Predicting Equity Liquidity

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Predicting Equity Liquidity

Abstract

In this paper we (a) quantify equity liquidity using a measure of price impact, the change in a firm's stock price associated with its observed net trading volume; (b) relate the measured price impact to a set of predetermined firm characteristics that serve as proxies for the severity of adverse selection in the equity market, the non-information based costs of making a market in the stock, and the extent of shareholder heterogeneity; and (c) compare, out-of-sample, our characteristic-based estimates of price impact to actual price impacts.

Increasing the magnitude of net turnover during a 5-minute interval by 0.1% of the shares outstanding produces an average incremental price effect of 2.65% for NYSE and AMEX listed firms and 1.85% for NASDAQ firms. These averages, however, mask considerable cross-sectional variation. We present evidence that liquidity varies cross-sectionally as a function of predetermined firm characteristics as predicted by theories based on adverse selection, market making costs, and shareholder heterogeneity. We also find intra-day patterns, with the price impact being higher at the beginning and end of the trading day relative to the middle of the day.

For a large set of institutional trades we examine the relation between actual price impact and that predicted out-of-sample using the cross sectional relation between firm characteristics and price impact. We find numerous aspects of trade execution which are significantly related to the price impact forecast error in intuitive ways: for example, the predicted price impact overestimates the actual price impact for very large trades, for trades executed in a more patient manner, and for trades where the institution pays higher commissions.

Predicting Equity Liquidity

Understanding the costs of trading securities is important for a variety of issues. In asset pricing, recent research suggests that asset prices are affected by expected trading costs. For example, Brennan and Subrahmanyam (1996) find that measures of price impact are positively related to equity returns, while Easley, Hvidkjaer, and O'Hara (2000) find that a measure of the probability of informed trading is also related to equity returns. Further, the inclusion of all relevant trading costs may nullify apparent asset pricing anomalies. For example, Knez and Ready (1996) find that trading costs eliminate the apparent profitability of a trading strategy based on the auto-correlation of small and large capitalization firms. In portfolio management, better measures of expected trading costs improve the ability to effectively implement portfolio strategies and to monitor the quality of trade execution. In corporate finance, investigating the behavior of trading costs surrounding corporate financial decisions may provide useful insights into firm policy. For example, while it has been argued that stock splits are used to increase the liquidity of a stock, the empirical evidence based on bid-ask spreads [Copeland (1979)], depth [Gray et. al. (1996)], and price impact [Breen, Hodrick, and Korajczyk (2000)] does not seem to support this argument.

Lack of liquidity generates an important cost of trading equities. While smaller equity trades are often executed inside the quoted prices [e.g., Petersen and Fialkowski (1994) and Knez and Ready (1996)], larger trades more often face prices far inferior to those quoted. In this paper we quantify equity liquidity using a measure of price impact, the change in a firm's stock price associated with its observed trading volume. Our measure of price impact is meant to capture the extent to which a trade is executed without influencing the stock price: a perfectly liquid asset trades without any price effect while a perfectly illiquid asset can not be traded at any price. The sample consists of 6513 firms from January 1993 through May 1997, with a typical month having data for 3699 firms. We find that increasing the magnitude of net turnover during a 5-minute interval by

0.1% of the shares outstanding (e.g., by 50,000 shares of a stock with 50 million shares outstanding) produces, on average, an incremental price effect of 2.65% for NYSE and AMEX listed firms and 1.85% for NASDAQ firms.

These averages, however, mask considerable cross-sectional variation. We relate the measured price impact to a set of predetermined firm characteristics. We would expect the liquidity of equity to be influenced by (i) the severity of the adverse selection problem faced by uninformed traders, (ii) the non-information based costs of market making, and (iii) the extent of shareholder heterogeneity. This builds on the work of Hasbrouck (1991a, b), who investigates the relation between firm size and a measure of price impact for samples of 80 and 177 firms, respectively, during the first quarter of 1989, and Glosten and Harris (1988), who investigate the cross-sectional relation between temporary and permanent price effects and proxies for adverse-selection and non-adverse-selection based costs of trading for a sample of 250 firms, over the period from December 1981 through January 1983.

We find that liquidity varies cross-sectionally as predicted by these theories. Moreover, the cross-sectional relation between our measure of price impact and firm characteristics is stable over the sample period, suggesting that the relation might be useful in predicting price impacts for other periods or for equities not used in the estimation stage. If so, researchers backtesting portfolio trading strategies would be able to incorporate an estimate of the cost of price impact. For example, Mitchell and Pulvino (2000) find that the estimated effects of price impact, using our cross-sectional regression results, have an important effect on the profitability of a merger-arbitrage portfolio strategy.

To gauge the ability of the cross-sectional relation to predict price impact out-of-sample, we compare the actual and predicted impact for trades made by twenty-one institutions over a twenty-seven month period. The sample was constructed by the Plexus Group and consists of 56,211 trades for a total of 1.9 billion shares representing $62.4 billion in value.[1] We find that numerous aspects of trade execution are significantly related to the price impact forecast error in intuitive ways. For example, the predicted price impact overestimates the actual price impact for very large trades, for trades executed in a more patient manner, and for trades where the institution pays higher commissions.

The paper proceeds as follows. Section I presents the data and the methodology employed. Statistics about the magnitudes of our measure of liquidity are reported. Section II presents the cross-sectional results. Section III confirms the robustness of the results and addresses a number of issues regarding the interpretation of the results. Section IV compares the actual and predicted price impact for a sample of institutional traders. Section V concludes.

I. Data and Methodology

The sample covers the period from January 1993 through May1997 and consists of all firms included in the COMPUSTAT Primary/Supplemental/Tertiary and OTC files. For each of these firms, we use the New York Stock Exchange's (NYSE) Trade and Quotation (TAQ) database to obtain net volume as well as price and quote information. If a transaction occurs within five seconds of a quote revision, we use the quote in the TAQ database as of five seconds prior to the trade as the quote prevailing at the time of a given trade. Lee and Ready (1991) suggest this five second rule because quote changes are often recorded more rapidly than the transactions that led to the quote changes. We classify every trade in the sample as buyer-initiated or seller-initiated on the basis of

[1]We thank Don Keim and Ananth Madhavan for providing us with the data and Wayne Wagner of the Plexus Group for granting us permission to use the data.

whether the trade price is greater than or less than the mid-point of the prevailing best bid and ask quotes.[2] Odders-White (2000) finds that this "quote method" of trade classification has the lowest frequency of mis-classification of the three methods she studies.[3]

Once all of the trades are classified, we calculate net turnover for each 5-minute and 30-minute trading interval during the trading day. Net turnover for period t (NTO_t) is defined as buyer-initiated volume less seller-initiated volume (times 1000) as a fraction of shares outstanding. That is, $NTO_t = 1$ corresponds to net turnover of 0.1% of shares outstanding. Additionally, we define $P_{i\tau}$ as the price at which the last trade occurred within the time period τ and $Q_{i\tau}$ as the quote midpoint prevailing at the end of period τ.[4] We calculate returns using both the percentage change in the last traded price and the percentage change in the end of period quote midpoints. Our sample does not include any over-night returns. Similarly, if a given stock begins trading after the open for the day, we only have return and net volume data for the periods after the initiation of trade.

For each month, t, where t = 1, ...,53, we have $\tau_i(t)$ 5-minute or 30-minute observations on firm i, i = 1, ...N(t). We estimate, for each month, the regression of firm i's $\tau_i(t)$ equity returns on its corresponding net turnover.[5]

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[2]Trades executed exactly at the midpoint are classified as neither buyer- nor seller-initiated, and they contribute zero net turnover.

[3]The other two alternatives are the "Tick Method," which classifies trades by comparing the trade price to the past trade's price, and a hybrid quote/tick method suggested by Lee and Ready (1991).

[4]We exclude intervals without any trades.

[5]We consider the sum of all transactions of the equity completed each interval, rather than considering either only the impact of the largest trades, or distinguishing each trade. Tolerating smaller trades as well as large-block transactions may prove important if, as posited in Kyle (1985), informed traders disguise their transactions by implementing numerous anonymous smaller trades rather than executing fewer large block trades.

$$(P_{i,\tau} - P_{i,\tau-1})/P_{i,\tau-1} = \alpha^{P}_{i\,t} + \beta^{P}_{i\,t}NTO_{i,\tau} + \epsilon_{i,\tau} \quad (1)$$

$$(Q_{i,\tau} - Q_{i,\tau-1})/Q_{i,\tau-1} = \alpha^{Q}_{i\,t} + \beta^{Q}_{i\,t}NTO_{i,\tau} + \epsilon_{i,\tau} \quad (2)$$

for $\tau = 1, 2, ..., \tau_i(t)$ and $i = 1, ..., N(t)$. This generates four time series of monthly price impact coefficients, $\hat{\beta}_{i,t}$, for every firm: two series using price returns (5-minute and 30-minute intervals) and two series using quotes returns (5-minute and 30-minute intervals).

We expect that $\beta^{P}_{i\,t}$ will be larger than $\beta^{Q}_{i\,t}$ given that the fixed-costs of trading are reflected in price returns. This is consistent with a number of microstructure models which imply both permanent and transitory effects of trade on prices [e.g., Glosten (1987), Easley and O'Hara (1987)]. For example, if the quoted bid and ask prices were constant through time with all transactions occurring at the quotes, quote returns would be zero and there would be no relation between them and NTO, while $\beta^{P}_{i\,t}$ would be positive.

The specification of (1) and (2) is motivated by the linear pricing rule of Kyle (1985), which expresses price changes as a linear function of net volume:

$$P_{i,\tau} - P_{i,\tau-1} = \lambda_{i,t}NVOL_{i,\tau} + \epsilon_{i,\tau}.$$

Notice that our specification scales net volume by shares outstanding to get net turnover, and scales price changes by beginning-of-period price to get returns. Using scaled measures provides more meaningful cross-sectional and intertemporal comparisons. For example, for firms engaging in stock splits, a given net volume corresponds to a larger fraction of the firm before the split than after the split. If the price reaction is solely a function of the *fraction* of the firm traded, then the coefficient λ would change around a stock split, while β should not.[6] Similarly, two otherwise identical firms that have a different number of shares outstanding, and consequently prices which differ by a scale factor, would have different values of λ even when trading the same dollar volume would lead to the same change in firm value, so the value of β would be the same across the two firms.

This last example also illustrates the rationale for using returns rather than price changes. Under the hypothesis of identical liquidity, trading the same dollar volume would lead to the same percent change in firm value but will not lead to the same dollar change in prices across the two firms. For example, if the price of one stock is \$100/share and the other stock, having twice as many shares, is priced at \$50/share, a trade that moves the first stock's price to \$110 should move the second stock's price to \$55, a 10% return in each case.

The use of returns in the specifications in (1) and (2) makes price or quote changes non-linear functions of NTO across multiple periods.[7] Some have argued that the potential for profitable market manipulation exists if the permanent component of the price effect of trade is not linear in net turnover [Huberman and Stanzl (2000)]. From a theoretical standpoint our impact measures may also include temporary components. From an empirical standpoint, using returns versus price changes does not seem to make a significant difference in the results [see Hasbrouck (1991b)] so we use returns for the advantages afforded in time-series and cross-sectional comparability.

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[6]It has been argued that stock splits might enhance liquidity by increasing the number of smaller investors in the stock. One could test this hypothesis by testing for changes in β around the split [Breen, Hodrick, and Korajczyk (2000)]. Changes in λ should occur around a split even absent any change in liquidity.

[7]Using (1) and the definition of returns we get that $P_{i,\tau} - P_{i,\tau-1} = P_{i,\tau-1}\,[\alpha^P_{i\,t} + \beta^P_{i\,t}NTO_{i,\tau} + \epsilon_{i,\tau}]$. This implies that trading a given NTO over two periods has a different price effect than trading the same NTO over one period (because of the effect of the first period of trading on the price at the beginning of the second period).

Table 1 contains summary statistics on the cross-sectional distribution of the price impact coefficient from equations (1) and (2) using 5-minute intervals. Given the differences in the extent of inter-dealer trading between the exchanges and NASDAQ [Gould and Kleidon (1994)], we report separate statistics for NYSE/AMEX versus NASDAQ firms. The average coefficients are 3.09% (exchanges) and 1.96% (NASDAQ) using price returns and are 2.15% (exchanges) and 0.25% (NASDAQ) using quote returns. There is, however, substantial cross-sectional dispersion, as can be seen from the average monthly standard deviations of 26.69% and 6.16%, using price returns for the exchanges and NASDAQ respectively, and 14.08% and 0.77%, using quote returns for the exchanges and NASDAQ respectively. Moreover, in all cases, the 85th percentile average coefficient is more than 10 times that of the 15th percentile value. Since there exist some coefficient outliers, we also report the mean price impact after truncating the sample.[8] This truncation leads to slightly smaller means: 2.65% for exchange-listed firms and 1.85% for NASDAQ firms for equation (1) and 1.95% for exchange-listed firms and 0.24% for NASDAQ firms for equation (2). The cross sectional standard deviation drops dramatically with the truncation. For equation (1) the standard deviation drops from 26.69% to 10.94% for the exchanges and from 6.16% to 3.67% for NASDAQ. Similarly for equation (2), the standard deviation drops from 14.08% to 8.07% for the exchanges and from 0.77% to 0.54% for NASDAQ. All subsequent analyses are performed on the truncated sample.

The time series of average monthly coefficients for all four specifications are plotted in Figures 1 and 2. The following observations can be made. First, the average coefficients are relatively stable. Second, the price impact coefficients estimated using the last traded prices in the

[8] In our truncation, we remove the high and low observations of $\beta_{i,t}$ each month. This eliminated 2 observations out of an average of 3699 each month. Similar results are obtained using alternative truncations based on the number of standard deviations from the mean for that month. Using a twenty-standard deviation truncation eliminates and average of 1.7 firms per month.

period are consistently larger than those estimated using the quote midpoints. This is consistent with a number of microstructure models which imply both permanent and transitory effects of trade on prices [e.g., Glosten (1987), Easley and O'Hara (1987)]. This difference is found to be more pronounced for NASDAQ firms.

It is instructive to compare the magnitudes of our price impact measures to similar variables estimated in previous work. Hasbrouck (1991 a, b) models the reaction of quote midpoints to classified trading volume using exchange-traded firms, so his results are most directly comparable to our quote specification in (2) for NYSE/AMEX firms. There the average price impacts per 1000 share trade are equal to 0.299% and 0.255% in (1991a, Table IV and 1991b, Table 1). In our sample, a 1000 share trade corresponds to an average net turnover of 0.00919% for exchange-traded firms so our average coefficient of 1.95% for turnover of 0.1% corresponds to a predicted impact of a 1000 share trade equal to 0.179%, close to, but slightly lower than, that found in Hasbrouck (1991a, b).[9]

Glosten and Harris (1988, Table 2) report a permanent price effect per 1000 share trade between $0.0102 and $0.0133 and a total (permanent plus transitory) price effect between $0.0375 and $0.0567. Their sample consists of 250 firms traded on the NYSE. Given an average price, in their sample, of $20.00 (Table1), these dollar price impacts correspond roughly to a percentage impact between 0.05% and 0.07% (permanent) and between 0.19% and 0.28% (permanent plus transitory). While their estimate of the permanent price impact is considerably smaller than our estimates and those of Hasbrouck (1991a, b), their total price impact is similar to our estimates for NYSE firms of 0.179% using quote returns and 0.244% using price returns.

[9] The average exchange-traded impact per 1,000 share trade is 0.1792% = (1.95%/0.1%) × 0.00919%.

II. Cross-Sectional Results

Equity liquidity can differ across firms at a point in time and can differ across time for the same firm. In this section we analyze cross-sectional variation, while in the next section we analyze intra-day effects. We consider a set of predetermined firm characteristics that proxy for (i) the severity of the adverse-selection costs faced by uninformed traders transacting with informed traders [e.g., Glosten and Milgrom (1985)], (ii) the non-information-based costs of making a market [e.g., Stoll (1978)], and (iii) the extent of shareholder heterogeneity [e.g., Bagwell (1992)]. Table 3 provides the time-series means of the cross-sectional average and cross-sectional standard deviation for each of the firm characteristics (except the constant term). The variables included in the cross-sectional analysis are as follows.

(1) The first variable is a constant.

(2) The second variable is the relative size of the firm. Relative market capitalization is measured as the market capitalization of the firm's common equity at the end of the previous month divided by the average market capitalization of firms in the CRSP index (which includes NYSE, AMEX, and NASDAQ firms), minus one. A firm with market capitalization larger (smaller) than the typical CRSP firm would have a positive (negative) relative size. This variable allows for the empirical regularity [e.g., Hasbrouck (1991 a, b)] that the stocks of large firms, all else equal, appear to trade with smaller price impact per share traded.

(3) The third variable is the historical relative trading volume of the firm's equity. Relative trading volume is measured as the total trading volume from the previous three months divided by the trading volume, over the same three months, of the average firm traded on the NYSE, minus one. Equities trading with high volume should pose less of an inventory risk to dealers since their expected holding period is shorter, and therefore, these equities should be more liquid.

Alternatively, causality might run in the opposite direction: assets for which there is greater liquidity are traded more frequently. These arguments suggest that liquidity should be positively related to volume.

(4) The fourth variable is the recent price appreciation of the stock. Price appreciation is measured as the firm's stock price at the end of the previous month divided by the price six months prior, minus one. Bagwell (1991) develops a model wherein basis value heterogeneity influences liquidity through the extent of "locked-in" capital gains. In her model, holders of recently-appreciated assets have a disincentive to sell those shares because of locked-in capital gains, while holders of recently-depreciated assets have an incentive to sell and realize capital losses. Thus, the supply of tax-motivated traders is expected to be smaller in the former case and larger in the latter case. This argument leads to the prediction that liquidity should be negatively related to past price appreciation.

(5) The fifth variable is recent price movement, measured as the absolute value of the previous variable, $|(P_{t-1}/P_{t-7}) - 1|$. Both gains and losses might induce non-information-based trading due to portfolio rebalancing [e.g., Constantinides (1986)]. For example, if an investor seeks a 50/50 allocation of stocks and bonds, any movement in stock prices would generate rebalancing toward the target proportions. This suggests that liquidity and price movement should be positively related.

(6) The sixth variable is a dummy variable equal to unity if the firm is included in the S&P 500 Index and equal to zero otherwise. The S&P 500 portfolio is a popular benchmark for passive index funds. Harris and Gurel (1986) find permanent increases in trading volume for firms added to the Index, consistent with increases in institutional demand. Pruitt and Wei (1989) find significant positive price effects which are related to changes in institutional demand. Harford and Kaul (1998)

find additional evidence suggesting increased firm liquidity following addition to the Index[10]

(7) The seventh variable is the dividend yield on the firm's stock. Dividend yield is measured as the most recent indicated annual dividend, found using the Quarterly COMPUSTAT item number 20, divided by the share price at the end of the previous month. Dividend capture, a non-information-based motive for trade, is less costly to implement when the yield is high.[11] If there is less adverse selection risk when the fraction of uninformed traders in the stock is large, then a negative relation between the price impact and dividend yield is expected.

(8) The eighth variable is the percentage of the firm's return variance explained by the return on an aggregate stock market portfolio. We use the coefficient of determination, or R^2, from a regression of the firm's monthly percentage stock price change on the monthly percentage change in the NYSE index, estimated over the previous 36 months. Some of the price impact may be compensation to market makers for bearing inventory risks, as developed in Scholes (1972), since firm-specific (idiosyncratic) risks should be more difficult to hedge. Also, insiders are more likely to have private information of a firm-specific nature, making the severity of the adverse selection risk increasing in the level of firm-specific risk. Finally, a dividend capture strategy, as discussed in (7) above, is more easily hedged when firm-specific risk is lower. Since R^2 measures the fraction of the firm's variance *not* due to firm-specific news, smaller price impact may be observed when the R^2

[10]Conversely, inclusion in the S&P 500 Index has been argued to reduce the liquidity of a firm's equity, as many of the outstanding shares are effectively taken off the market by the passive indexing.

[11]For historical evidence of increased trading volume around ex-dividend dates, as well as evidence that dividend capture activities are performed extensively in high dividend yield stocks, see Lakonishok and Vermaelen (1986). Tax-motivated dividend capture strategies designed to take advantage of the dividend received deduction need to be held for a minimum of 46 days during the 90-day period beginning 45 days before the stock goes ex-dividend [U.S. Department of the Treasury (1999)]. Because of this, we would expect non-information based trading to be high in both ex-dividend and non-ex-dividend months for high yield firms.

is larger.

Given the differences in market structure between the exchanges and NASDAQ, particularly differences in the meaning of volume, we estimate the cross-sectional regressions separately for exchange listed (NYSE/AMEX) and over-the-counter (NASDAQ) firms. Because of this, our next variable, which allows us to compare price impact across NYSE and AMEX firms, is not included in the cross-sectional analysis for the NASDAQ subsample.

(9) The ninth variable measures differences in liquidity between the equities of firms that listed on the NYSE versus the AMEX. It is a dummy variable equal to unity if the firm is traded on the NYSE and equal to zero otherwise. Any differences in liquidity could be due either to actual differences liquidity provision across exchanges or to different listing requirements of the exchanges which are correlated with liquidity. There is a large literature investigating the effects of different market structures on liquidity [e.g., Marsh and Rock (1986), Hasbrouck and Schwartz (1988), Huang and Stoll (1996 a, b)].

(10) The tenth variable is equal to unity if the firm's last earnings release was more than two months ago (and less than 40% of the other firms have at least a two month reporting lag) or if the firm's last earnings release was more than three months ago and equal to zero otherwise. Korajczyk, Lucas, and McDonald (1992) derive a model in which the precision of the managers' private information increases between regularly scheduled information releases, such as earnings. Thus, while immediately after such a release the adverse selection problem is small, when a long time has passed since any information release the asymmetry of information is large, implying a larger price impact.

(11) The eleventh variable is the institutional holdings of the equity. Institutional holdings are measured as the percentage of the firm's equity held by institutional investors as of the end of the

previous quarter (e.g., a value of 25 denotes 25%). For example, the institutional holding data for March are used in the cross-sectional regressions for April, May, and June. Hodrick (1999) argues that the extent of institutional holdings proxies negatively for both taxation-induced and information-induced illiquidity.

(12) The twelfth variable is a dummy variable equal to unity if options are traded on the firm's equity and equal to zero otherwise. While dealers may demand a smaller price concession if firm-specific risk can be hedged through option transactions, it may instead be the case that price concessions are larger for stocks with traded options, since, because of possible order fragmentation, a market maker cannot immediately determine whether the initiator of the trade is also trading in the options market [e.g., Bernhardt and Hughson (1997)].

(13) The thirteenth variable is the inverse of the stock price as of the previous month. Some of the measured price impact of trades using price returns might be due to bid-ask-bounce. In order to avoid having this effect attributed to the dividend yield variable, we include the inverse of the stock price. Given the discreteness of bid-ask spreads, price impact would be more pronounced for lower-priced stocks.

For each month, t, we estimate the cross-sectional relation between the estimates, $\hat{\beta}_{i,t}$, and the firm-level characteristics:

$$\hat{\beta}_t = X_t \Gamma_t + \nu_t \tag{3}$$

where N(t) represents the number of firms in the sample for month t, $\hat{\beta}_t$ is the N(t)×1 vector of estimated coefficients for month t, and let X_t is the N(t)×13 (N(t)×12) matrix of predetermined firm characteristics used to explain differences in $\hat{\beta}_{i,t}$ for NYSE/AMEX (NASDAQ) firms. As in Fama and MacBeth (1973), we use the time series of estimates $\hat{\Gamma}_1$, $\hat{\Gamma}_2$, ...,$\hat{\Gamma}_T$ (T = 53) to estimate the average $\bar{\Gamma} = (\hat{\Gamma}_1 + \hat{\Gamma}_2 + ... + \hat{\Gamma}_T)/T$ and to get standard errors for the elements of $\bar{\Gamma}$. We estimate (3) each month using ordinary least squares (OLS).[12]

Let $\hat{\gamma}_{j,t}$ denote the j^{th} element of $\hat{\Gamma}_t$. In Table 4 we show the autocorrelations of $\hat{\gamma}_{j,t}$ for lags one through six months. There is some evidence of time series dependence in the estimates, $\hat{\gamma}_{j,t}$, especially for the estimates of dividend yield and percent institutional for NYSE/AMEX firms (Panel A) and for the dividend yield, option dummy, and inverse price for NASDAQ firms (Panel B). In light of the autocorrelation in the $\hat{\gamma}_{j,t}$ series, we also estimate t-statistics for $\bar{\Gamma}$ that are adjusted for autocorrelation in the Γ coefficients.[13] Since these autocorrelation-consistent standard errors do not lead to substantive changes in inference we report t-statistics in Table 5 that are not adjusted for autocorrelation in the estimates.

The point estimates, $\bar{\gamma}_j$, and associated t-statistics are presented in Table 5. The results for price returns with 5-minute intervals are given in the first two columns. For NYSE/AMEX firms (Panel A), all of the variables have marginal effects on the price impact coefficient that are statistically significant at the 5% level except for the dividend yield, option dummy, and price movement variables. For NASDAQ firms (Panel B), all variables are significant except for the earnings release and price movement variables. The average monthly R^2 is 9.2% for the NYSE/AMEX subsample and 10.5% for the NASDAQ subsample.

The magnitudes of the coefficient estimates suggest significant cross-sectional variation in

[12]We also estimate (3) using weighted least squares, where the weights are inversely proportional to the standard error of the residuals from the first-stage regression (1). We find that the results are qualitatively similar and do not report them here.

[13]Our autocorrelation-consistent standard errors are estimated using the Newey and West (1987) version of the White (1980)/Hansen(1982) standard errors, with four lags.

liquidity. For exchange-traded firms (Table 5, Panel A), the coefficient of 0.24 for the relative market capitalization implies that if a firm's size were to increase by one (cross-sectional) standard deviation, 9.55, then the coefficient would increase by 2.28%. This positive relation stems from our measuring the coefficient per unit of net turnover: as the firm's size increases, the magnitude of a trade which represents a given percent turnover also increases. If we instead define volume as a fixed number of shares, we find that the relation between size and the coefficient is significantly negative, as found in Hasbrouck (1991 a, b).

The coefficient of −0.58 for relative trading volume implies that if a firm's trading volume in the previous quarter increases by one standard deviation, then the coefficient would decline by 1.38%. These findings are consistent with the conventional wisdom that the stocks of firms with greater trading volume tend to trade with more liquidity.

The coefficient of −0.65 for price appreciation implies that if a firm's price appreciation over the previous six months were to increase by one standard deviation, then the coefficient would decline by 0.46%. This result appears consistent with the finding in Brown and Ryngaert (1992) that tendering rates in self-tender repurchase offers are greater following a larger run-up in share prices, and inconsistent with the hypothesis that firms with greater price appreciation are less liquid due to the extent of "lock-in" induced by capital gains taxation. Odean (1998) also finds evidence that individual investors have a greater tendency to sell assets that have appreciated in value versus those that have depreciated in value. If this type of behavior results in a sufficiently large source of non-information-based traders after a stock appreciates, it would be consistent with our observed increase in liquidity for those firms.

Firms included in the S&P 500 Index have significantly lower price coefficients than do other firms after controlling for other independent variables, consistent with the documented permanent increase in institutional demand following inclusion in the Index. The coefficient of −2.34 on the R^2 variable implies that an increase in the R^2 by one standard deviation leads to a decrease in price impact of 0.29%. As discussed above, this is consistent with both adverse selection and non-adverse selection market making costs. Firms trading on the New York Stock Exchange have higher price impact coefficients than do American Stock Exchange firms. The coefficient of 0.72 on the earnings release dummy variable implies that firms that have not released earnings recently have a higher price impact. This is consistent with the severity of adverse selection increasing the longer since an earnings release.

The coefficient of −0.06 on the percentage of stock held by institutions implies that if a firm's institutional holdings were to increase by one standard deviation, then the price impact would decline by 1.41%. This finding might merely reflect the fact that institutional holders choose to hold more liquid stocks, or might instead reflect some additional influence that institutional holdings have on liquidity [e.g., Bagwell (1992)]. The coefficient of 2.68 on the inverse price variable implies that a one standard deviation increase in the inverse price would lead to an increase in the price impact of 1.06%.

For firms traded on NASDAQ (Table 5, Panel B), the coefficients have the same sign as for the exchange-traded firms with one exception: the coefficient for the option dummy is positive for NASDAQ. The late earnings release variable becomes insignificant (at the 5% level) for NASDAQ firms while the dividend yield and option traded variables become significant.

III. Robustness Analysis and Interpretation

In this section, we confirm the robustness of the results and address a number of issues regarding the interpretation of the results. First, we discuss endogeneity issues associated with interpretation of the results. Second, we compare the two alternative methods of calculating equity returns and the two alternative trading time intervals. Third, we look for intra-day patterns in the price impact. Fourth, we test for linearity and symmetry of the price impact specification. Finally, we look at the relation between our price impact measure and bid/ask spreads.

IIIa. Endogeneity of Chosen Trades

There are several issues that one should bear in mind when interpreting the results presented in this paper. As is true for all such transaction-based studies, only trades actually chosen to be implemented are observed in the data. This self-selection imparts a potential bias from those trading terms expected by a trade randomly brought to the market. This may cause the coefficient to underestimate the magnitude of the price response to trading volume, since trades not executed, and hence not observed in the data, may well be systematically those where the price response to the trade would have been less favorable.

IIIb. Price Returns versus Quote Returns

As shown in Figures 1 and 2, the estimated coefficients using returns calculated with quote midpoints (as in (2)) are generally smaller than those with transaction prices (as in (1)), with the difference being much larger for NASDAQ firms. There are periods of time over our sample when delays in reporting may cause prices and quotes to be misaligned [Schultz (1997)]. While this might explain some of the observed difference between β^{P}_{it} and β^{Q}_{it}, this is not likely to be a complete explanation since the time series properties of the misalignments in Schultz (1997) would imply far less stability in the differences than that observed in Figures 1 and 2.

The quote-return results for 5-minute intervals are reported in the third and fourth columns of

Table 5. The price- and quote-return results for 30-minute intervals are reported in columns five through eight of Table 5. For 5-minute intervals, the average price-return and quote-return cross-sectional regression coefficients have the same sign, except for the dividend yield for exchange-traded firms and the earnings release variable for NASDAQ firms. In each of these cases, one or both of the coefficients is insignificantly different from zero. For 30-minute intervals, the price-return and quote-return results agree in sign for exchange-traded firms, while for NASDAQ firms there are several cases where the signs differ across returns methods. In only two of those cases, the sign differences where both the price-return and the quote-return coefficients are significantly different from zero, the dividend yield and price movement variables. Thus, relation between the cross-section firm characteristics and price impact is qualitatively consistent across choice of return methods.

IIIc. Choice of Observation Interval

The process by which orders are filled has important implications for all studies which measure liquidity through the relation between trading volume and price changes. A broker may "shop" a large order, thus revealing some information about the order before its actual execution. Similarly, a large trade may be negotiated in the upstairs market and then brought to the floor for execution. In each of these cases some or all of the price impact will occur before the actual trade [Nelling (1996)]. One would expect that the shorter the time interval used, the more severe would be this problem. A day-long observation interval will include the full price impacts for orders shopped within the day but not for orders which take several days to execute. Analogously, a 30-minute (5-minute) observation interval will include the full price impact for orders only shopped within the 30-minute (5-minute) period but not for orders which take several hours to execute.[14] This argument suggests that the process of shopping a block would lead, other things equal, to a larger estimated impact measure for a longer time interval. Another reason why one might expect the impact measure to be larger for longer time periods is the possible contamination of the price/volume relation by feedback effects. That is, rather than volume driving price changes, price changes earlier in the period may generate volume later in the period. Both portfolio insurance and price change-induced portfolio rebalancing [e.g., Constantinides (1986)] are trading rules that might cause such a feedback. Feedback effects would lead to upward biases in the estimates of the price impact, with a less severe feedback problem for shorter observation intervals. A countervailing effect is that the same amount of net volume traded over a shorter period signals a greater demand for immediacy of execution. Thus, one might expect that, other things equal, there will be a larger coefficient when measured over a shorter time period. To determine which of these effects dominates, we compare the coefficients estimated using 5-minute intervals to those estimated over 30-minute intervals. The coefficients are found to be similar to each other, as can be seen from Figure 1, though we find that the 5-minute intervals lead to slightly higher measures when using transaction prices and slightly lower measures when using quote midpoints.

We also compare our inferences about the cross-sectional relation between the price impact and the predetermined firm characteristics in equation (3) using different time intervals (Table 5). When we use price returns to estimate the price impact, the inferences (at the 5% level) using 5-

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[14]Ideally, we would like to know the whole history of the order, from the decision to trade through execution, in order to obtain the full measure of the price impact. Given the anonymity of the trading process, this is not feasible with standard data sources like the TAQ database. Keim and Madhavan (1995, 1997) have a unique set of data for a group of institutional traders that allows inferences about the full price impact for their sample of trades. In Section IV, we use those data to compare the estimated price impacts to the actual price impacts.

minute and 30-minute price returns are identical for exchange-traded firms (one variable changes sign, but the coefficients are statistically insignificant). Turning to price impacts measured with quote returns, the signs of the coefficients are the same between 5-minute and 30-minute intervals. For NASDAQ, firms, two variables change sign between 5-minute and 30-minute returns but in each case at least one of the coefficients is insignificant. One variable changes sign between 5-minute and 30-minute quote returns (dividend yield) one of the coefficients is statistically insignificant.

IIId. Intra-Day Seasonality

We check for intra-day seasonality of the price impact coefficients by including dummy-variable interaction terms on net turnover:

$$(P_{i,\tau} - P_{i,\tau-1})/P_{i,\tau-1} = \alpha^P_{i\,t} + \beta^P_{i\,t,\,Mid}NTO_{i,\tau} + \Delta^P_{i\,t}\,\delta_{Early,t}NTO_{i,\tau} + \theta^P_{i\,t}\,\delta_{Late,t}NTO_{i,\tau} + \epsilon_{i,\tau} \quad (4)$$

$$(Q_{i,\tau} - Q_{i,\tau-1})/Q_{i,\tau-1} = \alpha^Q_{i\,t} + \beta^Q_{i\,t,\,Mid}NTO_{i,\tau} + \Delta^Q_{i\,t}\,\delta_{Early,t}NTO_{i,\tau} + \theta^Q_{i\,t}\,\delta_{Late,t}NTO_{i,\tau} + \epsilon_{i,\tau} \quad (5)$$

where $\delta_{Early,t}$ is a dummy variable equal to unity during the period 9:30 a.m. to 10:00 a.m. Eastern time, and equal to zero otherwise, and $\delta_{Late,t}$ equal to unity during the period after 3:30 p.m. Eastern time, and equal to zero otherwise. Thus, $\beta^P_{i\,t,\,Mid}$ and $\beta^Q_{i\,t,\,Mid}$ measure the price impact over the mid-day period from 10:00 a.m. to 3:30 p.m. while $\Delta^P_{i\,t}$ and $\Delta^Q_{i\,t}$ ($\theta^P_{i\,t}$ and $\theta^Q_{i\,t}$) measure any incremental price impact observed at the beginning (end) of the trading day. The time series means of the coefficients and t-statistics are presented in Table 6. The results show a pronounced increase in the price impact at both the beginning and the end of the day, with the exception of quote returns for NASDAQ firms. The average beginning (end) of day price impact $\beta^P_{i\,t,\,Mid} + \Delta^P_{i\,t}$ and $\beta^Q_{i\,t,\,Mid} + \Delta^Q_{i\,t}$ ($\beta^P_{i\,t,\,Mid} + \theta^P_{i\,t}$ and $\beta^Q_{i\,t,\,Mid} + \theta^Q_{i\,t}$) is approximately three times (1.5 to 2 times) the size of the mid-day price impact, again with the exception of quote- returns for NASDAQ firms. These results are consistent with the empirical findings of Hasbrouck (1991b) and Foster and Viswanathan (1993). Taken at face value, the lower middle-of-day price impact suggests that the midday seems to be a more liquid period. An alternative explanation of the smaller midday price impact is that large working orders or trades in the upstairs market that are shopped during the early part of the day, with much of the information revealed by the trade becoming public early in the day, are crossed in the middle of the day without much contemporaneous price impact [Nelling (1996)].

IIIe. Linearity and Symmetry of Price Impact

Empirically, it is common to find non-linear price impact functions [e.g., Hasbrouck (1991 a) and Hausman, Lo, and MacKinlay (1992)]. There are several reasons why we might expect such a non-linearity in the data. One is the existence of information leakages for block trades, as discussed above. Another reason relates to the endogeneity of observed trades discussed in Section IIIa. Since large trades desired by informed traders are likely to be broken into smaller components and traded anonymously, uniformed traders that are able to credibly signal that they are uniformed are more likely to be able to trade large volumes with small price impact without breaking-up the trade. Thus, for the trades observed in the data, we may see large trades that have proportionately smaller price impact than for small trades. Both of these reasons lead us to expect convexity in the price impact, as a function of net turnover.

To test for non-linearity, we add signed squared net turnover to the specifications in (1) and (2). Signed squared net turnover, NTOSQ, is defined as:

$$NTOSQ_{i,\tau} = \iota(NTO_{i,\tau})\times NTO^2_{i,\tau} \qquad (6)$$

where $\iota(NTO_{i,t}) = 1$ if $NTO_{i,t} > 0$ and $\delta(NTO_{i,t}) = -1$ otherwise. The estimated regressions are:

$$(P_{i,\tau} - P_{i,\tau-1})/P_{i,\tau-1} = \alpha^{P}_{i\,t} + \beta^{P}_{i\,t}NTO_{i,\tau} + \kappa^{P}_{i\,t}\, NTOSQ_{i,\tau} + \epsilon_{i,\tau} \tag{7}$$

$$(Q_{i,\tau} - Q_{i,\tau-1})/Q_{i,\tau-1} = \alpha^{Q}_{i\,t} + \beta^{Q}_{i\,t}NTO_{i,\tau} + \kappa^{Q}_{i\,t}\, NTOSQ_{i,\tau} + \epsilon_{i,\tau} \tag{8}$$

We provide only a summary of the results for brevity. The coefficients on NTOSQ are found to be negative and significant regardless of whether the firms are exchange-traded or NASDAQ, whether price or quote returns are used, or whether 5-minute or 30-minute intervals are used, with one exception. In the case of the 30-minute observation interval, using price returns for NYSE/AMEX firms the coefficient on NTOSQ is positive and insignificant.

Recall that the price appreciation variable in the cross-sectional analysis was motivated by the possibility of non-information, tax-based trading. If such trading exists, it might suggest an asymmetry in the price impact coefficients. For example, following a price increase, tax-loss selling does not occur increasing the probability of informed selling, while following a price decline the probability of uninformed, tax-motivated selling increases. This would imply that the price concession for seller-initiated trades would be relatively higher after price increases and relatively lower after price declines.

To test for asymmetry, we add to the specifications in (1) and (2) an interaction between net turnover and a dummy variable that is equal to unity if NTO is positive and equal to zero otherwise. The interaction term is defined as:

$$NTOBUY_{i,\tau} = \xi(NTO_{i,\tau})\times NTO_{i,\tau} \tag{9}$$

where $\xi(NTO_{i,t}) = 1$ if $NTO_{i,t} > 0$ and zero otherwise. The estimated regressions are:

$$(P_{i,\tau} - P_{i,\tau-1})/P_{i,\tau-1} = \alpha^{P}_{i\,t} + \beta^{P}_{i\,t,sell}NTO_{i,\tau} + \beta^{P}_{i\,t,dif}NTOBUY_{i,\tau} + \epsilon_{i,\tau} \quad (10)$$

$$(Q_{i,\tau} - Q_{i,\tau-1})/Q_{i,\tau-1} = \alpha^{Q}_{i\,t} + \beta^{Q}_{i\,t,sell}NTO_{i,\tau} + \beta^{Q}_{i\,t,dif}NTOBUY_{i,\tau} + \epsilon_{i,\tau}. \quad (11)$$

Here $\beta_{i,t,sell}$ measures the price impact for seller-initiated trades and $\beta_{i,t,dif}$ measures the difference between the price impact for buyer-initiated and seller-initiated trades. We provide only a summary of the results for brevity. The average coefficients on NTOBUY are positive and significant regardless of whether the firms are exchange-traded or NASDAQ, whether price or quote returns are used, or whether 5-minute or 30-minute intervals are used. Thus, buyer-initiated trades seem to have a larger price impact than seller-initiated trades, consistent with previous research [e.g., Holthausen, Leftwich, and Mayers (1990)]. However, our prediction of the negative cross-sectional relation between past price appreciation and $\beta_{i,t,dif}$ is not born out, since the cross-sectional relation is positive but insignificant.

IIIf. Bid/Ask Spreads and the Price Impact Coefficient

What if the price impact coefficient is merely picking up the effects of differential bid/ask spreads across stocks? As in Hasbrouck (1991 b), we examine the relation between a stock's price impact and its average percentage spread. For each time interval, we calculate the percentage spread as the difference between the prevailing ask and bid quotes divided by the average of the bid and ask quotes. We estimate the average spread each month by averaging over all available 5-minute or 30-minute observations.

We first investigate the power of the spread to explain the cross-sectional differences in the price impact coefficients. For each month t, we regress the price impact coefficient on (i) the average bid/ask spread and a constant and on (ii) the average bid/ask spread plus the firm characteristics used above in (3):

$$\hat{\beta}_t = c_t. + S_t \, \phi_t \; + \nu_t. \tag{12}$$

and

$$\hat{\beta}_t = [S_t \; X_t][\phi_t \;\; \Gamma_t']' + \zeta_t. \tag{13}$$

where S_t is the N(t)×1 vector of average bid/ask spreads in month t, X_t is the matrix of predetermined firm characteristics as in (3), and c_t is a regression intercept.

We provide only a summary of the results for brevity. The coefficient, $\overline{\phi}$, is always positive and statistically significant regardless of whether the firms are exchange-traded or NASDAQ, whether price or quote returns are used, or whether 5-minute or 30-minute intervals are used. For price returns, the average R^2 of the monthly spread regressions, (12), is 4% for NYSE/AMEX firms and 10% for NASDAQ firms, while for quote returns, the average R^2 of the monthly regressions is 3% for NYSE/AMEX firms and 1% for NASDAQ firms.[15] For the regressions that include both the spread and firm characteristics, (13), the coefficients for the other firm characteristics generally retain the sign and level of significance as originally found in (3). The main exceptions are variables which, like the percentage spread, have price in the denominator; (the dividend yield and inverse price variables). As in Hasbrouck (1991 b), our findings indicate that the

[15]As reported above, the regressions including only the firm characteristics have average R^2 values of 9.2% for the NYSE/AMEX subsample and 10.5% for the NASDAQ subsample

price impact coefficient and the bid/ask spread are related, but the spread does not prove to be a sufficient statistic for the price concession.

IV. Out-of-Sample Impact Prediction

There are many important applications for which an estimate of the expected price impact of a transaction would be extremely useful. There are many documented “anomalies” where profitable trading strategies seem to exist based on the comparison of the returns on simulated portfolio strategies to an appropriate (risk-adjusted) benchmark. While it is straight-forward to include the direct costs of commissions when comparing portfolio returns, it is more difficult to quantify the price impact that would be incurred. Moreover, a portfolio manager might wish to evaluate the trade execution provided by a broker. While commissions are the easiest component of transactions costs to observe, the price impact is likely to be much larger for sizable trades [Loeb (1983)]. An estimate of the expected price impact, that accounts for cross-sectional variation in liquidity, provides a benchmark against which given trade execution can be compared. The actual price impact will also reflect opportunistic behavior by the broker, such as front-running.

For a given hypothetical trade of size NTO in asset i, price impact can be estimated either directly, $\hat{\beta}_{i,t} \times \text{NTO}$, or out-of sample from the predicted value obtained from cross-sectional regressions such as (3), $X_{i,t}\bar{\Gamma} \times \text{NTO}$. The direct estimate has the advantage that it may incorporate firm-specific factors that are not explained by the cross-sectional regression, while the latter approach has the advantage that it can be applied to firms and time-periods not covered by the original tick-by-tick sample. For example, Mitchell and Pulvino (2000) use our cross-sectional regression coefficients to estimate the price impact incurred in a merger arbitrage trading strategy over a period far longer than that covered by our sample.

There are many reasons why the predicted price impacts might deviate from the actual price impacts. While either of the predicted price impacts, $\hat{\beta}_{i,t}\times$NTO or $X_{i,t}\bar{\Gamma}\times$NTO, implicitly assumes that a trade of size NTO is executed in the observation interval, large orders are often split up rather than executed in one trade [Kyle (1985), Back (1992), Bertsimas and Lo (1998)]. For those orders that are broken up, the actual average price impact should be less than that predicted. Similarly, while the predicted price impact implicitly assumes that market orders are used to execute the trade, patient traders, who supply liquidity, may choose to execute the trade with an alternative order, such as a limit order. Again, the actual price impact should be less than that predicted.

We compare the price impact, predicted out-of-sample, $X_{i,t}\bar{\Gamma}\times$NTO, to the actual price impact for a sample of trades executed by twenty-one institutional traders over the period from January 1991 to March 1993. The data, collected by the Plexus Group and including detailed information on the equity trades by these institutions, are those from Keim and Madhavan (1995, 1997). The comparison is out-of-sample in that the actual equity trades occur over a period, January 1991 to March 1993, that has very little overlap with the period used to estimate $\hat{\beta}_{i,t}$ and $\bar{\Gamma}$, January 1993 to May 1997. The actual price impacts provide a useful benchmark against which our out-of-sample impact predictions can be compared. We investigate how various aspects of the transaction relate to the price impact forecast error.

While the Plexus data set does not provide the identity of the participating institutions, Plexus does classify the institutions by investment styles: value-based, technical- or momentum-based, and index funds. We also know (a) the type of order placed including an index of the urgency of the order; (b) the price of the stock both prior to the decision to trade and prior to sending the order to the trading desk; (c) the desired number of shares to trade and the direction of trade (i.e., a buy vs. a sell); (d) the average trade price and the actual number of shares traded; (e) the dates at which the trading decision was made, the order was sent to the trading desk, trading began, and the order was completed. While we can determine the number of days that the broker took to execute the trade, we can not determine the number of separate trades used to execute the trade since the institutions only receive information on the total shares traded and the average price within a day. We exclude trades for which the time between either the decision date and the desk date, the desk date and the beginning of trade, or the beginning and completion of trade exceeds three weeks.[16] After matching the Plexus data with our sources for $X_{i,t}$ and applying the three-week screen, we have data from 56,211 trades for a total of 1.9 billion shares representing $62.4 billion in value.

Table 7 provides summary statistics on the timing of trade decisions, sending orders to the trading desk, and the initiation and completion of trading. On average there are 1.9 days between making the decision to trade and sending the order to the trading desk. The time between the receipt of the order by the trading desk and the initiation of trade averages 1.21 days. The average number of days between the beginning and end of trading is 0.37. Most orders (92.5%) are filled on the same day that trading begins.

We calculate two measures of actual price impact. The first is the average trade price minus the closing price the day before the trade is sent to the trading desk, expressed as a fraction of the prior closing price:

$$I_{Desk} = 100 \times \frac{\overline{P} - P_{Desk}}{P_{Desk}}.$$

The second is the average trade price minus the closing price the day before the decision is made to

[16]The results are not sensitive to this 21-day screen.

trade:

$$I_{Decide} = 100 \times \frac{\overline{P} - P_{Decide}}{P_{Decide}}.$$

If there is no leakage of information about the trade between the decision date and the date the order is sent to the trading desk, then I_{Desk} would be the better of the two measures since $I_{Decision}$ would be equal to I_{Desk} plus noise due to other information released between the decision and desk dates. Conversely, if there is leakage of information between the decision date and the date the order is sent to the trading desk, then I_{Desk} would not include the full price impact while $I_{Decision}$ would.

These two actual price impact measure are compared to two predicted price impact measures. The first, which assumes that the order is executed within one trading interval (five or thirty minutes), comes directly from our cross-sectional regression:

$$\hat{I}^{u}_{i,t} = X_{i,t}\overline{\Gamma} \times NTO_{i,t}. \tag{14}$$

The second measure is adjusted for the amount of time taken to execute the order:

$$\hat{I}^{a}_{i,t} = X_{i,t}\overline{\Gamma} \times NTO_{i,t} \times 0.5 \times (1 + 1/n), \tag{15}$$

where n is the number of days over which trading of the order extends. The superscripts *u* and *a* stand for unadjusted and adjusted. The second measure is meant to adjust for the effects of breaking up the order [Bersimas & Lo (1998)]. Since the data do not allow us to determine the actual manner in which the order is broken up, we use the simple approximation that there is one trade per day.

Table 8 provides averages of the actual and predicted impacts by investment style, by buy/sell order type, and by exchange-traded versus NASDAQ. The predicted price impact is generally larger (in absolute value) than the actual price impact. To aggregate across buy and sell orders we calculate the signed forecast error:

$$\delta_{i,t} \times (I_{z\ i,t} - \hat{I}_{i,t}^{y}), \tag{16}$$

where z = Decision or Desk, y = *a* or *u*, and $\delta_{i,t}$ = 1 for buys and −1 for sells. A positive (negative) average signed forecast error indicates that the predicted price impact was smaller (larger) than the actual price impact.

We first compare the actual impacts to the predicted impacts obtained using price returns. The consistently negative average forecast errors indicate that the actual price impacts tend to be smaller than the predicted price impacts. Across style categories, the predicted values are closest to the actual values for index funds, with an average (weighted by number of trades) signed forecast error of −6 basis points. The average signed forecast error is −91 basis points for value investors and −113 basis points for technical investors. When sorted by exchanges versus NASDAQ, we find an average signed forecast error of −71 basis points for firms listed in the NYSE and AMEX and −144 basis points for NASDAQ. When sorted by buy versus sell orders, we find an average signed forecast error of −74 basis points for buy orders and −98 basis points for sell orders.

When we use quote returns to estimate impact, the predicted values are closer to the actual values. Again, the predicted values are closest to actual price impacts for index funds, with an average (weighted by number of trades) signed forecast error of −0.04 basis points. The average signed forecast error is −65 basis points for both value and technical investors. When sorted by exchanges versus NASDAQ, we find an average signed forecast error of −57 basis points for firms listed in the NYSE and AMEX and 9 basis points for NASDAQ. When sorted by buy versus sell orders, we find an average signed forecast error of -45 basis points for buy orders and -54 basis points for sell orders.

To investigate potential explanations for the observed differences, we regress the forecast errors in (16) on characteristics of the trade. The trade characteristics are:

1) A constant

Trade size related variables

2) NTO - net turn over

3) NTO^2

Style related variables

4) A value dummy variable equal to unity if the institution is a value-style investor and equal to zero otherwise.

5) A technical dummy variable equal to unity if the institution is a technical investor and equal to zero otherwise.

Exchange related variable

6) A NASDAQ dummy variable equal to unity if the stock is listed on NASDAQ and equal to zero otherwise.

Variables measuring the time taken to complete the trade

7) The number of days between the decision date and the date the order is sent to the trading desk.

8) The number of days between the date the order is sent to the trading desk and the date trading begins.

9) The number of days between the beginning and end of trading.

Variables measuring urgency of trade and the type of order

10) An urgency Code (ranging from 1 for very urgent to 5 for not urgent).

11) A working order dummy variable equal to unity if the broker is instructed to work the order and equal to zero otherwise.

12) A limit order dummy variable equal to unity if the order is a limit order and equal to zero otherwise.

13) A market not held dummy variable equal to unity if the order is a market not held order and equal to zero otherwise.

14) A cross dummy variable equal to unity if the order was executed using a crossing network and equal to zero otherwise.

15) A principal dummy variable equal to unity if the order was executed through a principal trade and equal to zero otherwise.

Non-price impact costs

16) The commissions paid to the broker, expressed in percent of the trade value (determined by the stock price prior to sending the order to the trading desk).

17) A trade shortfall variable equal to the difference between the size of the desired trade size and the actual trade size (measured in 100,000 share units).

As argued in Section II, we expect that very large executed trades in the Plexus sample will have actual price impacts smaller than that predicted (i.e., a more negative forecast error) since the choice to execute the trade is endogenous. This could be manifested either as a negative coefficient on NTO^2 or as a negative coefficient on NTO with a non-positive coefficient on NTO^2.

Since we have incorporated dummy variables for both value and technical investing, the base-case investment style is index fund investing. Since value investors have less demand for immediacy, we would expect the coefficient on the value dummy to be negative. Keim and Madhavan (1995) find evidence that technical traders are willing to trade slightly more patiently than index traders but less patiently than value traders, which leads us to expect a coefficient for the technical dummy variable between zero and the coefficient for the value dummy variable.

There could be a number of cross-exchange differences that are picked-up by the NASDAQ dummy variable. Given that volume is "double counted" on NASDAQ versus the exchanges, we would expect the measured values of price impact to be downward biased for NASDAQ firms, leading to a positive forecast error.

The times between the date the decision is made to trade, the date the order is sent to the trading desk, and the date trading begins may reflect the portfolio manager's and the trading desk's judgement about the times at which market conditions are most favorable. If so, we would expect that actual price impact to be lower for longer waiting periods, leading to negative coefficients on these variables. However, the causality could also run in the opposite direction, if institutions must wait longer for difficult, low-liquidity trades. If the time between the beginning and ending of trade is a proxy for breaking up the trade, then we expect the forecast error to be more negative the longer the trading time when we use the unadjusted predicted impact. For the adjusted price impact, we would expect to find no relation between the forecast error and the trading interval if our approximation of one trade per day is reasonable. As above, an alternative interpretation would be that trades taking a long time are difficult, high impact trades.

We predict a negative coefficient on the urgency variable if a very urgent trade (i.e., one with a low urgency code) has a higher actual price impact. We predict that trades executed through limit orders will have a lower price impact, implying a negative coefficient on the limit order dummy variable. Other things equal, a working order or a market not held order should have lower price impact than a market order. Therefore, we expect that the coefficients on these two dummy variables would be negative. However, other things may not be equal in the sense that these types of orders are used for more difficult trades.

We expect that commissions and price impact may be substitutes as transactions costs. One reason to pay a higher commission rate is to get better execution. Therefore, we predict that the coefficient on commissions will be negative. The shortfall variable is related to the opportunity costs associated with failure to trade. There might be a very mechanical link between the forecast error and the shortfall. An institution might specify a desired trade size and a maximal (minimal) price to buy (sell). The closer that price boundary is to the current market price, the more likely it is that there is an incomplete fill of the order.

The results for the regressions are reported in Table 9. The signs of the coefficients and incidence of statistical significance (at the 5% level) are generally consistent across all four specifications. The coefficients on NTO are positive, but only significant for the adjusted measures. The coefficients on NTO^2 are consistently significantly negative. Thus, the predicted price impact will tend to overestimate the actual price impact for those large trades that are executed. Both the value and technical dummy variables are negative and statistically significant, suggesting that these institutions demand less liquidity than index funds. The coefficient for the technical dummy variable is smaller in absolute value than the value dummy variable, consistent with the evidence of relative demand for liquidity found in Keim and Madhavan (1995). The NASDAQ dummy variable is significantly negative.

The time between the decision to trade and sending the order to the trading desk has a significantly negative coefficient, as does the time between sending the order to the trading desk and the beginning of trading. This is consistent with portfolio managers waiting for favorable market liquidity before handing the order to the trading desk and with the trading desks waiting for favorable market liquidity before beginning to trade. The time it takes to execute the trade has a significantly negative coefficient when the unadjusted predicted impact is used, and an insignificant coefficient (that changes sign) when we use the adjusted predicted impact. As discussed above, this is what one would expect if both the trading interval is related to the manner in which the order is broken-up, and the approximation of one trade per day is reasonable.

Less urgent trades yield lower actual price impacts since the urgency code has a negative coefficient (trades that are not urgent have a more negative prediction error than urgent trades). The coefficients are significant only when we calculate the actual price impact using the price prior to the decision to trade.

As expected, the prediction errors are more negative for limit orders than for working orders, consistent with the fact that limit orders provide rather than demand liquidity. The coefficients on the not held order dummy variable are positive and significant only when we calculate the actual price impact using the price prior to the decision to trade. This might be due to portfolio managers choosing to submit not held order when the stock price has moved against them during the period prior to sending the order to the desk. The coefficients on the crossing network dummy are insignificantly positive. The coefficients for the principal trade dummy are negative and significant.

The coefficients on commissions are significantly negative, consistent with the argument that higher commissions are paid to execute trades with less price impact. The fact that the coefficients are greater than one in absolute value indicates a greater than one-for-one trade-off between extra commissions and lower price concessions. The coefficient on the trade size shortfall is significantly negative only when impact is measured relative to the trading desk date. Trades for which the trader specifies a smaller maximum price impact are likely to have a larger shortfall, on average.

All of the above results are for signed forecast errors obtained using price-return regressions to predict the price impact. We obtain similar results for signed forecast errors using the quote-return regressions to predict price impact. We only provide a summary of the results for brevity. The main differences are that with quote returns the coefficient on NTO becomes significantly negative and the coefficient on the NASDAQ dummy variable becomes significantly positive.

The institutional trade data collected by the Plexus Group provides an important out-of-sample benchmark against which our predicted price impacts may be compared. The results suggest that the magnitude of the predicted price impact tends to be larger than the actual price impact, especially when price-returns are used to estimate the predicted price impact. The prediction errors are significantly related to the investment style of the institution and the characteristics of the order: more patient trading leads to a more negative price impact forecast error.

V. Summary and Conclusions

We measure equity liquidity using a measure of price impact, the change in a firm's stock price associated with its observed net trading volume over 5-minute and 30-minute time intervals. The sample includes 6513 firms from January 1993 to May 1997, with a typical month having data for 3699. Increasing the magnitude of net turnover in the firm's equity during a 5-minute interval by 0.1% of the shares outstanding produces an average incremental price effect of 2.65% for exchange listed firms and 1.85% for NASDAQ firms.

These averages, however, mask considerable cross-sectional variation. We study the relation between our measure of price impact and a set of predetermined firm characteristics that serve as proxies for the severity of adverse selection, non-information-based costs of making a market, and the extent of shareholder heterogeneity. These results extend the existing literature examining the volume-volatility relation. For example, the finding in Hasbrouck (1991 a, b) that the average proportional price impact of a trade of a given size is negatively related to the size of the market capitalization of the stock is consistent with the findings here when trade size is defined in terms of shares instead of turnover. The inclusion of additional cross-sectional variables, such as the percentage of shares held by institutions and historical trading volume, improves the ability to explain price impact and allows for differentiation even among firms of similar size.

We employ the fitted cross-sectional relation between the price impact coefficient and firm characteristics to generate out-of-sample predicted price impacts for a sample of 56,211 stock trades made by twenty-one institutional investors over twenty-seven months. The trades represent a total of 1.9 billion shares and $62.4 billion in value. The predicted impact overstates the actual impact on average, with the difference being the smallest for trades by index funds. Numerous trade characteristics are significantly related to the signed price impact prediction error in intuitive ways. For example, the predicted price impact overestimates the actual price impact for very large trades, for trades executed in a more patient manner, and for trades where the institution pays higher commissions, suggesting a tradeoff between commissions paid and price impact.

This study can be extended in many directions. One important question in the asset pricing literature is whether the price impact has incremental explanatory power over other measures of liquidity [such as the spread as in Amihud and Mendelson (1986)] for the purposes of explaining asset pricing [e.g., Brennan and Subrahmanyam (1996), Easley, Hvidkjaer, and O'Hara (2000)]. Additionally, the price impact cost of trading might help explain why various asset pricing anomalies are apparently not exploited and might help explain differences in mutual fund performance.

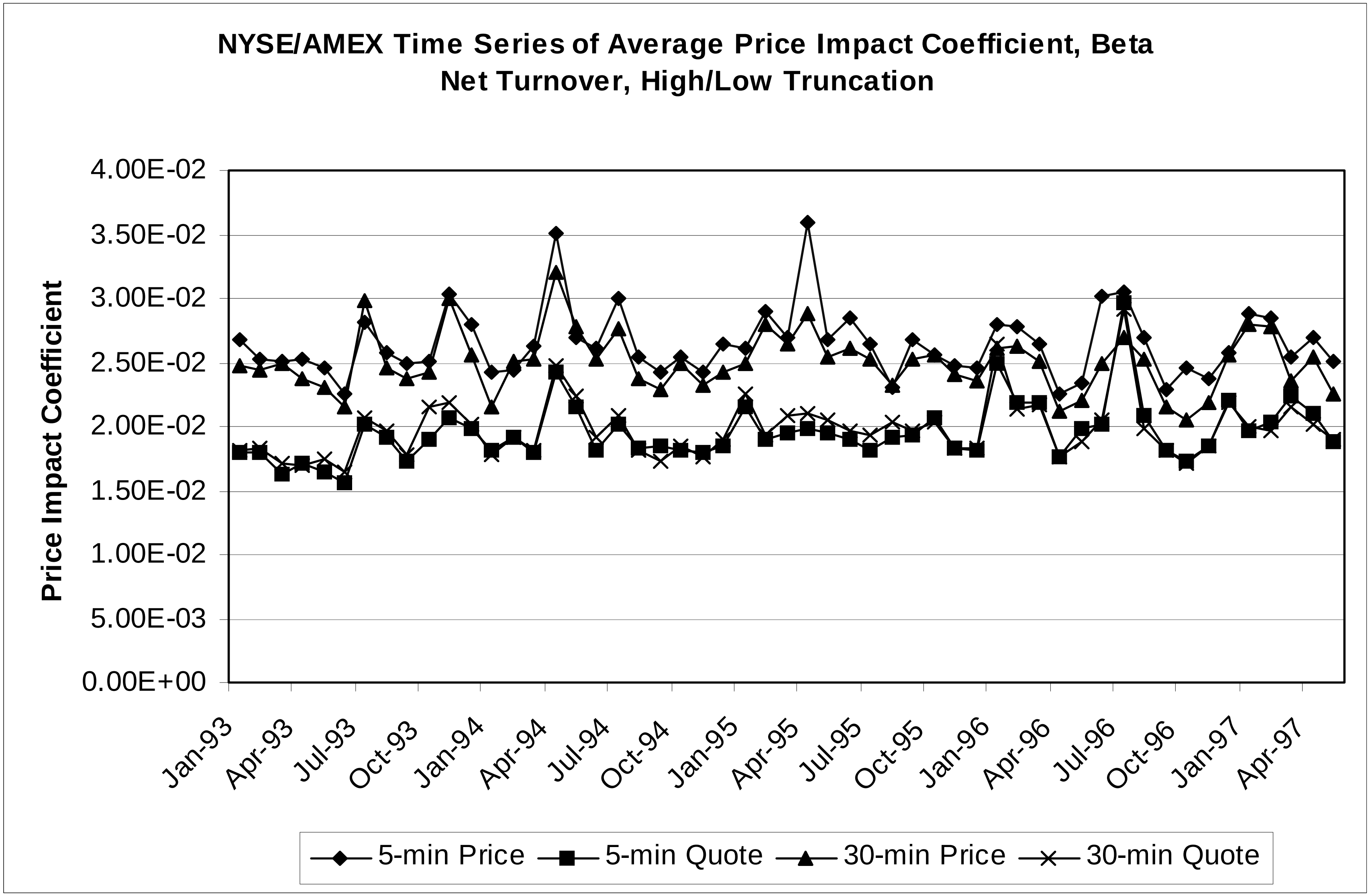
NYSE/AMEX Time Series of Average Price Impact Coefficient, Beta
Net Turnover, High/Low Truncation
Price Impact Coefficient
4.00E-02
3.50E-02
3.00E-02
2.50E-02
2.00E-02
1.50E-02
1.00E-02
5.00E-03
0.00E+00
Jan-93
Apr-93
Jul-93
Oct-93
Jan-94
Apr-94
Jul-94
Oct-94
Jan-95
Apr-95
Jul-95
Oct-95
Jan-96
Apr-96
Jul-96
Oct-96
Jan-97
Apr-97
5-min Price
5-min Quote
30-min Price
30-min Quote

Figure 1

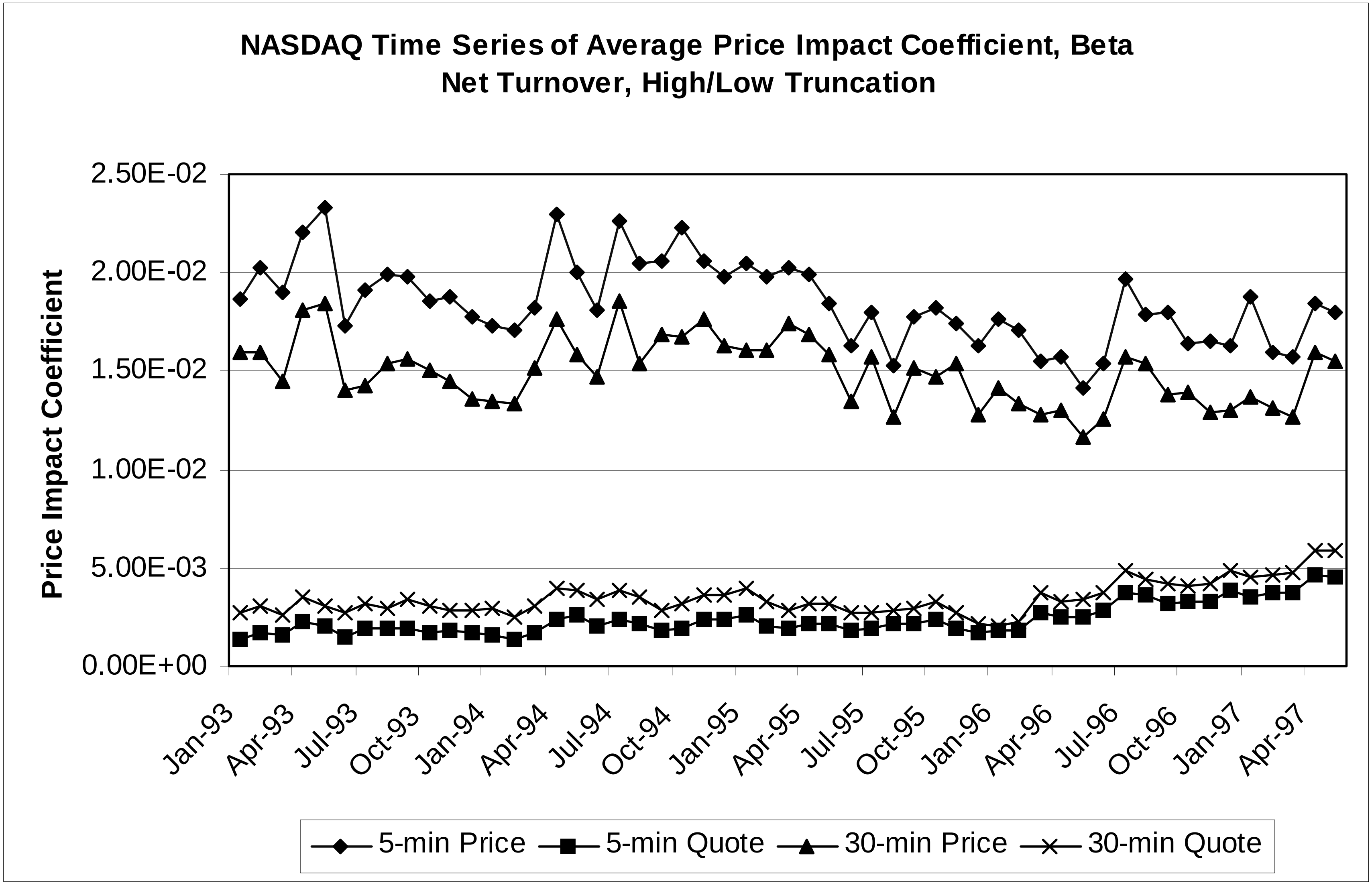
NASDAQ Time Series of Average Price Impact Coefficient, Beta
Net Turnover, High/Low Truncation
Price Impact Coefficient
2.50E-02
2.00E-02
1.50E-02
1.00E-02
5.00E-03
0.00E+00
Jan-93
Apr-93
Jul-93
Oct-93
Jan-94
Apr-94
Jul-94
Oct-94
Jan-95
Apr-95
Jul-95
Oct-95
Jan-96
Apr-96
Jul-96
Oct-96
Jan-97
Apr-97
5-min Price
5-min Quote
30-min Price
30-min Quote

Figure 2

Table 1: Time-series mean of the cross-sectional distributional statistics of the price impact coefficient, 5-minute intervals, January 1993 - May 1997, T = 53.

		NYSE/AMEX		NASDAQ	
		Beta Using Price Returns from (1)	Beta Using Quote Returns from (2)	Beta Using Price Returns from (1)	Beta Using Quote Returns from (2)
Mean, $\bar{\beta} = (\bar{\beta}_1 + \bar{\beta}_2 + ... + \bar{\beta}_T)/T$		3.09	2.15	1.96	0.25
Average Cross-sectional Standard Deviation		26.69	14.08	6.16	0.77
Average percentiles	15%	0.32	0.23	0.20	0.00
	25%	0.48	0.36	0.36	0.01
	75%	2.13	1.61	2.03	0.31
	85%	3.25	2.45	3.14	0.49
Mean, $\bar{\beta}$ with high/low Truncation		2.65	1.95	1.85	0.24
Average Cross-sectional Standard Deviation with high/low Truncation		10.94	8.07	3.67	0.54

Table 2: Predicted effect of firm characteristic on the price impact coefficient, $\beta_{i,t}$

	Predicted Effect		
Explanatory Variable	Adverse Selection	Non-Information Costs of Market Making	Heterogeneity
Intercept			
Relative Market Capitalization			
Relative Trading Volume		Decrease	
Price Appreciation	Increase		Increase
Price Movement	Decrease		
S&P Inclusion Dummy	Decrease	Decrease	
Dividend Yield	Decrease		
R^2 returns vs. NYSE	Decrease	Decrease	
NYSE Inclusion Dummy			
NASDAQ Inclusion Dummy			
Earnings Release Dummy	Increase		
Percentage Institutional			Decrease
Option Traded Dummy	Increase	Decrease	
Inverse Price			

Table 3: Time-series mean of the cross-sectional average and cross-sectional standard deviation of firm characteristics, January 1993 - May 1997.

	NYSE/AMEX		NASDAQ	
Firm Characteristic	Mean	Standard Deviation	Mean	Standard Deviation
Relative Market Capitalization	2.18	9.55	-0.24	3.27
Relative Trading Volume	0.10	2.37	-0.07	3.10
Price Appreciation $(P_{t-1}/P_{t-7}) - 1$	0.09	0.71	0.10	0.33
Price Movement $\|(P_{t-1}/P_{t-7}) - 1\|$	0.22	0.66	0.23	0.26
S&P Inclusion Dummy	0.19	0.39	0.03	0.17
Dividend Yield (decimal percent)	0.02	0.04	0.01	0.02
R^2 returns vs. NYSE (decimal percent)	0.15	0.13	0.13	0.10
NYSE Inclusion Dummy	0.56	0.49		
Late Earnings Release Dummy	0.42	0.46	0.40	0.45
Percentage Institutional (percent)	33.30	24.73	29.70	21.70
Option Traded Dummy	0.30	0.46	0.14	0.34
Inverse of Price	0.14	0.40	0.11	0.19

Table 4: Autocorrelation of monthly estimates of the cross-sectional relation between the price impact coefficient (measured by equation (1) over 5-minute intervals) and the predetermined firm characteristics, January 1993 - May 1997.

Panel A: NYSE/AMEX

Explanatory Variable	Lag 1	Lag 2	Lag 3	Lag 4	Lag 5	Lag 6
Intercept	0.36	0.07	0.22	0.14	0.14	-0.05
Relative Market Capitalization	-0.13	-0.09	-0.20	0.07	-0.01	-0.04
Relative Trading Volume	-0.16	-0.13	-0.22	0.10	0.00	-0.04
Price Appreciation	0.27	0.30	0.14	0.03	-0.21	-0.06
Price Movement	0.34	0.26	0.16	0.03	-0.24	0.01
S&P Inclusion Dummy	0.04	0.03	-0.01	0.11	0.14	-0.02
Dividend Yield	0.40	0.50	0.22	0.35	0.22	0.24
R^2 returns vs. NYSE	0.03	-0.06	0.01	0.09	-0.03	-0.13
NYSE Inclusion Dummy	0.08	0.11	-0.05	0.16	0.02	-0.10
Earnings Release Dummy	0.03	-0.10	0.33	-0.08	0.02	0.39
Percentage Institutional	0.69	0.56	0.55	0.57	0.56	0.45
Option Traded Dummy	0.35	0.24	0.13	-0.09	-0.12	-0.12
Inverse Price	0.05	0.00	-0.03	-0.08	-0.02	0.08

The standard error of the correlation coefficients is approximately $T^{-1/2} = 0.14$ for our sample size of T=53.

Panel B: NASDAQ						
Explanatory Variable	Lag 1	Lag 2	Lag 3	Lag 4	Lag 5	Lag 6
Intercept	0.28	0.26	0.09	0.11	0.09	0.00
Relative Market Capitalization	0.27	0.26	0.03	0.01	-0.07	-0.09
Relative Trading Volume	0.22	0.19	0.05	0.00	-0.04	0.03
Price Appreciation	0.17	0.29	0.16	-0.13	-0.17	-0.25
Price Movement	-0.22	0.15	0.21	-0.02	-0.04	-0.17
S&P Inclusion Dummy	0.34	0.23	0.16	0.10	0.17	0.18
Dividend Yield	0.32	0.43	0.25	0.21	0.31	0.20
R^2 returns vs. NYSE	0.46	0.16	0.13	0.25	0.22	0.06
Earnings Release Dummy	-0.35	-0.10	0.02	0.02	-0.20	0.41
Percentage Institutional	0.37	-0.01	-0.28	-0.07	0.21	0.25
Option Traded Dummy	0.34	0.34	0.13	0.14	0.33	0.14
Inverse Price	0.41	0.47	0.14	0.08	-0.06	-0.16

Table 5: Estimate of the average cross-sectional relation between $\hat{\beta}_i$, estimated using net turnover [equations (1) and (2)], and the firm-specific predetermined variables, using 5-minute and 30-minute intervals, with returns defined using transactions prices and quote midpoints: January 1993 - May 1997.

Panel A: NYSE/AMEX

	Prices: 5-Minute		Quotes: 5-Minute		Prices: 30-Minute		Quotes: 30-Minute	
Explanatory Variable	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.
Intercept	3.90	31.00	3.14	34.20	3.79	31.80	3.17	33.00
Relative Market Capitalization	0.24	8.69	0.20	7.18	0.22	12.20	0.22	6.93
Relative Trading Volume	-0.58	-9.91	-0.47	-8.97	-0.55	-13.90	-0.50	-8.64
Price Appreciation	-0.65	-3.91	-0.84	-5.32	-0.49	-3.06	-0.61	-5.12
Price Movement	0.16	0.95	0.49	2.80	0.05	0.29	0.34	2.45
S&P Inclusion Dummy	-0.70	-7.70	-0.57	-6.97	-0.58	-7.67	-0.60	-5.11
Dividend Yield	1.27	1.35	-3.26	-4.24	-0.69	-0.79	-3.87	-5.68
R^2 returns vs. NYSE	-2.34	-4.91	-2.17	-4.79	-2.07	-5.74	-2.32	-4.57
NYSE Inclusion Dummy	0.28	3.83	0.02	0.39	0.26	4.50	0.00	0.10
Earnings Release Dummy	0.72	4.52	0.55	4.22	0.67	4.10	0.48	4.25
Percentage Institutional	-0.06	-19.90	-0.04	-17.40	-0.05	-20.20	-0.04	-17.70
Option Traded Dummy	-0.03	-0.46	-0.03	-0.47	-0.15	-1.95	-0.05	-0.79
Inverse Price	2.68	13.60	0.86	9.26	2.50	13.80	0.95	8.87

Table 5: (continued)

Panel B: NASDAQ

	Prices: 5-Minute		Quotes: 5-Minute		Prices: 30-Minute		Quotes: 30-Minute	
Explanatory Variable	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.	$\bar{\gamma}_i \times 100$	t-Stat.
Intercept	2.36	31.10	0.26	18.50	2.05	33.70	0.37	23.50
Relative Market Capitalization	0.18	9.34	0.03	12.60	0.16	9.63	0.03	12.00
Relative Trading Volume	-0.14	-11.40	-0.02	-8.75	-0.11	-9.72	-0.01	-6.20
Price Appreciation	-0.63	-7.96	-0.04	-2.51	-0.27	-3.95	-0.10	-5.27
Price Movement	0.14	1.79	0.06	3.47	-0.15	-1.92	0.13	6.19
S&P Inclusion Dummy	-0.34	-7.48	-0.01	-1.50	-0.26	-5.79	0.02	1.22
Dividend Yield	14.60	9.15	0.29	1.46	13.20	9.01	-0.49	-2.32
R^2 returns vs. NYSE	-1.92	-11.30	-0.03	-1.18	-1.96	-13.30	-0.04	-1.67
Earnings Release Dummy	0.05	1.32	-0.01	-1.12	0.03	0.97	-0.01	-1.08
Percentage Institutional	-0.02	-27.50	-0.00	-8.22	-0.02	-27.20	-0.00	-11.00
Option Traded Dummy	0.09	4.05	0.14	11.30	-0.07	-2.46	0.20	13.80
Inverse Price	3.15	9.84	0.06	1.62	2.47	12.70	0.18	4.11

Table 6: Intra-day variation in price impact: mid-day versus first and last half hour of trading day. Time-series mean of the cross-sectional distributional statistics of price impact coefficient, 5-minute intervals, January 1993 - May 1997.

	NYSE/AMEX		NASDAQ	
	Beta Using Price Returns from (4)	Beta Using Quote Returns from (5)	Beta Using Price Returns from (4)	Beta Using Quote Returns from (5)
Mean, mid-day $\bar{\beta}_{Mid}$.	2.26 (77.8)	1.86 (67.2)	1.71 (81.9)	0.25 (22.0)
Mean Difference between first half hour and Mid-Day $\bar{\beta}_{Mid}$, $\Delta^{P}_{i\,t}$ and $\Delta^{Q}_{i\,t}$.	4.56 (13.7)	3.40 (32.1)	5.68 (20.3)	0.01 (2.7)
Mean Difference between last half hour and Mid-Day $\bar{\beta}_{Mid}$, $\theta^{P}_{i\,t}$ and $\theta^{Q}_{i\,t}$.	2.12 (24.1)	0.94 (8.3)	1.59 (25.8)	0.00 (1.5)

Table 7: Summary statistics for number of calendar days between decision to trade, order sent to trading desk, beginning of trading, and end of trading.

	Decision to Trade -- Order at Trading Desk		Order at Trading Desk – Begin Trading		Begin Trading – End Trading	
A: Mean Elapsed Time (Days)						
	1.90		1.21		0.37	
B: Frequency Distribution						
Days	Percent	Cumulative Percent	Percent	Cumulative Percent	Percent	Cumulative Percent
0	5.4	5.4	76.2	76.2	92.5	92.5
1	65.8	71.2	6.3	82.5	2.1	94.6
2	1.9	73.1	3.2	85.7	0.9	95.4
3	16.4	89.5	2.2	87.9	0.8	96.2
4	4.4	93.9	1.6	89.5	0.8	97.0
5	0.8	94.6	1.4	90.9	0.5	97.5
6	1.0	95.6	1.8	92.7	0.5	98.0
7	1.2	96.8	2.0	94.7	0.7	98.7
8	0.8	97.6	1.4	96.0	0.2	98.9
9	0.6	98.2	0.6	96.6	0.1	99.0
10	0.3	98.5	0.5	97.1	0.1	99.1
11	0.3	98.8	0.4	97.5	0.1	99.2
12	0.2	99.0	0.5	98.0	0.1	99.3
13	0.2	99.2	0.4	98.4	0.2	99.4
14	0.2	99.4	0.4	98.8	0.2	99.7
15	0.2	99.6	0.3	99.0	0.1	99.8
16	0.1	99.7	0.2	99.2	0.0	99.8
17	0.1	99.7	0.2	99.4	0.0	99.8
18	0.1	99.8	0.1	99.5	0.0	99.8
19	0.1	99.9	0.1	99.6	0.0	99.9
20	0.0	99.9	0.2	99.8	0.0	99.9
21	0.1	100.0	0.2	100.0	0.1	100.0

Table 8: Actual versus predicted price impacts by market, investment style, and order type. I_{Desk} is the actual impact measured relative to the price prior to sending the order to the trading desk and $I_{Decision}$ is the actual impact measured relative to the price prior deciding to trade.

				Actual Impact		Predicted Impact - Price Returns Regressions		Predicted - Quote Returns Regressions	
Order Type	Market	Style	Sample Size	I_{Desk}	$I_{Decision}$	Unadjusted [eqn. (14)]	Adjusted [eqn. (15)]	Unadjusted [eqn. (14)]	Adjusted [eqn. (15)]
Sell	NYSE/AMEX	Value	7066	-0.047	0.100	-1.046	-0.909	-0.864	-0.752
Sell	NYSE/AMEX	Technical	11779	-0.391	-0.594	-1.209	-1.193	-1.054	-1.043
Sell	NYSE/AMEX	Index	2198	-0.155	-0.239	-0.474	-0.446	-0.399	-0.376
Sell	NASDAQ	Value	512	-0.302	-0.081	-1.719	-1.540	-0.387	-0.341
Sell	NASDAQ	Technical	2150	-0.892	-0.915	-3.025	-3.023	-0.767	-0.766
Sell	NASDAQ	Index	203	-0.634	-0.730	-1.032	-0.993	-0.228	-0.217
Buy	NYSE/AMEX	Value	6230	0.179	0.139	1.241	1.056	1.021	0.870
Buy	NYSE/AMEX	Technical	11268	0.254	0.443	1.372	1.361	1.174	1.166
Buy	NYSE/AMEX	Index	10457	0.106	0.201	0.179	0.165	0.144	0.133
Buy	NASDAQ	Value	605	0.256	0.103	1.991	1.737	0.435	0.364
Buy	NASDAQ	Technical	2331	0.533	0.847	2.554	2.549	0.657	0.656
Buy	NASDAQ	Index	1412	0.524	0.676	0.198	0.178	0.042	0.039

Table 9: Regression of signed price impact forecast errors on trade and institutional characteristics.

	$\delta_{i,t}\times(I_{Desk: i,t} - \hat{I}^{u}_{i,t})$		$\delta_{i,t}\times(I_{Decision: i,t} - \hat{I}^{u}_{i,t})$		$\delta_{i,t}\times(I_{Desk: i,t} - \hat{I}^{a}_{i,t})$		$\delta_{i,t}\times(I_{Decision: i,t} - \hat{I}^{a}_{i,t})$	
	Coefficient	t-Stat	Coefficient	t-Stat	Coefficient	t-Stat	Coefficient	t-Stat
Intercept	1.89	2.21	3.73	4.02	1.63	1.96	3.47	3.82
NTO (0.1%)	0.00	0.56	0.00	0.57	0.02	2.40	0.02	2.25
NTO^2 ($0.1\%^2$)	-0.04	-132.19	-0.04	-119.35	-0.03	-130.43	-0.03	-117.27
Value dummy	-0.71	-11.86	-1.09	-16.81	-0.54	-9.35	-0.93	-14.60
Technical dummy	-0.55	-11.17	-0.64	-11.85	-0.51	-10.49	-0.59	-11.23
NASDAQ dummy	-0.49	-8.50	-0.44	-7.09	-0.47	-8.31	-0.42	-6.88
Decision to desk (days)	-0.11	-13.20	-0.10	-11.15	-0.12	-13.99	-0.11	-11.81
Desk to begin (days)	-0.09	-13.76	-0.05	-7.13	-0.09	-14.60	-0.05	-7.75
Begin to end (units = days)	-0.09	-8.55	-0.10	-9.02	0.00	0.08	-0.01	-1.10
Urgency (1=urgent to 5= not urgent)	-0.45	-1.60	-1.07	-3.47	-0.39	-1.41	-1.01	-3.33
Work order dummy	-0.10	-1.33	-0.04	-0.53	-0.07	-0.99	-0.02	-0.20
Limit order dummy	-0.42	-3.19	-0.48	-3.31	-0.41	-3.19	-0.47	-3.31
Not held order dummy	0.09	1.77	0.52	9.12	0.06	1.18	0.48	8.72
Cross dummy	0.14	1.15	0.07	0.53	0.09	0.78	0.02	0.18
Principal trade dummy	-0.50	-2.88	-0.82	-4.33	-0.41	-2.42	-0.73	-3.95
Commissions (%)	-1.71	-17.08	-1.21	-11.08	-1.54	-15.73	-1.03	-9.69
Shortfall (100,000 shares)	-0.10	-2.68	0.01	0.21	-0.09	-2.34	0.02	0.60
R^2	0.27		0.23		0.26		0.22	

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