Market Transparency: Who Wins and Who Loses?

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This study uses laboratory experiments to determine the effects of trade and quote disclosure on market efficiency, bid-ask spreads, and trader welfare. We show that trade disclosure increases the informational efficiency of transaction prices, but also increases opening bid-ask spreads, apparently by reducing market-makers' incentives to compete for order flow. As a result, trade disclosure benefits market makers at the expense of liquidity traders and informed traders. We find that quote disclosure has no discernible effects on market performance. Overall our results demonstrate that the degree of market transparency has important effects on market equilibria and on trader and market-maker welfare.

Transparency is a fundamental issue in the design and regulation of markets. In the United States, the Securities and Exchange Commission's (SEC) view is straightforward: "The Commission has long believed that transparency—the real time, public dissemination of trade and quote information—plays a fundamental role in the fairness and efficiency of the secondary markets . . . transparency helps to link dispersed markets and improves the price discovery, fairness, competitiveness and attractiveness of U.S. markets."[1] Consistent with this view, all U.S. market centers must immediately report trade prices and volumes, as well as provide the best outstanding bid and ask quote to traders.[2]

This beneficial view of transparency is not universally shared. The Securities and Investment Board (SIB), the U.K. regulatory body, has argued that there are important differences between quote transparency and trade transparency. Of particular importance is their view that there is "a tradeoff between liquidity and trade transparency." This presumed trade-off arises

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[1] See SEC Market 2000 Study, Chapter IV-1.

[2] Centralized exchanges are required to report all such information immediately. For a dispersed dealer market such as the NASDAQ, the rules are somewhat different. Prior to 1982, there were no explicit reporting requirements. As of June 1, 1982, the SEC required that the NASDAQ report all equity trades within 90 seconds of occurring. For more discussion of these reporting rules, see Porter and Weaver (1995).

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because knowledge of trades may expose market makers to undue risk as they unwind positions, and consequently "transparency [in this context the prompt publication of large trades] should be restricted if this is necessary to assure adequate liquidity."[3]

Some have argued, however, that restricting transparency provides benefits to large traders at the expense of small traders. Still others, for example, the Office of Fair Trading (OFT) in the United Kingdom, have questioned whether restricting transparency may also reduce the speed with which market makers adjust prices, thereby reducing market efficiency.[4] A further complication is introduced by the role of quote transparency. The SIB has suggested that the dissemination of quotes may, in a sense, substitute for posttrade information "by providing traders with sufficient information to make informed trading decisions." Yet this ameliorative role of quote transparency remains conjectural, and this speculation only serves to emphasize the general lack of knowledge surrounding the effects of transparency on market behavior.

These issues are of more than academic importance. The availability of posttrade information is a subject of intense debate on both the London Stock Exchange and the Paris Bourse, where transparency issues are viewed as fundamental to the competitiveness of each market.[5] Similarly, whether proprietary trading systems such as Instinet should be forced to reveal trade and quote information is an unsettled issue in the United States and elsewhere.[6] Transparency issues also arise in foreign exchange markets, where it is questioned whether the lack of transparency induces excessive volatility, and in derivative markets, where the very viability of such markets depends crucially on the transparency of the underlying equity market. Despite increasing research attention [see Biais (1993), Naik, Neuberger, and Viswanathan (1994), Board and Sutcliffe (1995, 1996), Madhavan (1995, 1996), Gemmill (1996), Lyons (1996), Pagano and Roell (1996), Porter and Weaver (1996), Flood et al. (1997a,b)] there is little consensus as to the overall effects of market transparency.

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[3] See the SIB discussion paper, February 1994, page 8.

[4] See Franks and Schaefer (1995) for a discussion of these issues.

[5] These competitive effects are illustrated by the difficulties of the Paris Bourse. The Paris market required full trade transparency, but found themselves losing trading volume to the London Stock Exchange where transparency was restricted. Paris changed from a transparent to a less transparent regime for large trades in an attempt to regain such trades. For more discussion of this change, see Gemmill (1996). See also Naik, Neuberger, and Viswanathan (1994) for a discussion of posttrade reporting on the London Stock Exchange, and Porter and Weaver (1996) for an interesting analysis of changes in pretrade efficiency on the Toronto Stock Exchange.

[6] It has been alleged that proprietary systems such as Instinet attract order flow from other markets because they are not subject to the same reporting requirements as are other market centers. While these reporting requirements were partially changed by the SEC order handling rules introduced in January 1996, differences in transparency, particularly with respect to quotes, still remain between alternative trading systems and regulated exchanges and markets [for more dicussion see Macey and O'Hara (1997)]. These issues also arise in a global context. For example, Australia does not permit Instinet to operate within its borders, but a significant percentage of volume in Australian stocks is now cleared by Instanet in Hong Kong.

In this study we use experimental economics to investigate the effects of transparency on informational efficiency, bid-ask spreads, and trader welfare in a dealer market. We are interested in a specific question: How does the observability of trade and quote information affect the market equilibrium? In actual markets, determining this is complicated by the difficulty of comparing markets that may differ on more dimensions than transparency, or may be subject to differences introduced by the passage of time.[7] Our research methodology allows us to control for these difficulties while explicitly varying the degree of market transparency. This allows us to investigate how different pieces of market information affect market performance and trader and market-maker welfare.

To address these issues, we conduct laboratory experiments in which two market makers quote prices at which a variety of traders can buy and sell.[8] The markets include computerized informed traders who know the true value of the security, computerized noise traders who buy and sell arbitrary numbers of shares in each round, and two human "active" traders who must raise or invest a predetermined amount of the laboratory currency before trading closes in each security. Trade takes place in three different settings. In a transparent setting, both market-makers' quotes and trades are publicly disclosed after each round of trading. In a semiopaque setting, quotes are disclosed but trades are not. In an opaque settings, neither trades nor quotes are disclosed.

Our experiments provide four principal results:

First, trade disclosure significantly improves the informational efficiency of the markets. The midpoint of the market bid and ask converges to true values more quickly when trades are disclosed, as do the value estimates of both market makers and active traders. This result is consistent with the analytical results of Madhavan (1995) and Pagano and Roell (1996).

Second, trade disclosure causes opening spreads to widen dramatically. This result appears to be driven by the market-makers' reduced need in transparent markets to compete for order flow in order to get information about security value. Spreads in later rounds are less affected by transparency, both because more information is already contained in market prices and because there is less future benefit to capturing late round order flow.

Third, trade disclosure benefits market makers at the expense of informed traders and liquidity traders who cannot time their trades. This welfare effect differs from that hypothesized in theoretical models, which assume that

[7] Board and Sutcliffe (1995, 1996) and Gemmill (1996) investigate the effects of changing trade reporting requirements on the London Stock Exchange. While in many ways a natural experiment on posttrade transparency, the different reporting regimes stretch over a decade, encompassing a period of dramatic change in the trading environment. This complicates interpretation of the results, and may partially explain the different findings of these studies.

[8] This framework is similar to that in Bloomfield (1996).

competing market makers earn zero (competitive) returns whether or not trades are disclosed. In our experiments, trade transparency allows market makers to set wider spreads and earn positive returns at the expense of both informed and liquidity traders. Such transparency has no welfare effect on active traders because they are able to delay trading in transparent markets until bid-ask spreads narrow sufficiently.

Finally, we find differential effects of quote transparency and trade transparency. Overall, we find that *trade disclosure* can have important effects on informational efficiency, bid-ask spreads, and trader welfare. In contrast, we find that *quote disclosure* seems to have little effect on any of these market characteristics. Thus our results suggest that trade transparency provides information that cannot be obtained from knowing only quotes.

Our results here may be contrasted with those of Flood et al. (1997a,b), who also investigate trade and quote transparency issues in two separate experimental studies. In Flood et al. (1997a), the authors investigate quote transparency in a setting in which trade information is never revealed, quote data may be available, and interdealer trade is the bulk of trading activity. In that setting, which is similar to a foreign exchange market, they find that quote transparency reduces opening spreads and reduces informational efficiency. While their results conflict with our findings that quote disclosure has little effect, the differences in the settings make this conflict more apparent than real. In their setting, without quote disclosure dealers must spend time searching for counterparties with whom to trade. Quote disclosure makes it easier for dealers to trade with each other, and this encourages competition in spreads as well as increased dealer speculative trading. This latter effect slows down informational efficiency. Interdealer trading is not a feature of our setting, and the results are not comparable. Conversely, in Flood et al. (1997b), the authors find results on trade transparency very similar to some of our findings; trade transparency increases bid-ask spreads and increases market efficiency.

This article is organized as follows. The next section sets out the issues connected with market transparency, reviews the relevant prior research, and details the hypotheses we will investigate. In Section 2 we describe our experimental design and the methodology we use to determine the effects of trade and quote information on market equilibrium. Section 3 then presents and analyzes our results. The article's final section summarizes our findings, discusses their limitations, and outlines their implications for the design and regulation of markets.

1. Market Transparency

How should transparency affect market behavior? Numerous authors have investigated aspects of this question, but no definitive answer has emerged. One reason may be that even defining a transparent market is problem-

atic. Publicly available quote and trade data are certainly characteristics of transparent markets, but so too may be data on trade size, trader identity, order type, and the size and distribution of any limit orders. Even restricting the transparency debate to trade and quote data, as we will do in this research, reveals a wide disparity in predicted effects. In this section we outline some of these predictions and determine the specific hypotheses we will investigate in our experimental markets.

Perhaps the natural starting point is to consider whether transparency should matter at all. The theoretical research [see Biais (1993), Naik, Neuberger, and Viswanathan (1994), Madhavan (1995, 1996), Lyons (1996), Pagano and Roell (1996)] generally suggests that it should, although the empirical evidence [see Board and Sutcliffe (1995, 1996), Gemmill (1996), Porter and Weaver (1996)] is mixed. Pagano and Roell (1996) consider this issue theoretically by analyzing how market outcomes differ across various types of auction and dealer markets. Their analysis predicts that transparency matters because patterns in trades, such as imbalances of buy or sell orders across the market, may be more easily discerned in transparent markets. This, in turn, allows market makers to learn any information from trades more quickly, and thereby set their prices more efficiently. Focusing on these informational efficiency effects, this suggests testing the following hypothesis:

Hypothesis 1. *Transparency increases the informational efficiency of the market.*

A related issue is how transparency affects bid-ask spreads. In general, we would expect spreads to decrease over the trading rounds as market makers learn from the order flow. The speed with which this occurs will depend on how quickly the market makers learn (which we investigate in Hypothesis 1) and on their particular pricing strategy. Madhavan (1995) predicts that dealers in less transparent markets will price more aggressively in early rounds so as to use their private knowledge from trades to extract rents in future rounds. Informed traders benefit in this case, as do large uninformed traders, because dealers are willing to pay to learn trade information that can be exploited later. A finding that initial spreads are smaller in less transparent markets would be consistent with this hypothesis [this effect is also predicted by Naik, Neuberger, and Viswanathan (1994)]. To investigate these effects, we test the null hypothesis:

Hypothesis 2. *Transparency does not affect spread behavior in markets.*

If transparency affects informational efficiency or bid-ask spreads, then it may also affect the gains and losses of traders. Both Madhavan (1995; 1996) and Pagano and Roell (1996) argue that the reduced ability of informed traders to trade on their information in transparent markets should translate into lower rents for informed traders and higher rents for unin-

formed traders. But there are other possible outcomes to consider as well. For example, the trading strategies of uninformed traders may differ between transparent and nontransparent settings, dictating a complexity to predicting their actual trading rents in each setting. Moreover, while greater informational efficiency dictates that the midpoint of the spread is closer to the true value, this does not necessarily mean that the spread is smaller. Consequently, determining the effect of transparency on trader rents need not be straightforward. This suggests testing the following null hypotheses:

Hypothesis 3. *Transparency does not affect the rents earned by informed traders.*

Hypothesis 4. *Transparency does not affect the rents earned by uninformed traders.*

One factor not yet considered is whether other trader characteristics might also be important. Uninformed traders may prefer a less transparent venue, as it facilitates their ability to "hide" their liquidity needs from the market. This reflects the problem that a market maker who knows, for example, that a large uninformed trader is buying may set higher prices to take advantage of these trading needs [see Roell (1990)]. Empirical evidence on this issue, however, is less clear, as Gemmill's (1996) study finds fairly weak effects of differing large trade reporting rules on the London Stock Exchange. This suggests testing the following null hypothesis:

Hypothesis 5. *Transparency does not affect the rents earned by large uninformed traders.*

If traders have preferences over market transparency, then so too may market makers. Most theoretical analyses are mute on this point because by construction most models have assumed that market makers act competitively and so make zero profits in any market setting [see O'Hara (1995) for more discussion]. Yet market makers and dealers have been among the most vociferous participants in transparency discussions [see Gemmill (1996)], suggesting that indifference to transparency regimes is hardly accurate. Moreover, recent experimental evidence of Lamoureux and Schnitzlein (1997) demonstrates that dealer profits may be greatly affected by market structure.[9] This suggests testing the following hypothesis:

Hypothesis 6. *Market-maker profits are zero across all transparency settings.*

[9] Lamoureux and Schnitzlein analyze the effect on market equilibrium of fragmentation of trading. Though not directly related to our analysis here, there results do suggest that dealer profits are lower when traders can search for better prices. This is consistent with the notion of greater transparency in the trading venue.

Experimental economics provides a natural methodology for investigating these hypotheses. Using the controlled setting of a laboratory market, we can analyze the many factors listed above, such as dealer and trader profits, which cannot easily be observed in actual markets. There are now dozens of laboratory studies examining the behavior of many types of financial markets [see Davis and Holt (1994) and Sunder (1995) for a review]. In the financial markets we employ in this study, only some participants (market makers) are permitted to post prices; all other traders are restricted to buying and selling at those posted prices [see Bloomfield (1996) for more discussion]. This setting captures the trading framework most frequently modeled in the microstructure literature—a dealer market in which each dealer sets bid and ask quotes, and must compete with other market makers (typically not modeled) for order flow—and thus provides the natural venue in which to test our hypotheses.[10]

In the next section we detail our experimental design and methodology. The experiments we perform bear similarities to those in Flood et al. (1997a,b). In our research, we manipulate transparency by altering the amount of information traders can observe about other traders' activity, and examine how this manipulation affects the efficiency of prices. This allows us to construct independent manipulations of whether or not quotes (bids and asks) are observable and whether or not trades are observable.

2. Experimental Design

In this section we describe the nature of our experiment and the specific features of our markets. We conclude the section with a discussion of how our experimental controls improve the power of the experiment and minimize the chance that "nuisance variables" might interfere with the interpretation of the results. As a useful preliminary, we note the following definitions. A *cohort* is a group of four subjects who always trade together. A *security* is a claim on a terminal dividend, and is identified by the value of the security and the traders' liquidity incentives (described below). A *market* is a sequence of trading rounds for a single security. A *session* is a 3-hour period during which a cohort participates in a series of markets (one for each security).

2.1 Overview

We construct markets in which two market makers compete to trade with several traders. The true value of each security (denominated in "francs," a laboratory currency ultimately converted to cash) is either 110, 130, 150,

[10] In contrast, most previous laboratory studies of financial markets employ a double auction in which all traders can post prices at which they are willing to buy and sell shares [see, e.g., Copeland and Friedman (1987, 1991) and O'Brien and Srivastava (1991)].

170, or 190. Each value is equally likely. As in most previous laboratory market studies, we use a small number of discrete values to simplify the subjects' task. Each security is traded for five rounds. In each round, the two market markets each estimate the value of the security, and then set bid quotes at which they will buy shares in that round and ask quotes at which they will sell shares. The only restrictions on a market-maker's bids and asks are that they must lie in the interval [100, 200], that the bid can be no greater than the market-maker's value estimate, and that the ask can be no less than that estimate. Traders observe the market makers' quotes, estimate the value of the security, and choose to buy up to 10 shares at the low ask (called the "market ask") or sell up to 10 shares at the high bid (the "market" bid). There are no wealth constraints or short-selling constraints in the market.

After each round of trading, investors receive some information about market-makers' quotes and trades. We manipulate market transparency by altering this information. As shown in panel A of Table 1, trade takes place in three different settings. In a "transparent" setting, both market-makers' quotes and trades are publicly disclosed after each round of trading. In a "semiopaque" setting, quotes are disclosed publicly, but trades are not—thus every participant sees only their own trades. In both the transparent and semiopaque settings, market makers do not observe the other market-makers' quotes until after they set their own quotes. In an "opaque" setting, quotes are disclosed only to traders, and trades are not disclosed.

The market settings described above are intended to correspond to the general structure typically found in dealer-based equity markets. In such markets, traders are assumed able to see the outstanding quotes and so can choose which market maker to trade with. Dealers set quotes and must be willing to trade up to a specific amount at their quotes. In our transparent and semiopaque settings, a dealer sees the other dealers' quotes only after setting his quotes, and so he may then want to revise his quotations. Dealers are free to do so in the next round, but they must clear orders (if there are any) submitted to them in this round at their current quote. This protocol corresponds to that found on both the London Stock Exchange and on the SOES system used on the NASDAQ, where dealers must trade up to a certain amount if orders have been submitted.[11]

[11] The SOES system requires that dealers trade up to 2,000 shares at their posted prices (this amount has been lowered for some stocks following the implementation of the new SEC order handling rules). In that setting, dealers each post a quote and they presumably then watch the posted quotes of other dealers. Seeing other dealers revise their quotes may induce a dealer to revise his as well. SOES "bandits," however, also watch dealer quotes and they attempt to send in orders before the dealers revise their quotes. For a discussion of SOES trading see Harris and Schultz (1997). On the London Stock Exchange, dealers also are required to trade at any posted quote up to one NMS (notional market size) in each stock. Reiss and Werner (1997) argue that this binding nature of the quote is a fundamental feature of the London market.

Table 1
Experimental design and analysis

Panel A: Description of treatments. This panel describes the three different experimental settings.

Opaque setting	Semiopaque setting	Transparent setting
Market makers don't see each other's quotes or trades.	Market makers see each others quotes, but not their trades.	Market makers see each other's quotes and trades.
Traders see market-makers' quotes, but not their trades.	Traders see market-makers' quotes, but not their trades.	Traders see market-makers' quotes and trades.

Panel B: Experimental design. This panel depicts the experimental design. Securities 1–18 are traded during the first session, while securities 19–42 are traded during the second session. A cohort participating in ordering 1 of the design trades securities 1–6 in session 1 and securities 19–26 in session 2 in the opaque setting (O), trades securities 7–12 in the first session and 27–34 in the second session in the semiopaque setting (SO), and trades securities 13–18 in the first session and 35–42 in the second session in the transparent setting (T). In every ordering, the cohorts trade in all three settings in each session, and trade the 42 securities in the same order.

Ordering number	Securities 1–6 and 19–26	Securities 7–12 and 27–34	Securities 13–18 and 35–42
1	O	SO	T
2	SO	T	O
3	T	O	SO
4	O	T	SO
5	T	SO	O
6	SO	O	T

2.2 Trader types

After the market makers set their quotes for a given round, traders decide how many shares they wish to buy or sell at those quotes. In addition to the two market makers, the markets include two computerized informed traders, two computerized constant liquidity traders, and two "active traders." Each type of trader is described in turn.

Each computerized informed trader buys 10 shares at the low ask if that price is strictly less than the true value, and sells 10 shares at the high bid if that price is strictly greater than the true value. Such nonstrategic behavior is consistent with informed traders competing with each other to place orders.[12] In earlier pilot studies we incorporated human informed traders, but their behavior never deviated from the simple mechanistic rule described above. Consequently we used the simpler framework of computerized informed trade in this study.

One computerized constant liquidity trader always buys 20 shares in each round at the low ask; the other constant trader always sells 20 shares at the high bid in each round. These constant traders induce smaller bid-ask spreads by providing a significant flow of uninformed trade which market

[12] Nonstrategic informed traders are a standard feature of many microstructure models [see O'Hara (1995)].

makers can use to offset losses to the informed traders. We use nonrandom liquidity traders to keep the setting as simple as possible.[13] Like the informed traders, these traders have no opportunity to delay trading. This restriction, similar to that imposed in many theoretical models, reflects the notion that liquidity concerns can force traders to trade at a specific point in time, regardless of the prevailing market price.

Finally, two human "active traders" are each given a target number of francs to raise or invest over the five rounds of trading. In each market, one active trader is "large" and has a target of 4,000 francs, while the other is "small" and has a target of 2,000 francs.[14] Whether a given active trader must raise or invest that number of francs is determined randomly and independently for each trader, with each possible outcome equally likely. Unlike the computerized traders, active traders can choose when they wish to trade. Thus active traders may attempt to delay trading in order to conceal their liquidity needs or to avoid wide opening bid-ask spreads. In each round, active traders choose to buy or sell from 0 to 10 shares. They cannot buy and sell in the same round. Active traders (like market makers) are not told security value until trading concludes.

2.3 Subjects and incentives

The participants in the experiments are primarily MBA students in the Johnson Graduate School of Management at Cornell University, and all had participated in previous laboratory market studies. Each subject attended an initial training session, and each received detailed written instructions, a copy of which is given in Appendix A.

Both market makers and active traders earn francs through their trading decisions and their estimates of security value. Every share bought results in a gain or loss of (Price − Value) and every share sold results in a gain or loss of (Value − Price), where Value is the true value of the security, and Price is the price at which the trade occurred. Every value estimate earns the participant a bonus of $50 - 2 \times |\text{Estimate} - \text{Value}|$, to a minimum of 0 francs. Thus participants have an incentive to estimate the value of the security accurately in each round. However, the bonus is small enough (relative to trading gains and losses) that participants are unlikely to hedge their estimates and quotes against one another to reduce payoff risk.

[13] An additional advantage of nonrandom liquidity traders is that it shortens the time necessary for market makers to learn true values. This allows us to investigate the effect of transparency differences in a parsimonious manner. Introducing random liquidity trades should not affect the nature of our results but would be expected to slow the speed with which markets adjust to information.

[14] Setting a target in francs instead of shares seems more faithful to the typical story underlying liquidity shocks: that investors need to raise or invest cash, not shares. Our treatment could potentially induce behavior that depends on share value, since it is harder to raise or invest large amounts of cash if the stock price is low. We mitigate this effect by using securities that have a value that is high relative to the variance in possible values.

Active traders also lose francs by failing to meet the target level of francs they need to raise or invest over the five rounds of trading. Every franc by which an active trader fails to meet the target results in a penalty of 3 francs. This penalty is large enough that in most cases the active trader is better off meeting the target than not trading and avoiding a trading loss. (For example, a trader who sells a share for 120 francs when it is worth 190 reduces his or her penalty by 210 francs, while incurring a trading loss of only 70 francs).

Cash winnings for each session are determined by matching each subject with a subject from another cohort but playing the same role (i.e., market maker or active trader). Matched subjects divide $50 pro rata on the basis of francs earned, with a minimum payment of $5.[15] Francs won in one session do not offset francs lost in another session. Although this method superficially resembles a "tournament" scheme, it has none of its undesirable properties. In particular, the marginal value of a franc is roughly constant (but unknown). Thus the pro rata scheme does not induce risk-seeking behavior as tournament schemes do. This pro rata method reduces the extent to which investors' earnings depend on the role they are asked to play, while simultaneously ensuring that each investor's winnings are independent of the performance of other investors in the same market (conditional on their own performance).

2.4 Experimental design issues

A primary benefit of experimental methodology is the ability to exercise control over features of the experiment that might influence behavior but are not the focus of the study. Our experimental design (panel B of Table 1) is intended to control for several such features: differences across securities, differences across subjects, and differences due to learning effects, and the unknown effects of various aspects of the experiments that are constant across treatments.

First, we control for variation across securities. The specific characteristics of a security are expected to affect several of our dependent variables. For example, securities with more extreme values tend to be priced less efficiently in experimental settings [Bloomfield (1996)]. Informed traders' trading gains are likely to be largest for securities with extreme values, while active traders' trading losses are likely to be largest for securities for which they have larger liquidity targets. We control for variation over securities by having each security traded in all three experimental settings. Each time

[15] A minimum payment may induce risk-seeking behavior among subjects who have a low or negative balance (and are insured against further loss). To mitigate such concerns, subjects are told that their balance in francs is equal to their change in wealth plus an unknown endowment. Because subjects do not know the endowment, they cannot know whether they were close to the minimum payment level and should be less likely to engage in risk-seeking behavior.

a security is traded, the security value and the active traders' targets remain the same. This control guarantees that differences across settings are not driven by differences in securities.

Second, we control for differences across cohorts. It is well known that different subjects in laboratory markets possess different levels of intelligence, motivation, and familiarity with the experimental environment [Davis and Holt (1994), Kagel and Roth (1995)]. Such differences can make it difficult to draw inferences about the effect of a treatment variable (such as transparency) if one cohort of subjects trades in one setting and another group trades in another setting. In such a case, the apparent effect of the treatment might actually reflect differences in the cohorts' intelligence or motivation. We avoid this potential confounding factor by having each of our six cohorts trade securities in all three transparency settings, as shown in panel B of Table 1. As a result, we are able to observe the effect of transparency within a given cohort.

Third, we control for learning effects. Because laboratory markets are complex, even the same cohort may behave differently in later repetitions of the task than in early repetitions [Bloomfield and Libby (1996), Forsythe and Lundholm (1990)]. Thus it is very important to avoid running one setting first and then the other, as any differences could be due to learning rather than to transparency differences. We control for such effects by having each cohort of subjects participate in the different settings in different orders, as shown in panel B of Table 1. Each cohort still trades the same securities in the same order—the only change is in the order of the settings in which they trade.

Lastly, we control for the effects of various characteristics of the financial markets that have effects on behavior that are unknown but similar across treatments. For example, the wording of the instructions, the incentive system, and discrete security values may affect behavior in unknown ways. However, these unknown effects are unlikely to vary systematically across treatments. As a result, they cannot account for the differences across cells that are the focus of our analysis. To the extent that such effects increase the amount of random error in the data, they may make it more difficult to observe differences across cells, but they cannot create such differences.

As a final point, we note that a crucial feature of any experimental design is its ability to provide a well-defined statistical structure within which to interpret results. Our framework is designed to provide data that can be compared with statistical precision across our three transparency settings. This is a result of our use of discrete trading periods which allows us to aggregate data from many markets and many cohorts. Our analysis can thus employ the standard statistical tools to discriminate effects between our treatment settings.

3. Results

We now turn to the testing of our hypotheses and our experimental results. Recall that multiple cohorts trade each security in each setting. Given that we

have six cohorts each trading 42 securities, our total sample size is $6 \times 42 = 252$ securities. Because these observations are not completely independent, we compute three averages within each cohort for each dependent variable for each security—one for each setting. This results in a single (averaged) observation in each of the three treatments for each of the 42 securities, for a total of $3 \times 42 = 126$ observations. To determine the effect of trade disclosure, we compute t-statistics using the 42 paired differences in the (averaged) dependent variable across the transparent and semiopaque settings for a sample size of 42 averaged difference observations. To determine the effect of quote disclosure, we compute t-statistics using the 42 paired differences in the (averaged) dependent variable across the semiopaque and opaque settings, for a sample size of 42 averaged difference observations.[16]

3.1 Informational efficiency

We begin by testing Hypothesis 1 which posits that transparency increases the informational efficiency of market prices. We proxy the market price in each trading round by the midpoint of the market bid and ask. Figure 1 shows that price errors decline more rapidly in the transparent markets than in the opaque and semiopaque markets. This suggests that transparent markets reveal information more rapidly and completely than less transparent markets, which is strong evidence in support of our Hypothesis 1. This result is consistent with the analytical findings of Madhavan (1995) and Pagano and Roell (1996), but is at variance with the conclusions of Flood et al. (1997a).

To assess the statistical significance of this difference, we compute the cumulative errors over rounds 2 through 5 of trading. (Errors in round 1 are not expected to differ across settings because subjects provide their initial value estimates before observing any market information.) This data is presented in Table 2. The cumulative errors are 20.7 francs in the transparent markets, compared to 35.8 francs in the semiopaque markets. The difference of 15.2 francs is statistically significant ($p = .002$). In contrast, quote disclosure appears to have little effect on informational efficiency: the difference in cumulative errors across the opaque and semiopaque markets is only 0.5 francs, not statistically significant at any conventional level.

It is possible that the effect of trade disclosure on informational efficiency shown in Figure 1 is driven not by the information contained in quotes, but by a decrease in the influence of noninformational factors on quotes. Quotes can represent biased proxies for their true value estimates if market makers

[16] Some cells include three cohorts, while others include only one. The only effect of this imbalance (which was a result of a programming flaw) is that some of the averages are averages of three observations, while others are of only one or two. As a result, the averages for some securities possess less random error in the semiopaque setting than the transparent setting, while the averages for other securities possess less random error in the transparent setting than in the semiopaque setting. However, this does not introduce any bias into the results.

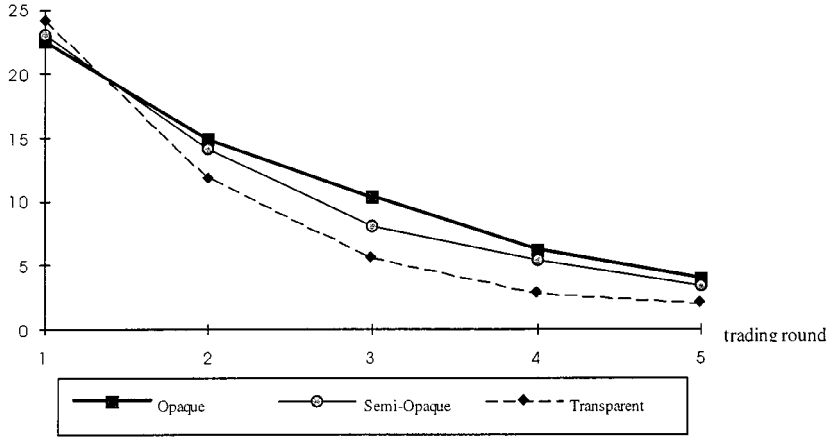

Figure 1
Absolute errors in market prices by trading round
This figure shows that absolute errors in market prices decline in all three settings, but decline more quickly in transparent setting, in which trades are disclosed. Prices are defined as the midpoint of the market (high) bid and market (low) ask. The absolute error is defined as |Price − Value|.

Table 2
Cumulative absolute errors in prices and estimates

Dependent variable used to compute error	Opaque setting	**Effect of quote disclosure (*t*-statistic) *p* value**	Semiopaque setting	**Effect of trade disclosure (*t*-statistic) *p* value**	Transparent setting
Price	35.3	**0.5 (0.104) ns**	35.8	**−15.2 (−3.209) *p* = .002**	20.7
Market-maker estimates	34.0	**−0.8 (−0.157) ns**	33.2	**−15.5 (−3.477) *p* = .0001**	17.7
Active trader estimates	32.7	**−2.3 (−0.436) ns**	30.4	**−14.3 (−3.775) *p* = .001**	16.1

This table depicts absolute errors in market prices, market-maker estimates, and active trader estimates cumulated over rounds 2–5. Price is defined as the midpoint of the market (low) ask and the market (high) bid.

T-statistics on the effects of quote and trade disclosure are computed by first taking the average of all observations for a given security in each setting, which results in a single (averaged) observation in each setting for each security. The statistical analysis of the effect of quote disclosure is based on the 42 paired differences (one for each security) between the opaque and semiopaque settings. The statistical analysis of the effect of trade disclosure is based on the 42 paired differences (one for each security) between the semiopaque and transparent settings.

are concerned about controlling inventory [see Biais (1993), O'Hara (1995), Bloomfield (1996)]. If such effects are smaller in the transparent setting, the midpoint of quotes may lie closer to value, even though market-makers' information is no better. We investigate this possibility by analyzing both

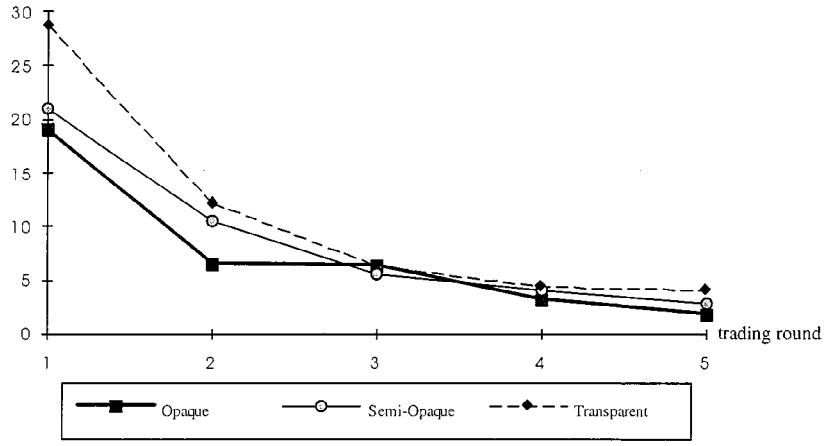

Figure 2
Bid-ask spreads by trading round
This figure shows that market bid-ask spreads decline over time in all three settings, but that opening spreads are widest in the transparent setting, in which trades are disclosed. The spread in a given round is defined as the market (low) ask minus the market (high) bid.

market-makers' and active traders' estimates of value. Because subjects have an incentive to minimize absolute error in their estimates (and they need not worry about inventory risk), estimates should be an unbiased measure of market makers' true best estimates of value. As shown in Table 2, analysis of value estimates provides results nearly identical to those regarding price errors. Thus it does not appear that the ability of trade disclosure to improve the informational efficiency of market prices is driven by inventory or other noninformational effects.[17]

3.2 Bid-ask spreads

We next examine the behavior of bid-ask spreads across the three markets. Hypothesis 2 posits the null hypothesis that transparency has no effect on market spreads. As shown in Figure 2, opening spreads are much wider in the transparent setting (in which trades are disclosed) than in the semiopaque and opaque settings (in which they are not). In contrast, quote disclosure appears to have no effect on opening spreads, which are similar in the opaque and semiopaque settings. Trade disclosure also appears to have little effect on spreads in later rounds: spreads decline rapidly to near zero in all three settings.

Table 3 reports tests of the statistical significance of these differences. The increase in opening spreads from 18.0 to 26.8 francs due to trade

[17] The absence of hedging behavior, together with the similarity of quote and estimate data, make it unlikely that subjects are hedging their quotes and value estimates against one another in order to reduce payoff risks.

Table 3
Bid-ask spreads

Dependent variable	Opaque setting	Effect of quote disclosure (t-statistic) p value	Semiopaque setting	Effect of trade disclosure (t-statistic) p value	Transparent setting
Opening spread	19.1	**−1.1 (−0.412) ns**	18.0	**8.8 (3.535) p < .001**	26.8
Rounds 2–5	4.4	**1.7 (1.465) p = .08**	6.2	**2.9 (1.991) p = .03**	7.4
Average spread over all rounds	7.35	**1.2 (1.155) ns**	8.5	**2.7 (2.574) p = .014**	11.3

This table depicts market bid-ask spreads in the opening round, rounds 2–5, and all rounds. The spread in a given round is defined as the market (low) ask minus the market (high) bid.

T-statistics on the effects of quote and trade disclosure are computed by first taking the average of all observations for a given security in each setting, which results in a single (averaged) observation in each setting for each security. The statistical analysis of the effect of quote disclosure is based on the 42 paired differences (one for each security) between the opaque and semiopaque settings. The statistical analysis of the effect of trade disclosure is based on the 42 paired differences (one for each security) between the semiopaque and transparent settings.

disclosure is statistically significant ($p < .001$), while the decrease from 19.1 to 18.0 francs due to quote disclosure is not statistically significant. Trade disclosure also increases average spreads, with the average taken over trading rounds 2 through 5, or over all five trading rounds. Quote disclosure has only a marginally significant effect in rounds 2 through 5 ($p = .08$), and no significant effect over all rounds. We interpret these results as strongly rejecting Hypothesis 2; transparency of trade information does have a statistically significant effect on bid and ask prices.

The slight positive effect of trade disclosure on late spreads and the much larger effect on initial spreads are consistent with theoretical predictions in Madhavan (1995). In that article, trade disclosure reduces market-makers' incentive to compete for order flow because they can extract information about security value and active traders' net demand from trade information even if they, themselves, do not trade. In contrast, trading is the only way a market maker can extract information about security value when trades are not disclosed. Thus market makers may be willing to buy for more and sell for less in the opening round of trade in the opaque and semiopaque settings, in order to collect information. In late rounds, more information about value and demand has already been impounded into market prices (see Figure 1), and there are few remaining rounds over which to recoup the costs of collecting that information. Therefore market makers should not be willing to pay as large a premium for order flow in later rounds. In Mad-

Table 4
Average trading gains

Participant type	Opaque setting	Effect of quote disclosure (t-statistic) p value	Semiopaque setting	Effect of trade disclosure (t-statistic) p value	Transparent setting
Market makers	−146.4	**222.4 (0.961) ns**	76.0	**636.4 (3.965) p = .001**	712.4
Constant traders	−735.2	**−116.7 (−1.155) ns**	−852.0	**−273.6 (−2.574) p = .007**	−1125.6
Small active trader	−21.1	**11.0 (0.331) ns**	−10.1	**0 (0) ns**	−10.1
Large active trader	−35.5	**−69.1 (−1.283) ns**	−104.6	**−0.7 (−0.015) ns**	−105.3
Informed traders	938.3	**−47.6 (−0.414) ns**	890.7	**−362.1 (−4.142) p = .001**	528.6

This table presents the average trading gains for each of the five types of participants. Market makers compete to set prices at which the other participants may trade. Constant traders are computerized participants who trade shares in each round; one constant trader always buys 20 shares, while the other always sells 20 shares. The small (large) active trader must raise or invest 2,000 (4,000) francs over the five rounds of trading, or suffer a substantial penalty. The informed traders are computerized participants who always buy at prices below true value and sell at prices above true value. There are two market makers, two constant traders and two informed traders. The numbers in the table depict *combined* winnings of both participants in each of these groups.

T-statistics on the effects of quote and trade disclosure are computed by first taking the average of all observations for a given security in each setting, which results in a single (averaged) observation in each setting for each security. The statistical analysis of the effect of quote disclosure is based on the 42 paired differences (one for each security) between the opaque and semiopaque settings. The statistical analysis of the effect of trade disclosure is based on the 42 paired differences (one for each security) between the semiopaque and transparent settings.

havan's analysis, market makers still compete such that overall expected profits are zero, a result we investigate in the next section.

3.3 Winnings

The effect of trade and quote disclosure on informational efficiency and spreads suggests that these treatments will also affect the distribution of trading gains and losses across various classes of participants. These trading gains are the focus of several of our hypotheses, and we now turn to their testing. Summary information on winnings (given in francs) is provided in Table 4.

The trading gains and losses of liquidity and informed traders are the focus of our Hypotheses 3 and 4. Hypothesis 3 addresses whether transparency affects the welfare of informed traders, while Hypothesis 4 addresses whether it affects the welfare of uninformed traders. Turning first

to the informed traders, we find results that accord well with theoretical predictions. Informed traders' gains are 938.3 francs/security in the opaque market and 890.7 francs/security in the semiopaque market, with the two numbers statistically indistinguishable. In contrast, informed traders gain only 528.6 francs/security in the transparent setting, significantly less than in the semiopaque setting (t-statistic on difference $= -4.142$, $p < .001$). This reduction in earnings appears to be due partly to faster information revelation and partly to wider spreads. Thus our results strongly reject Hypothesis 3.

The liquidity traders' outcomes are more unexpected. Theory predicts that liquidity traders will on average lose relative to more informed traders, and this is confirmed by the data. Liquidity traders lose in every market setting, but transparency affects the size of the losses. The liquidity traders' losses are insignificantly different across the opaque and semiopaque settings, but they are significantly larger in the transparent setting. This is the opposite direction of what is predicted by extant theories, but it is consistent with the spread effects found earlier. Thus these results strongly reject Hypothesis 4 that transparency has no effects.

We view these results on the welfare effects of transparency as both intriguing and important. While transparency does reduce the gains of informed traders, it does not correspondingly increase the gains of uninformed traders as is typically surmised. To understand this result better, we examine in Table 4 how transparency affects the winnings of the active (liquidity) traders, who can choose how to time their trades. We also test Hypothesis 5 that transparency does not affect large uninformed traders.

The results show that not all liquidity traders fare the same. Traders with small liquidity targets lose 21.1 francs/security in the opaque setting, 10.1 francs/security in the semiopaque setting, and 10.1 francs/security in the transparent setting. However, none of the differences are statistically significant. Traders with large liquidity targets lose 35.5, 104.6, and 105.3 francs/security in the three settings. The pattern of losses for the large traders is more in line with theoretical predictions, but, while stronger, the results are still not statistically significant. Thus we fail to reject Hypothesis 5 that transparency has no effect on large uninformed traders.

That transparency differences do not affect active traders is an intriguing and important result.[18] Why this occurs is debatable, but one possibility is that active traders use the timing of their trades to offset transparency differences.[19] As support for this conjecture, we first examined the number of shares large and small active traders choose to trade in the first two rounds of trading. Table 5 reports that large traders trade approximately 14

[18] Our results that active traders are essentially unaffected by transparency changes may provide a partial explanation for Board and Sutcliffe (1996) and Gemmill's (1996) findings that changes in trade reporting rules on the London Stock Exchange do not appear to have much effect.

[19] Such timing effects are predicted by the work of Foster and Viswinathan (1990).

Table 5
Number of shares traded in the first two trading rounds of each market

Trader type	Opaque setting	**Effect of quote disclosure (t-statistic) p value**	Semiopaque setting	**Effect of trade disclosure (t-statistic) p value**	Transparent setting
Small active trader	11.9	**−0.9 (−0.666) ns**	11.0	**−3.0 (−1.994) p = .053**	8.0
Large active trader	14.3	**0.389 (0.289) ns**	14.6	**−0.345 (−0.227) ns**	14.3

This table depicts the number of shares traded by each type of active trader in the first two rounds of trade of each market in each of three settings.

T-statistics on the effects of quote and trade disclosure are computed by first taking the average of all observations for a given security in each setting, which results in a single (averaged) observation in each setting for each security. The statistical analysis of the effect of quote disclosure is based on the 42 paired differences (one for each security) between the opaque and semiopaque settings. The statistical analysis of the effect of trade disclosure is based on the 42 paired differences (one for each security) between the semiopaque and transparent settings.

shares/security in the first two trading rounds in each of the three settings, with no significant differences across the settings. However, small traders trade only 11.9 shares/security in the first two trading rounds in the opaque setting, compared to 11 shares in the semiopaque setting, and 8 shares in the transparent setting. The difference between the transparent and semiopaque settings is significant (t-statistic on difference $= -1.994$, $p = .053$), as is the difference between the transparent and opaque setting (t-statistic on difference $= -3.348$, $p < .001$).

One motivation for this behavior may be an ability by active traders to recognize when spreads are higher or lower than average. To investigate this possibility, we calculated the correlations between the trades of active traders and spreads. The results confirm our intuition: spreads and active trades are negatively correlated ($\rho = -.41$, $p = .0001$), suggesting that active traders delay trading when spreads are large. This negative correlation is found in every round but round 5 (the last round), a result consistent with traders having to meet targets in the final round regardless of spreads. We also found that the sensitivity of trades to spreads differed for large and small traders. Looking at round 1, the correlation of spreads and large trader trades was $-.12$ ($p = .03$), while the correlation of spreads with small trader trades was $-.28$ ($p = .001$). Thus, not surprisingly, traders with small trading needs were better able to time their trades than were traders with large needs.

Finally, we test Hypothesis 6, that market-maker rents are zero in all trading regimes. Consistent with this hypothesis, the market makers appear to be in a competitive equilibrium in the opaque setting, with market makers losing an average of only 146.4 francs/security, not statistically dis-

tinguishable from zero. Quote disclosure has no significant effect on market-makers' winnings: market makers gain an average of 76 francs/security in the semiopaque setting, again not significantly different from zero ($p > .5$), and not statistically different from the losses in the opaque setting.[20] The failure of quote disclosure to affect market-maker winnings is consistent with its failure to affect either price efficiency or bid-ask spreads.

Trade disclosure has a dramatic impact on market-makers' winnings. In the fully transparent setting, market makers earn average trading gains of 712.4 francs/security, significantly greater from zero ($p < .001$) and significantly greater than the winnings in the semiopaque setting ($p < .001$). This is a strong rejection of Hypothesis 6, and it suggests that disclosing trades apparently reduces the need for market makers to compete for order flow by narrowing spreads, allowing market makers to earn greater returns.

The effect of trade disclosure on market-makers' winnings represents a significant departure from standard microstructure theory. Microstructure models with competing market makers typically assume that those market makers earn zero returns. Such models therefore predict that trade disclosure affects only the distribution of gains across liquidity and informed traders, and has no effect on the gains of market makers. That this does not hold in transparent markets suggests that market makers are setting prices in ways not envisioned by the standard models, and it is to this issue that we now turn.

3.4 Capturing order flow

We now examine more carefully whether trade disclosure widens spreads because it reduces the benefit to capturing order flow. To test this conjecture, we focus on those markets in which one market maker captures all of the order flow in the first round of trading. (In the remaining markets, one market maker traded with buyers, while the other traded with sellers.) There are a total of 63 such markets in the opaque setting, 62 in the semiopaque setting, and 51 in the transparent setting; of the 42 securities, there is at least one such market for 39, 38, and 34 securities in the opaque, semiopaque, and transparent settings, respectively. As we have done throughout our analysis, we use a t-test (over the available securities) to compute statistical significance on the differences across settings. We call the market maker who captures all order flow in the first round of trading the "capturing" market maker and call the dealer who did not trade in the first round the "nontrading market maker."

Table 6 provides striking evidence on the informational advantages of capturing first-round order flow. The top two panels report the changes in estimate accuracy of the capturing and nontrading market makers over the

[20] Thus in both the opaque and semiopaque settings, markets appear to be in competitive equilibria with market makers earning on average zero profits. This result accords well with the predictions of theoretical models.

course of trading. In the transparent setting, nontrading market makers increase the accuracy of their value estimates by 12.56 francs, a change that is statistically significant. In the semiopaque setting, by contrast, nontrading market makers improve the accuracy of their value estimates by only 2.17 francs. This increase is statistically significant, and the difference between the increases is also significant. This shows that nontrading market makers who cannot observe first round trades are at a severe disadvantage in the second round. However, the disadvantage essentially disappears in later rounds—the disclosure of second round quotes apparently eliminates the informational advantage of the capturing market maker. These results contrast with those of the capturing dealers, who increase their estimate accuracy by roughly the same amount in all three settings. The size of the accuracy increase in round 2 is also larger for the capturing dealers than the nontrading dealers in all but the transparent setting. Thus capturing order flow provides a dealer with valuable information, and this allows him to improve the accuracy of his value estimates over dealers who do not see the order flow.

The next panel of Table 6 shows how the informational disadvantage of the nontrading market makers affects their quotes. In the transparent setting, nontrading market makers reduce their spread by a statistically significant 23.75 francs in the second round. In the semiopaque settings, by contrast, they reduce their spreads by an insignificant 0.79 francs. The significant difference in spread reduction suggests that market makers not observing first round trades are unable to narrow their quotes to remain competitive. The fourth panel of Table 6 shows one important difference between capturing and nontrading dealers: the latter decrease their spreads much more in the transparent markets. However, this result is probably driven by the fact that capturing dealers set much narrower spreads in the first round and are thus unlikely to reduce the spreads by as much.

The next panel of Table 6 shows how the better information and narrower spreads of the capturing market makers create trading gains. This part of the table reports the difference in trading gains enjoyed by the capturing market maker, minus the gains enjoyed by the nontrading market maker. In all three settings, there is a statistically significant benefit in round 2 to capturing order flow in all round 1. However, the benefit of 454.3 francs in the semiopaque settings is significantly larger than the benefit of 138.5 francs in the transparent setting. This suggests that the second-round benefit to capturing order flow is significantly larger when nontrading market makers are unable to observe the capturing market maker's first round trades. In the semiopaque and transparent settings, the benefits to capturing order flow are statistically insignificant after the second round. This suggests that the capturing market-maker's information is largely revealed by his or her posted quotes in the second round. However, the benefit remains significant through the third trading round in the opaque setting, in which quotes are never disclosed.

Table 6
Benefit of capturing round 1 order flow

	Opaque setting	**Effect of quote disclosure**	*Semiopaque setting*	**Effect of trade disclosure**	*Transparent setting*
Accuracy increases for market makers who capture all order flow in round 1					
Round 2	**9.14**	3.43	**12.57**	1.37	**13.94**
Round 3	**3.94**	−0.67	**3.27**	1.61	**4.88**
Round 4	2.15	1.48	**3.63**	−2.62	1.01
Round 5	**2.71**	−0.89	1.82	0.18	**2.00**
Accuracy increases for market makers who do not trade in round 1					
Round 2	2.08	0.09	**2.17**	**10.39**	**12.56**
Round 3	**11.24**	1.21	**12.45**	−5.46	**6.99**
Round 4	**4.73**	1.08	**3.65**	−2.25	**1.40**
Round 5	**2.19**	0.06	**2.25**	−2.06	0.19
Spread decrease for market makers who capture all order flow in round 1					
Round 2	0.70	−3.71	−3.01	**13.17**	**10.16**
Round 3	**4.40**	3.62	**8.02**	0.19	**8.21**
Round 4	**3.03**	2.52	**5.55**	−1.01	**4.54**
Round 5	**4.21**	−1.59	**2.62**	−1.06	**1.56**
Spread decrease for market makers who do not trade in round 1					
Round 2	**7.88**	**−7.09**	0.79	**22.96**	**23.75**
Round 3	**12.68**	4.37	**17.05**	**−7.8**	**9.25**
Round 4	**6.62**	−0.88	**5.75**	−1.25	**4.49**
Round 5	**2.86**	0.75	**−3.61**	−1.94	1.67
Excess winnings of market makers who capture all round 1 order flow					
Round 1	**137.9**	41.5	**179.4**	119.0	**298.4**
Round 2	**715.7**	−261.4	**454.3**	**315.8**	**138.5**
Round 3	**289.7**	−218.8	70.9	−91.8	−20.93
Round 4	37.3	−31.1	6.6	−30.9	−24.3
Round 5	97.5	−75.6	21.9	−49.5	−27.6
Total gains of market makers					
Round 1	**105.1**	54.0	**159.1**	**166.3**	**325.4**
Round 2	**−151.0**	171.8	20.78	**76.7**	**97.5**
Round 3	−36.6	50.5	13.9	49.3	**63.2**
Round 4	−37.4	60.6	23.2	59.0	**82.2**
Round 5	−26.5	45.2	18.7	70.0	**88.7**

This table presents the effects of capturing all order flow in round 1 of a market. A market maker is said to capture all order flow in round 1 if the market maker sets the market bid *and* the market ask in round 1, so that the other market maker does not trade that round. The benefit to capturing order flow in any given round is defined as the winnings of the capturing market maker minus the winnings of the other market maker through those rounds. Estimate accuracy is measured as the absolute difference between the market-maker's value estimate and the true value of the security.

T-statistics on treatment means and the effects of quote and trade disclosure are computed by first taking the average of all observations for a given security in each setting, which results in as a single (averaged) observation in each setting for each security. In the opaque, semiopaque, and transparent settings, this yields 39, 38, and 34 observations, respectively. **Bold** text indicates a cell mean that is significantly different from 0 at the $p < .05$ level (two-tailed).

The last panel of Table 6 reports the total market-maker winnings in each of the five rounds of trading. The data show that market makers in all three settings actually benefit *in round 1* from capturing order flow in that round. Differences in skill levels or aggressiveness between subjects may account for this, as more skilled dealers would be expected to make greater profits.

Trade disclosure increases the benefit to capturing first-round order flow, demonstrating the informational role conveyed by orders.

An alternative explanation for the wider opening quotes in the transparent markets is that market makers may be able to use trade information to maintain a collusive outcome. While we cannot definitively refute this alternative explanation, two aspects of the data make it seem unlikely. First, if market makers are able to collude to set wide opening spreads, one would expect them to be able to maintain wider spreads in later trading rounds as well.[21] However, spreads are similar and small after the second trading round, a result more consistent with an information-demand explanation. Second, the data show that quote disclosure has no effect on opening spreads. However, such information should serve as a useful coordinating device, and allow colluding market makers to widen spreads.[22] Given that quote disclosure has no effect, an improved ability to collude seems an unlikely explanation of the effect of trade disclosure on opening quotes.

3.5 Experience effects

As the final stage of our analysis, we examine whether the effects of quote and trade disclosure change as the subjects become more familiar with their tasks. We find very little evidence of such "experience effects." When we delete the first 18 securities traded by each cohort, our results are qualitatively unchanged—results that are (are not) significant when examining all of the data remain significant (insignificant) when examining only the last 24 securities. We also include a dummy variable for experience that takes on a value of 0 for the first 18 securities and a value of 1 for the remaining 24 securities. We then use t-tests to determine whether treatment effects differ across early and late securities. We find no evidence of experience effects in price errors, spreads, market-maker winnings, or benefits of capturing first-round order flow. We do find that experience actually *reduces* the extent to which small liquidity traders delay their trades in response to trade disclosure, even though this change would reduce their winnings. Given that this is the only significant experience effect among the many we examined, we believe it reflects nothing more than a statistical fluke.

4. Conclusions and Policy Implications

Who wins and who loses with market transparency? As we show in this research, the answers are complex, reflecting the multiple influences that market transparency has on market performance. We find that trade disclosure increases the informational efficiency of transaction prices, with

[21] Such persistence effects are found, for example, in Bloomfield and O'Hara (1997) where they investigate the effects of order preferencing on market behavior.

[22] For more discussion of collusion, see Cason (1996) who investigates collusive behavior in an experimental setting in which dealers are allowed to explicitly communicate before setting price quotes.

prices converging faster in more transparent markets. This increased efficiency is accompanied, however, by increases in opening bid-ask spreads and, to some extent, later spreads as well. This occurs primarily because of market-makers' reduced incentive to compete for order flow. As a result, trade disclosure introduces welfare effects into the market, with market makers benefiting at the expense of liquidity traders and informed traders. We also find that quote disclosure has no discernible effects on market performance.

From a theoretical perspective, these results provide both reassurance and concern. That our experiments confirm the predictions of a number of extant theories is encouraging. Indeed, we find strong support for theoretical predictions of informational efficiency effects, informed trader theories, and overall pricing behavior. But our work also reveals a number of areas where theoretical models fall short. It is not the case in our research that market makers earn zero profits across trading regimes. That the market makers use trade information to, in effect, substitute for price competition is a particularly important divergence from some established theories. Similarly, that uninformed traders do not benefit from increased transparency is at variance with virtually all model predictions. This suggests that models of market behavior may need to include a richer set of strategies and trading environments if they are to capture actual market behavior.

As with any microstructure research study, the results we obtain may not generalize to markets with different trading rules. In particular, trade in our markets takes place in discrete time: all dealers simultaneously choose quotes, which are presented in a batch to all traders. After traders choose their trades, dealers simultaneously revise their quotes. In contrast, the markets in Flood et al. (1997a,b) allow continuous revision and acceptance of quotes. The discrete-time mechanism has several important benefits. First, a discrete-time structure makes it much easier for us to compare our results to extant theory: the discrete-time models of Glosten and Milgrom (1985) and Easley and O'Hara (1987) provide a natural point for comparison. Second, it allows us to identify more clearly how information events (such as the disclosure of trades in a particular round) affect quotes, trades, and value estimates in each subsequent round.[23] Third, discrete time dramatically improves our ability to perform statistical tests on the informational effects of transparency because we can compare behavior in identical trading rounds across the different settings. In continuous time, transparency might have effects on trading frequency [as in Flood et al. (1997a)] that would interfere with inferences about informational effects. Finally, discrete time allows us

[23] For example, we are able to show in Table 6 that dealers who capture order flow in the first round benefit by having better information in the second round. We also show that this benefit disappears in the third round of the semiopaque setting, because the informational advantage is largely eliminated when second-round quotes are displayed. This detail allows us to provide an explanation of the results that would be difficult to provide in a continuous-time market.

to avoid the many statistical problems that arise when a small number of traders take hundreds or thousands of actions during a short length of time. Because these actions are statistically dependent in continuous markets, the appropriate statistical treatment is unclear.

A discrete-time market structure also has potential costs; in particular, our results might differ in a more realistic continuous-time setting. However, switching to such a setting would appear to change only the magnitude of the informational forces that drive our results, and would not change their fundamental nature. Thus we would not expect our results to change dramatically. Flood et al. (1997b) provides support for this position. That study finds that trade disclosure in a continuous-time market increases informational efficiency, dealer profits, and opening bid-ask spreads. Thus we believe that our results will generalize reasonably well to continuous-time markets.

Our research has a number of implications for regulatory policy. We find that transparent markets are more informationally efficient. If informational efficiency is viewed as the goal of market design, then the clear "winner" is the most transparent market. But as our results make clear, this conclusion may be too simplistic. The enhanced informational efficiency of the transparent market is purchased at the expense of greater transactional inefficiency. In particular, because increased transparency reduces the market-makers' need to compete for order flow, they increase spreads and therefore trading costs.

This trade-off between transactional versus informational efficiency suggests that the crucial issue in market design may be the degree of transparency rather than its absolute attainment. From a traders' perspective, transactional efficiency is of paramount importance, and it is natural for trade to gravitate to centers that provide it. This suggests that the SEC's endorsement of transparency noted in the introduction may be short-sighted; transparent markets need not be more efficient, nor does transparency necessarily enhance market competitiveness.

That transparent markets are not always welfare improving is an equally important implication of our research. From a welfare perspective, the clear "winners" from transparency were the market makers; the "losers" were the traders without the ability to time their trades. These include both informed traders and those uninformed traders with immediate liquidity needs. The active liquidity traders were unaffected by transparency, suggesting that patient active traders can basically take care of themselves. These results suggest a policy prescription: if the number of immediate liquidity traders is large, or if productive decisions depend on private information, then opaqueness of markets may be preferred. Otherwise, transparency may be preferred.

More immediate policy implications also arise from these findings. Our finding that liquidity may be enhanced for large uninformed traders when markets are less transparent suggests that large trade reporting protocols

may have some value in promoting market liquidity. Our results also suggest why it need not be surprising if multiple market makers, as are found on the NASDAQ, set wider spreads than would be expected in a competitive market: the ability to learn from trade and quote information can reduce the need to compete via prices

Recently transparency issues have arisen both in the United States and elsewhere with respect to whether trades should be internalized or not, whether price improvement schemes impede or enhance transparency, whether after-hours trading is permissible, whether limit orders must be exposed, and whether reporting protocols should differ for proprietary trading systems. While the complexity of these individual questions is beyond our analysis here, the disparate effects detailed above suggest that policy decisions regarding market transparency issues involve a complexity not captured by the SEC's blanket endorsement of transparency. We believe that experimental studies provide a valuable tool for evaluating the overall effects of various market regimes.

Finally, we end with a discussion of some future research. Our experiments investigate the effects on market equilibrium under differing degrees of market transparency. Having established substantial differences in market behavior, the natural question arises whether one market type is likely to dominate another. Investigating this requires allowing markets of differing transparency to coexist and compete. Such a topic is well suited to exploration in an experimental market setting, where both traders and dealers can be allowed to choose their markets endogenously. We believe this research will provide additional insights into the effects of transparency on market behavior.

Appendix A: Laboratory Stock Market—Instructions to Participants

Overview

In this market, you will trade shares of a number of securities. Each security has a value denominated in a laboratory currency called "francs." The value of each security is either 110, 130, 150, 170, or 190 francs, with each value equally likely. Francs are converted into cash at the end of the session.

Types of Traders. The market includes four types of traders:

Market Makers, two participants who set prices at which they will buy and sell shares.

Active Traders, two participants who must raise or invest francs by trading in each security, but can choose when to do so.

Informed Traders, two computerized participants who buy whenever a market maker is willing to sell for a price below value, and sell whenever a market maker is willing to buy for a price above value.

Constant Traders, two computerized participants, one who buys and one who sells a constant number of shares in each round of trading at the most favorable price.

Sequence of Events. The sequence of events in the market is as follows:

Start Trading a New Security
Active Traders observe the number of francs they must raise or invest.
Informed Traders (computerized) observe true value.

Trading round (five rounds per security)

- Computers reveal information on others' actions in the previous round.
- Market makers quote prices at which they will buy and sell.
- Traders buy or sell at the market-makers' quotes.

End Trading in Security
All participants observe the true security value and their gain or loss from trading in that security.

The Market-Makers' Task. If you are a market maker, you will set three numbers in each round of trading:

Ask Quote: The price at which you will sell a share.
Estimate: Your best estimate of value.
Bid Quote: The price at which you will buy a share.

A quote is a promise to buy or sell as many shares as traders wish to trade at that price in that round; at the end of that round, you may set new bid and ask quotes. The bid and ask must lie between 100 and 200, inclusive. The estimate must lie in between the ask and the bid.

The Active Traders' Task. If you are an active trader, you must enter your estimate of the security's value each time you observe the market-makers' bid and ask quotes. You then must decide whether you wish to buy at the lower of the market-makers' asks or sell at the higher of the market-makers' bids (you may not do both in one round). You may trade up to 10 shares in each round.

For each security, each active trader must raise or invest some minimum number of francs. This minimum number is referred to as a "target." Whether a given trader must raise or invest francs is determined randomly and independently for each trader. However, in each market, the target is always 2,000 francs for one trader and 4,000 francs for the other. Traders are penalized 3 francs for each franc they fail to raise or invest. There is no penalty for exceeding the target (i.e., raising or disposing more than required).

Computerized Traders. There are two types of "computerized traders" who can enter orders to buy and sell at the market-makers' quotes. When computerized traders trade, they always buy at the lowest ask, or sell at the highest bid.

The Informed Traders act as if they know the true value of the security. They each buy 10 shares whenever a market maker sets an ask below true value, and sell 10 shares whenever a market maker sets a bid above true value.

The Constant Traders each trade 20 shares in each period. One constant trader always buys 20 shares, while the other always sells 20 shares.

In the event of a tie. If the two market makers set the same bid, all traders who wish to sell will sell shares to the market maker whose quotes were received first. Similarly,

if the two market makers set the same ask, all traders who wish to buy will buy shares from the market maker whose quotes were received first.

Incentives. Market participants earn francs in three ways: by trading, by accurately estimating the value of the security, and by avoiding penalties for not meeting liquidity targets.

Trading. Whether you are a market maker or an active trader, every share you buy increases or decreases your wealth in francs according to the formulas:

$$\begin{aligned} \text{Buying:} &\quad \text{Change in wealth per share} = (\text{Value} - \text{Price}) \\ \text{Selling:} &\quad \text{Change in wealth per share} = (\text{Price} - \text{Value}) \end{aligned}$$

Estimates. In each round of trading, if your estimate is exactly right, you will earn a 50 franc bonus. For every franc your estimate deviates from true value, your bonus will decrease by 2 francs; thus if your estimate is off by 13 francs, you will earn a bonus of 50 - 2*13 = 24. If your estimate is off by more than 25 francs, you earn no bonus (but you are not penalized).

Penalties for not meeting trading targets. Active traders are penalized 3 francs for each franc they fail to raise or invest. This penalty is large enough that active traders are almost always better off raising or investing the target number of francs, no matter how large the trading losses resulting from doing so. Of course, it is still better to minimize such losses or maximize trading gains, as long as this does not interfere with meeting the target. There is no penalty for buying or selling more than required.

Converting Francs to Dollars. Your cash winnings for each session will be determined by the number of francs you won in that session, relative to the average francs won by participants in other markets who play the same role as you. If you are a market maker, you split $50 with a market maker from another market, with the split proportional to the number of francs you each earn. Similarly, active traders' winnings are compared to active traders' winnings in another market. Before computing proportions, we add a positive number to everyone's earnings in francs. Thus, even if you are losing francs, you can earn more money by losing fewer francs than the participant you are being compared to. Note that by this method, **your winnings are not affected by whether the other traders in your group do well or poorly.**

Winnings are determined separately for each session. Losses in one session do not offset gains in another session. No matter what happens, you will receive at least $5 for completing each session.

Watching the Market. After every round of trading, market makers and traders will observe something about other participant's actions. Exactly what you see will depend on which of three settings you are in.

Setting 0:

- Neither market maker observes the other's quotes or trades.
- Each trader observes both market-makers' quotes, but not their trades.

Setting 1.

- Each market maker observes the other's quotes, but not their trades.
- Each trader observes both market-makers' quotes, but not their trades.

Setting 2:

- Each market maker observes the other's quotes and trades.
- Each trader observes both market-makers' quotes and trades.

You will trade different securities in different settings. You will always be told whether you are in setting 0, 1, or 2; you may play in these settings in any order, so pay careful attention to your instructions.

I consent to participate in this experiment, and agree to abide by all of the rules determined by the experiment coordinator throughout my participation. I recognize that (1) if I breach any of the rules governing the market, I forfeit my right to any money I might have earned by participating; (2) I have the right to leave the experiment at any time, without penalty, but that in doing so I forfeit my right to any money I might have earned by trading; (3) this experiment has been approved by the Cornell University Committee on Human Subjects as research that uses no deception of any kind.

Signature _____ Date _____

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