, \ldots, \widetilde{s}^I}_{n}; \underbrace{\widetilde{s}^{II}, \ldots, \widetilde{s}^{II}}_{n})$$

of the market game $\mathcal{E}(n)$ is not a μ-SESS for any $\mu \in [0, \mu_0)$.

Proof. We will demonstrate that, if a market is closed, there exists a coalition of agents (one agent per type) such that if the coalition opens the market, at least one member of the coalition can always become better off after trading than any non-deviant agent of his type. In this sense, the proof of the theorem is "destructive." We will describe a coalitional deviation which guarantees a higher payoff for *all* of the deviating agents.

Consider the market game $\mathcal{E}(n) = \langle 2n, (X, Y), (w, W), (U^1; U^2)\rangle$. Each agent obtains utility w^2W in the symmetric autarkic Nash equilibrium,

$$(\underbrace{\widetilde{s}^I, \ldots, \widetilde{s}^I}_{n}; \underbrace{\widetilde{s}^{II}, \ldots, \widetilde{s}^{II}}_{n}),$$

for any n.

Consider a coalition, C, consisting of exactly one agent per type (2 agents in total), and suppose that each agent in C deviates to the strategy prescribed by the full Nash equilibrium of the market game $\mathcal{E}(1)$, $(\widehat{s}^I; \widehat{s}^{II})$. Except for the two deviating agents, all other agents still obtain utility w^2W. On the other hand, each deviating agent receives utility $(w + \widehat{b}_y)^2(W - \widehat{b}_y) > w^2W$. Define

$$\mu_0 = (w + \widehat{b}_y)^2(W - \widehat{b}_y) - w^2W > 0.$$

The theorem now follows for any $\mu \in [0, \mu_0)$. □

Summarizing our findings, Theorems 1–3 demonstrate that for any $\epsilon, \mu > 0$, if the economy $\mathcal{E}$ is "large," the symmetric full Nash equilibrium profile of the market game associated with $\mathcal{E}$ is an ϵ-(μ-)SESS and the autarkic Nash equilibrium is not. Further as we showed in Section 2 for the 2×2 market game studied in the experiment, as $n \to \infty$, the full Nash equilibrium approaches the unique Walrasian competitive equilibrium for the economy.[15] Hence, SESS provides support for competitive outcomes in sufficiently large economies.[16] We next ask whether behavior in our experiment is consistent with the approximate-SESS concept.

4.3. Empirical support for SESS

In this section, we consider approximate SESS for the competitive equilibrium and the full NE using the parameterization of our experimental design. Our aim is to assess the extent to which

[15] In a related paper, Postlewaite and Schmeidler [15] assert that full Nash equilibrium allocations in a large enough economy are approximately competitive.

[16] As we mentioned earlier, these results will *not* hold in general if the economy is populated by a small number of agents. In that case, by having a non-negligible effect on prices, an agent deviating from the full Nash equilibrium allocation may be able to make himself better off relative to other agents of his type. Therefore, full Nash equilibria may not correspond to ϵ-*SESS* if agents have significant market power.

behavior in our experiment is consistent with the approximate-SESS concept. Consistent with Theorems 1–2, we focus on the case where the endowments are given by: $w = 10$, $W = 200$.[17]

Let us start with the competitive equilibrium. Theorem 1 states that for any positive number $\epsilon > 0$, there exists a $n_\epsilon > 1$ such that the competitive equilibrium profile of the market game $\mathcal{E}(n_\epsilon)$ is an ϵ-SESS. In our experiment we have $n = 2$ and 10, so we want to find $\epsilon_2 > 0$ and $\epsilon_{10} > 0$ such that the competitive equilibrium profile of the market game $\mathcal{E}(n_{\epsilon_i})$ is an ϵ_i-SESS, $i = 2, 10$. Our approach is to find a deviation (by one agent on each side of the market) that gives the highest utility in relative terms to one of the two deviating agents, given that all other $n - 2$ agents are playing according to the competitive equilibrium strategy. Without loss of generality we examine the maximal relative utility gain by a deviating type I agent where the deviating type II's deviation is maximally helpful for the deviating type I agent. Using Definition 4 we are looking for the smallest ϵ such that the competitive equilibrium is an ϵ-SESS. Formally, this can be found in the $n = 2$ case by solving the following maximization problem:

$$\max_{0 \leqslant d_y^I, d_x^{II} \leqslant W} \left[U^I\big((0, d_y^I); \gamma^1, \gamma^2\big) - U^I\big((0, b_y^I); \gamma^1, \gamma^2\big)\right]$$

$$= \max_{0 \leqslant d_y^I, d_x^{II} \leqslant W} \left[\left(w + d_y^I \frac{b_x^{II} + d_x^{II}}{b_y^I + d_y^I}\right)^2 \big(W - d_y^I\big) - \left(w + b_y^I \frac{b_x^{II} + d_x^{II}}{b_y^I + d_y^I}\right)^2 \big(W - b_y^I\big)\right],$$

where d_y^I is the deviation by the type I agent and d_x^{II} is the deviation by the type II agent. Numerically solving this maximization problem we find that $d_y^I = 130.7$ and $d_x^{II} = 200$. Substitution of these deviations into the utility function implies that the deviating type I agent's utility is higher by the amount 162.7 as compared with the utility of the other type I agent who played the competitive equilibrium strategy. More precisely, following the logic of Theorem 1 we can say that for any $\epsilon \geqslant \epsilon_2 = 162.7$, the competitive equilibrium is an ϵ-SESS for any $n \geqslant 2$.

Analogously, for the $n = 10$ case we find $d_y^I = 130.17$ and $d_x^{II} = 200$. Substitution of these deviations into the utility function implies that the deviating type I agent's utility is higher by the amount 6.7 as compared with the utility of the other 9 type I agents who played the competitive equilibrium strategy. More precisely, following the logic of Theorem 1 for any $\epsilon \geqslant \epsilon_{10} = 6.7$, the competitive equilibrium is an ϵ-SESS for any $n \geqslant 10$.

In order to understand the magnitude of these findings, we compare the additional utility amounts, 162.7 and 6.7, with an agent's utility in the competitive equilibrium, U^W. Since $U^W = 1{,}372{,}000$, the relative improvements, ϵ_2/U^W and ϵ_{10}/U^W are very small: just 0.012 and 0.0005 percent higher than U^W!

Let us next consider the full NE. Theorem 2 states that for any positive number $\mu > 0$, there exists an $n_\mu > 1$ such that the full NE of the market game $\mathcal{E}(n_\mu)$ is a μ-SESS. In our experiments, we have $n = 2$ and 10, so we want to find $\mu_2 > 0$ and $\mu_{10} > 0$ such that the full NE equilibrium profile of the market game $\mathcal{E}(n_{\mu_i})$ is a μ_i-SESS, $i = 2, 10$. We again look for a deviation (by one agent on each side of the market) that gives the highest utility in relative terms to one of those deviating agents, given that the other $n - 2$ agents are playing according to the full Nash equilibrium strategy. For the $n = 2$ case, we find $d_y^I = 125.8$ and $d_x^{II} = 200$. In this case, the deviating type I agent's utility is 384,357 higher than the other type I agent who played according to the full NE strategy. By the logic of Theorem 2, for any $\mu \geqslant \mu_2 = 384{,}357$, the full NE is an μ-SESS for any $n \geqslant 2$.

[17] The details of the calculations in this section can be found in the online supplementary materials, Appendix A: Numerical calculations.

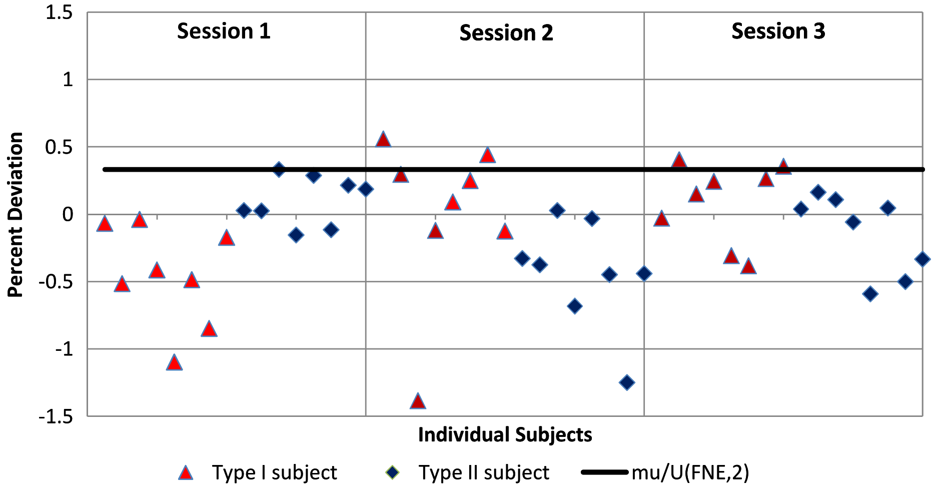

Fig. 10. Individual % deviation from full NE payoff in last 5 rounds of Treatment 1. Lower bound for μ-SESS, $\mu = 384{,}357$; $\mu/U^{NE}(2) = 0.332$.

Analogously, for the $n = 10$ case we find $d_y^I = 130.14$ and $d_x^{II} = 200$. In this case, the deviating type I agent's utility is 6099 higher than the other 9 type I agents who played according to the full Nash equilibrium strategy. According to Theorem 2, for any $\mu \geqslant \mu_{10} = 6099$, the full NE is a μ-SESS for any $n \geqslant 10$.

In order to understand the magnitude of these deviations for the full NE, we compare the additional utility amounts, 384,357 and 6099, with the agent's utility at the corresponding full NE, U^{NE}. Since $U^{NE}(n = 2) = 1{,}157{,}625$ and $U^{NE}(n = 10) = 1{,}366{,}875$ the relative improvements are $\mu_2/U^{NE}(2) = 33.2$ percent in the $n = 2$ case and $\mu_{10}/U^{NE}(10) = 0.45$ percent in the $n = 10$ case.

Recall that the values μ_2, μ_{10}, ϵ_2, and ϵ_{10} give rise to *lower* bounds for μ-SESS and ϵ-SESS starting from the full NE or the competitive equilibrium; the full NE and competitive equilibria can also be a μ-SESS or ϵ-SESS for any higher values of μ and ϵ. We now ask how well the experimental data compare with these lower bounds, focusing on the μ-SESS predictions for the full NE in Treatments 1 and 2 of our experiment where $w = 10$, $W = 200$.

We first calculated the average utility that each subject earned over the last 5 rounds of an experimental session—call this number for player i, $\bar{U}^i$. For each subject i, we then calculated $(\bar{U}^i - U^{NE}(n))/U^{NE}(n)$, where $U^{NE}(n)$ is the utility in the full Nash equilibrium for the relevant treatment condition, $n = 2$ or $n = 10$. The ratio gives the percent by which a player's utility exceeds the relevant full Nash equilibrium level and it is shown for each subject in Figs. 10–11 for Treatments 1 and 2 (please note the different vertical scale in Fig. 11 as compared with Fig. 10). The figures also indicate the relevant lower bound percent deviation, $\mu/U^{NE}(n)$.

We observe that the relevant lower bound percent deviation, $\mu/U^{NE}(n)$, characterizes 80–100% of individual payoff deviations in 5 of the 6 sessions of Treatments 1–2 suggesting that the minimal μ-SESS is close to characterizing the behavior of most subjects. The exception, Session 1 of Treatment 2, is discussed below.[18] Further, as $\mu_2/U^{NE}(2)$ is 73 times greater than $\mu_{10}/U^{NE}(10)$, we should expect to see greater variance in payoff differences in the $n = 2$ treatment as compared with the $n = 10$ treatments. And, indeed, the standard deviation in the payoff difference $\bar{U}^i - U^{NE}(2)$ for all subjects in Treatment 1 is 434,047, which is significantly greater

[18] Obviously with the choice of a higher μ we could explain 99% or more of the individual payoff deviations. Following the tradition in the literature, we have focused on the smallest possible μ that is consistent with our theory.

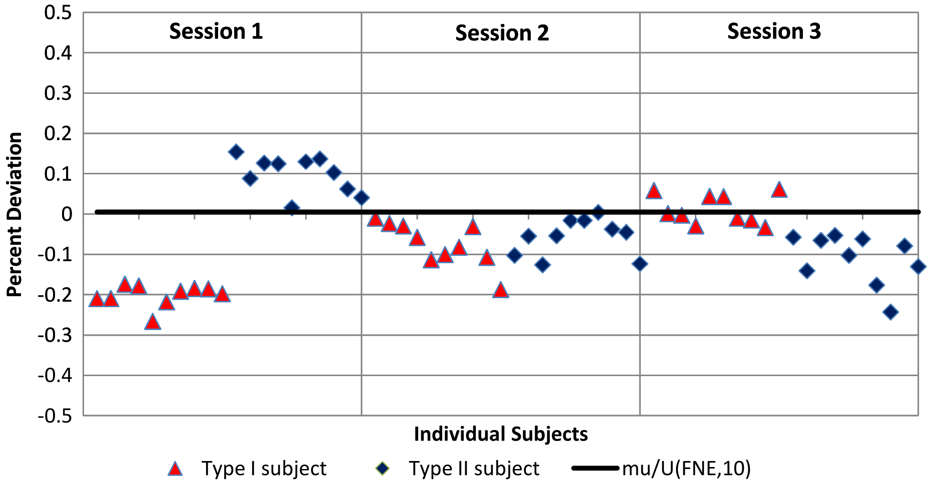

Fig. 11. Individual % deviation from full NE payoff in last 5 rounds of Treatment 2. Lower bound for μ-SESS, $\mu = 6099$; $\mu/U^{NE}(10) = 0.0045$.

than the standard deviation in the difference $\bar{U}^i - U^{NE}(10)$ for Treatment 2, 132,922. Using session-level averages, we can reject the null hypothesis of no difference in favor of the alternative that these standard deviations in payoff differences are significantly greater in Treatment 1 as compared with Treatment 2 (Mann–Whitney test, $p = 0.10$).

Consider the session where the variance in payoff deviations was the greatest, Session 1 of Treatment 2. Over the last 5 rounds, all 10 type I subjects are earning below full NE payoffs, on average, 20% less, while all 10 type II subjects are earning above full NE payoffs, on average, 10% more. While this is not typical of any other sessions of Treatments 1–2, it nicely illustrates a difficulty with core stability arguments that assume rationality of coalition members. The payoff evidence from this session suggests that players are exhibiting boundedly rational behavior consistent with our evolutionary solution concept. In particular, rational type I agents ought to have coordinated on a payoff-increasing strategy in which they bid less of their endowment of good y for good x.

Finally, and perhaps most significantly, Theorem 3 states that autarky in not a μ_0-SESS for small enough μ_0. In Treatments 1–2, the evidence (as discussed in Section 3) is clear that subjects are moving away from the strict autarky Nash equilibrium, in the direction of the competitive equilibrium, but they stop short, in a neighborhood of the relevant full NE, which, along with the competitive equilibrium is an approximate SESS for the economies of these two treatments. By contrast, in Treatment 3, the unique Nash equilibrium *is* autarky, so according to Theorem 3, the unique NE of Treatment 3 is *not* a μ_0-SESS. Our evolutionary approach thus predicts that in Treatment 3 the market should be opened with trades taking place. By contrast, the unique Nash equilibrium prediction calls for no trade to occur in this case. The small, but significantly greater than zero trade that takes place in Treatment 3 thus supports the prediction of our evolutionary solution concept. Further, by Theorem 1, the competitive equilibrium, while not a Nash equilibrium of the game we consider, *is* an approximate SESS. Trades should thus move in the direction of this competitive equilibrium and as discussed in Section 3, the evidence clearly supports this prediction.

5. Conclusion

Experimental results on double auctions (DA) give remarkably strong support for competitive equilibrium (WE) outcomes. This is true even when there is a small number of agents in both sides of the market.[19] Vernon Smith [23] referred to this property as a "scientific mystery." This raises the important question of whether competitive price formation crucially depends on the choice of market institution. Is this support particular to the way a DA "aggregates" agents' actions and information, or is it a property of WE itself? In the second case, the apparent emergence of WE should be shared by a *variety of market mechanisms*, in addition to the DA. However, little experimental work has been done to investigate the performance of different "general equilibrium" market structures. Our work constitutes a first effort to fill this gap.

Market games as in Shapley and Shubik [20] offer such mechanisms in that they involve the general equilibrium properties of the competitive paradigm while, at the same time, they are fully specified non-cooperative games, with sharp predictions as to how the number of agents affects equilibrium outcomes. These two features make them particularly suitable for studying whether competitive prices will emerge under different specifications of the underlying economic environment; i.e., the agents' preferences, and endowments and the agents' market power.

Clearly, for autarky not to be stable, one needs to require stability against coalitional deviations. It is worth emphasizing that our concept, SESS, is consistent with certain members of the deviating coalition being worse off than in the status quo. In future research, we would like to consider other solution concepts, such as the core. One important difference is that SESS follows the tradition in evolutionary game theory in emphasizing *relative* as opposed to absolute performance. We would also like to study experimentally other non-cooperative models of exchange, for example, models with sequential bargaining as in Gale [7].

Peck and Shell [13] and Ghosal and Morelli [8] study variations of market games in which competitive outcomes prevail even when the number of traders is small. Peck and Shell [13] consider the case where agents can temporarily sell more than their endowment with the proviso that they buy back the excess so that the aggregate endowment is not altered in equilibrium. They show that allowing such "wash trades" can enable agents to achieve Nash allocations that are very close to the competitive equilibrium. We do not allow wash trades in our framework. Ghosal and Morelli [8] consider a dynamic market game where agents can sequentially re-open markets and initiate further rounds of trading starting from the allocation achieved in the prior round. They show that this process yields Nash equilibrium allocations that come closer and closer to the competitive equilibrium allocation. By contrast, we consider a static market game. Nevertheless, it would be interesting to study whether our evolutionary story can be embedded in these alternative setups.

Finally, an important extension of our analysis concerns the relation between our static *SESS* concept and the asymptotically stable points of a suitably defined dynamic system describing the learning process. This extension is left to future research. Future work might also include studying economies with many goods, more general preferences, and multiple competitive equilibria.

Appendix A. Supplementary material

The online version of this article contains additional supplementary material.
Please visit doi:10.1016/j.jet.2011.05.013.

[19] Smith [22], Isaac and Plott [11] find little support for the notion that market power matters under the double auction (DA) mechanism. However, the structure of these DA experiments does not typically give significant market power to traders on either side of the market. When the DA experimental design is modified to allow unilateral market power by a couple of traders on one side of the market as in Holt et al. [9], prices do depart slightly from competitive levels in about half of all experimental sessions. As Holt [10, p. S122] concludes, "Even when asymmetric power is present, it is not always recognized and exercised, and, therefore, the DA is a surprisingly competitive institution."

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