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ABSTRACT

We analyze the rationale for limit order trading. Use of limit orders involves two risks: 1) an adverse information event can trigger an undesirable execution, and 2) favorable news can result in a desirable execution not being obtained. On the other hand, a paucity of limit orders can result in accentuated short-term price fluctuations that compensate a limit order trader. Our empirical tests suggest that trading via limit orders dominates trading via market orders for market participants with relatively well balanced portfolios, and that placing a network of buy and sell limit orders as a pure trading strategy is profitable.

AN IMPORTANT DESIGN FEATURE of a securities market is whether it is quote driven (such as Nasdaq in the United States and SEAQ in London), order driven (such as the Tokyo Stock Exchange), or both (such as the New York Stock Exchange, which has a specialist system, a public limit order book, and floor traders). A market is *quote driven* if dealers announce the prices at which other market participants can trade; it is *order driven* if some investors, by placing limit orders, establish the prices at which other participants can buy or sell shares. Although limit orders are routinely submitted to markets such as the New York and Tokyo Stock Exchanges, their desirability for investors has received little attention in the literature until recently.[1]

This article analyzes the rationale for, and profitability of, limit order trading. The issue is clearly important to market participants individually. It is also important from a market structure point of view: the profitability of limit order trading is essential to the viability of an order driven market. Moreover, an understanding of the order flow dynamics that make limit order trading viable enhances our knowledge of how a market's microstructure affects the return generation process.

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[1] Glosten (1994) examines the role of limit orders in the context of an open electronic book. In a recent working paper, Harris and Hasbrouck (1993) empirically assess the profitability of limit orders versus market orders on the New York Stock Exchange.

In a world where transaction prices move solely in response to information, trading via limit order is costly because the individual who places a buy (sell) limit order has written a free put (call) option to the market. Consider an investor who wishes to buy shares of xyz common at $50 or better, and let that investor select between submitting a market order (that would execute, say, at $50) and submitting a limit order at $49. If news causes the share price to fall below $49, the option will be exercised, and the individual can lose from trading with a better informed investor. Alternatively, the price of shares on the market may not fall to $49, and the individual might miss the investment opportunity.

Copeland and Galai (1983) address the adverse selection problem from the viewpoint of a dealer. Building on Bagehot (1971), they show that the market maker's spread generates returns that can cover the cost of trading with informed participants. Limit order traders resemble dealers in that they provide liquidity and immediacy to the market. However, the primary objective of limit order traders is to implement their investment decisions, and they do not continuously post two-sided quotes.

In a recent article, Glosten (1994) derives an equilibrium where limit order traders implicitly gain from liquidity driven price changes but lose from information driven price changes. Glosten assumes two distinct classes of participants, those who trade by limit order and those who trade by market order, and does not model a participant's decision to trade via limit order or market order. In this article, we suggest that Glosten's analysis can be extended to consider the choice faced by an investor who wishes to buy or sell a share of the risky asset over a trading window.

Our investor can choose to trade via limit order and supply liquidity to the market or choose to trade via market order and demand liquidity from the market. The choice depends critically on the investor's beliefs about the probability of his or her limit order executing against an informed or a liquidity trader. Transaction price changes due to the arrival of a liquidity trader are temporary and reversible, and having a limit order execute against such price changes is desirable.[2] In contrast, transaction price changes due to the arrival of an informed trader are permanent and irreversible, and having a limit order execute against such price changes is undesirable. In addition, if a limit order fails to execute in a trading window, a decision has to be made at the end of the window of whether to trade at the prevailing transaction price or to forego trading, and this decision has cost implications.

We endogenize the decision to trade via market or limit order and view the trading environment as an ecology where the supply of, and demand for, liquidity can be in natural balance. We conduct empirical experiments that use this conceptualization to assess the relative profitability of limit and market order trading.

[2] That the transaction price can change without change in expected future payoffs is also found in Easley and O'Hara (1987), Grossman and Miller (1988), Glosten (1989), and Leach and Madhavan (1993), among others.

Our empirical tests use 1988 transaction price data for the thirty Dow Jones Industrial stocks. The market order strategy involves buying or selling at market at the start of a trading session. The limit order strategy involves placing an experimental limit order that is subsequently converted to a market order if it does not execute within a specified period of time.[3] We find that returns to the limit order strategy conditional on the orders executing are greater than unconditional market order returns. We also find that returns to the limit order strategy conditional on the orders not executing are lower than the unconditional market order returns. Finally, we assess the profitability of placing a network of buy and sell limit orders around an asset's current price, and find evidence of what we call a *limit order spread* that is larger than the spreads posted on the market.[4]

The article is organized as follows. In Section I, we examine the rationale for trading via limit orders. Section II contains empirical evidence on the profitability of the limit order versus market order trading strategy. In Section III, we examine a multiple limit order strategy (buy and sell) and empirically document the existence of a limit order spread. Our conclusions are presented in Section IV.

I. A Rationale for Limit Order Trading

A. The Analytical Framework

In this section we present an analytical framework to assess an investor's decision to trade via limit order versus market order. Glosten (1994) provides a rationale for limit order trading. In his framework, there are two types of investors: patient traders, who supply liquidity to the market, and other traders, who wish to trade immediately. The former place limit orders, and the latter place market orders. A limit buy (sell) order trader can expect to lose if the order executes upon the arrival of an informed trader with a valuation below (above) the limit price, and can expect to gain if the order executes upon the arrival of a liquidity trader. The trader will not choose to place a limit order unless the expected gain from transacting with a liquidity trader exceeds the expected loss from transacting with an informed trader.

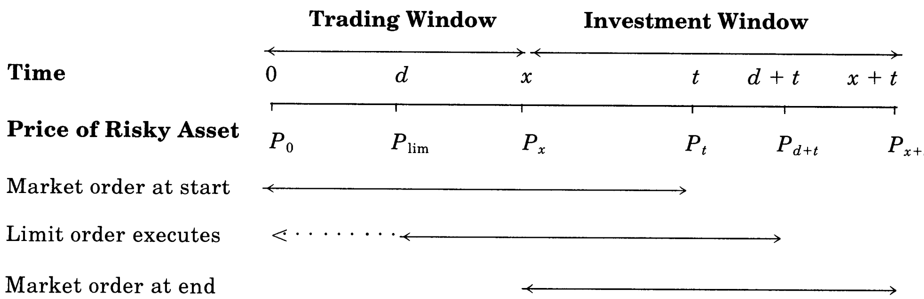
Glosten (1994) does not explicitly model an investor's decision to trade via a limit order or a market order. In what follows, we suggest that his analysis can be extended to do this. We consider an investor who wishes to buy a share of the risky asset over a trading window (the analysis for a sell order is symmetric). Let the trading window be followed by an investment period of length t.[5]

[3] In contrast, Harris and Hasbrouck (1993) base their tests of limit order trading on orders that have actually been submitted to the market.

[4] The documentation of the limit order spread is consistent with Grossman and Miller (1988), who postulate that a market maker can earn a different amount than the spread quoted at the time of the placement of the limit order.

[5] Assuming that the stock is held over an investment period of a fixed length t facilitates comparing returns across different strategies.

Our investor faces the following microstructure. The opening price of the trading window is P_0. A limit order book exists at the start of the trading window and a trade is made during the window if a public market order arrives. For simplicity we assume that just one public trader, who is either informationally motivated or liquidity motivated, arrives during the trading window to buy or sell at market. That trader's order executes against the book and moves the transaction price to P_x, which holds at the end of the trading window. If the arriving trader is informed, the security's price conditional on the new information is P_x. If the arriving trader is liquidity motivated, the security's price conditional on the existing information is the opening price, P_0.[6] The following schema represents the sequencing of events:



Consider the situation faced by the investor at the start of the trading window. If he or she uses a market order, the trade occurs immediately at the price P_0. The position is then sold at a price P_t after an investment period of length t. If the investor uses a limit order, the trade occurs within the trading window if a market order to sell clears the book down to a transaction price at or below the limit price P_{lim}. Otherwise a trade fails to occur within the trading window. If that happens, a second decision must be made: 1) withdraw the order and not trade, or 2) buy by market order at the last price of the trading window.

We write the closing price P_x for the trading window as S for notational convenience and denote the probability density function of S, conditional upon the arrival of an informed trader, by $f(S|I)$ and the probability density function of the last price S, conditional upon the arrival of a liquidity trader, by $f(S|L)$. We let F denote the probability that the last price of the trading window is equal to or below P_{lim} conditional upon a liquidity trader arriving. We assume that an informed trader arrives with an exogenously specified probability p, and that a liquidity trader arrives with a probability $1 - p = q$.

[6] The opening price itself may be perturbed by liquidity events. With positive and negative deviations equally likely, the opening price is an unbiased estimate of the true price, but has more variability that is undesirable for a limit order trader. If the opening price is above true price, the limit order is placed too high and is apt to result in an undesirable execution. If the opening price is below true price, the limit order is placed too low and is more apt to not execute. We do not model this variability component here.

When the limit order executes within the trading window and the arriving trader is informed, the expected gain is given by:

$$p \cdot \int_{-\infty}^{P_{\lim}} (S - P_{\lim}) f(S|I)\ \mathrm{d}S \leq 0 \tag{1}$$

Assume that the investment period is long enough for the liquidity imbalance to be corrected. Thus, when the limit order executes because of the arrival of a liquidity trader, our investor's expected gain is:

$$q \cdot \int_{-\infty}^{P_{\lim}} (P_0 - P_{\lim}) f(S|L)\ \mathrm{d}S = q \cdot F \cdot (P_0 - P_{\lim}) \geq 0 \tag{2}$$

Thus, our investor's net expected gain when the limit order executes can be written as:

$$p \cdot \int_{-\infty}^{P_{\lim}} (S - P_{\lim}) f(S|I)\ \mathrm{d}S + q \cdot F \cdot (P_0 - P_{\lim}) \tag{3}$$

Our investor expects to gain from the execution of a limit order only if the expected loss from the arrival of an informed trader is more than offset by the expected gain from the arrival of a liquidity trader. For this to be the case, the probability of the arrival of a liquidity trader with a reservation value equal to or less than the limit price must be sufficiently large. When there are no liquidity driven changes in transaction price to cause the execution of a limit order (i.e., $q = 0$), a limit order trader faces a negative expected gain from limit order execution. We refer to this negative expected gain as a *bagging cost.* On the other hand, sufficient fluctuation in transaction price due to market order trades by liquidity traders (i.e., $q \cdot F$ sufficiently large), can result in a positive expected gain from limit order execution. This positive expected gain corresponds to a negative bagging cost.

We now turn to the case where the buy limit order fails to execute within the trading window. The limit order trader must then choose between 1) foregoing trading, and 2) buying by market order at the last price of the trading window, S. When the arrival of an informed trader results in the last price, the closing purchase is at the true price and the expected gain is zero. On the other hand, when the arrival of a liquidity trader results in the last price, the closing purchase is at a price other than the true price and results in a non-zero expected gain. Ex-ante, when the arrival of a liquidity trader causes S to be above $P_{\lim}$, the closing purchase results in:

$$q \cdot \int_{P_{\lim}}^{\infty} (P_0 - S) f(S|L)\ \mathrm{d}S < 0 \qquad \text{given that} \quad \mathrm{E}(S|L) = P_0 \tag{4}$$

When there are no liquidity driven changes in transaction price (i.e., $q = 0$), a limit order trader faces a zero expected gain from a closing purchase at the end of a trading window. On the other hand, fluctuation in transaction price due to liquidity motivated trades (i.e., $q > 0$) results in a negative expected gain from a closing purchase, which we refer to as a positive *nonexecution cost.* This means that if $q > 0$, an investor will choose to purchase at the end of the trading window only if the loss from foregoing a purchase exceeds the loss from a closing purchase. Hence, the unconditional expected gain from submitting a limit order and subsequently trading by market order if the limit order does not execute, can be written as:

$$\left[p \cdot \int_{-\infty}^{P_{\lim}} (S - P_{\lim}) f(S|I)\, \mathrm{d}S\right] + [q \cdot F \cdot (P_0 - P_{\lim})] + \left[q \cdot \int_{P_{\lim}}^{\infty} (P_0 - S) f(S|L)\, \mathrm{d}S\right]. \quad (5)$$

When there are no liquidity driven changes in transaction price (i.e., $q = 0$), only the first term is nonzero and it is negative. This means that the unconditional expected gains from trading by limit order are negative for all investors and no investor will choose to trade via limit order in the absence of liquidity events.

Now consider the case where transaction prices also fluctuate due to liquidity motivated trades. From the previous expression, the unconditional expected return from trading via limit order is negative.[7] To see this, recall that the first term of the expression is negative and write the last two terms (ignoring q) as

$$\int_{-\infty}^{P_{\lim}} (P_0 - P_{\lim}) f(S|L)\, \mathrm{d}S + \int_{P_{\lim}}^{\infty} (P_0 - S) f(S|L)\, \mathrm{d}S < \int_{-\infty}^{\infty} (P_0 - S) f(S|L)\, \mathrm{d}S = 0 \quad (6)$$

Thus, forced execution leads to a negative expected return, regardless of q, the probability that a liquidity trader arrives during the trading window. Clearly a limit order strategy is suboptimal for a trader who must obtain an execution by the close of the trading window. Nevertheless, the second term of equation (5) (representing gains from the arrival of a liquidity trader) is positive and, with a sufficiently high probability that a liquidity trader will arrive during the trading window, can outweigh the first term (representing losses from the

[7] We thank the referee for this result.

arrival of an informed trader). At this stage, consider an investor whose portfolio is relatively well-balanced at current market prices and whose opportunity cost of not trading is low. We refer to such an investor as a *patient* investor. A *patient* investor will ignore the third term in equation (5), and will choose to trade via a limit order if the gain from the second term exceeds the loss from the first by a sufficient amount to outweigh the expected opportunity cost of not trading at all. The presence of *patient* traders keeps order driven markets from failing and provides liquidity in markets where trading depends on public participants. We suggest that the market can be viewed as an ecology with liquidity-driven price changes being just sufficient for the flow of market and limit orders to be in balance.

We now write the expected returns from the limit order strategy, conditional on execution and nonexecution, relative to the returns from a market order strategy. We use the notation R_{m} for the return to the market order strategy and the notation R_{l} for the return to the limit order strategy. When we condition a return on execution we use the notation e and when we condition on nonexecution we use the notation n. Also $\mathrm{E}(\cdot)$ denotes the expectation operator at the start of the trading window.

When q is zero, our earlier result was a negative expected gain from a limit order execution, i.e., a positive *bagging cost*. The expected gain from a closing purchase was zero, i.e., a zero *nonexecution cost*. Hence, for $q = 0$, the relative return from the limit order strategy, conditional upon execution and nonexecution is:

$$\mathrm{E}(R_{\mathrm{l}}|e) - \mathrm{E}(R_{\mathrm{m}}) \leq 0, \tag{7}$$

$$\mathrm{E}(R_{\mathrm{l}}|n) - \mathrm{E}(R_{\mathrm{m}}) = 0. \tag{8}$$

When $q \cdot F$ is sufficiently large, we have a positive expected gain from limit order execution, i.e., a negative *bagging cost*, and a negative expected gain from a closing purchase, i.e., a positive *nonexecution cost*. Hence, when the probability of arrival of a liquidity public trader is sufficiently large, the return from the limit order strategy, conditional upon execution and nonexecution is:

$$\mathrm{E}(R_{\mathrm{l}}|e) - \mathrm{E}(R_{\mathrm{m}}) \geq 0, \tag{9}$$

$$\mathrm{E}(R_{\mathrm{l}}|n) - \mathrm{E}(R_{\mathrm{m}}) \leq 0, \tag{10}$$

II. Empirical Evidence on Limit Order versus Market Order Trading

Limit orders can capture the higher level of short-run price volatility that is caused by liquidity driven price changes that are temporary, and our experiments may be interpreted as a *joint* test of 1) the limit order trading strategy, and 2) the underlying price process. Our primary objective is, of course, to assess the profitability of limit order trading.

We conduct our experiments by replaying the transaction record and assessing the profitability of entering experimental, one-share market and limit orders. The one-share orders are certainly small enough so that, if they had in fact been entered, they would not have altered the transaction record. Our entries should be interpreted as marginal orders, and their profitability would suggest that the intramarginal orders that did actually execute at the same price were indeed profitable.

Our experiments are run for the thirty Dow Jones Industrial firms that trade on the New York Stock Exchange (NYSE). The primary data source is the 1988 "Trades and Quotes" transaction file for NYSE stocks supplied by the Institute For The Study of Security Markets (ISSM). The importance of these firms and the frequency with which their shares trade make them particularly suitable for testing the limit order model.

A. Experimental Design

Our theoretical model considers the returns realized over an investment window after a purchase has been made using a market order or a limit order strategy. Our empirical analysis, which is based on this framework, requires the specification of three parameters: the length of the trading window (x days), the difference (expressed as a percent) between the current price (P_0) and the limit order price (l percent), and the length of the investment window (t days).[8,9] We examine four limit order trading strategies corresponding to $l =$ 0.5 percent, 1 percent, 2 percent, and 3 percent.[10] We choose a trading window of $x = 1$ day for $l = 0.5$ percent and 1 percent, $x = 2$ days for $l = 2$ percent, and $x = 3$ days for $l = 3$ percent. We avoid a window of less than one day so as to be consistent in determining the opening and closing prices in a trading window. In addition, we use a larger trading window when the limit order is placed further from the opening price so as to allow more time for the liquidity and information events to occur that may result in an execution. We hold the investment window constant at $t = 3$ days to facilitate comparison of returns from the different trading strategies.

In our tests, a market buy order at a purchase price equal to P_0 on day 0 (the first day of the trading window) is compared with a limit buy order placed l percent below P_0 (rounded to the nearest eighth). The limit order is followed until it executes or until the last price in the trading window is reached. If the limit order does not execute during the trading window, the stock is purchased at the opening price on the day following the trading window. For each trading

[8] One of these parameters, the length of the investment window, need not be specified in our multiple limit order tests reported in Section 4 below.

[9] The empirical analysis deviates from the theoretical model insofar as 1) unlike our theoretical framework where only one event is assumed to occur in a trading window, a sequence of events may take place in our empirical analysis, and 2) the empirical analysis uses the opening call price for the subsequent day as the closing price for the end of a trading window.

[10] Supplemental tests for $l < 0.5\%$ and $l > 3\%$, not reported here, yield consistent results.

window, all prices are standardized by setting the opening price on day 0 equal to 100 and rescaling the ensuing price series.

The return to the market order is defined as the log of the opening price on Day 3 minus the log of the opening price on Day 0. The limit order return is defined as the log of the opening price on day $d + 3$ minus the log of the purchase price. For shares bought because the limit order executes during the trading window, the purchase price is either the limit order price (P_{lim}) if the execution occurs during the day or an opening price equal to or less than P_{lim} if the execution occurs at an opening. For $d = x$, the purchase price is the opening price of the day following the trading window.

Transactions data for the first 250 trading days of 1988 are used. The year is partitioned into ten 25-day subperiods. Each subperiod is further subdivided into windows. For the 0.5 percent and 1 percent strategies, there are six windows per stock per subperiod (each window being 4 days long); for the 2 percent strategy, there are five windows per stock per subperiod (each window being 5 days long); and for the 3 percent strategy, there are four windows per stock per subperiod (each window being 6 days long).[11] Subperiod results for each stock form our basic observations. Hence, the aggregate subperiod and full-year results that we report in this article, along with the statistical tests, are based upon the corresponding values for each stock in a subperiod.[12]

B. Transaction Prices

The average standardized purchase prices for the four limit order strategies are shown in Table I. Panel A of the table reports overall and subperiod results for all (executed and unexecuted) limit orders. For the 0.5 percent limit order strategy, the average standardized purchase price ranges from 99.825 to 100.727 for the subperiods, and is 100.119 overall. We test the null hypothesis that the average limit order purchase price is equal to the market order purchase price of 100, both for the individual subperiods and overall. For each subperiod, we run t-tests using the mean and variance of price across firms. None of the subperiod averages is significantly below 100, two are significantly above 100, and the remaining eight differ insignificantly from 100. We also test the hypothesis that the limit order purchase price equals 100 using the grand mean and variance of price across all firms and subperiods and reject it at the 5 percent level. Finally, we run the nonparametric Wilcoxon signed rank test using subperiod means to test the null hypothesis that the overall median purchase price is 100. The p-value is 0.138, which does not enable us to reject the null hypothesis.

For the 1 percent strategy, two subperiod average purchase prices are significantly below 100, two are significantly above 100, and the rest are

[11] For three of the strategies, 0.5 percent, 1 percent, and 3 percent, the last day of each sub-period was discarded.

[12] We also run tests using individual stock returns *within* subperiods as our basic observations, and the results are similar.

Table I

Average Standardized Purchase Price Of A Limit Order For The 0.5%, 1%, 2%, and 3% Limit Order Tests For The Thirty Dow Jones Industrial Stocks in 1988

All prices are expressed as percentages of the opening price at the start of a trading window (which is also the purchase price of the market order). The limit order test parameter sets the limit order price with respect to the opening price. Specifically, the limit price for the $x\%$ limit order ($x = 0.5, 1, 2, 3$) is $x\%$ below the opening price, rounded to the nearest one-eighth. The purchase price of a limit order is its limit price if the stock price crosses the limit price during the trading window, and is the opening price of the day following the trading window if the limit price is not crossed during that period. Subperiod t-tests use observations on the thirty stocks for each subperiod. The overall t-test uses the observations on thirty stocks for ten subperiods. In addition, we report a nonparametric test of the null hypothesis that the median standardized purchase price (of the ten subperiod means) equals 100, the standardized market order price, versus the alternate that it does not equal 100.

		Limit Order Test			
		0.5%	1%	2%	3%
		Panel A: All Limit Orders			
Subperiod	1	100.727**	100.539**	100.249	100.712*
	2	100.021	99.861	100.074	99.543
	3	100.281**	100.281*	100.030	99.787
	4	100.097	100.001	99.361**	99.524*
	5	100.127	100.138	99.878	100.552*
	6	99.825	99.483**	99.492**	98.780*
	7	100.005	99.932	99.655*	100.208
	8	100.082	100.030	100.158	99.796
	9	100.042	99.970	99.589*	100.280
	10	99.907	99.815*	99.772	99.648
Overall 1988		100.119*	100.000	99.814**	99.909
Wilcoxon p-value		0.138	0.958	0.138	0.646
		Panel B: Unexecuted Limit Orders			
Subperiod	1	105.342**	104.331**	102.620**	103.155**
	2	101.193**	100.864**	101.321**	101.166*
	3	101.587**	101.385**	101.736**	101.092
	4	101.107**	100.827**	100.510**	101.320*
	5	101.913**	101.186**	100.835*	102.408**
	6	100.964**	100.366*	100.623*	99.729
	7	101.058**	100.768**	100.868**	101.918**
	8	100.935**	100.665**	101.443**	100.909
	9	101.098**	100.754**	100.566*	101.613**
	10	100.550**	100.327**	100.514**	100.714**
Overall 1988		101.613**	101.156**	101.148**	101.466**
Wilcoxon p-value		0.000	0.000	0.000	0.000

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

insignificantly different from 100. The overall purchase price is 100. For the 2 percent limit order strategy, four subperiod average purchase prices are significantly below 100 and the rest are insignificantly different from 100. The overall purchase price is 99.814, which is significantly different from 100. The 3 percent limit order strategy yields two subperiod averages significantly below 100, two significantly above 100, and the remaining six insignificantly different from 100. The overall purchase price is 99.909 and not significantly different from 100. The Wilcoxon signed rank test gives a p-value of 0.646, and we cannot reject the null hypothesis at the 5 percent level of significance.

The purchase price for limit orders that execute is distributed around $100(1 - l$ percent) because of price rounding, and these values are not shown in the Table. Panel B of Table I gives subperiod and overall means for limit orders that do not execute. Ex-ante, the expected purchase price of an unexecuted limit order, conditional on the fact that price does not drop l percent or more during the trading window, is greater than 100. For the 0.5 percent limit order strategy, the purchase price ranges from 100.550 to 105.342, and has an average value of 101.613, which is significantly above 100. For the 1 percent limit order strategy, the purchase price ranges from 100.327 to 104.331, while the overall average is 101.156, which is significantly above 100. Similar results are obtained for unexecuted limit orders based on the 2 percent and 3 percent limit order strategies; the overall purchase price is 101.148 and 101.466, respectively, and significantly above 100 in both cases.

In sum, the unconditional purchase price of all limit orders is predominantly insignificantly different from the market order purchase price. On the other hand, the purchase price of unexecuted limit orders is almost consistently significantly higher than the overall market order purchase price of 100.

C. Returns

We define the following variables for each stock to measure and to compare returns for the limit and market order strategies over the 3-day investment windows:[13]

$r_{\mathrm{o}}^{\mathrm{m}}$: the unconditional return on the market order strategy.
$r_{\mathrm{o}}^{\mathrm{l}}$: the unconditional return on the limit order strategy.
$r_{\mathrm{e}}^{\mathrm{l}}$: the return on the limit order strategy conditional on execution.
$r_{\mathrm{n}}^{\mathrm{l}}$: the return on the limit order strategy conditional on nonexecution.

Table II presents results on average limit order and market order returns, and on the percentage of limit orders that execute. The returns reported are the average of individual subperiod averages for the ten subperiods in 1988. The unconditional market order return is 0.121 percent and 0.045 percent for the 0.5 percent and 1 percent strategies, respectively, −0.302 percent for the 2 percent strategy, and 0.260 percent for the 3 percent strategy; it is significantly above zero for the 0.5 percent and the 3 percent strategies and is significantly below zero for the 2 percent test.[14] The comparable unconditional limit order return is 0.177 percent for the 0.5 percent strategy, 0.261 percent for the 1

[13] The stock index i is suppressed to conserve notation.

[14] Average market order returns can differ for the four limit order tests because the specific calendar days included in these tests are not identical.

Table II

Average Return On Market Order And Limit Order Purchases For Dow Jones Industrial Stocks In 1988

Stock returns in a subperiod form our basic unit of observation. Hence, average returns on the thirty stocks for the ten subperiods are reported. The limit order test parameter sets the limit order price with respect to the opening price (which is also the price at which the market order executes). Specifically, the limit price for the $x\%$ limit order ($x = 0.5, 1, 2, 3$) is $x\%$ below the opening price, rounded to the nearest one-eighth.

		Limit Order Test			
		0.5%	1%	2%	3%
The average $ size of the limit order test parameter		0.267	0.522	1.026	1.532
Percentage of limit orders that executed		63.441	46.398	39.029	34.589
Average Return					
All market orders	r_o^m	0.121*	0.045	−0.302**	0.260**
All limit orders	r_o^l	0.177*	0.261**	0.175	0.379**
Executed limit orders	r_e^l	0.222*	0.406**	0.213	1.865**
Unexecuted limit orders	r_n^l	0.076	0.149	−0.076	−0.507**

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

percent strategy, 0.175 percent for the 2 percent strategy, and 0.379 percent for the 3 percent strategy, and it is significantly above zero except for the 2 percent test. Hence, the limit order strategy outperforms the market order strategy for all four tests.

Except for the 2 percent test, the average limit order return conditional on execution increases in the percentage parameter, and is always significantly higher than both the unconditional limit order and the unconditional market order return. It is 0.222 percent for the 0.5 percent strategy, 0.406 percent for the 1 percent strategy, 0.213 percent for the 2 percent strategy, and 1.865 percent for the 3 percent strategy; except for the 2 percent test, it is significantly greater than zero. The average limit order return conditional on non-execution is always lower than the overall limit order return, but not necessarily lower than the unconditional market order return. It is 0.076 percent for the 0.5 percent strategy, 0.149 percent for the 1 percent strategy, −0.076 percent for the 2 percent strategy, and −0.507 percent for the 3 percent strategy; it is significantly below zero for the 3 percent test.

Overall, the results are indicative of short-run negative autocorrelation in returns. Stock price changes appear to be noisy signals of informational change, and have a tendency to revert to previous levels. The exaggerated price movements during a trading window are presumably liquidity driven. Our theoretical model suggests that the noise may, in part, be endogenous. That is, if an insufficient percentage of limit orders execute, fewer will be placed and short-run price volatility will increase, until the marginal limit order trader is adequately compensated.

Table III

Differential Limit Order Returns When Limit Price is Reached in a Trading Window Compared to Unconditional Average Market Order Returns, for Dow-Jones Industrial Stocks for Ten Subperiods in 1988

The limit order test parameter sets the limit order price with respect to the opening price (which is also the price at which the market order executes). Specifically, the limit price for the $x\%$ limit order ($x = 0.5, 1, 2, 3$) is $x\%$ below the opening price, rounded to the nearest one-eighth. The returns reported are simple averages of the observations on the thirty stocks for the ten subperiods. The subperiod t-tests use subperiod observations on the thirty stocks. The overall t-test uses the observations on thirty stocks for ten subperiods. We also report results from a nonparametric test of the null hypothesis that the median return (of the ten subperiod means) equals zero versus the alternate that it is more than zero.

Differential Return to Executed Limit Orders $r_e^l - r_o^m$		Limit Order Test			
		0.5%	1%	2%	3%
Subperiod	1	−0.067	0.120	1.633**	1.813**
	2	0.114	0.392	1.116*	3.290**
	3	−0.153	−0.231	−2.232**	1.989
	4	0.242	1.063**	0.743*	1.477**
	5	0.828**	1.380**	2.113**	2.918**
	6	0.531**	0.777**	0.145	1.087
	7	−0.159	−0.448	0.279	−1.543
	8	0.270	0.635**	1.311*	2.276**
	9	−0.373	−0.175	1.602**	0.647
	10	−0.116	−0.117	0.834*	0.328
Overall 1988		0.100	0.361**	0.516**	1.605**
Wilcoxon p-value		0.254	0.084	0.037	0.011

** Indicates significance at the 1 percent level.
** Indicates significance at the 5 percent level.

D. Bagging

As discussed in Section I, if prices follow a random walk, limit order traders suffer from the winner's curse: a buy limit order executes if and only if the equilibrium price jumps to or below the limit order price, a phenomenon referred to as bagging. With bagging, if the limit price is reached in a trading window, the return on the limit order strategy is expected to be lower than the *unconditional* market order return. Table III presents the difference between the limit order return *conditional on execution*, and the *unconditional* market order return (where, for each stock, the unconditional market order return is the average market order return in a given subperiod). The grand averages reported in Table III are the simple averages of all observations. Overall, the differential limit order return is 0.100 percent for the 0.5 percent strategy, which is insignificantly greater than zero, and with a Wilcoxon test p-value of 0.254. It is positive for five of the subperiods and significantly greater than zero for two subperiods.

The differential limit order return for the 1 percent strategy is 0.361 percent, which is significantly above zero at the 1 percent level and has a Wilcoxon p-value of 0.084. It is positive for six subperiods and is significantly greater than zero for four subperiods. For the 2 percent strategy, the differential limit order return increases to 0.516 percent, and is significantly greater than zero at the 1 percent level with a Wilcoxon p-value of 0.037. It is positive for all but one subperiod and is significantly greater than zero for seven subperiods. Finally, the 3 percent strategy yields a differential limit order return of 1.605 percent, which is significantly greater than zero at the 1 percent level and has a Wilcoxon p-value of 0.011. It is positive for all but one subperiod and is significantly greater than zero for five subperiods.

The differential limit order returns conditional upon execution, which are consistently positive and steadily increase from the 0.5 percent test to the 3 percent test, are inconsistent with the hypothesis that a positive bagging cost dominates the gains from liquidity events. There is no evidence that executed limit orders, on average, earn lower returns than the benchmark return. On the contrary, it appears that limit order executions occur due to liquidity driven price changes with sufficient frequency, and that prices tend to rebound over relatively short investment horizons. This being the case, why do not all investors find it profitable to place limit orders? The answer, provided in the next section, involves the nonexecution cost.

E. Nonexecution Cost

We capture the nonexecution cost by forcing the investor to buy at the market price prevailing at the end of a trading window. We compare returns to the limit order strategy *conditional on nonexecution* to the *unconditional* market order return (where, for each stock, the unconditional market order return is the average market order return in a given subperiod). Table IV presents the differential returns over subperiods and overall for the four strategies. The grand averages reported in Table IV are the simple average of all observations. Overall, the differential limit order return is −0.045 percent for the 0.5 percent strategy. This is insignificant based on the t-test and with a Wilcoxon test p-value of 0.439.

For the 1 percent strategy, the differential limit order return is 0.104 percent, which is insignificant based on the t-test and with a Wilcoxon test p-value of 0.193. For the 2 percent strategy, the differential limit order return is 0.227 percent, which is significantly positive at the 5 percent level. It is significantly greater than zero for three subperiods, and significantly less than zero for one subperiod. Finally, for the 3 percent strategy, the differential limit order return of −0.767 is significantly less than zero at the 1 percent level. It is negative for six subperiods, significantly so for three, and significantly positive for two subperiods.

In conclusion, some of the evidence on nonexecution costs reported in Table IV differs from what our model predicts. The cost is negative for the 2 percent strategy (as the differential return is positive). This discrepancy could be

Table IV

Differential Limit Order Returns When Limit Price is Not Reached in a Trading Window Compared to Unconditional Average Market Order Returns, for Dow-Jones Industrial Stocks for Ten Subperiods in 1988

The limit order test parameter sets the limit order price with respect to the opening price (which is also the price at which the market order executes). Specifically, the limit price for the $x\%$ limit order ($x = 0.5, 1, 2, 3$) is $x\%$ below the opening price, rounded to the nearest one-eighth. The returns reported are simple averages of the observations on the thirty stocks for the ten subperiods. The subperiod t-tests use subperiod observations on the thirty stocks. The overall t-test uses the observations on thirty stocks for ten subperiods. We also report results from a nonparametric test of the null hypothesis that the median return (of the ten subperiod means) equals zero versus the alternate that it is more than zero.

Differential Return to Unexecuted Limit Orders $r_n^l - r_o^m$		Limit Order Test			
		0.5%	1%	2%	3%
Subperiod	1	0.403	0.664	−1.689**	−4.160**
	2	0.638	0.378	0.974**	1.885**
	3	0.117	−0.164	0.260	−1.843**
	4	−0.973*	−1.213*	1.172**	−2.249*
	5	−0.302	−0.035	1.583**	0.126
	6	0.030	0.762	0.198	1.616**
	7	−0.059	0.440*	−0.063	−0.331
	8	−0.775*	−0.418*	−0.703	−1.192
	9	0.565	0.227	0.428	−0.726
	10	0.142	0.343*	0.284	0.227
Overall 1988		−0.045	0.104	0.227*	−0.767**
Wilcoxon p-value		0.439	0.193	0.142	0.142

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

attributed, at least in part, to our use of next day's opening price rather than the current day's closing price if mean reversion occurs during the overnight period. But the cost is insignificantly different from zero for the 0.5 percent and 1 percent strategies, and significantly positive for the 3 percent strategy. Thus, an eager investor (with a high intensity to trade) could find that nonexecution is costly and, accordingly, choose to place a market order. A patient investor (with a well-balanced portfolio), on the other hand, can avoid the cost of nonexecution by not trading at all if the limit order does not execute.

F. Overall Differential Returns

The unconditional differential limit order returns $r_o^l - r_o^m$ are reported in Table V for the four strategies. As in Tables III and IV, the grand averages reported in Table V are the simple averages of all observations. The differential limit order return to all limit orders is a statistically insignificant 0.055

Table V

Unconditional Limit Order Returns Compared to Unconditional Market Order Returns, for Dow-Jones Industrial Stocks for Ten Subperiods in 1988

The limit order test parameter sets the limit order price with respect to the opening price (which is also the price at which the market order executes). Specifically, the limit price for the $x\%$ limit order ($x = 0.5, 1, 2, 3$) is $x\%$ below the opening price, rounded to the nearest one-eighth. The returns reported are simple averages of the observations on the thirty stocks for the ten subperiods. The subperiod t-tests use subperiod observations on the thirty stocks. The overall t-test uses the observations on thirty stocks for ten subperiods. We also report results from a nonparametric test of the null hypothesis that the median return (of the ten subperiod means) equals zero versus the alternate that it is more than zero.

Differential Return to All Limit Orders $r_o^l - r_o^m$		Limit Order Test 0.5%	1%	2%	3%
Subperiod	1	0.042	0.274*	0.004	−1.706**
	2	0.227	0.390**	1.215**	2.308**
	3	−0.105	−0.150	−0.574*	−0.509
	4	−0.323*	−0.213	0.986**	−0.383
	5	0.503**	0.553**	1.851**	1.174**
	6	0.368*	0.781**	0.336	1.430**
	7	−0.049	0.092	0.175	−0.656
	8	−0.016	0.050	−0.026	−0.181
	9	−0.038	0.021	0.926**	−0.254
	10	0.078	0.214	0.449	0.196
Overall 1988		0.055	0.215**	0.477**	0.119
Wilcoxon p-value		0.508	0.046	0.046	0.958

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

percent for the 0.5 percent test, and positive for five subperiods (being positive and significant for two subperiods). The corresponding return for the 1 percent test is 0.215 percent, being positive for eight subperiods; it is significant at the 1 percent level and has a Wilcoxon test p-value of 0.046. For the 2 percent test, the overall differential limit order return is 0.477 percent, being positive in eight subperiods and significantly greater than zero at the 1 percent level with a Wilcoxon test p-value of 0.046. For the 3 percent test, the overall differential limit order return of 0.119 percent is insignificantly different from zero.

Overall, the limit order strategy performs as well or better than the market order strategy. This is not consistent with the strict prediction of our model that, overall, limit orders must underperform market orders when execution is forced at the end of a trading window. As with our evidence on the nonexecution cost, use of next day opening prices rather than current day closing prices could be the explanation if prices mean revert during the overnight period. For the most part, however, the positive differential returns are driven primarily by limit order executions that are followed by a rebound in prices.

G. Market-adjusted Returns

In this section, we compare market-adjusted returns to the limit order strategy with market-adjusted returns to the market order strategy. The objective is to determine whether the differential performance of limit orders and market orders for individual stocks is attributable to order flow dynamics that are unique to individual stocks, or to the behavior of the aggregate order flow for the broad market. That is, we examine the extent to which the execution of a limit order for a specific stock at a particular point in time is the outcome of a liquidity event specific to that stock, or to a more pervasive, mean-reversion producing order flow imbalance that causes limit orders on the same side of the market to execute for a wide cross-section of stocks. We start by defining the differential return for stock i in window t as

$$\mathrm{R}_{it} = r_{\mathrm{o}}^{\mathrm{l}} - r_{\mathrm{o}}^{\mathrm{m}} \tag{11}$$

where $r_{\mathrm{o}}^{\mathrm{l}}$ is the overall limit order return of stock i in window t, and $r_{\mathrm{o}}^{\mathrm{m}}$ is the average market order return of stock i in the subperiod that contains window t. Similarly, for an equally-weighted portfolio we define

$$\mathrm{R}_{pt} = r_{\mathrm{po}}^{\mathrm{l}} - r_{\mathrm{po}}^{\mathrm{m}} \tag{12}$$

where $r_{\mathrm{po}}^{\mathrm{l}}$ is the overall limit order return of the portfolio in window t, and $r_{\mathrm{po}}^{\mathrm{m}}$ is the average market order return of the portfolio in the subperiod that contains window t. While R_{it} can be interpreted as the excess return for stock i, R_{pt} can be interpreted as the excess return for the portfolio. We run the regression,

$$\mathrm{R}_{it} = \alpha_i + \beta_i\, \mathrm{R}_{pt} + \eta_{it} \tag{13}$$

where η_{it} is the differential return for a stock that is uncorrelated with the differential return on the market portfolio. Because our portfolio comprises only 30 stocks, we eliminate the effect of each stock's own return on the portfolio return by constructing a customized index for each stock, using only the returns for the other 29 stocks in the sample.[15] For stock i, a positive covariance term, β_i would suggest that the profitability of a limit order as compared to a market order in a trading window is in part attributable to limit orders in aggregate having outperformed market orders for that period. The residual term, η_{it} captures the component of limit order profitability that is specific to stock i.

An analysis of η_{it} is interesting for two reasons. First, removing the common market element for each stock allows us to view the observations for the 30 companies as independent samples, which increases our confidence in the significance of the findings. Second, one might expect that liquidity events are unique to individual stocks, and that they cause limit order execution to be profitable on a stock-by-stock basis. To test this hypothesis, we examine the

[15] This procedure, known as the "Lachenbruch method," is described in Lachenbruch (1967).

Table VI

Market-Adjusted Differential Returns to Limit Order Strategy Relative to Market Order Strategy for Dow-Jones Industrial Stocks for Ten Subperiods in 1988

Overall and subperiod results for the panel regression $\eta_{it} = \gamma_1 + \gamma_2 L_{it} + \varepsilon_{it}$, where i represents the stock, t represents the window, η_{it} is the market-adjusted differential return, and L_{it} is an indicator variable that equals 1 when the limit price is reached, and 0 otherwise. The vector of market-adjusted differential returns (η_{it}) for the entire year is obtained from a first-pass market model regression given by

$$R_{it} = \alpha_i + \beta_i R_{pt} + \eta_{it}.$$

Where R_{it} is the differential return for stock i in window t and R_{pt} is the differential return for portfolio p in window t.

		Limit Order Test							
		0.5%		1%		2%		3%	
		γ_1	γ_2	γ_1	γ_2	γ_1	γ_2	γ_1	γ_2
Subperiod	1	−0.808	1.017*	−0.448	0.663	−0.429	0.851	−1.028**	2.201**
	2	−0.116	0.174	−0.147	0.312	−0.061	0.453	−0.431	2.103**
	3	−0.602	0.916*	−0.399	0.817*	−0.064	0.020	−0.392	1.666**
	4	−0.093	0.106	−0.362	0.822	−0.470	1.177**	−0.359	1.747**
	5	−0.763**	1.131**	−0.528**	1.252**	−0.065	0.629	−0.415	2.383**
	6	−0.546	0.696	0.027	0.011	−0.219	0.814	−0.043	0.437
	7	−0.535	0.755*	−0.155	0.337	0.004	−0.046	−0.205	0.708
	8	−0.527*	0.896**	−0.260	0.745*	−0.221	1.678**	−0.337	2.218**
	9	0.216	−0.371	0.082	−0.242	−0.305	1.509**	−0.225	1.362
	10	−0.088	0.152	−0.028	0.084	−0.302	1.406**	−0.129	0.728
Overall 1988		−0.341**	0.505**	−0.213**	0.465**	−0.209**	0.718**	−0.339**	1.494**

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

behavior of the market-adjusted component of differential returns by running the following panel regression:

$$\eta_{it} = \gamma_1 + \gamma_2 L_{it} + \varepsilon_{it} \tag{14}$$

where i represents the stock, t represents the window, and

η_{it} is the market-adjusted differential return,
$L_{it} = 1$ when the limit price is reached in a window, and
$L_{it} = 0$ when the limit price is not reached, and ε_{it} is an error term.

Hence, a stacked vector of the market-adjusted differential returns *for all stocks* is regressed on a stacked vector of indicator values L_{it}, to obtain estimates of the coefficients γ_1 and γ_2, and their standard errors. As in our previous analysis, finding that $\gamma_1 = 0$ and $\gamma_2 = 0$ is consistent with a random walk. On the other hand, $\gamma_1 < 0$ and $\gamma_2 > 0$ is consistent with mean reversion.

The overall and subperiod results after we control for the systematic market component in limit order trading, are shown in Table VI. These results provide clear and strong support for the hypothesis that trading via limit orders provides gains from execution and has costs when a limit order fails to execute. For the 0.5% test, the market-adjusted differential return for limit orders that execute (given by $\gamma_1 + \gamma_2$) is 0.164%, while the market-adjusted differential return for limit orders that do not execute (given by γ_1) is -0.341%. The parameters γ_1 and γ_2 are well-behaved: γ_1 is significantly less than zero overall and takes a negative value in nine of the ten subperiods, while γ_2 is significantly greater than zero overall, greater than γ_1 in nine subperiods, and takes a positive value in the same nine subperiods.

For the 1 percent test, the market-adjusted differential return for limit orders that execute is 0.252 percent and for limit orders that fail to execute it is -0.213 percent. The parameter γ_1 is significantly less than zero overall and is negative in eight subperiods, while γ_2 is significantly greater than zero overall, positive in nine subperiods, and exceeds γ_1 in eight subperiods. For the 2 percent test, the market-adjusted differential return for limit orders that execute is 0.509 percent, and the market-adjusted differential return for limit orders that do not execute is -0.209 percent. The parameter γ_1 is significantly less than zero overall and is negative in all but one subperiod, while γ_2 is significantly greater than zero overall, positive in nine subperiods and greater than γ_1 in the same subperiods. Finally, for the 3 percent test, the market-adjusted differential return for limit orders that execute is 1.155 percent and the market-adjusted differential return for limit orders that do not execute is -0.339 percent. The parameter γ_1 is significantly less than zero overall and is negative in all ten subperiods, while γ_2 is significantly greater than zero overall, and greater than γ_1 and positive in all subperiods.

The results on adjusted differential returns support the hypothesis that differential limit order returns conditional on execution are positive, and that differential limit order returns conditional on nonexecution are negative. We find, after controlling for the common market effect in individual stock returns, that bagging is not a cost to limit order traders, but that nonexecution is. Again we conclude that investors who are particularly eager to transact may prefer the market order strategy, while investors who gain relatively little by trading at current prices (and who are thus willing to risk not executing) may prefer the limit order strategy.

H. Robustness Tests

We have thus far tested the profitability of buying by limit orders using transaction data for the year 1988. This particular year was marked by generally rising stock prices, and the impact of trend in prices is not clear. A possibility is that an upward trend could result in a reduction in bagging costs and an increase in nonexecution costs for buy orders.

To investigate the impact of a price trend on our results, we invert the price series, treating the last price observation as the first, the second to last price

Table VII

Robustness Tests of Limit Order Trading With the Price Record Run Backward (Inverted Buy) and With a Limit Sell Order Strategy (Regular Short-Sell), for Dow-Jones Industrial Stocks in 1988

The robustness tests reverse the price trend by either running the price series backward or by selling instead of buying without altering the correlation structure of the prices. We compare results from the *regular buy strategy* with (1) an *inverted buy strategy*, where the entire 1988 transaction price series for each stock is run backward and buy limit orders compared with buy market orders, and (2) *a regular short-sell strategy*, where the price series is not disturbed but instead limit sell orders are compared to market sell orders, with the short position being covered at the end of the investment window by a purchase, and the return being given by $\ln(P_x) - \ln(P_{x+t})$.

Robustness Test	Limit Order Test Parameter	Differential Return to Executed Limit Orders $r_e^l - r_o^m$	Differential Return to Unexecuted Limit $r_n^l - r_o^m$	Differential Return to All Limit Orders $r_o^l - r_o^m$
Regular Buy	1%	0.361**	0.104	0.215**
	2%	0.516**	0.227*	0.477**
	3%	1.605**	−0.767**	0.119
Inverted Buy	1%	0.096	−0.007	0.141*
	2%	0.965**	0.243	0.595**
	3%	0.904**	−0.415*	0.075
Regular Short-Sell	1%	−0.124	−0.519**	−0.235**
	2%	−0.254*	−0.622**	−0.356**
	3%	0.539**	0.314*	0.485**

** Indicates significance at the 1 percent level.
* Indicates significance at the 5 percent level.

observation as the second, etc. We examine the profitability of a buy limit order strategy on this inverted price series using a test design identical to the one reported in the earlier sections. One advantage of using the inverted price series is that we are able to preserve both the experimental design and the correlation structure of the price series, while altering the trend in prices. In an alternate test that we perform, the profitability of short-selling by limit order is also examined during the trading period, with the short position subsequently covered at the end of the investment window (i.e., the return to a short-sell can be stated as $\ln(P_x) - \ln(P_{x+t})$). In this formulation of the robustness test, the experimental design assumes that there are no constraints or requirements to short-selling, and that short-selling can proceed in a manner similar to purchasing stock.

Table VII compares the relative returns we obtain from a regular (noninverted) buy test, with the relative returns from a buy limit order strategy using the inverted price series, and with the relative returns from a short-selling limit order strategy. An examination of all limit orders that execute, irrespective of the strategy being tested, reveals that such orders perform significantly better than a market order in six cases, not significantly different in two cases, and significantly worse in only one case. Second, an examination of limit orders that fail to execute, irrespective of the strategy tested, reveals that such orders perform significantly worse than a market order in four cases, not significantly different in three cases, and significantly better in two cases. Third, the results on the overall performance of limit orders are mixed as expected: in the inverted buy tests, limit orders outperform market orders for all strategies and do so significantly for the 1 percent and 2 percent strategies; in the short-sell tests, limit orders significantly out-perform market orders for the 3 percent strategy, but significantly under-perform in the 1 percent and 2 percent tests. Overall, our robustness tests suggest that our results are not just an artifact of the price trends that characterized 1988.

III. Profitability of a Multiple Limit Order Trading Strategy

Our tests have thus far assessed the profitability of either buying or selling by limit order, and of holding the position for a relatively short period before closing it out. We now assess the profitability of placing a network of buy and sell limit orders, and of allowing a sequence of purchases and sales to occur over an extended calendar period before closing out any accumulated position that may have developed. The multiple limit order test we use to assess the profitability of limit order trading is structured as follows.

For each Dow stock, our hypothetical trader casts a network of limit orders around the opening price for the first trading day of a test period. The orders are placed a fixed distance apart where the distance is measured as a percentage (s = 1 percent, 2 percent, 3 percent, 4 percent, 5 percent) of the opening price. That is, if the opening price is P_0, limit orders to buy are entered at $(1 - s)P_0$, $(1 - 2s)P_0$, $(1 - 3s)P_0$, etc., and limit orders to sell are entered at $(1 + s)P_0$, $(1 + 2s)P_0$, $(1 + 3s)P_0$, etc. All entered orders are rounded to the nearest eighth. The dollar spread implied by the parameter s depends, of course, on the price level at which the stock is trading. For the 1 percent strategy, the unrounded dollar spreads average \$0.48 and range from \$0.05 to \$1.58. As s increases to 5 percent, the average, minimum, and maximum rise monotonically to, respectively, \$2.42, \$0.26, and \$7.08.

As executions are realized, limit orders are appropriately reinstated to keep the network intact and centered on the price of the last limit order that executed. We restrict our tests to a naive limit order strategy, without allowing for a revision in the quotes in response to current market conditions, and without any inventory control. We follow the trades, inventory levels, and profits for three 80-day periods during the year 1988.[16] The total return to the multiple limit order strategy can be written as the sum of all proceeds from sales less the sum of all payments for purchases. Purchases and sales are made as price fluctuates during a test period and, because price can drift, the trader generally holds an unbalanced inventory at the end of each period. We measure returns for the multiple limit order strategy in two ways: 1) as the gain per round trip (referred to as RTGAIN later in this section) that is realized as prices fluctuate during a test period, not including the closing (inventory

[16] Three periods of eighty trading days each are used as data are available for all stocks in our sample for a period of 242 days in 1988.

rebalancing) transaction, and 2) as a limit order spread (referred to as LOS later in this section) which includes the closing trade.[17] The limit order spread is obtained by dividing the total dollar return including the closing transaction, by the total number of round trips, including the closing transaction. A positive bagging cost is, thus, manifest in a negative RTGAIN, and a positive nonexecution cost is manifest in an LOS less than RTGAIN.

More formally, we use the following notation to assess the returns to the limit order strategy:

N_a = number of shares sold during the test period (excluding sales at the closing trade),
N_b = number of shares bought during the test period (excluding purchases at the closing trade),
P_a = average selling price per share over all trades except the closing trade,
P_b = average buying price per share over all trades except the closing trade, and
P_c = closing price per share.

Letting Π be the total return, we have:

$$\Pi = (P_a \cdot N_a - P_b \cdot N_b) - P_c \cdot (N_a - N_b) \tag{15}$$

Rearranging the equation, we can write the total return as:

$$\Pi = (P_a - P_b) \cdot \mathrm{Min}(N_a, N_b) + (\bar{P} - P_c) \cdot (N_a - N_b)$$
$$\text{where } \bar{P} = P_a \quad \text{if} \quad N_a > N_b \qquad \text{and} \qquad \bar{P} = P_b \quad \text{if} \quad N_a < N_b. \tag{16}$$

and the limit order spread (LOS) as

$$\mathrm{LOS} = \frac{\Pi}{\mathrm{Max}(N_a, N_b)} \tag{17}$$

The first term on the right hand side of the expansion for Π, $(P_a - P_b) \cdot \mathrm{Min}(N_a, N_b)$, captures the *returns* from the round trips.[18] The *bagging cost* is given by $-(P_a - P_b) \cdot \mathrm{Min}(N_a, N_b)$. Similarly, the second term, $(\bar{P} - P_c) \cdot (N_a - N_b)$, gives the *returns* from closing the position at the end of the period. Therefore, the *non-execution cost* is given by $-(\bar{P} - P_c) \cdot (N_a - N_b)$.

In the case of bagging, the difference between the average ask price and the average bid price, $(P_a - P_b)$ establishes whether bagging is, in fact, a positive cost. If bagging is a positive cost, $(P_a - P_b)$ is, on expectation, negative, and if bagging is a negative cost (because of mean reversion in prices) this component

[17] This is similar to the standard bid-ask spread which gives the per share profits of one round trip, i.e., buying at the bid and selling at the ask. The limit order spread can be interpreted as the returns per share by buying as limit bids execute and selling as limit asks execute, followed by a rebalancing to restore the opening inventory position.

[18] We are grateful to the referee for suggesting this line of analysis.

is, on expectation, positive. To see this more clearly, consider the following. If during a test period, news is on net bearish, limit buy orders will execute as price falls and P_b will, on average, be greater than P_a. Alternatively, if news is on net bullish, limit sell orders will execute as price rises and P_a will, on average, be less than P_b. Hence, when prices follow a random walk $(P_a - P_b)$ will, on average, be zero. On the other hand, with mean reversion, prices will tend to rise after limit buy orders execute and to fall after limit sell orders execute, leading to P_a greater than P_b. For the empirical results that follow, we represent the bagging cost as

$$\mathrm{BC} = (-1) \cdot \mathrm{RTGAIN} \cdot \mathrm{RTRIPS}$$

$$\begin{array}{ll} \text{where} & \mathrm{BC} = \text{Bagging Cost} \\ & \mathrm{RTGAIN} = \text{Gain per Round Trip} = (P_a - P_b) \\ \text{and} & \mathrm{RTRIPS} = \text{Number of Round Trips} = \mathrm{Min}(N_a, N_b) \end{array} \quad (18)$$

Now consider the nonexecution cost. Any buy imbalance that develops during the test period can be eliminated either by sell limit orders executing during the test period, or by a closing sale at the end of the test period. Similarly, any sell imbalance can be eliminated either by buy orders executing during the test period, or by a closing purchase at the end of the test period. The component $(N_a - N_b)$ is the size of the closing trade at the end of a test period. Rising prices cause $(N_a - N_b) > 0$ and $(\bar{P} - P_b) < 0$, and falling prices have the reverse effect; hence $\mathrm{sign}[(N_a - N_b)] = -\mathrm{sign}[(\bar{P} - P_c)]$ and the nonexecution cost is, on expectation, positive. We use the following notation to represent the nonexecution cost:

$$\mathrm{NEC} = (-1) \cdot \mathrm{DC} \cdot \mathrm{IMB}$$

$$\begin{array}{ll} \text{where} & \mathrm{NEC} = \text{Non-execution Cost} \\ & \mathrm{DC} = \text{Differential Per Share at Closure} = (\bar{P} - P_c) \\ \text{and} & \mathrm{IMB} = \text{Share Imbalance} = (N_a - N_b) \end{array} \quad (19)$$

Note that, if prices do not follow a mean reverting process, the hypothetical limit order trader is doubly cursed: a bagging cost is incurred when bullish or bearish news causes orders predominantly on one side or the other of the market to execute, and a nonexecution cost is incurred when the imbalance is eventually restored. On the other hand, with mean reversion, the limit order strategy may be profitable (that is, a negative bagging cost can outweigh the positive non-execution cost). If so, limit orders may be used as a *volatility capture* trading strategy.

Table VIII reports the total return from the multiple limit order test as well as the bagging and nonexecution costs associated with it, for the five limit order strategies (s = 1 percent, . . . , 5 percent). It also reports the (implied) limit order spread for each of the five strategies. Consistent with the results reported in the previous sections, the bagging cost (BC) is negative, i.e., there

Table VIII

Limit Order Spread From a Multiple Limit Order Trading Strategy When a Network of Limit Orders is Placed Around the Opening Price, from one to five Percentage Points Apart, and all Transactions are Closed at the End of an Eighty Trading-Day Period, for Three Trading Periods in 1988 for all Dow Jones Industrial Stocks

We test the profitability of a *multiple limit order trading strategy* by monitoring purchases and sales during the period and also the profitability of the closing trade. Our hypothetical limit order trader is expected to gain from round trips very much like a market maker gains from the spread, and to lose at the closing trade when the inventory imbalance is corrected. In Column 1, figures in parentheses refer to the average rounded (to an eighth) dollar value of the percentage parameter. For all other columns, the t-statistic corresponding to the null hypothesis that the value is zero, is reported in parentheses.

Percentage Parameter	Round-trip Gain (RTGAIN) (in \$)	Number of Round Trips (RTRIPS)	Differential Per Share at Closure (DC) (in \$)	Share Imbalance at Closure (IMB)	Bagging Cost (BC) (in \$)	Nonexecution Cost (NEC) (in \$)	Total Return (II) (in \$)	Limit Order Spread (LOS) (in \$)
1%	0.54	83.18	−0.31	4.50	−36.86	20.53	16.33	0.23
(\$0.50)	(11.28)	(13.52)	(−1.13)	(4.31)	(−11.73)	(4.93)	(3.37)	(3.74)
2%	1.02	25.27	−0.31	2.49	−19.40	12.41	6.99	0.47
(\$1.00)	(10.79)	(15.40)	(−1.12)	(4.12)	(−11.10)	(3.08)	(1.42)	(4.03)
3%	1.49	12.28	−0.24	1.72	−12.89	7.84	5.05	0.67
(\$1.50)	(10.61)	(10.48)	(−0.88)	(4.00)	(−11.55)	(2.91)	(1.66)	(4.51)
4%	1.83	6.83	−0.31	1.25	−10.03	5.37	4.67	0.95
(\$2.00)	(9.89)	(8.78)	(−1.21)	(3.39)	(−11.09)	(2.87)	(2.12)	(5.05)
5%	2.26	4.05	−0.12	0.78	−7.49	3.68	3.80	1.24
(\$2.375)	(11.03)	(11.91)	(−0.46)	(3.18)	(−11.00)	(2.26)	(2.14)	(5.43)

is a positive return rather than a loss from executing limit orders. Moreover, this return is statistically significant for all five strategies. Examining the components of this gain, the round trip gain (RTGAIN) is always positive and increasing in the spread parameter. The number of round trips (RTRIPS) declines rapidly in the spread parameter but is always significantly greater than zero, i.e., our hypothetical trader does indeed get an opportunity to reverse some of the trades.

The nonexecution cost (NEC) captures the closing trade at the end of a test period and this cost turns out to be always positive. The first component of the nonexecution cost is the differential per share at closure (DC), a number which, on average for our sample, is negative though not significantly different from zero. The second component of the nonexecution cost is share imbalance (IMB). This number is always positive, indicating a tendency to sell more than to buy in a period. Overall, because prices were generally rising in 1988, the hypothetical trader oversells during the test periods and has to regain a zero inventory position by purchasing at the higher closing price. This results in a loss at closure, i.e., in a positive nonexecution cost. Both of the components of nonexecution cost have highly skewed distributions (with a few heavy losses and several smaller gains as well as losses at closure).

The net profitability of the multiple limit order strategy is clearly brought out by the *limit order spread* numbers. As one moves from a 1 percent to a 5 percent strategy, the *limit order spread* (LOS) increases steadily from \$0.23 to \$1.24. This result is our strongest confirmation that mean reversion exists in prices in an order-driven market, and that it makes limit order trading profitable for some public participants.

The *limit order spread* is a function of two countervailing forces: a negative *bagging* cost that results in the hypothetical trader actually gaining from the execution of his or her limit orders, and a positive *nonexecution cost* that results in a loss to the trader if he or she closes a position to return to zero inventory. In order to better understand how the limit order spread is determined, we study the correlation between its various components. The bagging cost arises when a trader, on average, sells low and buys high (as captured by the term RTGAIN), and on the number of round trips made within a period, RTRIPS. In our sample, a negative bagging cost arises when the trader, on average, buys low and sells high, i.e., RTGAIN is positive, and, moreover, some round trips are actually achieved, i.e., RTRIPS is significantly greater than zero. The correlation between RTGAIN and RTRIPS is -0.336 and not significantly different across strategies, which indicates that a lower per share gain is associated with a higher number of round trips. The limit order spread (LOS) is only weakly negatively related to RTRIPS (correlation of -0.174), but strongly positively related to RTGAIN (correlation of 0.619) and RTGAIN seems to drive the gains for the trading strategy.

The nonexecution cost arises when the closing price is high (i.e., DC is negative), and the trader does buy at close (i.e., IMB is positive), or vice versa. The evidence is that DC and IMB are, indeed, highly negatively correlated (correlation of -0.558 for all strategies pooled together, and not significantly different across strategies) which is consistent with the significant positive nonexecution cost we document. Further, the limit order spread is positively correlated with DC (correlation of 0.405) and negatively correlated with IMB (correlation of -0.336), indicating that rising prices which cause a trader to oversell, cause lower limit order spreads.

We regress the limit order spread (LOS) on its four components, RTGAIN, RTRIPS, DC, and IMB, and on four indicator variables (IND_1, IND_2, IND_4, and IND_5) to capture any systematic differences between the strategies that are not accounted for by the four component variables.[19] The indicator variable IND_1 takes the value 1 when the 1 percent strategy is used and the value zero otherwise, and so on. The regression uses data pooled across time periods, across stocks and across the five trading strategies. Our objectives are a) to understand the partial effect of each component variable on LOS after controlling for the other component variables and after controlling for the specific trading strategy employed, and b) to assess whether LOS has any systematic component that depends on the specific trading strategy employed which is not

[19] As our primary interest is to get some insights into partial correlations between LOS and its component variables, for simplicity we restrict the regression test to a linear fit.

explained by the four component variables. We expect the four component variables to explain a significant portion of LOS even across strategies, and we expect the indicator variables not to capture any systematic effects beyond those captured by the four component variables. Our regression results are presented below. The dependent variable LOS, and the independent variables RTGAIN and DC are measured in cents, and the independent variables RTRIPS and IMB in number of shares.

$$\begin{aligned} \text{LOS} = &\underset{(-1.98)}{-25.68} + \underset{(16.90)}{0.64} \cdot \text{RTGAIN} + \underset{(1.24)}{0.24} \cdot \text{RTRIPS} + \underset{(9.91)}{0.24} \cdot \text{DC} + \underset{(0.82)}{0.93} \cdot \text{IMB} \\ &\underset{(-0.10)}{-2.12} \cdot \text{IND}_1 + \underset{(0.43)}{6.81} \cdot \text{IND}_2 + \underset{(0.56)}{8.96} \cdot \text{IND}_4 + \underset{(0.46)}{7.37} \cdot \text{IND}_5 \\ &R^2 = 0.529 \end{aligned} \tag{20}$$

The regression clearly indicates that the key components driving the limit order spread in 1988 are RTGAIN and DC. The number of round trips within a period (RTRIPS) and the share imbalance at closure (IMB) have an insignificant effect on the limit order spread. Further, the indicator variables are all insignificant as expected, indicating that no systematic differences exist beyond those captured by RTGAIN and DC. Hence, the significant difference that exists in the limit order spread (LOS) across strategies ($0.23 for the 1 percent test monotonically increasing to $1.24 for the 5 percent test), is explained largely by RTGAIN and DC.

IV. Conclusion

The viability of an order driven market depends on limit order trading being profitable for a sufficient number of public participants. When transaction prices change solely in response to information, trading via limit order is suboptimal for all traders because the advent of adverse news can trigger an undesired trade, while the advent of favorable news can result in the limit order not executing. We suggest that limit orders are placed by public traders only because liquidity driven price changes (caused by temporary order imbalances in the market) can offset the cost of being bagged by informed traders. Furthermore, in an order driven market, a paucity of limit orders can cause temporary order imbalances that lead to short-run changes in transaction price, which eventually reverts to true price. Hence, equilibrium levels of limit order trading and of short-run price volatility can exist in the sense that an increase (decrease) in short-run volatility encourages (discourages) the placement of limit orders and, in turn, an increase (decrease) in limit order trading decreases (increases) short-run volatility.

We test the limit order trading strategy for the thirty Dow Jones Industrial stocks for the year 1988. The results are generally consistent, both with our theoretical framework and across the ten subperiod tests. Overall, the evidence indicates that limit order purchases (including closing purchases at the end of each trading window) and market order purchases result in comparable prices and returns. We find no evidence of systematic bagging; on the contrary, on average, prices tend to bounce back toward original levels after limit order executions. On the other hand, nonexecution costs appear to be positive, but not always statistically significant. We also examine limit order trading as a *pure* trading strategy and, in so doing, document the existence of a *limit order spread* that averages $0.23 for the 1 percent strategy and monotonically increases to $1.24 for the 5 percent strategy.

Our results show that some proprietary traders (those who, having minimal nonexecution costs, are relatively patient) have an incentive to submit limit orders, that others (those who, having high nonexecution costs, are relatively eager) prefer to submit market orders, and that this trading ecology can be self-sustaining. These results are consistent with the viability of an order driven market, where public participants supply liquidity to themselves without the intervention of dealers. We believe that future research on issues such as trade size and the profitability of executed limit orders, and on the cross-sectional differences in stock price volatility and the profitability of limit order trading, can provide further useful insights into the functioning of an order driven market.

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