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The Effect of Sequential Information Arrival on Asset Prices: An Experimental Study

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ABSTRACT

A complete understanding of security markets requires a simultaneous explanation of price behavior, trading volume, portfolio composition (ie., asset allocation), and bid-ask spreads. In this paper, these variables are observed in a controlled setting—a computerized double auction market, similar to NASDAQ. Our laboratory allows experimental control of information arrival—whether simultaneously or sequentially received, and whether homogeneous or heterogeneous. We compare the price, volume, and share allocations of three market equilibrium models: telepathic rational expectations, which assumes that traders can read each others minds (strong-form market efficiency); ordinary rational expectations, which assumes traders can use (some) market price information, (a type of semi-strong form efficiency); and private information, where traders use no market information. We conclude 1) that stronger-form market models predict equilibrium prices better than weaker-form models, 2) that there were fewer misallocation forecasts in simultaneous information arrival (*SIM*) environments, 3) that trading volume was significantly higher in *SIM* environments, 4) and that bid-ask spreads widen significantly when traders are exposed to price uncertainty resulting from information heterogeneity.

I. Introduction

IN MOST CONTEMPORARY ASSET markets, traders receive information during the trading day. Some receive a particular piece of information early, some later and perhaps some not at all. Such *sequential* information arrival has been studied theoretically by Copeland (1976) under the simplifying assumption that traders glean no new information from price changes. The main testable predictions from this line of research (see also Jennings, Fellingham, Starks, 1981) are that, as compared to simultaneous information arrival, trading volume will be increased, and there will be a positive correlation between volume and the absolute value of price changes when information arrives sequentially.

Although it has not been cast in a sequential information setting, the Grossman-Stiglitz (1976) theory of fully revealing asset prices makes a striking prediction: if each trader can invert the relationship between asset prices and private information then he will not have to personally receive a message to react optimally to it; asset prices will reveal it. In this case, prices will respond as if all information were publicly known [See also Grossman (1981)]. That is, the market exhibits strong-form informational efficiency (Fama, 1970), or full aggregation.

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Thus we have the following important empirical question: to what extent do asset markets with sequential information arrival exhibit strong-form efficiency? Econometric analysis of the usual field data is of quite limited value for this question: "event studies" can only examine public information, treated as if it were simultaneous, and "trading rule tests" are very indirect (since information sets are unobservable) and require a maintained assumption that some benchmark model (e.g., the CAPM) adequately characterizes market equilibrium. On the other hand, laboratory methods are well-suited to investigate this question, since private information can be controlled experimentally.

Simultaneous information arrival in asset markets has been studied experimentally by Plott and Sunder (1982), Forsythe, Palfrey and Plott (1982), and Friedman, Harrison and Salmon (1984), using oral Double Auction markets. For the most part, these studies confirmed Rational Expectations theories of asset prices, and when some traders ("insiders") had better information than others, prices tended to reveal this information. (This latter point was also explored, with mixed results, in Plott and Sunder [1983].)

The work we report below constitutes the first experimental study of sequential information in asset markets. We employ a computerized Double Auction market and allow the content as well as the timing of messages to vary across traders. In this doubly heterogeneous environment, as well as in simpler environments, we test a strong-form efficiency equilibrium (which we call telepathic rational expectations, *TRE*), against several alternatives, including a semi-strong form Rational Expectations equilibrium (ordinary rational expectations, *ORE*). Generally speaking, we find that the data is better explained by *TRE* than any of the (static) alternative models. However, we detect several empirical regularities that are inconsistent with *TRE* so we do not regard the question as closed; we suspect that an alternate dynamic theory may better account for the data. Other results that may be of special interest include:

(a) In our simpler environments, we are broadly able to replicate the results of previous experimental studies, despite our present use of a different laboratory setting (computerized *vs.* oral Double Auction).

(b) We exploit the real-time bid and ask data to measure price convergence when trading is thin and to test an information-based theory of the bid-ask spread. This theory is generally confirmed.

(c) We employ a fractional factorial design that allows paired comparisons across four treatment variables. The "nuisance" (or artifactual) variables generally have little effect. Less happily, the "focus" (or theoretically important) variables also often have insignificant effects.

(d) We systematically examine trading volume as a measure of market performance, and find to our surprise that it is lower in a sequential than in a simultaneous information environment.

We begin in Part 2 below with a description of our experimental design. Part 3 then lays out our formal theory and presents its testable predictions. Our data and statistical tests are presented in Part 4 and some conclusions are offered in Part 5.

II. Experimental Design

A. Basic Market Structures

Our six asset market experiments employed a new computerized Double Auction (*DA*) program implemented on an HP3000 system. During a trading period, each participant can enter a bid (an offer to buy an asset unit for a specified amount of cash) or an ask (a similar offer to sell) from his or her interactive terminal, can use the terminal to accept the current best (highest) bid or best (lowest) ask offered by his or her fellow traders and can cancel an outstanding bid or ask. The computer serves primarily as a communications and record-keeping device, and also enforces the rules. For instance, transaction requests that would result in a negative cash or asset position are not executed, but rather generate descriptive error messages. The only communication between market participants is via their computer terminals.

Our *DA* market is conceptually similar to Williams' (1980) original PLATO Double Auction. Perhaps the most salient differences are: (1) our screen displays are not modeled on the written record sheets employed in oral double auctions, but rather are designed to contain only information (including real-time messages) relevant to traders' immediate decisions, (2) "function" or "soft" keys are used to enter bids, etc., (3) confirmation is not required to execute a transaction, (4) traders can cancel existing bids or asks with a single keystroke, and (5) several bids (or asks) by different traders that are tied for best are queued in the order received, with the length of the queue being publicly displayed.[1]

We recruited our subjects by sending a flyer to all regular MBA students at the John E. Anderson Graduate School of Management, UCLA, and conducted the six experiments between October 1984 and October 1985. Each experiment involved nine subjects (henceforth referred to as "traders"), each initially endowed with $20 cash and three units ("shares") of the asset. We induced value for asset shares by specifying for each trader a state-contingent payoff for each share held at the end of the five-minute trading period. We randomly assigned three subjects ("clones") to each of three payoff schedules as specified in Table 1. For example, in Experiment #1 in Table 1, the three type I clones would receive $2.00, while type II clones would each receive $1.80, for each share held at the end of trade if state 1 eventuates. Thus we induced (information-contingent) potential gains from trade.[2]

B. Information Variables

Traders begin each trading period ("repetition" or "rep") with full knowledge of their own state-contingent payoffs, but uncertain about what payoffs other traders might have and about which state will be realized that period. Therefore,

[1] Further details about the HP3000 double auction, as well as copies of our instructions to the subjects are available from the authors upon request.

[2] These gains are intended as counterparts of trading incentives for participants in contemporary asset markets such as differing tax brackets, differing non-marketable assets held in portfolios, and differing risk preferences.

Table 1

Table of Subject State Contingent Payoffs (cents)

clone type	prob	Exp. #1 state 1	Exp. #1 state 2	Exp. #2 state 1	Exp. #2 state 2	Exp. #3 state 1	Exp. #3 state 2
I	50/50	200	40	180	40	205	105
II	50/50	180	100	170	100	175	115
III	50/50	120	120	160	150	145	125

clone type	prob	Exp. #4 state 1	Exp. #4 state 2	Exp. #5 state 1	Exp. #5 state 2	Exp. #6 state 1	Exp. #6 state 2
I	50/50	180	40	200	70	200	30
II	50/50	170	100	175	95	170	80
III	50/50	160	150	135	135	120	100

they are uncertain about the market value of shares. Each trader receives private information on his or her screen during the course of trading in the form of messages regarding the final state of nature. Equal (.50, .50) probabilities over the two states are induced for every trader before trading opened, and each trader receives a single conclusive message (e.g., state 2 with probability 1.00) before the end of the trading period. The exact timing of message receipt is unknown to traders. Other private information includes a continuously updated record of the traders' cash and share inventories, and state contingent wealth. Public information, which is displayed for all traders at all times, includes the best bid and best ask, the size of the queue at the best bid or ask, and a continuously updated sequence of transaction prices.

In this set of experiments, we are primarily interested in how the information arrival process and information content affect market performance. Specifically, we define the two treatment variables *INFOARRIVAL* and *INFOCONTENT*, each with two levels. For *INFOARRIVAL* = *SIM*, we have simultaneous messages (revealing the actual state for that trading day) sent to each trader at a predetermined time, *e.g.*, at $t = 120$ sec. Recall that the length of the trading day is $T = 300$ sec. For *INFOARRIVAL* = *SEQ*, we send messages to all clones of one type at $t = 70$ sec, to another type at $t = 140$ sec and to the final type at $t = 215$ sec. The ordering of the types in the sequence of messages is randomly selected.

For *INFOCONTENT* = *HOM*, a single ("homogeneous") random event determines the state for everyone; i.e., either everyone receives the "Good news" message that the high-payoff state (state 1) has occurred, or everyone receives a "Bad news" message (state 2). Alternatively, for *INFOCONTENT* = *HET*, a different random event determines the state for each different type;[3] *e.g.*, the heterogeneous information of "Good news" for clones of types I and III, but "Bad news" for type II.

Each complete experiment consisted of 16 market "trading" days, or repetitions. At the start of each experiment, subjects knew only that there would be

[3] Shareholder votes on corporate policy are rarely unanimous (proxy fights are an example). Clearly then, when a decision is announced, some traders may interpret the message as good news while others may interpret it as bad news.

between 5 and 20 trading days. It has become customary in market experiments for repetitions to be *stationary* in the sense that endowments and payoff schedules are the same in every repetition for each trader. We extend this definition of stationary repetition to our informational variables with the convention that the random process generating the information arrival sequence (under *SEQ*) and the process generating the realized states (under *HOM* or *HET*) must be the same in every repetition, but we do not require that the *realizations* of these random processes are the same across repetitions.

C. Nuisance Variables

Inevitably an experimental examination of a theory requires arbitrary choices for variables of little or no theoretical importance. For instance one always must pick specific numerical values for parameters; those already discussed include payoff schedules, length of trading period, information arrival times, number of clones, number of types, etc. Typically the experimenter holds most of those "nuisance" variables constant at conventional levels, under the tentative assumption that the specific choices either have no effect on experimental results or effects that are independent of the effects of the theoretically important ("focus") variables. Of course, this tentative assumption can always be tested, budget permitting, by systematically varying the levels of nuisance variables. Generally speaking, it is good practice to vary a few such variables as are thought *a priori* most likely to affect performance.

In our market experiments, we identified three such nuisances. First, Williams (1980) noted that trader experience seems especially important in computerized DA markets; inexperienced subjects often fail to perceive and take advantage of obvious profit opportunities. Thus we wanted subjects with previous experience in our HP3000 DA market, and this required two training experiments (E1 and E2). The remaining four experiments employed only experienced subjects (a pool of about 25 people).

From the standpoint of the equilibrium theories we examine here, learning is a second nuisance. Virtually all experimentalists employing stationary repetition note subjects' performance "improves" in later reps as subjects gain a better understanding of the experimental environment and form more consistent beliefs regarding the behavior of the other subjects. We have sought to enhance this learning process by displaying for each trader a transaction-by-transaction analysis of his or her performance on the previous rep before starting each new rep. Individual "interim screens" which appear for 90 seconds after each trading round, contain information on the profitability of each trade, total profit for the last round, and cumulative profit for all trading rounds to date. We thought it quite likely, then, that there will be systematic differences in market performance in later reps as compared to early reps in any experiment, and these effects might interact with the effects of our focus variables. Hence we defined the nuisance variable *LEARNOPS* (learning opportunities) with two levels: *EREPS* for reps 1–8 and *LREPS* for reps 9–16. Our main experiments E3–6 employed stationary replication in the traditional sense across all reps, but the stronger definition given above (involving informational variables) held only within a given level of *LEARNOPS* as detailed in the next subsection.

The final nuisance we consider is the payoff procedure, *PAYMETHOD*. In most previous asset market experiments, a subject would still earn substantial profits even if he refused to transact. Indeed, these "status-quo" profits, arising from the payoff on endowment,[4] typically exceeded the "trading profits" arising from purchasing shares at prices below the payoff value and/or selling shares at prices above the payoff value. Thus the "salient" (see Smith 1982) part of the reward structure was attenuated. In some experiments, we used the standard payoff procedure *SP* (total profits = status-quo profits + trading profits) but in other experiments we tested an alternative payoff procedure, *TPO* (Trading profits only, no status-quo profits paid). In the latter case, endowed shares as well as cash could be regarded as a loan. We adjusted payoff parameters so that under either procedure our subjects earned an average of approximately \$15–\$20 for a 2-hour session.

For completeness, we should note that there are many nuisance variables that can't be controlled experimentally (either held constant or systematically varied). These include observables such as temperature[5] as well as unobservables such as subjects' attention span. One eliminates the effects of such variables (at least in large samples)[6] by the standard device of randomizing the chosen levels of the controlled variables.

D. Fractional Factorial Design

To generate the strongest possible evidence from a given number of observations on a given set of independent variables, one requires that these variables be mutually orthogonal. Hence a principle of economical experimental design is that controlled variables be orthogonal within a set of experiments. The *factorial design*, in which each combination of the possible levels of controlled variables appears exactly once, is the most straight-forward approach. Experimental error then can be further reduced by replicating this design two or more times.

The problem is that even for a moderate number, n, of controlled variables, each of which assumes several levels (*e.g.*, k each), the required number of experimental runs quickly becomes very large (rk^n for r replications) and the design is then prohibitively expensive. Perhaps the most useful approach to reducing cost, particularly in "heuristic" or "boundary" experiments (see Smith 1982) in which one wishes to examine superficially a relatively large number of variables, is to run some balanced subset of the factorial design set of experiments. Latin squares and related designs are one classical method, but the method known as *fractional factorial design* (also classical) seems superior when the controlled variables all have two levels. In effect, one chooses the replication factor r to be a proper fraction, usually ½, ¼ or ⅛.

[4] Typically traders were paid for retained endowment of shares but not endowed cash, the latter being in effect an interest-free loan to relax liquidity constraints.

[5] The air conditioning in the new computer room failed during two of our experiments.

[6] That is, these uncontrollable nuisances are asymptotically orthogonal to the controlled variables given appropriate randomization procedures.

Table 2
Experimental Design

Run Number	Variable Number 1	2	3	4	Experiment Number
1	EREP	TPO	SIM	HOM	E5
2	LREP	TPO	SIM	HET	
3	EREP	SP	SIM	HET	E4
4	LREP	SP	SIM	HOM	
5	EREP	TPO	SEQ	HET	E3
6	LREP	TPO	SEQ	HOM	
7	EREP	SP	SEQ	HOM	E6
8	LREP	SP	SEQ	HET	

Table 2 shows our experimental design for Experiments 3–6, a half-fractional factorial design with four dichotomous (2-level) controlled variables. Comparisons within the E3–6 set allow us to test for systematic effects of our focus variables *INFOARRIVAL* and *INFOCONTENT* as well as the "nuisances" *LEARNOPS* and *PAYMETHOD* with experienced subjects. Note that *INFOCONTENT* (*HET vs. HOM*) is deliberately confounded with the three-way interaction of the other variables. Such confounding is unavoidable in fractional designs, and is justified by the implausibility that the three-way interaction would be significant.

III. Theoretical Predictions

In this section we review the relevant theory and derive testable predictions regarding prices and trading volume. The notation follows. Let Z be the set of states which depend on the type of information and the method of information arrival.

$$Z = \textit{set of states}\text{: } Z = \{G, B\} \quad \text{for} \quad HOM,$$
$$Z = \{GGG, GGB, \ldots, BBB\} \quad \text{for} \quad HET;$$

in the latter case $z = GBG$ means the "good" state (state 1), for type I and III traders, and the "bad" state (state 2) for type II. Next, let $\pi(z)$ = the unconditional probability of final equilibrium state z; note

$$\pi(z) = 1/8 \quad \text{for all} \quad z \text{ under } HET$$
$$= 1/2 \quad \text{for all} \quad z \text{ under } HOM.$$

Also let,

$$p_i(z) = \text{agent type } i\text{'s } \textit{payoff} \text{ in state } z, \quad i = \text{I, II and III}.$$

Information at t can be represented by an "event" $A_t \subset Z$. The interpretation is that the true state $z \in A_t$ with probability 1. Note that conditional probability for $z \in A_t$ is $\pi(z)/\pi(A_t)$, where $\pi(A_t) = \sum_{z \in A_t} \pi(z)$. The number of shares held by trader type i is denoted x_i; note $\sum x_i = 27$ in our experiments.

A. Theories of Equilibrium Prices and Allocations

Prior to the time when all traders have received information about the final state of nature, one can distinguish four different static equilibrium concepts. We shall call them clairvoyant rational expectations (also, final equilibrium), private information equilibrium, ordinary rational expectations, and telepathic rational expectations.

Final Equilibrium and Clairvoyant Rational Expectations

Suppose the true state is known to be z. Then the *final equilibrium* price is

$$p(FE, z) = \max\{p_i(z): \quad i = \text{I, II, III}\}. \tag{1}$$

The corresponding allocation of shares, x, is unique up to trader type, but not up to an individual trader: $x_i = 0$ unless $p_i(z) = p(z)$. This can be justified by supply-and-demand theory (demand is infinitely elastic at $p(FE)$ and supply is fixed), by sealed-bid auction theory (second-highest price among agents = highest price among types, since there are more than 2 clones), or by game-theoretic approaches to the DA (see Friedman, 1984). *FE* is the only applicable concept when every agent knows his true payoff.

Suppose that all traders behave as though they know the final true state, z, even before any of them has received information about what it will be. We will call this *clairvoyant expectations*, (*CRE*). The instant the market opens, prices would move to the *FE* level, the level attained when all traders are actually informed so that the final true state has been obtained with certainty.

Private Information Equilibrium

Suppose each trader pursues a buy-and-hold strategy based only on A_{it}, summarizing the messages he has personally received. Maintaining the assumption of risk-neutrality, then, his "reservation price" would be the conditional expected value of his payoff $E_{it}p_i(z) = \pi(A_{it})^{-1} \sum_{z\in A_{it}} \pi(z)p_i(z)$. The same arguments listed in the previous subsection then yield the *Private Information* (*PI*) equilibrium as

$$p_t(PI) = \max_i E_{it}p_i(z). \tag{2}$$

As usual, the corresponding allocation is characterized by $x_i = 0$ unless $E_{it}p_i(FE) = p_t(PI)$. Note that $p_t(PI) = p(FE, z)$ for the realized z, when each agent knows his true payoff, *e.g.*, in a sequential experiment for $t > 215$ sec.

Ordinary Rational Expectations Equilibrium

Suppose every agent knows the full structure of the asset market, including all contingent *FE* prices, $p(FE, z)$, and probabilities, $\pi(z)$. Then each can confidently pursue strategies more complicated than buy-and hold; indeed, each trader's (risk-neutral) reservation price becomes his conditional expected *FE* price, rather than his payoff. For example, suppose that he has received a message that his final payoff will be "good." If he knows that the clones of his type receive the same message at the same time, then he can reduce the set of final states (in the *HET* environment) from 8 to 4. He can rule out half of the possible final equilibria, and revise his trading strategy accordingly. If he conditions only on messages he personally has received, summarized by A_{it}, we have the *Ordinary Rational Expectations* equilibrium price

$$p_t(ORE) = \max_i E_{it}p(z), \quad \text{where } E_{it}p(z) = \pi(A_{it})^{-1} \sum_{z\in A_{it}} \pi(z)p(FE, z). \tag{3}$$

The numerical example in section IIIB will clarify *ORE* price predictions. *ORE* allocation predictions are that $x_i = 0$ unless $E_{it}p(z) = p_t(ORE)$. In other words, *ORE* rational traders will not hold the asset unless, given the information which they have received, they are among those holding the most optimistic expectations regarding the *FE* price.

Telepathic Rational Expectations

As noted in the introduction, information may also be transmitted by the market itself. An extreme case is that each trader has available the aggregate of *all* private information, *i.e.*, each trader conditions on $A_t = \cap_i A_{it}$. We refer to this as the *Telepathic Rational Expectations* equilibrium, denoted

$$p_t(TRE) = E_t p(z) = \pi(A_t)^{-1} \sum_{z\in A_t} \pi(z)p(FE, z). \tag{4}$$

Telepathic rational expectations is the same as strong form market efficiency in the Fama (1970) sense that equilibrium prices reflect all information, whether publicly available or not. This property is also known as strong-form aggregation and as fully revealing rational expectations.

Of course, $p_t(TRE)$ and $p_t(ORE)$ both coincide with $p(FE, z)$ at the realized outcome z for $t > 215$. Note that no allocation predictions are associated with *TRE* since everyone has the same reservation price.

Some Variants

TRE corresponds rather closely to the Grossman-Stiglitz idea that agents condition on (equilibrium) price as well as private information, but in a real-time trading process traders observe only transacted prices (and bids and asks) rather than exact equilibrium prices, so the correspondence is not exact. One could probably suggest several alternative formalizations of *RE* equilibrium in which observed prices play a more direct role. Probably the most natural approach would employ a *Bayesian Rational Expectations* (*BRE*) framework in which $\pi(z)$ and/or $p(FE, z)$ were replaced by estimates in the *ORE* or *TRE* formulas. For example, $\pi(z)$ could be based on the relative frequency of state realizations in previous reps.

Another possibility is that traders infer information from the price dynamics within each repetition as well as from *FE* prices. For lack of a better term we coin this idea *Dynamic Rational Expectations* (*DRE*). In order to contrast it with *ORE*, recall that *ORE* rational traders use two types of information to condition their behavior: 1) an assumed prior knowledge of all possible *FE* prices, $p(FE, z)$, and their probabilities, $\pi(z)$, and 2) private information about the state which will obtain for them ("good" or "bad"). In addition to these types of information, a *DRE* trader is able to infer some of the information received by other market participants by observing price changes and volume during a trading round. It lies beyond the scope of this paper to work out these ideas formally.

We note in passing that risk aversion would seem generally to lower equilibrium prices (other than *FE*) slightly, and to widen bid-ask spreads. Again, in the interest of simplicity, we will not try to compute the precise effects of risk aversion.[7]

B. A Numerical Example

The reader's intuition may be aided by considering a numerical example that illustrates most of the complications. Experiment 6 is characterized by sequential arrival of heterogeneous information so that 8 final equilibria are possible. As Table 1 indicates, type II traders know at the outset their payoffs will be either \$1.70 or \$0.80 with equal probability, for an expected value of \$1.25. Since type I and type III traders' expected values are lower (at \$1.15 and \$1.10 per share, respectively), the *PI* equilibrium price initially is \$1.25 and the associated allocation prediction is that type II clones will hold all 27 shares before the first information event.

By rep 12 however, type I traders will anticipate reselling their shares, if their "bad" state eventuates, at a price above their \$0.30 payoff. Table 3 shows that if type I traders receive bad news they can rule out states 1, 2, 3 and 5. Using true *FE* prices and true probabilities, they obtain an expected resale price of \$1.40 (= (.5)(\$1.70) + (.25)(\$1.20) + (.25)(\$1.00)) in the "Bad" state, and actual experience in previous "Bad" reps should lead to a similar estimate. Hence their initial (i.e., when $T < 70$ sec) *ORE* value of each share is \$1.70 = (.5)(\$2.00) + (.5)(\$1.40), the first term arising from buy-and-hold given the "good" state, and the latter from reselling given the "bad" state. Note that appropriate contingent selling strategies lead traders of other types to the same \$1.70 valuation, so allocations prior to information arrival are completely indeterminate for ORE. To summarize, in rep 12 for $T < 70$ sec., the PI prediction is p = \$1.25 with all shares held by trader type II. The ORE prediction is p = \$1.70 with no allocation prediction. And the TRE prediction is also p = \$1.70 with no allocation prediction.

In rep 12, type III traders are the first to receive messages. At $t = 70$ each is notified that the "bad" state had occurred, as indicated in Table 3. Hence each type III clone rules out states 1, 3, 4 and 7 then revises his ORE price[8] down to \$1.675(=(.5)(\$2.00) + (.25)(\$1.70) + (.25)(\$1.00)). Under the HET assumption that other types' news is uncorrelated with his own, this revision has no effect on the equilibrium price, thus the type III clones sell off their shares to the type

[7] The stakes involved in any particular transaction are small relative to trader's earnings for the experiment (much less relative to his total wealth) so *a priori* one would expect approximate risk neutrality.

[8] Of course, *PI* reservation prices would also fall for these clones, but that would have no effect on prices or allocations.

Table 3

A Numerical Example (Experiment 6, rep 12)

clone type	Payouts good	bad	avg.	Final Equilibrium States 1	2	3	4	5	6	7	8
I	2.00	.30	1.15	G	G	G	B	G	B	B	B
II	1.70	.80	1.25	G	G	B	G	B	G	B	B
III	1.20	1.00	1.10	G	B	G	G	B	B	G	B
		FE price:		2.00	2.00	2.00	1.70	2.00	1.70	1.20	1.00

I's and II's who still consider $1.70 the right price. The stronger TRE assumption that all agents learn about type III's "bad news" leads to an equal downward adjustment by all agents, and the TRE price prediction becomes $1.675. There is an even stronger RE equilibrium applicable here, namely clairvoyant rational expectations (CRE). If traders realize the state is actually BBB (*i.e.*, homogeneous) and aggregate information, then the CRE prediction $p(FE, BBB) = \$1.00$ would apply to the entire period after $t = 70$.

The second information event is "bad news" for type I clones at $t = 140$. At this point, type II agents still have the highest expected payoff, so the PI price remains at $1.25, with type II holding all shares. Under ORE assumptions the uninformed type II's would maintain a reservation price of $1.70, while previously informed type III's remain at $1.675, and newly informed type I's fall to the resale value $1.40 computed above. Hence $p_{140}(ORE) = \$1.70$ with all shares held by type II's.[9] Under *TRE* assumptions, all states but *BGB* and *BBB* (states 6 and 8) have been ruled out, so $p_{140}(TRE) = (.5)(\$1.70) + (.5)(\$1.00) = \$1.35$ at this point.

The last information event at $t = 215$ is a "bad news" message for type II clones. At this point all information is out and all equilibrium prices coincide with the *FE* price $p(BBB) = \$1.00$. The *FE* allocation prediction (all shares held by type III's) applies by the end of trade, $t = 300$.

Note that the *SIM* experiments have only two subperiods: before information arrival (*e.g.*, $0 \leq t \leq 140$) and after information arrival ($140 < t \leq 300$). Thus the first and last of the four information subperiods under *SEQ* apply to *SIM* experiments, but the middle two *SEQ* subperiods have no *SIM* counterpart. Also, note that *ORE* and *TRE* coincide in the first subperiod, so they can be denoted simply by *RE*, but in the second and third subperiods of the *SEQ* experiments they give different predictions. It is the sequential information arrival experiments which allow us to discriminate between the *TRE* and *ORE* theories, something not possible in *SIM* experiments. This is a major motivating factor for this set of experiments.

C. *Theoretical Trading Volume*

Associated with each equilibrium concept is a sequence of allocation forecasts. All forecasts coincide for $t = 0$ because initial allocations are pre-specified, and

[9] Note that this *ORE* concept suggests a phenomenon akin to the "winner's curse" of the common-value sealed-bid auction: by conditioning only on their own private information, and not taking into account that they are the "highest bidders", the type II clones in this case will suffer losses.

for $t = T = 300$ because all coincide with the *FE* forecast for the realized outcome, z. However, the forecasts generally differ at the information arrival times (*e.g.*, $t_1 = 70$, $t_2 = 140$, $t_3 = 215$ under *SEQ*), and consequently they generally differ as to the minimum volume of trade required to support the equilibrium. For example, the discussion in the preceding subsection notes that in Rep 12 of E6, the *ORE* prediction is that type III clones will sell between $t = 70$ and $t = 140$ generating a trading volume of 9 shares. The expected shareholdings for clone types I and II are 13.5 shares each. Next, type I clones will sell between $t = 140$ and $t = 215$ generating an expected volume of 13.5 shares. At this time all shares are held by type II. Between $t = 215$ and $t = 300$ all shares are repurchased by type III thereby generating another 27 shares of volume. The *theoretical trade volume* prediction in this case is $TV_{12}(ORE) = 49.5$.

In general one can define TV for a given equilibrium concept in terms of the allocation forecasts $X = \{x_i(t_j)\}$ at information times $0 = t_0 < t_1 < \cdots < t_k = T$, as follows. Since a single trade changes the allocations of two traders, we obtain the volume formula $V(t_2, t_1) = \frac{1}{2} \sum_{i=1}^{III} \min\{|x_i(t_2) - x_i(t_1)|: x \text{ an Eq. Alloc.}\}$; note that V generally depends on the realization of z and the sequence of messages as well as the specific equilibrium concept used. Then the *theoretical trade volume* prediction for the given repetition r is

$$TV_r = \max[\textstyle\sum_{i=1}^{k} V(t_i, t_{i-1}): 0 \le t_0 \le t_1 \le \cdots \le t_k \le T]. \tag{5}$$

Table A2 in Appendix 1 provides the theoretical trading volume predictions for the *PI* and *ORE* hypotheses. Note that *ORE* always (and *PI* usually) predicts lower volume in *SIM* experiments (4 and 5) than in *SEQ* experiments (3 and 6). *TRE* and *CRE* always predict $TV_r = 18$ and are not shown in the table.

Actual trading volume could be higher or lower simply because subjects may make mistakes. However, systematic departures from the theoretical predictions may indicate gaps in the theory. For example, volume much higher than predicted may indicate scalping behavior which accelerates convergence of prices to their equilibrium levels. Alternately, volume much lower than predicted may indicate a form of risk averse behavior.

D. Bid-Ask Spreads

An essential feature of our experiment is that it allows the observation of price behavior in markets with heterogeneous information, both in the sense of differing message timing in a *SEQ* environment (*i.e.*, when some traders are informed and others are not, they possess heterogeneous information) and also in the sense of differing message content in a *HET* environment.

Copeland and Galai (1983) and Glosten and Milgrom (1985) have modeled bid-ask price behavior in markets where participants are heterogeneously informed. Risk neutral traders set optimal bid-ask spreads by balancing expected gains from market participants who are equally well informed as they (liquidity traders), with expected losses to participants who are better informed (informed traders). The bid-ask spread in these models increases with greater price uncertainty and as the percentage of informed traders increases. In our computerized double auction market, we should therefore expect to see greater bid-ask spreads before the first information event due to price uncertainty and lower bid-ask spreads after the last information event when price uncertainty is resolved. Also, we should observe widening bid-ask spreads immediately after each information event because the percentage of informed traders increases.

Finally, apart from their hypothesized behavior vis-a-vis information heterogeneity, bid-ask data are interesting in their own right. For example, they allow tests of equilibrium prices even when actual transactions are thin or nonexistent.

E. Predictions

Each applicable equilibrium concept provides a forecast of transaction prices and allocations. One then can use statistics such as mean squared errors, MSE, to summarize the quality of each forecast in each subperiod, and then can make overall comparisons. Theory and experimental tradition suggest the following testable predictions:

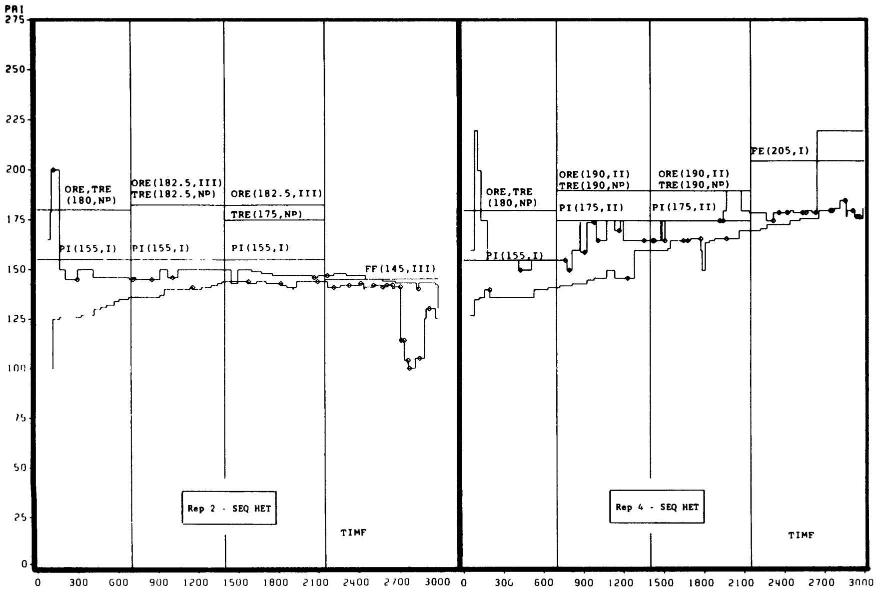
1. Stronger-form equilibria predict prices, allocations and trading volume better in later reps than early ones. Specifically, *ORE* and *TRE* will eventually outperform *PI* in pre-information subperiods, and *TRE* will eventually outperform *ORE* in middle subperiods of *SEQ* reps.
2. The nuisance variable *PAYMETHOD* will have no significant effect on observable market behavior. The nuisance variable *LEARNOPS* will have minor effects: prices and allocations will display less variability and will be closer to *FE* in final subperiods in *LREPS* than in *EREPS*.
3. The focus variables *INFOARRIVAL* and *INFOCONTENT* will have similar effects on market behavior: prices and allocations will be closer to *FE* predictions in the final subperiod and *RE* predictions in the initial subperiod in the simpler environments (*SIM* and *HOM*) than in the more informationally heterogeneous environments (*SEQ* and *HET*).
4. Trading volume will be higher with sequential than with simultaneous information arrival.
5. Bid-ask spreads will be wider in the presence of greater price uncertainty, *i.e.*, in earlier subperiods than in later subperiods, and immediately after information events than immediately before.

IV. Results

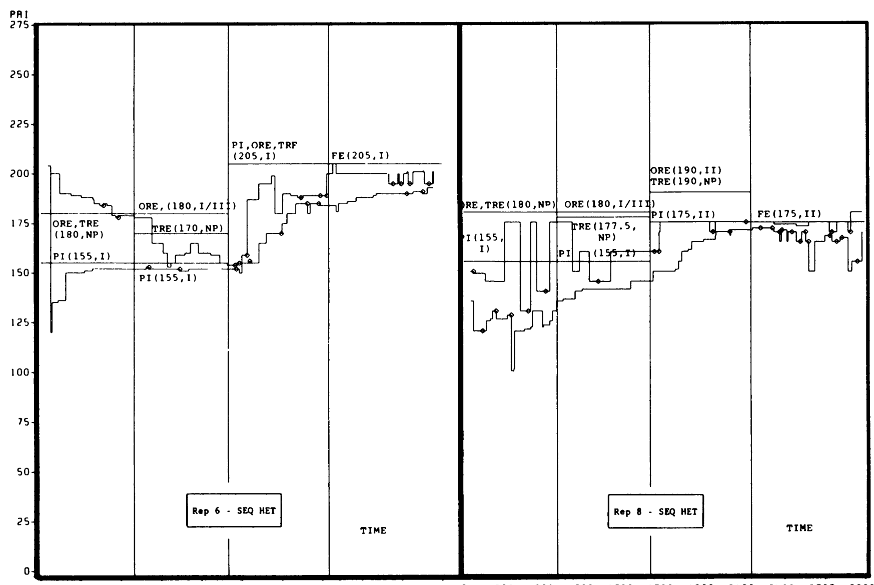
We present our results in five sections: treatment variables, bid-ask spreads, allocation data, volume, and price convergence. We employ a variety of statistical tests, both parametric and nonparametric in each section. First, however, a visual presentation of raw data is in order.

Figures 1–4 are graphs of bid-ask spreads and transactions (in cents) as a function of time (in tenths of a second) for even-numbered reps in experiments E3–6.[10] Vertical lines indicate information events with the specific information listed in the *NEWS* row below. For example, the first information event in

[10] Only even-numbered reps are reported in order to conserve space. Results for odd-numbered reps are available upon request.

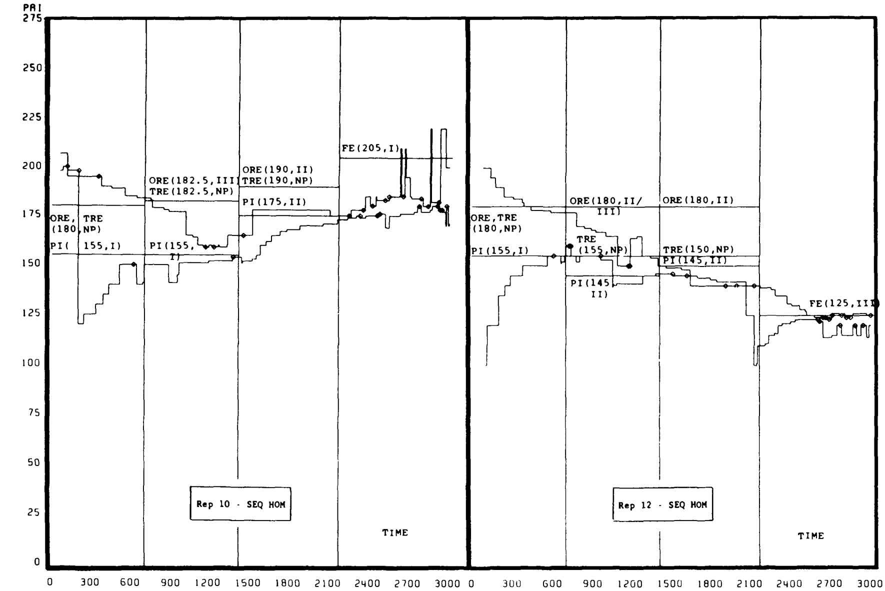


NEWS		IIIG	IIB	IB		IIG	IIIG	IG	
VOLUME/stage		3	3	4	16	3	8	9	12
	I	4+4+3 = 11	4+4+3 = 11	4+4+3 = 11	2+3+4 = 9	3+3+3 = 9	3+2+3 = 8	1+2+3 = 6	2+7+6 = 15
share	II	3+1+4 = 8	6+0+4 = 10	6+0+2 = 8	0+2+1 = 3	2+3+2 = 7	3+10+2 = 15	9+10+2 = 21	5+6+0 = 11
holdings	III	3+3+2 = 8	3+3+0 = 6	3+3+2 = 8	9+6+0 = 15	3+3+5 = 11	1+1+2 = 4	0+0+0 = 0	1+0+0 = 1
	PI	16	16	16	12	18	12	6	12
misalloc	ORE	NP	21	19	12	NP	12	6	12

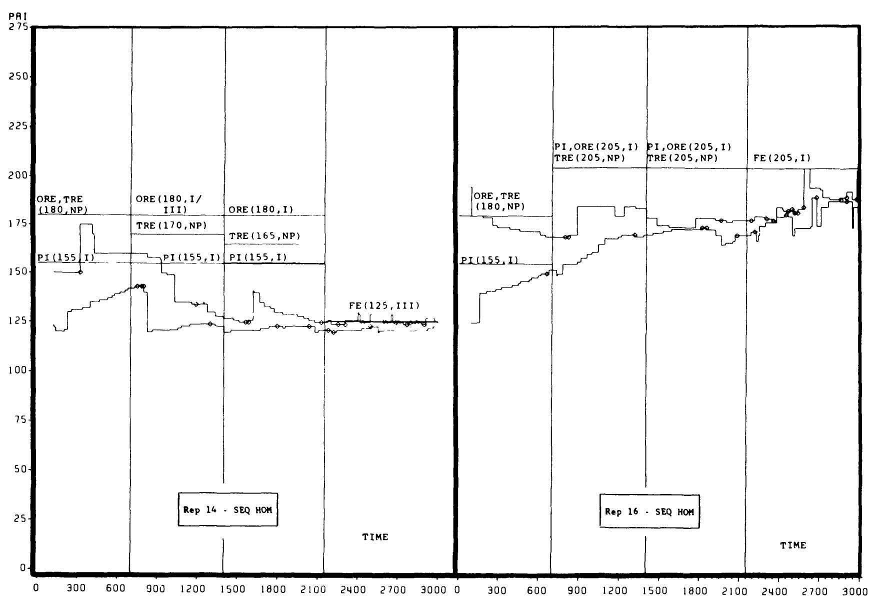


NEWS		IIB	IG	IIIB		IIIB	IIG	IB	
VOLUME/stage		2	2	11	6	6	1	5	14
	I	3+4+3 = 10	3+4+3 = 10	6+9+4 = 19	6+13+7 = 26	3+4+1 = 8	2+5+1 = 8	2+6+1 = 9	0+0+0 = 0
share	II	4+3+3 = 10	4+2+2 = 8	0+0+0 = 0	0+0+0 = 0	5+4+3 = 12	5+4+3 = 12	5+6+4 = 15	11+10+6 = 27
holdings	III	3+3+1 = 7	4+3+2 = 9	5+2+1 = 8	0+0+1 = 1	3+3+1 = 7	3+3+1 = 7	1+2+0 = 3	0+0+0 = 0
	PI	17	17	8	1	19	19	12	0
misalloc	ORE	NP	8	8	1	NP	7	12	0

Figure 1. Trading Data, Experiment 3, SEQ:HET

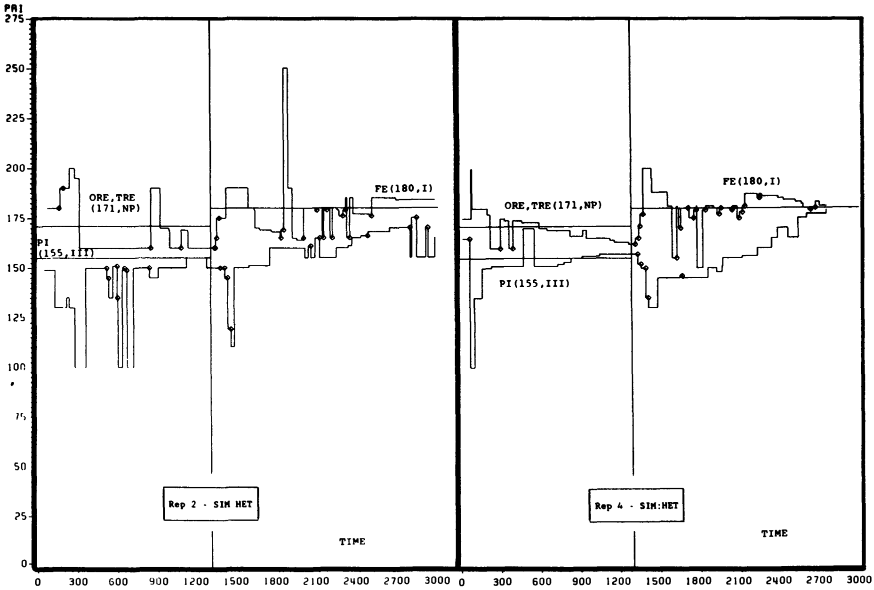


NEWS		IIIG	IIG	IG		IB	IIIB	IIB	
VOLUME/stage		4	3	1	17	1	5	5	13
	I	3+3+3 = 9	3+3+3 = 9	3+3+3 = 9	8+10+5 = 23	3+3+3 = 9	3+2+2 = 7	1+1+2 = 4	0+0+0 = 0
share	II	3+2+3 = 9	4+4+3 = 11	4+5+3 = 12	3+1+0 = 4	3+2+3 = 8	3+1+3 = 7	3+4+3 = 10	0+1+0 = 1
holdings	III	4+3+3 = 10	4+3+0 = 7	3+3+0 = 6	0+0+0 = 0	4+3+3 = 10	6+3+4 = 13	6+2+5 = 13	14+6+6 = 26
	PI	18	18	15	4	18	20	17	1
misalloc	ORE	NP	20	15	4	NP	7	17	1

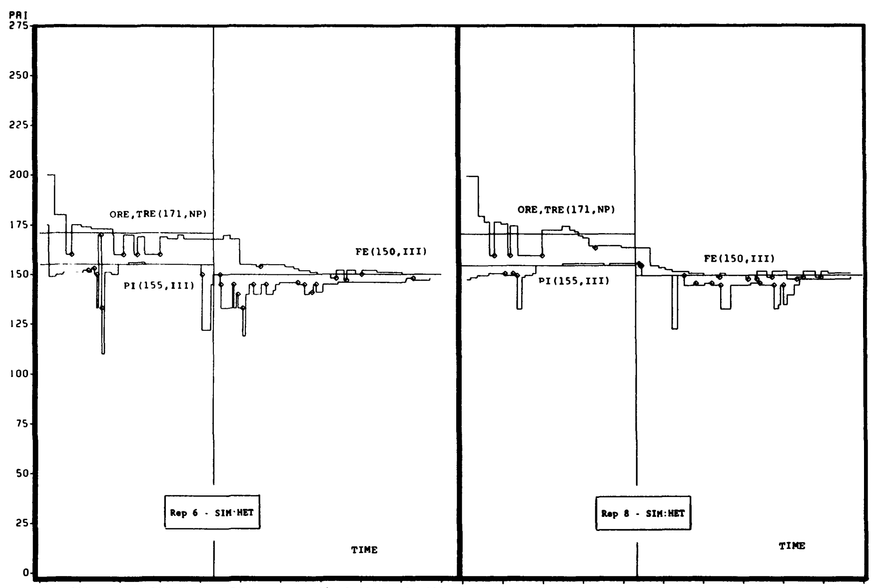


NEWS		IIIB	IIB	IB		IG	IIG	IIIG	
VOLUME/stage		1	5	5	13	1	3	4	15
	I	4+3+3 = 10	4+3+3 = 10	4+2+3 = 9	0+0+0 = 0	3+3+3 = 9	3+3+3 = 9	3+4+3 = 10	9+11+5 = 25
share	II	3+3+3 = 9	2+3+1 = 6	1+0+1 = 2	0+1+0 = 1	3+2+3 = 8	4+1+4 = 9	5+0+4 = 9	0+0+0 = 0
holdings	III	3+3+2 = 8	5+3+3 = 11	8+3+5 = 16	17+4+5 = 26	4+3+3 = 10	4+3+2 = 9	4+1+3 = 8	0+0+2 = 2
	PI	17	17	18	1	18	18	17	2
misalloc	ORE	NP	6	18	1	NP	18	17	2

Figure 1 *continued.* Trading Data, Experiment 3, SEQ:HOM

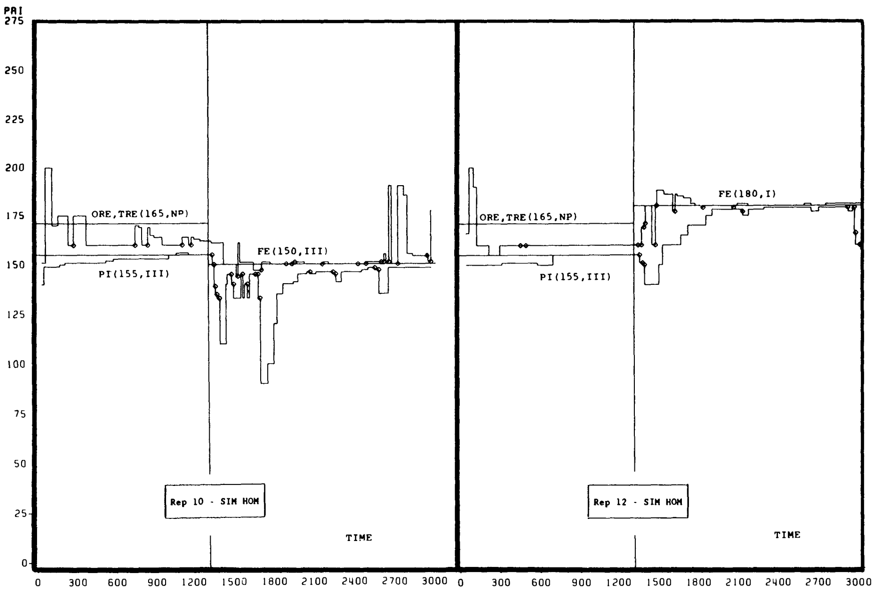


NEWS		IG, IIB, IIIG		IG, IIB, IIIB	
VOLUME/stage		11	25	3	25
	I	5+0+0 = 5	11+9+4 = 24	3+2+2 = 7	10+9+8 = 27
share	II	2+1+2 = 5	0+0+0 = 0	3+3+4 = 10	0+0+0 = 0
holdings	III	8+4+5 = 17	3+0+0 = 3	4+5+1 = 10	0+0+0 = 0
misallo-	PI	10	3	17	0
cations	ORE	NP	3	NP	0

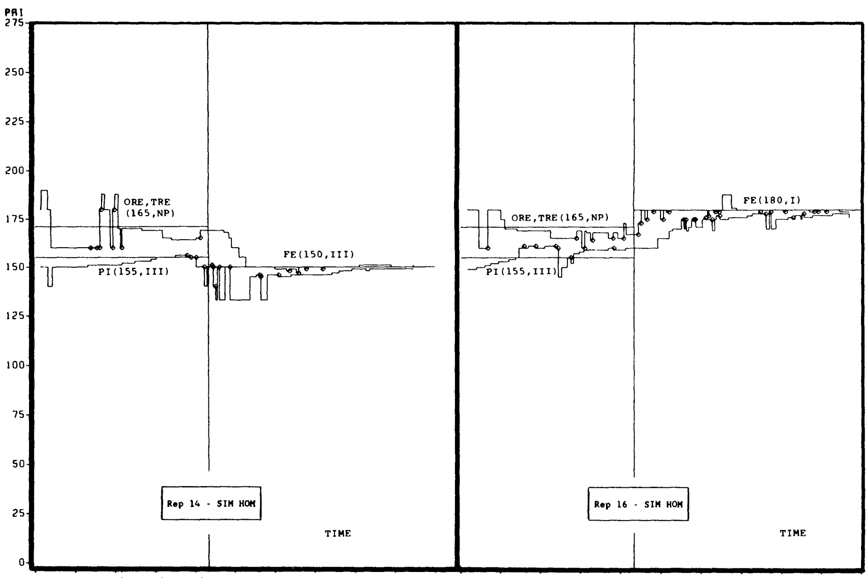


NEWS		IB, IIB, IIIB		IB, IIB, IIIB	
VOLUME/stage		10	16	7	17
	I	0+0+3 = 3	0+0+0 = 0	3+0+0 = 3	0+0+0 = 0
share	II	3+3+5 = 11	0+0+0 = 0	3+3+4 = 10	0+0+0 = 0
holdings	III	6+6+1 = 13	10+8+7 = 27	6+5+3 = 14	12+5+10 = 27
misallo-	PI	14	0	13	0
cations	ORE	NP	0	NP	0

Figure 2. Trading Data, Experiment 4, SIM:HET

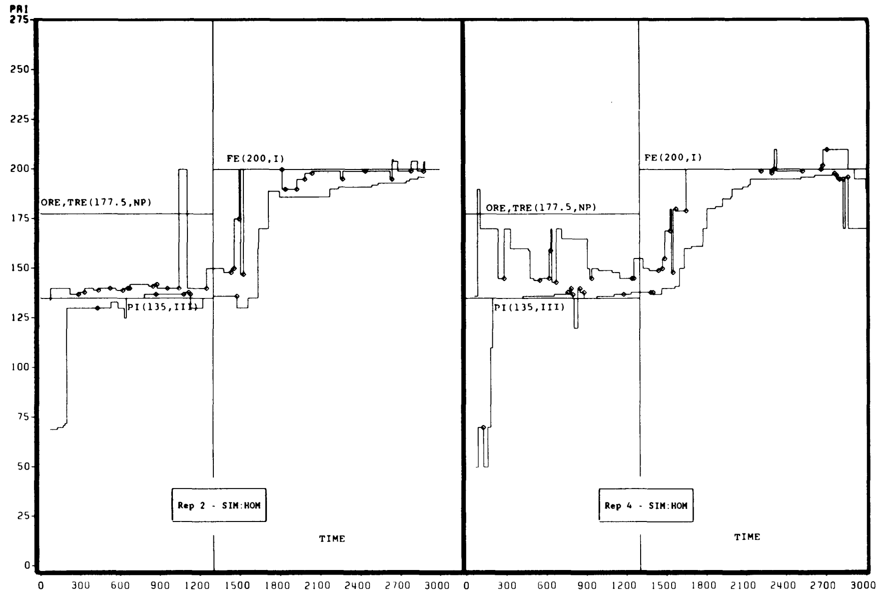


		Rep 10		Rep 12	
NEWS		IB,IIB,IIIB		IG,IIG,IIIG	
VOLUME/stage		5	31	2	17
share holdings	I	1+3+3 = 7	0+0+0 = 0	5+3+3 = 11	9+8+7 = 24
	II	3+3+7 = 13	0+0+0 = 0	3+4+2 = 9	1+1+0 = 2
	III	3+4+0 = 7	9+16+2 = 27	3+2+2 = 7	0+1+0 = 1
misallo-cations	PI	20	0	20	3
	ORE	NP	0	NP	3

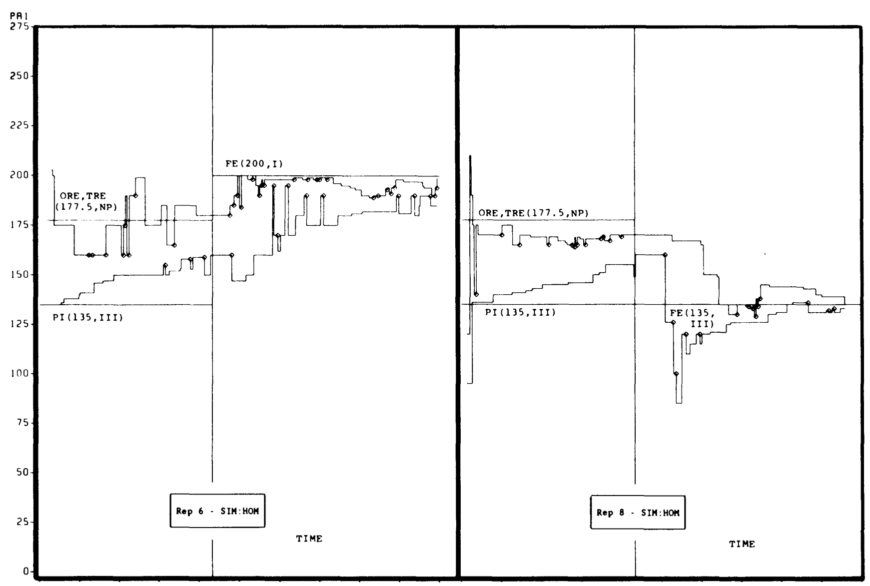


		Rep 14		Rep 16	
NEWS		IB,IIB,IIIB		IG,IIG,IIIG	
VOLUME/stage		12	12	12	25
share holdings	I	5+0+3 = 8	0+0+0 = 0	2+0+2 = 4	9+13+4 = 26
	II	3+2+1 = 6	0+0+0 = 0	3+3+7 = 13	0+0+1 = 1
	III	3+9+1 = 13	10+9+8 = 27	3+7+0 = 10	0+0+0 = 0
misallo-cations	PI	14	0	17	1
	ORE	NP	0	NP	1

Figure 2 *continued.* Trading Data, Experiment 4, SIM:HOM

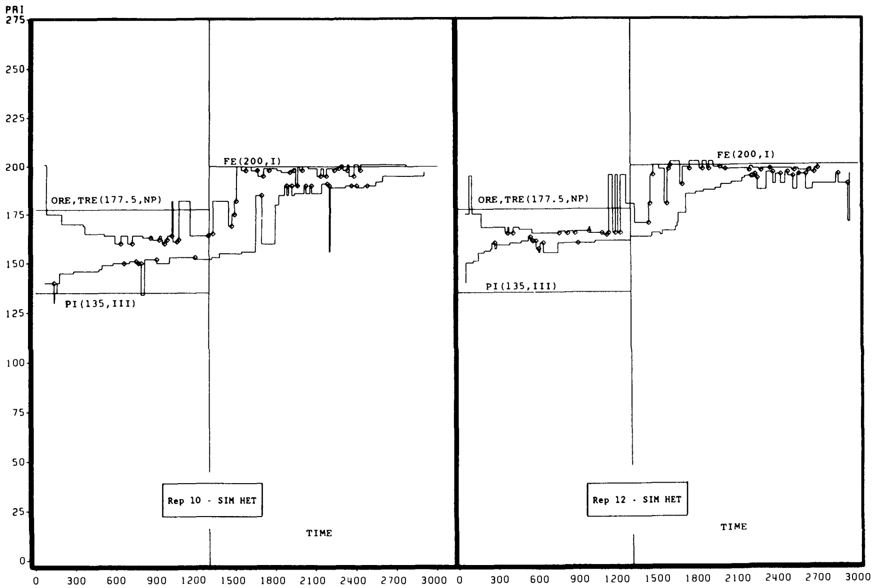


		Rep 2		Rep 4	
NEWS		IG,IIG,IIIG		IG,IIG,IIIG	
VOLUME/stage		17	15	15	21
share holdings	I	3+1+4 = 8	11+3+9 = 23	7+3+3 = 13	15+3+3 = 21
	II	4+10+1 = 15	0+1+0 = 1	1+6+4 = 11	1+4+1 = 6
	III	3+0+1 = 4	3+0+0 = 3	1+2+0 = 3	0+0+0 = 0
misallocations	PI	23	4	24	6
	ORE	NP	4	NP	6

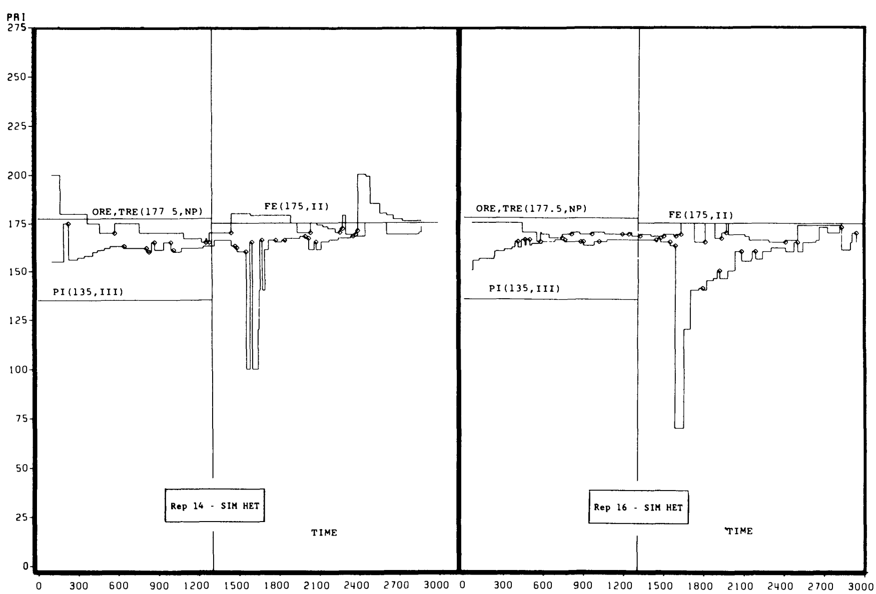


		Rep 6		Rep 8	
NEWS		IG,IIG,IIIG		IB,IIB,IIIB	
VOLUME/stage		11	29	12	14
share holdings	I	0+0+3 = 3	12+7+6 = 25	0+3+0 = 3	0+0+2 = 2
	II	4+2+2 = 8	1+0+1 = 2	2+0+3 = 5	0+0+0 = 0
	III	3+7+6 = 16	0+0+0 = 0	2+3+14 = 19	3+10+12 = 25
misallocations	PI	11	2	8	2
	ORE	NP	2	NP	2

Figure 3. Trading Data, Experiment 5, SIM:HOM

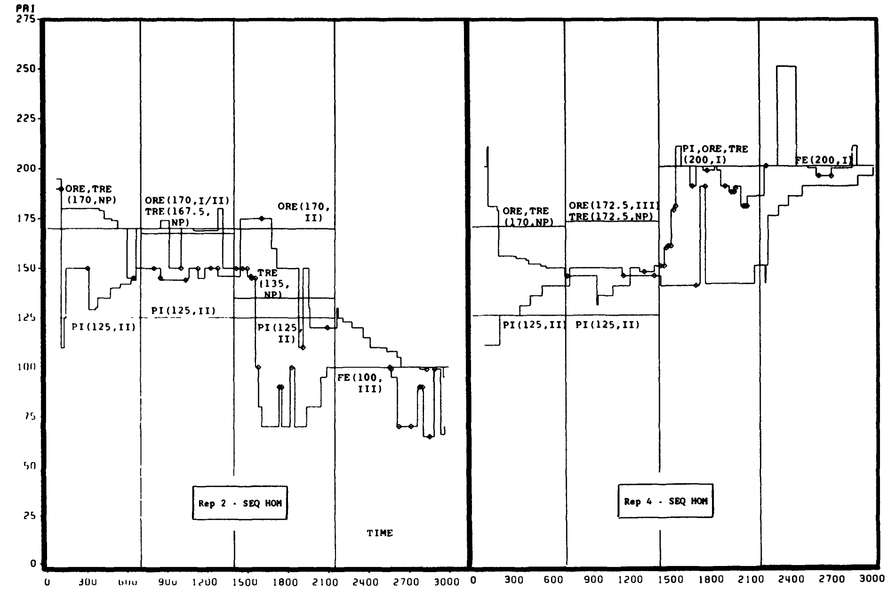


		Rep 10		Rep 12	
NEWS		IG,IIB,IIIC		IG,IIC,IIIB	
VOLUME/stage		16	30	18	30
	I	0+2+2 = 4	9+8+10 = 27	1+1+0 = 2	12+10+5 = 27
share	II	2+2+3 = 7	0+0+0 = 0	3+3+3 = 9	0+0+0 = 0
holdings	III	2+5+9 = 16	0+0+0 = 0	3+5+8 = 16	0+0+0 = 0
misallo-	PI	11	0	11	0
cations	ORE	NP	0	NP	0

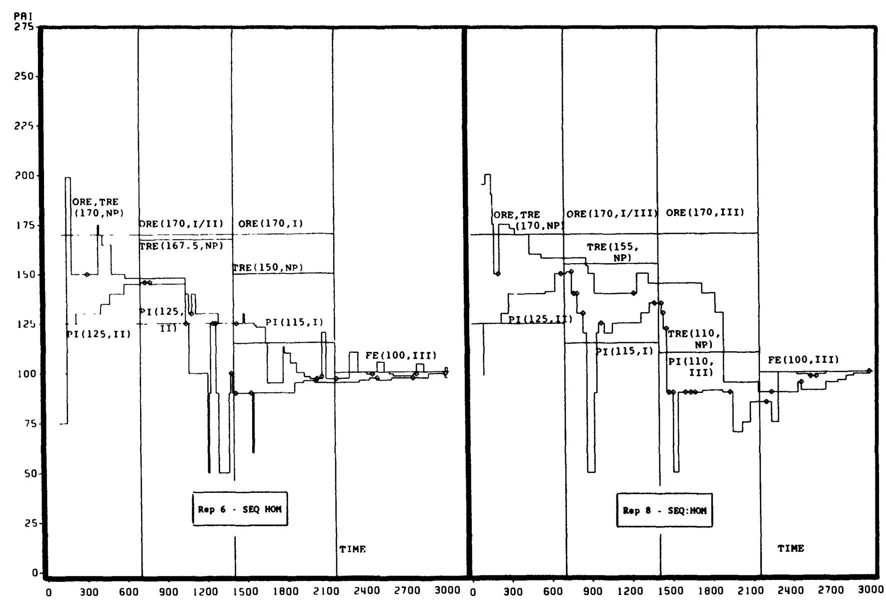


		Rep 14		Rep 16	
NEWS		IB,IIC,IIIB		IB,IIC,IIIB	
VOLUME/stage		10	17	13	19
	I	1+0+4 = 5	0+0+0 = 0	6+0+2 = 8	0+0+0 = 0
share	II	3+4+3 = 11	2+12+11 = 25	2+5+3 = 10	3+13+9 = 25
holdings	III	5+5+2 = 12	2+0+0 = 2	4+5+0 = 9	2+0+0 = 2
misallo-	PI	15	2	18	2
cations	ORE	NP	2	NP	2

Figure 3 ***continued.*** Trading Data, Experiment 5, SIM:HET

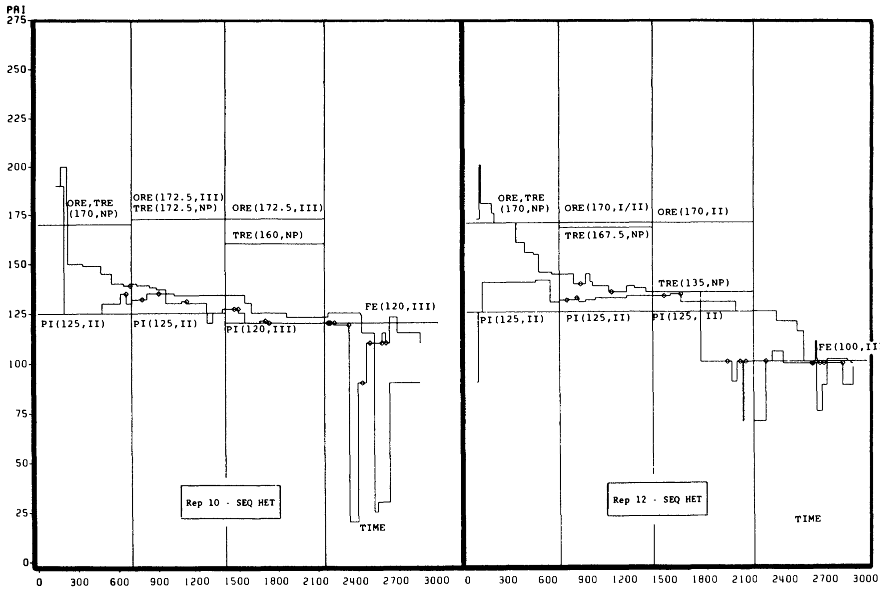


NEWS		IIIB	IB	IIB		IIG	IG	IIIG	
VOLUME/stage		3	7	13	9	0	4	18	3
	I	3+5+4 = 12	4+9+4 = 17	0+5+2 = 7	0+0+0 = 0	3+3+3 = 9	1+3+6 = 10	7+8+9 = 24	7+9+10 = 26
share	II	3+2+2 = 7	2+2+3 = 7	4+4+5 = 13	3+7+4 = 14	3+3+3 = 9	2+3+3 = 8	0+0+2 = 2	0+0+1 = 1
holdings	III	3+3+2 = 8	0+3+0 = 3	2+2+3 = 7	3+3+7 = 13	3+3+3 = 9	3+3+3 = 9	0+1+0 = 1	0+0+0 = 0
	PI	20	20	14	14	18	19	3	1
misalloc	ORE	NP	3	14	14	NP	18	3	1

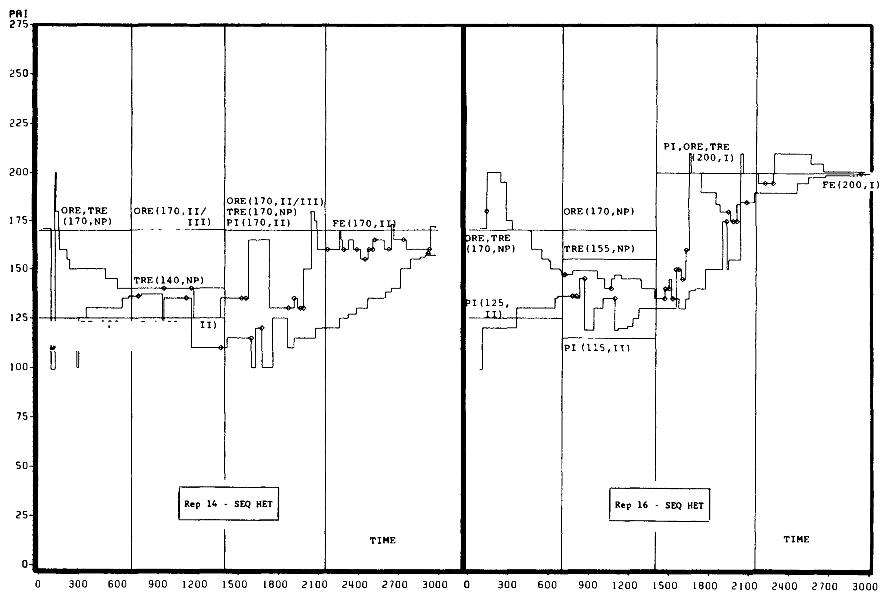


NEWS		IIIB	IIB	IB		IIB	IB	IIIB	
VOLUME/stage		2	7	6	6	2	7	9	6
	I	3+3+3 = 9	2+0+1 = 3	0+0+0 = 0	0+0+0 = 0	4+3+3 = 10	6+3+3 = 12	2+3+1 = 6	1+1+0 = 2
share	II	3+3+3 = 9	4+3+4 = 11	5+1+3 = 9	4+0+0 = 4	3+2+3 = 8	1+2+0 = 3	1+1+0 = 3	1+0+0 = 1
holdings	III	2+3+4 = 9	5+4+4 = 13	6+6+6 = 18	5+8+10 = 23	3+2+4 = 9	4+3+5 = 12	6+6+7 = 19	5+7+12 = 24
	PI	18	16	27	4	19	15	8	3
misalloc	ORE	NP	13	27	4	NP	3	8	3

Figure 4. Trading Data, Experiment 6, SEQ:HOM



NEWS		IIIG	IIB	IB		IIIB	IB	IIB	
VOLUME/stage		2	3	4	9	0	4	5	7
	I	4+3+3 = 10	6+4+3 = 13	8+5+3 = 16	6+3+0 = 9	3+3+3 = 9	4+3+3 = 10	3+2+3 = 8	2+1+1 = 4
share	II	4+2+3 = 9	4+2+3 = 9	2+0+3 = 5	2+1+2 = 5	3+3+3 = 9	4+3+3 = 10	4+2+3 = 9	3+1+2 = 6
holdings	III	3+3+2 = 8	3+2+0 = 5	4+2+0 = 6	4+6+3 = 13	3+3+3 = 9	3+2+2 = 7	3+3+4 = 10	5+4+8 = 17
	PI	18	18	21	14	18	17	18	10
misalloc	ORE	NP	22	21	14	NP	7	18	10



NEWS		IB	IIG	IIIG		IIB	IG	IIIB	
VOLUME/stage		1	5	8	11	1	6	13	3
	I	3+4+3 = 10	2+5+2 = 9	0+0+2 = 2	0+0+0 = 0	3+3+3 = 9	3+5+4 = 12	5+14+5 = 24	5+15+7 = 27
share	II	3+3+3 = 9	3+3+3 = 9	7+5+4 = 16	16+6+5 = 27	3+3+4 = 10	1+0+4 = 5	0+0+0 = 0	0+0+0 = 0
holdings	III	3+2+2 = 8	3+1+5 = 9	2+1+6 = 9	0+0+0 = 0	2+3+3 = 8	2+3+5 = 10	0+1+2 = 3	0+0+0 = 0
	PI	18	18	11	0	17	15	3	0
misalloc	ORE	NP	9	2	0	NP	5	3	0

Figure 4 *continued.* Trading Data, Experiment 6, SEQ:HET

Experiment 3, rep 2 (Figure 1) was III*G* (good news received by clone type III) at time $t = 70$. The second news event was II*B* at $t = 140$ and the third was I*B* at $t = 215$. Horizontal lines indicate the relevant equilibrium price and allocation forecasts. For example (using Exp 3, rep 2 again) for $0 \leq t < 70$ the *ORE* and *TRE* price forecasts are $p = 180$ with no allocation prediction, *NP*, and the *PI* price forecast is $p = 155$ with clone type I predicted to hold all shares. Actual transactions are indicated with diamonds while the solid lines (step functions) represent the ask (upper line) and bid prices (lower line). Volume per stage is given below the news line, and actual share holdings by clone type below that. For example, during the interval $0 \leq t < 70$ three shares were traded and at $t =$ 70 sec, the three traders of clone type I held $4 + 4 + 3 = 11$ shares, the three traders of clone type II held $3 + 1 + 4 = 8$ shares and the three traders of clone type III held $3 + 3 + 2 = 8$ shares. Finally, the last two lines below the figure show the shareholding misallocations for the *PI* and *ORE* theories. At $t = 70$ *PI* predicted that all 27 shares would be held by clone type I when, in fact, they held only 11 shares, a 16 share misallocation. There was no prediction, *NP*, made by *ORE*.

With only a few exceptions (Experiments 3 reps 3, 4, 15, and 16; and experiment 6, rep 13) prices converged closely to the *FE* prediction for the last subperiod, when all traders were informed of the final state of nature (good or bad for them). Convergence seems to be nearly as good in early reps as in late reps.[11]

A. Treatment Variables

To uncover systematic effects of our treatment variables, we employ data on share misallocations, transaction prices, and bid-ask spreads. The fractional factorial experimental design lends itself to both paired and pooled comparisons. To keep matters relatively simple, we confine our comparisons to the final subperiod, for which there is a unique equilibrium forecast.

Table 4, Panel A shows the experimental design. We begin by looking at average *FE* misallocation percentages for each set of reps (a "run") with identical treatments. We then hold constant two of the three remaining variables to generate matched pairs. Thus, to test *LEARNOPS*, we match run 1 (*EREPS*) with 13.9% misallocations against run 2 (*LREPS*) with 6% misallocations, 3 versus 4, etc. The summary data are given in Table 4, Panel B, and the raw data at the bottom of each figure. In four of the four cases, fewer shares were misallocated in *LREPS*. The average misallocation for *EREPS* was 12.8% while it was only 8.3% for *LREPS*. This seems to support our prediction regarding the *LEARNOPS* hypothesis.

We employed two parametric and two nonparametric tests on the data. The first parametric test was a *t*-test which utilized deviations from *FE* price forecasts. Deviations were defined either as mean-squared errors (based on transaction

[11] In the interest of brevity, we omit analysis of our training experiments E1 and E2. These experiments exhibit the usual "sloppiness" one expects in asset markets with inexperienced subjects: major misallocations by some traders, occasionally erratic price fluctuations, etc. They were also marred by computer malfunctions and we were not able to complete all 16 scheduled reps. Raw data for all unreported experiments is available from the authors upon request.

Table 4

Tests of Treatment Variables Based on FE Forecasts

Panel A: Experimental Design

Run Num.	Exp. Num.	Rep. Num.	Treatment Variables LEARNOPS	PAYMETHOD	INFOARRIVAL	INFOCONTENT
1	E5	1–8	EREPS	TPO	SIM	HOM
2	E5	9–16	LREPS	TPO	SIM	HET
3	E4	1–6, 8	EREPS	SP	SIM	HET
4	E4	9–16	LREPS	SP	SIM	HOM
5	E3	1–8	EREPS	TPO	SEQ	HET
6	E3	9–16	LREPS	TPO	SEQ	HOM
7	E6	1–8	EREPS	SP	SEQ	HOM
8	E6	9–16	LREPS	SP	SEQ	HET

Panel B: Results by Run

Run Num.	Exp. Num.	Rep. Num.	FE percent misalloc.	FE Price MSE	FE Bid-Ask TWMSE
1	E5	1–8	30/216 = 13.9	147.535	512.286
2	E5	9–16	13/216 = 6.0	88.874	313.424
3	E4	1–6, 8	8/189 = 4.2	115.671	207.226
4	E4	9–16	3/216 = 1.4	52.919	106.429
5	E3	1–8	32/216 = 14.8	159.218	613.086
6	E3	9–16	24/216 = 11.1	92.305	288.213
7	E6	1–8	39/216 = 18.1	78.731	416.394
8	E6	9–16	32/216 = 14.8	127.199	584.775

Panel C: Summary Statistics

Treatment		Run Num.	misalloc. pairwise	misalloc. percent	Mean-squared errors Prices	Mean-squared errors Bid-Ask
LEARNOPS	EREPS	1, 3, 5, 7	0	12.8	501.155	1,748.992
	LREPS	2, 4, 6, 8	4	8.3	361.297	1,292.841
PAYMETHOD	TPO	1, 2, 5, 6	2	11.5	487.932	1,727.009
	SP	3, 4, 7, 8	2	9.6	374.520	1,314.824
INFOARRIVAL	SIM	1, 2, 3, 4	4	6.3	404.999	1,139.365
	SEQ	5, 6, 7, 8	0	14.7	457.453	1,902.468
INFOCONTENT	HOM	1, 4, 6, 7	3	11.2	371.490	1,323.322
	HET	2, 3, 5, 8	1	10.8	490.962	1,718.511

Panel D: Parametric statistics—*F*-tests and *t*-tests

Hypothesis	misalloc. *t*-test	MSE *t*-test	MSE *F*-test	TWMSE *t*-test	TWMSE *F*-test
LEARNOPS: EREPS vs LREPS	1.61	.63	1.39	.54	1.35
PAYMETHOD: TPO vs SP	.25	.60	1.28	.38	1.31
INFOARRIVAL: SEQ vs SIM	1.33	.29	1.13	.89	1.67
INFOCONTENT: HET vs HOM	−.27	.51	1.32	.45	1.30

Panel E: Nonparametric statistics—signs test and Wilcoxon rank sum

Hypothesis	misalloc. *z*	misalloc. *T*	MSE *z*	MSE *T*	TWMSE *z*	TWMSE *T*
LEARNOPS: EREPS vs LREPS	2.00	1.16	1.00	1.16	1.00	.87
PAYMETHOD: SP vs TPO	.00	.00	1.00	1.16	1.00	.87
INFOARRIVAL: SIM vs SEQ	2.00	2.02	1.00	.58	1.00	1.44
INFOCONTENT: HOM vs HET	1.00	.00	1.00	1.16	1.00	.87

prices)

$$MSE = \frac{1}{N} \sum_{t=1}^{N} (Act_t - Pred_t)^2 \tag{6}$$

where N is the number of transactions in the relevant subinterval, or as time-weighted mean-squared errors. If a subperiod begins at $t = S$ seconds and ends at $t = E$, and the prices change at times $t_i \in [S, E]$ then the time-weighted mean squared errors are defined as:

$$TWMSE = \frac{1}{E - S} \sum_{i=S}^{E} (t_{i+1} - t_i)[Avg_i - Pred_i]^2. \tag{7}$$

That is, we square the difference between the observed average of the ask and bid prices at time t_i, Avg_i, and the price forecast under a given equilibrium concept, $Pred_i$, then weight the squared error by the length of time that Avg_i remains unchanged. Dividing this result by the total number of seconds $E - S$ gives a new estimate of the *MSE* based on *all* real-time data for the relevant subperiod. To construct t-tests, we pooled the data for each run then subtracted the relevant run pairs from each other. For example, using *MSE* data, one of four differences was run 1 minus run 2 (*EREPS* = 147.5 minus *LREPS* = 88.9). If X_i is the paired difference, the t-test is defined as

$$t = \frac{X_i}{[1/3 \sum_{i=1}^{4} (X_i - \bar{X})^2]^{.5}} \tag{8}$$

with three degrees of freedom.[12] The t-test results are shown in Table 4, Panel D. None of the treatment variables have a significant effect on *FE* prices.

The second parametric test was an F-test. Using the *LEARNOPS* treatment as an example, we summed the squared *FE* forecast errors for all reps in runs 1, 3, 5, and 7 ($i = 1, \ldots, 31$ observations) and summed the corresponding squared errors for runs 2, 4, 6 and 8 ($j = 1, \ldots, 32$ observations). The resulting F-ratio

$$F = \frac{1/31 \sum_i (P_i - Act_i)^2}{1/32 \sum_j (P_j - Act_j)^2} \tag{9}$$

of 1.314 fails to reject the null hypothesis that forecast errors are no lower for *LREPS* than for *EREPS*. The critical $F(30, 31)$ values are approximately 2.38 at the 10% confidence level and 1.84 at the 5% level. As shown in Table 4, Panel D, none of the F-tests reveal significant differences between the treatment variables.

Because the distributional assumptions of the parametric tests are not valid for our data, we also conducted two nonparametric tests: a signs test, and a Wilcoxon rank sum test. The signs test compares the number of paired comparisons, Θ, which are positive with the number expected under the null hypothesis of a 50-50 probability. The resulting z-statistic has a unit normal distribution for

[12] An alternative t-test can be constructed by pairing rep numbers instead of runs. We did this, and obtained approximately the same qualitative conclusions even though the degrees of freedom were larger. We prefer the method described in the text because individual reps are not properly matched pairs, owing to differing realizations of random events.

large sample sizes. If N is the number of observations, the z-statistic is

$$z = \frac{\Theta - .5N}{[N(.5)(.5)]^{1/2}} \tag{10}$$

The second nonparametric statistic is a Wilcoxon rank-sum test. It is implemented by rank ordering the data from two treatments and then summing the ranks r_i, for the first treatment

$$T = \sum_{i=1}^{n} r_i \tag{11}$$

If n_1 and n_2 are the number of observations for each treatment then the mean and variance for T are:

$$\bar{T} = \frac{n_1(n_1 + n_2 + 1)}{2} \tag{12}$$

$$\sigma_T^2 = \frac{n_1 n_2(n_1 + n_2 + 1)}{12} \tag{13}$$

and T has an asymptotically unit normal distribution. Table 4, Panel E shows the results for the nonparametric tests. There is some support for *EREPS* over *LREPS* in the *LEARNOPS* hypothesis for *SIM* over *SEQ* in the *INFOARRIVAL* hypothesis.

Our conclusions regarding treatment variables are that the payment method (trading profits only versus the standard paymethod) and the heterogeneity of payouts (*HOM* vs *HET*) seem to make no significant difference in either the *FE* misallocations or the mean-squared error (*MSE* or *TWMSE*). There is limited support for late reps being better than early reps, and simultaneous information arrival being better than sequential information arrival, based on misallocation data. More experimental runs would be required to draw stronger conclusions here.

B. Bid-Ask Spreads

Ask and bid prices as a function of time are shown as step functions in each figure. Often, when trading activity is thin, the bid-ask spread provides evidence that prices are converging. For example, in rep 16 of experiment 3 there was only one transaction prior to the first information event, yet the bid-ask spreads clearly converge. Hence, bids and asks provide useful data for comparing equilibrium pricing theories even when no transactions are recorded. It would be impossible to compute time-weighted mean-squared errors, Eq. 7, without using bid-ask spreads.

Copeland and Galai (1983) predict that bid-ask spreads widen when risk-neutral traders believe they are faced with greater information uncertainty. The bid-ask graphs seem to support this hypothesis in three ways. First, bid-ask spreads in early reps are greater than in late reps (*e.g.*, compare rep 2 with rep 16 in experiment 4, Figure 2). Second, within a rep, bid-ask spreads prior to the first information event are larger than those after the last information event (*e.g.*, see experiment 4, rep 8). And third, bid-ask spreads widen immediately after an information event has occurred, then narrow after the market "settles down." For example see experiment 6, rep 4 where bid-ask spreads are wide at the beginning, but narrow at the end of each sub-period.

Tests based on the bid-ask data are summarized in Table 5. The *SPREAD* was defined as the ask price minus the bid price (in cents) each time either one changed. An *OLS* regression of the form

$$SPREAD = a + bTIME \tag{14}$$

was run on various bid-ask data sets, where *TIME* is number of seconds elapsed from the start of the relevant time period. The pooled data (with 4894 degrees of freedom) showed a significant negative relationship, indicating that the bid-ask spread decreased as a function of time. Data taken from subintervals indicates that the bid-ask spread decreased after information arrival events for all subintervals except the third in the *SEQ* experiments. Also, note that the declining intercept term between subintervals indicates that the bid-ask spread decreased on average from early to late reps. We conclude that the data provide strong support for the hypothesis that bid-ask spreads increase with greater uncertainty.

C. Volume Data

Evidence on trading volume, share allocations, and price convergence can be used to test our four theories of equilibrium pricing (*CRE*, *TRE*, *ORE*, and *PI*). We begin with volume. Theoretical trading volume predictions were given in Section IIIC, and raw volume data may be found in Appendix 1, Table A2 or at the bottom of each figure.

As shown in Table 6, (Panel A) only one of our treatment variables had a significant effect on trading volume. Both nonparametric tests indicate higher volume in the simultaneous information arrival environment. This is just the opposite of our prior beliefs, as well as all (static) equilibrium theories considered (Table A2). Perhaps traders took a "wait and see" attitude in the more uncertain *SEQ* environment, or the *SIM* environment allowed more sophisticated subjects to act as scalpers. This finding remains a puzzle to us.

Of our four equilibrium price theories (*CRE*, *TRE*, *ORE*, and *PI*) only *ORE* and *PI* give interesting volume predictions (see Appendix 1, Table A2). Actual

Table 5

Bid-Ask Spread Regressions

regression type:	a	$t(a)$	b	$t(b)$	r^2	F	df
pooled data:	46.899	7.740	−.014	−4.041	.0033	16.33	4894
subintervals:							
first (T = 0–70)	215.340	3.749	−.371	−2.691	.0137	7.24	520
first (T = 0–130)	32.367	22.392	−.022	−11.506	.1230	132.38	944
second (T = 70–140)	32.060	9.540	−.013	−3.941	.0291	15.53	519
third (T = 140–215)	14.932	1.684	.003	.629	.0007	.39	599
fourth (T = 215–300)	25.577	1.634	−.002	−.293	.0001	.09	789
last (T = 130–300)	38.002	17.578	−.010	−9.375	.0550	87.89	1511

Table 6

Tests Based on Volume Data

Panel A: Tests of Treatment Variables Effects

Run Num	Exp Num	Rep Num	Total Vol.	Hypothesis	*t*-test	Statistics *z*	*T*
1	E5	1–8	285	LEARNOPS			
2	E5	9–16	283	EREP-LREP:	.34	1.00	.29
3	E4	1–6, 8	241*	PAYMETHOD			
4	E4	9–16	228	TPO-SP:	.65	.00	.00
5	E3	1–8	191	INFOARRIVAL			
6	E3	9–16	184	SEQ-SIM:	−.73	−2.00	−2.31
7	E6	1–8	201	INFOCONTENT			
8	E6	9–16	202	HET-HOM:	1.08	1.00	.29

* The average volume from reps 1–6 and 8 was added back to replace the missing rep 7

Panel B: Tests of Equilibrium Pricing Theories

	PI	ORE
Average error: all reps	5.95	−.64
Average error: 30 reps with different pred.	19.30	4.50
Mean squared error: 30 reps with diff. pred.	683.53	321.61

z − score = 3.29, $F = 2.13$

volume was closer to the *ORE* prediction in 21 out of 30 reps where *ORE* and *PI* predictions were different. Note that *PI* predicts greater average volume than *ORE*. The average *PI* error was 5.95 shares too high while the average *ORE* error was only −.64 shares too low (Table 6, Panel B). For the 30 reps where the two theories provided different predictions, the *ORE* theory was significantly better than *PI* with a signs test of $z = 3.29$ and an *F*-test of 2.13. The *TRE* volume forecast is always 18 shares, independent of the treatment variables random events. *TRE* is clearly not supported by the data; however, in the 26 cases that they differ, *TRE* outperforms *ORE* 15 times, an insignificant difference.

D. Allocation Predictions

There are two generally accepted measures of allocational efficiency in experimental markets. The first, which has already been used to test treatment variables, is the difference between actual shareholdings and those theoretically predicted for each subinterval. The raw data is provided at the bottom of each figure. Only the *ORE* and *PI* theories provide allocation predictions for each subinterval. Of the 29 subintervals where these allocation predictions differ, the *ORE* misallocation is lower in 22 cases, giving it a significant signs test of $z = 2.79$.

A second conventional measure of allocational efficiency is the percentage of potential profits actually realized by traders at the end of each rep. This measure weights a misallocation according to how large an unrealized gain from trade it produces. We define efficiency in terms of the profits received by all nine traders by

$$\text{Efficiency} = \frac{\text{Total Actual Payoff}}{\text{Total } FE \text{ Payoff}} \tag{15}$$

This definition assumes Paymethod = *SP* in all experiments to maintain comparability. (The opposite convention would yield somewhat lower percentages.) Appendix 1, Table A3 shows that overall efficiencies are fairly high, comparable to results found by previous investigators.

E. Price Convergence Tests

Perhaps the most interesting statistical tests are "horseraces" of the various price forecasts. In particular, we can compare the *PI* to *RE* forecasts for the first sub-period in all reps. For the 43 reps where transactions took place in the pre-information sub-period, in experiments E3–E6, we obtained a F-ratio, based on the ratio of *PI* to *RE* mean-squared errors, of 1.31, so this evidence is inconclusive. The results for the *TWMSE*'s based on 47 reps were also inconclusive with an F-test of 1.26. Our strongest argument for rejecting *PI* is that the hypothesis is rejected whenever transactions prices exceed *PI* predictions. This happened in dozens of transactions. Experiments 4 and 5 are particularly clear (see Figures 2 and 3). After the early reps, almost all transactions in the first sub- interval exceeded the *PI* price.

To test the prediction that *RE* will eventually overtake *PI*, we regressed the differences between the *RE* and *PI* forecast errors in the first subperiod on a constant and the rep number (*REPNO*). The result for 43 *MSE* differences was

$$\underset{}{DIFF} = \underset{(2.33)}{12.417} - \underset{(-1.24)}{.684}\ REPNO$$

with $F = 1.55$, $R^2 = .025$, Durbin Watson = 1.79 and first-order autocorrelation = .058. For the 47 *TWMSE* differences, the result was

$$\underset{}{DIFF} = \underset{(3.00)}{35.988} - \underset{(-1.71)}{2.113}\ REPNO$$

with $F = 2.92$, $R^2 = .046$, Durbin-Watson = 1.69, and first-order autocorrelation = 1.48. The coefficient of *REPNO* has the predicted sign, although it is not statistically significant at conventional levels.

ORE and *TRE* price predictions were different in only the more complex *SEQ* environment of experiments 3 (16 out of the 32 possible subintervals) and 6 (also 16 out of the 32 possible subintervals). F-ratios of *ORE* over *TRE* were 1.60 for the *MSE*'s and 1.76 for the *TWMSE*'s. Neither F-ratio is significant at the 5% level ($F(32, 32) = 1.82$). A paired comparison t-test was 1.40, also marginally insignificant. However, the *MSE*'s and *TWMSE*'s were lower for *TRE* in *every* paired comparison (32 out of 32) giving a highly significant signs test of $z = 5.66$. In addition, the Wilcoxon rank sum statistic was also highly significant with $T = 4.40$. Consequently, we reject *ORE* in favor of *TRE*.

V. Conclusions

Our experimental examination of sequential information arrival allowed us to study more closely the information aggregation properties of an asset market than has been possible in previous studies. Specifically, we were able to sharply distinguish full aggregation or strong-form informational efficiency (*TRE*) from nonaggregation or semi-strong form efficiency Rational Expectations equilibrium (*ORE*). We tested these equilibrium concepts against each other and two other alternatives (*CRE* and *PI*) in a variety of environments, using price, allocation and trading volume data.

In terms of the testable predictions obtained in Part IIIE, our principal findings may be summarized as follows:

(1a) We broadly confirmed the findings of previous investigators that *RE* forecasts eventually outperform *PI* forecasts. Specifically, *ORE* allocation forecasts were significantly more accurate overall (produced fewer "misallocations") than *PI* forecasts according to a nonparametric test; first subperiod *RE* price forecasts produced (not quite significantly) smaller errors than *PI* forecasts, and the errors were much more often of the right sign; and the *RE* errors tended to decline over reps relative to the *PI* errors (though the difference again was not quite significant). Thus in all cases examined this prediction was never rejected.

(1b) *TRE* price forecasts clearly outperformed *ORE* forecasts, with highly significant non-parametric test statistics. Since *TRE* makes no distinctive allocation forecasts, we were unable to compare them by an allocation measure, and a comparison of volume predictions is inconclusive.

(2) As predicted, we detected no effect from the nuisance variable *PAYMETHOD*. Non-parametric tests provided only weak evidence for the *LEARNOPS* prediction that convergence is better (*i.e.*, *FE* price and allocation forecasts in the last subperiod are more accurate) in later reps than in early reps. We conclude that our two nuisances need not be taken seriously with experienced subjects.

(3) Our non-parametric tests detected (marginally significant) fewer misallocations in simultaneous than in sequential information environments. Other tests indicated insignificantly better price and allocation convergence in *SIM* than in *SEQ*, and in *HOM* than in *HET* environments. Thus our predictions with respect to our focus variables were only weakly supported.

(4) Actual trading volume was significantly higher in *SIM* than in *SEQ* environments according to our non-parametric tests, contrary to the Copeland (1976) prediction as well as all four equilibrium concepts. This was perhaps our most unexpected finding, and if confirmed by future experiments, it indicates a need for new theory.

(5) Bid-ask spreads increase significantly according to our regression results when traders are exposed to price uncertainty—in early reps, in early subperiods, and immediately after information arrival—as predicted. This broadly confirms the Copeland–Galai [1983] model.

As noted in the introduction, we conclude that the full aggregation equilibrium concept *TRE* better explains our data than the alternatives considered, including the *a priori* plausible *ORE* concept corresponding to semi-strong information efficiency. However, *TRE* is unable to account for regularities in the volume, subperiod allocation, and bid-ask data. Although we have confined ourselves here to tests of static equilibrium concepts, we suggested further theoretical development of concepts such as Bayesian and Dynamic Rational Expectations in section IIIA. Ultimately it may turn out that such theories provide a better explanation than *TRE* for asset market behavior; clearly both new theory and empirical work are required to settle this matter. In the meantime, we trust that our present study will provide a solid foundation for more refined theory and new experiments investigating the role of information in asset markets.

Appendix 1 - Table A1 Mean Squared Errors

Panel A: Experiment 3									
Rep. Num.		0-70 sec		70-140 sec		140-215 sec		215-300 sec	
		MSE	TWMSE	MSE	TWMSE	MSE	TWMSE	MSE	TWMSE
1	TRE	56.0	130.5	18.1	49.2	33.1	102.5	17.7	233.1
	ORE	56.0	130.5	43.1	127.8	48.0	145.3	17.7	233.1
	PI	32.9	53.6	8.3	18.2	33.1	102.5	17.7	233.1
2	TRE	30.8	127.5	39.0	119.3	30.8	90.9	22.8	37.1
	ORE	30.8	127.5	39.0	119.3	38.3	114.1	22.8	37.1
	PI	27.2	50.1	11.6	33.1	10.8	29.2	22.8	37.1
3	TRE	46.3	151.8	41.1	94.5	60.5	183.4	59.9	189.2
	ORE	46.3	151.8	43.6	102.4	60.5	183.4	59.9	189.2
	PI	22.7	84.5	20.2	24.1	60.5	183.4	59.9	189.2
4	TRE	77.2	92.3	30.9	106.8	22.7	65.3	26.0	67.2
	ORE	77.2	92.3	30.9	106.8	22.7	65.3	26.0	67.2
	PI	64.0	37.8	17.1	59.7	8.9	22.6	26.0	67.2
5	TRE	6.0	64.6	40.8	127.2	16.2	41.1	1.0	7.4
	ORE	6.0	64.6	40.8	127.2	16.2	41.1	1.0	7.4
	PI	19.0	21.3	40.8	127.2	16.2	41.1	1.0	7.4
6	TRE	3.2	380.4	19.0	44.1	37.0	90.6	11.7	34.7
	ORE	3.2	380.4	29.0	76.7	37.0	90.6	11.7	34.7
	PI	26.2	407.0	4.1	18.8	37.0	90.6	11.7	34.7
7	TRE	-2.0	82.8	52.3	42.8	30.5	92.3	7.0	24.0
	ORE	-2.0	82.8	61.0	72.0	38.0	116.1	7.0	24.0
	PI	-2.0	49.9	40.3	14.0	10.6	30.8	7.0	24.0
8	TRE	59.7	129.2	32.5	78.7	21.6	84.4	13.0	20.3
	ORE	59.7	129.2	35.0	85.7	21.6	84.4	13.0	20.3
	PI	36.9	55.1	10.0	18.5	7.7	37.6	13.0	20.3
9	TRE	0.0	182.1	28.7	93.8	17.7	52.2	7.9	17.5
	ORE	0.0	182.1	28.7	93.8	17.7	52.2	7.9	17.5
	PI	25.0	201.4	28.7	93.8	17.7	52.2	7.9	17.5
10	TRE	63.0	58.2	25.3	72.1	25.0	63.7	25.6	73.5
	ORE	63.0	58.2	25.3	72.1	25.0	63.7	25.6	73.5
	PI	54.9	57.7	3.3	17.1	10.0	20.5	25.6	73.5
11	TRE	4.0	57.1	24.6	54.1	7.9	42.3	7.5	24.6
	ORE	4.0	57.1	27.1	61.9	21.7	89.3	7.5	24.6
	PI	21.0	42.9	2.8	16.7	14.8	35.0	7.5	24.6
12	TRE	43.4	50.3	40.6	18.4	8.4	44.0	2.7	16.6
	ORE	43.4	50.3	55.5	80.0	38.1	133.0	2.7	16.6
	PI	21.9	34.6	37.7	37.1	3.9	32.6	2.7	16.6
13	TRE	31.1	81.0	35.7	126.4	33.6	29.4	2.8	5.2
	ORE	31.1	81.0	45.4	157.5	70.7	164.1	2.8	5.2
	PI	6.4	12.6	21.5	80.3	33.6	29.4	2.8	5.2
14	TRE	30.0	109.6	34.0	119.0	40.6	123.3	1.9	5.1
	ORE	30.0	109.6	43.8	150.3	55.6	170.0	1.9	5.1
	PI	5.0	37.0	19.6	73.2	30.6	92.2	1.9	5.1
15	TRE	36.4	88.1	27.1	84.7	23.1	71.6	21.5	70.8
	ORE	36.4	88.1	27.1	84.7	23.1	71.6	21.5	70.8
	PI	12.7	37.3	3.7	9.6	9.7	26.8	21.5	70.8
16	TRE	30.0	64.8	35.7	106.0	31.4	94.0	22.4	74.9
	ORE	30.0	64.8	35.7	106.0	31.4	94.0	22.4	74.9
	PI	5.0	13.5	35.7	106.0	31.4	94.0	22.4	74.9

* – 2.00 means that there were no transactions during the subinterval

Appendix - Table A1 (continued) Mean Squared Errors

Panel B: Experiment 4						Panel C: Experiment 5			
Rep. Num.		0-140 sec		140-300 sec		0-140 sec		140-300 sec	
		MSE	TWMSE	MSE	TWMSE	MSE	TWMSE	MSE	TWMSE
1	RE	17.5	102.3	35.3	46.6	35.9	110.2	7.9	52.8
	PI	16.3	113.3	35.3	46.6	8.2	90.7	7.9	52.8
2	RE	21.2	78.7	20.1	43.9	58.4	133.5	29.5	73.1
	PI	15.2	66.4	20.1	43.9	31.9	33.0	29.5	73.1
3	RE	13.5	46.2	17.7	24.7	37.8	108.8	20.6	63.3
	PI	9.8	38.8	17.7	24.7	6.2	42.6	20.6	63.3
4	RE	9.9	39.2	16.6	41.3	51.8	118.7	24.3	91.2
	PI	7.1	25.4	16.6	41.3	26.7	41.7	24.3	91.2
5	RE	17.5	33.9	15.9	26.5	28.3	80.5	14.1	55.9
	PI	4.3	31.4	15.9	26.5	17.1	65.5	14.1	55.9
6	RE	19.0	33.3	6.5	10.1	16.7	55.3	12.6	53.2
	PI	9.3	24.6	6.5	10.1	30.4	87.4	12.6	53.2
7	RE	-	-	-	-	14.6	43.0	18.7	78.9
	PI	-	-	-	-	29.3	91.8	18.7	78.9
8	RE	15.6	34.7	3.6	14.2	15.2	66.6	19.8	43.9
	PI	5.5	27.3	3.6	14.2	30.3	65.0	19.8	43.9
9	RE	11.3	34.6	8.4	13.4	22.4	80.1	19.2	59.5
	PI	5.0	18.7	8.4	13.4	23.2	54.3	19.2	59.5
10	RE	11.3	39.1	6.7	28.1	24.0	64.0	10.6	48.9
	PI	5.0	15.8	6.7	28.1	22.1	72.0	10.6	48.9
11	RE	11.8	38.2	4.6	8.7	14.8	48.5	12.3	39.8
	PI	4.5	20.7	4.6	8.7	32.9	104.7	12.3	39.8
12	RE	11.3	36.1	16.1	52.9	14.4	43.8	8.8	45.1
	PI	5.0	12.4	16.1	52.9	28.4	90.4	8.8	45.1
13	RE	9.7	26.3	4.0	16.3	13.1	39.9	12.4	40.9
	PI	6.8	32.0	4.0	16.3	29.6	95.9	12.4	40.9
14	RE	13.1	39.3	3.8	10.3	12.7	53.5	8.4	19.7
	PI	11.2	19.1	3.8	10.3	30.8	96.6	8.4	19.7
15	RE	7.9	33.1	5.0	13.5	17.4	45.6	4.7	12.2
	PI	13.6	31.6	5.0	13.5	26.9	89.3	4.7	12.2
16	RE	9.9	29.1	4.3	16.0	10.9	32.8	12.4	47.2
	PI	8.9	24.0	4.3	16.0	31.8	99.4	12.4	47.2

* – 2.00 means that there were no transactions during the subinterval

Appendix 1 - Table A1 (continued) Mean Squared Errors

Panel D: Experiment 6									
Rep.		0-70 sec		70-140 sec		140-215 sec		215-300 sec	
Num.		MSE	TWMSE	MSE	TWMSE	MSE	TWMSE	MSE	TWMSE
	TRE	51.1	116.7	51.8	126.7	28.7	71.2	16.8	38.5
1	ORE	51.1	116.7	51.8	126.7	28.7	71.2	16.8	38.5
	PI	11.7	41.8	38.3	81.8	15.0	33.3	16.8	38.5
	TRE	21.8	49.7	27.2	35.5	33.9	82.8	19.1	35.2
2	ORE	21.8	49.7	29.5	42.7	57.5	165.0	19.1	35.2
	PI	41.8	113.2	21.3	97.7	31.3	75.5	19.1	35.2
	TRE	62.2	138.7	35.7	98.6	18.3	52.9	5.9	18.2
3	ORE	62.2	138.7	35.7	98.6	18.3	52.9	5.9	18.2
	PI	22.2	46.8	23.2	58.9	18.3	52.9	5.9	18.2
	TRE	-2.0	83.2	27.0	88.1	27.6	160.5	2.9	174.4
4	ORE	-2.0	83.2	27.0	88.1	27.6	160.5	2.9	174.4
	PI	-2.0	63.0	20.5	59.4	27.6	160.5	2.9	174.4
	TRE	-2.0	74.1	4.1	5.0	41.5	83.5	10.1	39.0
5	ORE	-2.0	74.1	34.0	95.5	72.5	173.7	10.1	39.0
	PI	-2.0	65.5	11.0	50.7	34.1	69.4	10.1	39.0
	TRE	20.0	73.5	48.6	173.8	52.0	152.2	2.5	6.3
6	ORE	20.0	73.5	50.9	181.7	71.7	213.2	2.5	6.3
	PI	25.0	62.1	19.7	64.8	19.7	46.9	2.5	6.3
	TRE	35.2	99.1	34.0	81.0	26.0	71.4	14.3	80.0
7	ORE	35.2	99.1	34.0	81.0	26.0	71.4	14.3	80.0
	PI	10.8	47.4	14.3	73.3	14.4	40.6	14.3	80.0
	TRE	20.0	158.6	9.4	43.0	27.0	50.5	7.1	24.8
8	ORE	20.0	158.6	36.9	124.0	79.4	213.7	7.1	24.8
	PI	25.0	216.7	21.5	71.1	27.0	50.5	7.1	24.8
	TRE	20.0	113.9	25.5	73.7	19.9	59.3	21.6	199.3
9	ORE	20.0	113.9	25.5	73.7	19.9	59.3	21.6	199.3
	PI	25.0	87.9	11.0	28.5	5.8	14.0	21.6	199.3
	TRE	33.1	263.5	39.9	128.0	38.0	91.9	16.2	83.0
10	ORE	33.1	263.5	39.9	128.0	50.4	123.7	16.2	83.0
	PI	12.2	317.2	7.9	23.7	3.9	12.9	16.2	83.0
	TRE	65.5	213.1	39.5	145.2	26.3	57.1	10.1	23.6
11	ORE	65.5	213.1	39.5	145.2	39.7	102.8	10.1	23.6
	PI	21.4	82.9	27.2	101.9	26.3	57.1	10.1	23.6
	TRE	-2.0	76.2	33.4	102.2	27.2	66.4	2.2	28.3
12	ORE	-2.0	76.2	35.9	110.1	59.4	171.1	2.2	28.3
	PI	-2.0	69.0	9.8	33.2	19.8	41.9	2.2	28.3
	TRE	69.8	191.3	32.5	85.4	32.5	101.5	53.4	149.7
13	ORE	69.8	191.3	61.7	176.4	67.1	211.8	53.4	149.7
	PI	25.8	70.4	18.9	44.9	22.8	70.0	53.4	149.7
	TRE	60.0	104.5	13.7	39.8	41.7	129.7	9.7	63.9
14	ORE	60.0	104.5	39.4	128.3	41.7	129.7	9.7	63.9
	PI	15.0	42.7	13.4	28.5	41.7	129.7	9.7	63.9
	TRE	25.0	70.6	21.1	58.7	20.0	57.1	10.6	29.1
15	ORE	25.0	70.6	21.1	58.7	20.0	57.1	10.6	29.1
	PI	20.0	73.4	28.9	91.3	5.7	15.9	10.6	29.1
	TRE	10.0	77.2	18.6	58.4	46.2	128.7	2.9	7.9
16	ORE	10.0	77.2	33.3	105.1	46.2	128.7	2.9	7.9
	PI	55.0	71.4	22.8	70.3	46.2	128.7	2.9	7.9

* – 2.00 means that there were no transactions during the subinterval

	Appendix 1: Table A2 - Theoretical and Actual Total Volume											
	Exp. #3			Exp. #4			Exp. #5			Exp. #6		
rep #	PI	ORE	Act.	PI	ORE	Act.	PI	ORE	Act.	PI	ORE	Act.
1	45	22.5	21	18	18	33	18	18	38	45	45	22
2	45	18	26	45	18	36	18	18	32	45	49.5	32
3	18	22.5	25	45	18	27	18	18	42	45	45	28
4	72	45	32	45	18	28	18	18	36	45	45	25
5	18	18	19	45	18	37	18	18	42	45	49.5	20
6	18	22.5	21	18	18	26	18	18	40	72	49.5	21
7	45	22.5	21	NA	NA	NA	18	18	29	45	72	29
8	45	22.5	26	18	18	24	18	18	26	72	22.5	24
9	18	18	22	18	18	30	18	18	38	45	45	26
10	72	72	25	18	18	36	18	18	46	45	18	18
11	72	49.5	17	18	18	34	18	18	27	18	18	27
12	72	49.5	24	45	18	19	18	18	48	45	49.5	15
13	45	22.5	27	45	18	20	18	18	30	18	22.5	38
14	45	49.5	24	18	18	24	18	18	27	18	22.5	25
15	72	72	22	45	18	28	18	18	35	18	45	30
16	18	18	23	45	18	37	18	18	32	45	22.5	23
Avg.	45	34	23.4	31.5	18	26.3	18	18	35.5	41.6	38.8	25.2

Appendix 1: Table A3 Allocational Efficiency in FE (Percentages)				
	Experiment #			
Rep	3	4	5	6
1	100.0	88.9	95.4	95.0
2	88.5	98.8	95.9	89.6
3	91.9	98.9	96.0	100.0
4	93.0	100.0	97.2	99.0
5	100.0	100.0	100.0	96.3
6	98.6	100.0	53.2	97.0
7	98.2	NA	96.7	98.3
8	100.0	100.0	69.9	95.0
9	100.0	100.0	100.0	96.1
10	97.8	100.0	100.0	93.5
11	96.7	100.0	100.0	100.0
12	99.7	99.2	100.0	85.2
13	100.0	100.0	84.0	98.5
14	99.7	100.0	98.5	100.0
15	95.7	100.0	100.0	100.0
16	97.8	99.8	98.3	100.0

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