TWO ESSAYS IN EMPIRICAL FINANCIAL ECONOMICS: THE STOCK PRICE EFFECTS OF INSIDER TRADING AND A COMPARISON OF FUTURES AND FORWARD PRICES

by

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ABSTRACT

The first essay uses previously unexplored data on illegal insider trading from the Securities and Exchange Commission to investigate the stock price impact of insider trading. The findings indicate that insider trading leads to large immediate price movements: the cumulative abnormal return on insider trading days is 50% of the abnormal return on the day the inside information becomes public. In addition, an examination of the stock price run-up before takeovers reveals that almost half of the run-up is attributable to insider trading.

The second essay investigates the effect of marking-to-market on the observed differences between futures and forward prices using the pricing model described in Cox, Ingersoll and Ross (CIR) (1981). Prior research supports the weak CIR implication predicting the sign of the average price difference, but fails to support the stronger CIR prediction that specific covariances are important explanatory variables for this price difference. To increase the power of tests of the weak and strong implications of the CIR model, this essay uses previously unavailable data from an interest rate sensitive financial asset. Unlike prior empirical studies, test results support both the weak and strong model predictions, successfully explaining intra-sample price difference variations.

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CHAPTER 1

AN EMPIRICAL ANALYSIS OF INSIDER TRADING AND THE STOCK MARKET

1. Introduction

Insider trading, the illegal trading in securities by individuals or firms possessing important non-public information, has been the subject of much news media and congressional attention. Alarmed by charges that insider trading impairs the integrity of the market, Congress substantially increased insider trading penalties in 1984, and again in 1988. The catalyst for the recent legislation and for the continuing debate on how best to regulate insider trading is the view that insider trading is both harmful and pervasive.

Proponents of insider trading regulation argue that insider trading decreases stock market liquidity, produces abusive managerial practices, and is unfair to uninformed investors.[1] Specifically, insider trading increases the cost of trading as the market maker, aware that he may be trading against informed investors, boosts the bid-ask spread. Insider trading by managers also leads to divergent managerial and shareholder interests: managers maximize stock price volatility rather than shareholder wealth. This divergence induces managers to delay corporate disclosures until they trade on the information, and to make ambiguous corporate disclosures or adopt excessively risky projects to increase stock price volatility.

Others, however, continue to question the harm induced by insider

[1] See Schotland (1967) and Seligman (1985).

trading, and some even champion its benefits. Manne (1966) and Carlton and Fischel (1983) argue that insider trading by corporate insiders reduces traditional agency problems associated with managerial compensation by allowing managers to capture some of the benefits of increased entrepreneurial effort. Carlton and Fischel also suggest that insider trading offers the firm a valuable alternative to directly disclosing information to the market. Finally, Manne and Carlton and Fischel assert that insider trading promotes efficient capital markets by improving the accuracy of stock prices.[2]

Allowing insider trading also reduces costly information production. Information production is often socially wasteful because the temptation to speculate induces many agents to produce the same information. Quick price discovery mitigates this incentive and therefore reduces socially redundant information production. In the context of takeovers, risk arbitragers consume considerable resources collecting and analyzing information concerning the terms and success of takeovers. While the arbitragers provide a valuable risk sharing service, the resources they consume are also socially wasteful because many arbitragers collect the

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[2] Security prices which reflect all relevant information enhance the allocative efficiency of capital markets. A firm's stock price determines its cost of capital; accurate stock prices improve the firm's investment decisions. A firm's stock price also guides decisions made by other firms. For instance, a potential entrant may judge the incumbent firm's profitability by its stock price and base its entry decision on this estimation.

same information. If insider trading effectively increases the accuracy of securities prices, permitting insider trading would reduce this redundant information collection.

This efficiency enhancing view of insider trading is not restricted to academia. For example, the *Wall Street Journal* points to the dangers of strict insider trading regulation which

> ...attack[s]...anyone who acquires information that may give him a market advantage. In stock trading, as in so many other areas of American public life, misplaced egalitarianism has to be stopped before it starts impeding the spread of market information instead of removing the obstacles to its free flow.[3]

The belief that insider trading is pervasive also fuels the demand for legislation. Alleging widespread insider trading, Keown and Pinkerton (1981) note that, on average, 40-50% of the price gain experienced by a target firm's stock occurs *before* the actual takeover announcement.[4] Jarrell and Poulsen (1989), however, caution against concluding that insider trading causes the target firm's stock price run-up. Instead, Jarrell and Poulsen find that legitimate sources, such as media speculation concerning the upcoming takeover and the bidder's purchase of shares in the target

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[3] *Wall Street Journal* editorial, July 7, 1983, p. 22.

[4] Keown and Pinkerton (1981) document the pre-announcement price run-up and conclude that "...impending merger announcements are poorly held secrets, and trading on this non-public information abounds." (p. 866)

firm, help drive up the target's stock price.[5]

Underlying the belief that insider trading is harmful and widespread is the assumption that insider trading affects stock prices. The price discovery and social efficiency arguments of the opponents of insider trading regulation rest on this assumption. The conclusion that price run-ups prior to takeover announcements reflect insider trading is also contingent on the assumption that insider trading creates significant stock price movements. Empirically, however, this assumption remains untested.

This paper investigates the stock price effects of insider trading. To increase the accuracy of stock prices, insider trading must move prices towards values which incorporate the inside information. Hence, a determination that insider trading does not affect stock prices would bolster the arguments of opponents of insider trading.[6] [7] At the same time, a

[5] Jarrell and Poulsen also find that stock price run-up is not significantly higher for the stock of target firms that are later identified in government insider trading allegations than for firms not so identified.

[6] Even if insider trading does not affect stock prices, it still might reduce agency problems associated with managerial compensation. An investigation of how insider trading affects managerial effort is beyond the scope of this paper.

[7] Insider-induced price movements could lead to detection of the insider trading by the SEC. If so, a finding of no insider trading effect in an already regulated environment does not imply that insider trading will fail to move prices in a deregulated environment. An investigation of sample selection bias in Section 4(B), however, suggests that the size of the insider trading induced price movement does not lead to detection. Therefore, data from the currently regulated environment should not underestimate the price effects of insider trading in a deregulated environment.

finding that insider trading does not move stock prices also should lead one to challenge the contention that stock price run-ups before takeover announcements reflect widespread insider trading.

Conversely, a finding that insider trading does increase security price accuracy has more ambiguous social welfare implications. More accurate securities prices may come at the expense of a less liquid securities market if the bid-ask spread increases or if uninformed traders refrain from trading. Insider trading also creates wealth transfers from the uninformed to the informed that could be socially undesirable.

Whether insider trading affects stock prices assumes particular importance in the context of takeovers. Because of their large effect on stock prices, takeovers offer inside traders an attractive profit opportunity. At the same time, the potential welfare costs of insider trading in takeover stocks are large. Insider trading will decrease the probability of a successful takeover if it increases the bidder's cost. Insider trading could increase the acquisition cost by increasing the bidder's cost of its foothold stake, or by raising the premium the bidder must pay for the target by running up the stock price. In either case, insider trading increases the bidder's cost by increasing the target's stock price. This paper's investigation of the stock price effects of insider trading is a necessary first step for a future evaluation of the effect of insider trading on the premiums and outcomes of takeovers.

Past empirical research, however, reveals little about whether insider trading produces stock price movements. Studies of insider trading typically use legal transactions by corporate insiders (officers, directors, and owners of at least 10% of any equity class) to estimate returns accruing from insider trading.[8] These studies suffer from two problems. First, they investigate the stock returns earned by corporate insiders over an extended period following the transaction date. The abnormal return on the insider transaction date more accurately measures the stock price effect of the insider transaction by separating the effect of the trading from the effect of the eventual public dissemination of the inside information. Second, and more important, these studies only examine routine transactions that corporate insiders must report to the Securities and Exchange Commission (SEC).[9] Corporate insiders cannot legally trade based upon material, non-public information. If corporate insiders choose to violate the law, one suspects that they would refrain from reporting their violative transactions to the SEC.

Seyhun (1986), investigating transactions reported to the SEC, finds

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[8] See Lorie and Niederhoffer (1968), Pratt and DeVere (1970), Jaffee (1974), Finnerty (1976), Elliot, Morse, and Richardson (1984), Givoly and Palmon (1985), Seyhun (1986), and Rozeff and Zaman (1988).

[9] Corporate insiders must report their trades within 10 days following the last day of the month in which the trading occurs. The SEC publishes these transactions in its monthly *Official Summary of Insider Transactions*.

that corporate insiders earn excess returns that are on average small, reinforcing the conjecture that these corporate insiders refrain from trading on material non-public information. More precisely, Seyhun estimates that after accounting for overall market movements, corporate insiders earn excess returns (calculated *before* transactions costs) of 3% following a corporate insider purchase, and -1.7% following a corporate insider sale during the 100 trading days following the month of a purchase or sale. Givoly and Palmon (1985) examine whether the abnormal returns earned by corporate insiders result from trades based on inside information. They find that insider purchases do not precede good news and insider sales do not precede bad news, and conclude that corporate insiders do not trade on inside information.[10] [11]

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[10] To test their hypothesis, Givoly and Palmon collect news announcements for a sample of firms from the *Wall Street Journal Index*. News includes earnings and dividends announcements, management forecasts of earnings or sales, expansions and acquisitions, new products, discoveries, and patents, litigation, etc. They classify news as "good" if it produces a positive abnormal price reaction upon announcement; "bad" news produces a negative reaction.

[11] Elliot, Morse, and Richardson (1984) also investigate the association between corporate insider transactions and information announcements; they examine whether insiders buy relatively more before good news and sell relatively less before bad news. They find that insider transactions surrounding specific public information events are often insignificantly different from insider transactions at other times, and conclude that the use of private information explains only a small proportion of corporate insider transactions.

Because this evidence indicates that corporate insiders do not base their transactions (at least the ones they report to the SEC) on inside information, previous research has not addressed how insider trading affects stock prices. This paper uses a previously unexplored data source, illegal insider trading detected by the SEC and subsequently cited in a civil case, to examine excess returns on the days of illegal insider trading. Unlike the corporate transactions used in prior work, the SEC has alleged that the trades in this data source are based upon material, inside information. In fact, few of the defendants in insider trading cases are corporate insiders required to report their transactions to the SEC. Only 24% of the defendants are employees of the firm whose stock they trade.

The results reveal that insider trading, on average, moves stock prices on the day of the inside trade in the same direction as the subsequent public announcement of the inside information. This finding suggests that insider trading does, in fact, lead to immediate price movements and quick price discovery. Moreover, on average the insider induced stock price movement is about 50% of the stock price reaction from public revelation of the inside information. This relatively large ratio implies that insider trading impounds a large proportion of the insider information into the stock price before the inside information becomes public. Finally, an examination of insider trading preceding takeovers shows that 44% of the price run-up before the takeover announcement occurs on insider trading days.

This paper consists of five sections. The next section reviews the laws regulating insider trading and their enforcement by the SEC. Section 3 contains the sample construction and description. Section 4 consists of the empirical analysis. It investigates potential sample selection bias, the source of the price run-up that occurs prior to takeovers, and the relation of trading volume to price movements observed on insider trading days. Section 5 concludes and outlines next steps in the analysis.

2. The Laws Regulating Insider Trading

The Securities Exchange Act of 1934 and the 1968 Williams Act Amendments regulate insider trading.[12] [13] SEC rule 10b-5, implementing section 10(b) of the 1934 Act, states, in part, that an insider must disclose material inside information or refrain from trading.[14] This rule applies

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[12] Also see Netter, Poulsen, and Hersch (1988) for a description of the laws regulating insider trading.

[13] Section 16 of the 1934 Act requires certain corporate insiders, in particular officers, directors, and 10 percent owners of any class of equity securities, to report their registered equity holdings in the company's stocks to the SEC. Section 16 also requires corporate insiders to return to the issuer any profit earned on holding periods of less than six months, and to refrain from short sales.

[14] Unlike section 16, section 10(b) of the 1934 Act applies to more than registered corporate insiders. Under section 10(b), an insider is anyone who obtains material, non-public information from a corporate insider, or from the issuer, or steals such information from another source.

not only to insiders who trade, but also to insiders who tip others who in turn trade, as well as to the individuals that the insider tips. Information is material if a substantial likelihood exists that a reasonable investor would consider it important in making his/her investment decisions. An intent to deceive, manipulate, or defraud is generally required for violation.

SEC rule 14e-3, adopted under the Williams Act, declares that if substantial steps have been taken to commence a tender offer, or if a tender offer has commenced, trading while in possession of material non-public information acquired directly or indirectly from an insider is fraud, regardless of how or for what reason a person received it. Rule 14e-3 is stricter than rule 10b-5 in the sense that a 14e-3 violation does not require scienter, that is, an attempt to deceive, manipulate, or defraud, nor does it require that the insider breach a fiduciary or other duty in disclosing the tender offer information. In practice, however, the SEC has not brought charges of 14e-3 violation without accompanying 10b-5 charges.

The SEC has primary responsibility for enforcing insider trading regulations. Although the SEC typically brings civil charges against a defendant, it may also refer cases to the Justice Department for criminal prosecution, or, if the defendant is a regulated market professional, the SEC may suspend or revoke his/her license.[15] The majority of insider

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[15] Regulated market professionals include brokers, dealers, and investment advisors.

trading cases are civil cases in which the SEC seeks the return of the insider's profit gained or loss avoided, and asks the court to issue an injunction prohibiting further insider trading violations.[16] Approximately 70% of defendants charged in a civil insider trading case settle with the SEC rather than litigate.

During the 1980's, Congress increased insider trading penalties substantially through the Insider Trading Sanctions Act of 1984 (ITSA) and the Insider Trading and Securities Fraud Enforcement Act of 1988 (1988 Act). ITSA boosted both civil and criminal penalties, and extended these penalties to trading in derivative instruments.[17] Specifically, ITSA allowed, but did not require, the SEC to seek a civil penalty of up to three times the insider's profit gained or loss avoided. ITSA also raised maximum criminal fines from $10,000 to $100,000.

Interest in insider trading regulation intensified following the Dennis Levine and Ivan Boesky cases, and in 1988 Congress again stiffened insider trading sanctions. The 1988 Act increased maximum criminal fines to $1,000,000 for individuals and required that securities firms actively set up procedures to prevent insider trading by the firm or its employees.[18]

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[16] If the defendant receives an injunction and continues to violate insider trading regulations, the court may cite the violator with contempt, a criminal violation.

[17] These instruments include puts, calls, straddles, options, and privileges.

[18] The maximum jail term also increased from 5 to 10 years.

Whereas ITSA protected a firm from treble damage liability if an employee accused of violating insider trading law did not act on behalf of the firm, the 1988 Act held a firm liable if its employees engaged in insider trading while the firm knowingly or recklessly disregarded this fact.[19]

3. Sample Construction and Description

The sample consists of individuals charged with insider trading by the SEC in civil or administrative cases during 1980-1989.[20] The starting point for sample construction is a list, prepared by the SEC's Enforcement Division, of all defendants formally charged with insider trading during 1980-1989. Public and non-public SEC documents provide trading information for the defendants on this list.

The information available in public sources is typically quite limited. Litigation releases (SEC news releases describing the charges and outcome in a case) usually reveal the type of inside information, the name of the security traded, and sometimes the profit gained or the loss avoided from

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[19] In addition, the 1988 Act authorized private citizens who traded at the same time as the insider to seek damages in federal court limited to the profit gained or the loss avoided by the insider trade. It also allowed the SEC to pay bounties (up to 10% of the ITSA penalty) to informers, and permitted the SEC to cooperate with foreign governments. See Pitt and Groskaufmanis (1989) for further information.

[20] In an administrative case, the SEC disciplines brokers, dealers, and investment advisors. The SEC may suspend or revoke the license of these regulated market professionals in an internal hearing.

insider trading. Court complaints, documents the SEC files with the Courts to bring charges against a defendant, also lack detailed information, often contributing little more than the litigation releases. The scarcity of publicly available information about insider trading cases is largely attributable to the high proportion of defendants that settle before trial. Since 70% of complaints are filed with an accompanying settlement document, these complaints need not be as detailed as complaints filed for litigated cases. In fact, by limiting the information in a complaint, the SEC controls information leakage concerning its investigative techniques.

SEC non-public case files supplement the information available from public sources.[21] The case files contain more detailed trading and descriptive information than either the litigation releases or the court complaints. Missing and incomplete case files prevented the inclusion of every defendant charged with insider trading. As Table 1 reflects, the sample includes roughly 70% (320 of 464) of all defendants during the 1980-1989 period.[22] The number of 1989 defendants appears low relative

[21] Data made available by the Office of Commissioner Joseph Grundfest.

[22] This figure slightly understates the proportion of defendants in the sample, since the number listed as the number of defendants charged with insider trading (464) also includes some defendants charged in the same civil suit as an inside trader, but who are not themselves charged with insider trading. For example, a recent suit charged Drexel Burnham Lambert with numerous securities violations, including insider trading. The same complaint also charged Victor Posner with failure to disclose beneficial ownership. Both Drexel and Posner are included in the count of 464 total defendants, but only

to other years since the sample includes only completed, not pending, 1989 cases. Table 1 also presents the distribution of insider trading episodes by year. Although the sample of inside traders begins with defendants charged in 1980, the events on which the defendants trade begin as early as 1974. The insider trading episodes occur from 1974 to 1988, with 66% of the sample clustered between 1981 and 1985.[23] The information collected for each defendant includes the charges brought, penalties incurred, profits earned, number of securities traded in, type and source of the inside information, and trade specific information about transaction date, size, and price paid. Appendix A details the type of data collected for each defendant.

Table 2 presents summary statistics describing the defendants' trading, profits, and assessed penalties. The *profit gained* reflects insider trading profits when the defendant buys based upon positive inside information.[24] Conversely, the *loss avoided* reveals the loss an insider avoids by selling prior to negative news. The *profit per security* sums

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Drexel is in the insider trading sample.

[23] In 1981, John Shad became SEC Chairman and vowed to "come down on insider trading with hobnail boots."

[24] The profits in Table 2 are the SEC's profit calculations. In most cases, the profits are the defendant's holding period profits rather than the profits as measured by abnormal returns. Later analysis does not rely on the SEC provided profit measure, but instead calculates the abnormal returns.

these two profit measures and divides by the number of securities in which the defendant trades.[25]

Table 2 shows that among the defendants who traded, the average defendant (as measured by the median) transacted in one security and reaped a little over $17,628 in profit per security. The fact that the minimum figures for each profit measurement are negative may seem puzzling at first, but the defendant sometimes maintains his position after the inside information becomes public. Without his informational advantage, the defendant faces the same risks as an ordinary investor.

Notice that each category in table 2 includes only the defendants appropriate to that category, causing the number of defendants per category to differ. More specifically, 255 defendants contribute to the calculations of *number of securities traded*. This category does not include the 14% of defendants who do not trade at all, and are charged only with tipping. The count also excludes several defendants for whom information concerning the number of securities traded is unavailable. Similarly, the calculations for the *number of immediate tippees* category includes only defendants who tip and for whom information on the number of tippees exists.[26] The

[25] The insider may transact in more than one security, earning profits in some securities and avoiding losses in others.

[26] Most inside trading defendants (65%) do not disclose the inside information to others.

penalties assessed the typical inside trader are low relative to the amount gained by trading. The median ratio of penalty to profit is 1.00 (the *penalty to profit ratio* in table 2), indicating that often the defendants have to repay only the profit they obtain.

Despite extensive media publicity concerning large insider trading profits and long prison terms, table 2 shows that most cases involve small amounts and minor penalties. The following examples illustrate typical defendants that comprise the sample. In 1986, the SEC brought civil charges against Anthony A. DePalma, an officer of Diasonics, Inc.. DePalma was primarily responsible for a division that accounted for 75% of Diasonics' revenue. Before a 1983 third quarter earnings announcement that revealed anticipated losses, DePalma sold Diasonics stock, thereby avoiding losses of approximately $71,125. DePalma settled prior to trial, consenting to a permanent injunction and agreeing to repay $71,125.[27] (See Litigation Release No. 11333, Dec. 30, 1986).

The second example concerns a Prudential-Bache broker, Robert D'Elia, who arranged to buy information from an employee of a financial printer. From July 1980-October 1981, D'Elia and his father, Albert D'Elia, obtained material, non-public information about forthcoming

[27] An injunction prohibits further inside trading violations. The courts may cite a defendant that receives an injunction and continues to violate insider trading regulations with contempt, a criminal charge.

merger and acquisition bids, including Humana's bid for Brookwood Health Services, BTR's bid for Posi-Seal International, and National Medical Enterprises' bid for Cenco, Inc.. The D'Elias' traded in the takeover targets and tipped others who traded. Robert D'Elia repaid $22,257 in illegal profits, and each defendant consented to a permanent injunction barring him from further violations of Rule 10b-5 and 14e-3.

In a criminal case arising out of the insider trading scheme, the D'Elias were convicted of criminal conspiracy and Robert D'Elia was also convicted of fraud in the purchase of securities and fraud in connection with tender offers. Robert D'Elia was sentenced to 3 years of probation, 1000 hours of community service, and fined $30,000. Albert D'Elia received 3 years of probation and a $10,000 fine. (See Litigation Release No. 11499, July 28, 1987).

From the sample of defendants, I construct a sample of stocks in which insider trading occurred. Often more than one defendant trades in a given stock. If a defendant discloses the inside information to a friend who trades on the information, for example, they will both trade in the same stock. I consolidate the defendants' trading activity by stock. The next section describes the sample of stocks and proceeds with a preliminary empirical analysis of the data.

4. Empirical Analysis

The 320 defendants engaged in 229 different episodes of insider trading, representing trading in the securities of 218 companies. For the most part, each episode of insider trading corresponds to trading in the securities of a different firm, although several firms had more than one episode of insider trading. For example, insider trading occurred prior to the Limited's April 1984 hostile tender offer for Carter Hawley Hale, and again before the Limited's second hostile tender offer for the same firm in November, 1986.

Firms are excluded from the sample if their stock does not trade on the NYSE, AMEX, or NASDAQ, if the inside information never becomes public, if no record of the public announcement of the inside information exists in the *Wall Street Journal* or on the *Dow Jones Broadtape*, if the only day the insider trades is on the day the inside information becomes public, or if recent listing of the stock on a major exchange prevents the collection of enough price data to estimate the market model parameters. I eliminated the cases where the inside information never becomes public because without such a public announcement, determining whether insider trading increases stock price accuracy requires a subjective evaluation.[28]

[28] A breakdown of the 46 exclusions is as follows. Eleven firms exit the sample because stock price data is unavailable from CRSP. The inside information never becomes public for 5 firms. For 16 firms, no announcement of the inside information could be found in the *Wall Street Journal* or the *Dow Jones Broadtape*. Nine firms are dropped because the inside trader only transacts on the day the inside information becomes public. Incomplete price data prevents the inclusion of 5 firms.

Table 3 displays descriptive statistics of the final sample. The sample includes 98 (54%) NYSE stocks, 54 (30%) NASDAQ stocks, and 34 (17%) AMEX stocks. Table 3 indicates that most insider trading episodes (79% of the sample) are associated with corporate control transactions: friendly and hostile tender offers, mergers, LBOs, restructurings, and major share acquisitions. Also, most insider trading episodes (87%) involve news that positively affects the stock price.

Some inside traders used options and warrants to take advantage of their inside information. The insider trading involved stocks on 529 (91%) of the 582 different days of insider trading in the sample, call options on 47 days (8%), put options on 2 days (.3%), and warrants on 4 days (.7%). For consistency, the analysis to follow investigates the effects of both stock and non-stock trading on the stock price.[29]

I use event study methodology to estimate abnormal returns on insider trading days and on the day the inside information becomes public. This approach requires the dates of insider trading and the public announcement date of the inside information. The dates of the insider trades are from SEC documents, both public and non-public.

The predominance of takeover related events in the sample compli-

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[29] A cross-sectional analysis fails to detect any significant difference in the stock price reaction on insider trading days based upon whether the trade involved stocks or non-stock securities.

cates the analysis since frequently speculation concerning an upcoming takeover occurs in the media prior to the formal announcement of the takeover. More precisely, news announcements occurring on the same day as the insider trading make isolating the effect of the insider trading difficult. To control for the impact of the confounding news announcements, I collect the dates of interim news announcements relating to the inside information, as well as the formal announcement date.

For each stock, I searched three related sources for dates of the final announcement of the inside information and any preceding news announcements: the SEC's *Dow Jones Headline Tapes*, the *Dow Jones News Retrieval Service* (*DJNS*), and the *Wall Street Journal Index* (*WSJ Index*). The *Headline Tapes* cover news events dating back to 1982. They give the story headline and the date the story crosses the *Broadtape* or appears in the *WSJ* if the story never crosses the *Broadtape*. The *DJNS* has the entire story, as well as the date and time of day that an announcement crosses the *Broadtape* for selected news events dating back to June 1979.[30] For example, if the inside information involved a tender offer, I used the *Headline Tapes* to find the dates of 13D filings, possible prior tender offers, acquisition rumors, and the final announcement of the tender offer itself. Using the *Headline Tapes* story date offers two advantages over

[30] The *DJNS* is not comprehensive; it covers major news stories, while the *Headline Tapes* cover every story.

the *WSJ* Index date. First, information reported in the *WSJ* often comes across the *Broadtape* a day before the story appears in the *WSJ*. The *Headline Tapes* date is the earlier of the two dates, and therefore more accurately reflects the date the information became public. Second, the *Headline Tapes* also report stories, such as acquisition rumors, that come across the *Broadtape*, which the *WSJ* never reports. Prior to 1982, I use the *WSJ Index* to find news announcements and dates. To determine whether the news releases occurred before or after the market closed for the day, I used the *DJNS*.

A. *Basic Specification*

I use a modified market model to estimate the stock price impact of insider trading, with a separate OLS regression for each insider trading episode. The basic specification for the modified market model is as follows:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \gamma_i Announce_{it} + \delta_i Inside_{it} + \sum_{j=1}^{J} \mu_{ij} News_{ijt} + \epsilon_{it} \quad (1)$$

The term R_{it} is the return at time t for firm i (i=1,...,183), R_{mt} is the CRSP value-weighted market index return at time t, $Announce_{it}$ is a dummy variable which equals one for firm i if the inside information is publicly announced on day t and zero otherwise, $Inside_{it}$ is a dummy variable which equals one for firm i on days of illegal insider trading in firm i's stock and zero otherwise, and $News_{ijt}$ are J dummy variables which control for confounding interim news announcements. The number of news dummies included, J, differs by firm, depending on the number of interim news announcements appearing in the *Wall Street Journal* or on the *Dow Jones Broadtape*. The jth news dummy equals one only on the day of the jth interim news announcement.

The coefficient δ_i directly tests whether insider trading affects stock prices. In addition to returns over the event period, the regressions incorporate an additional 150 trading days of returns that contribute to the estimates of the market model parameters, α_i and β_i. The 150 day estimation period ends with the day prior to the earlier of the first insider trade or the first interim news announcement.

For example, in 1985, following speculation in the *Wall Street Journal* and on the *Broadtape*, Coastal launched a hostile tender offer for American Natural Resources. In this case, $News_1$ equals one on 2-28-85, the day the Heard on the Street column reported a potential bid and news of American Natural Resources' defensive spinoff plan appeared on the *Broadtape*, and zero on other days; *Announce* equals one on 3-04-85, the day of the actual bid, and zero other days; and *Inside* equals one on each of the six insider trading days occurring from 2-14-85 through 3-01-85, and zero otherwise.

If insider trading increases the accuracy of securities prices, the abnormal returns on insider trading days will have the same sign as the abnormal return on the day the inside information becomes public. For example, the stock price of the target of a tender offer usually increases when the tender offer is publicly announced. If insider trading in the target's stock results in a more accurate stock price, then insider trading will move the price closer to the stock price realized after the tender offer announcement. More precisely, in this case positive abnormal returns will occur on insider trading days.

Table 4 summarizes the estimates of the basic specification for the 183 OLS regressions. The table groups the means and standard errors of the regression coefficients by type of insider trading episode, using the same event classification as Table 3. Panel A separates events related to takeovers from other events, such as bankruptcy; Panel B partitions the takeover sample by type of takeover event: friendly tender offer, LBO, etc..

The first column, *Public Ann. Day AR*, corresponds to the γ_i coefficient in the regression equation. The *Public Ann. Day AR* is the abnormal return on the day the inside information becomes public. In the first row, labelled *Takeover Related Events*, the average public announcement day abnormal return across all takeover related events is 17.46%.

The standard error of the average public announcement day abnormal return, in parentheses, is 0.92%. Column two, *AR on Insider Trade Day*, displays the means of the δ_i coefficients from the regression equation. For takeover related events, the average abnormal return on an insider trading day is 2.55%, with a cross-sectional standard error of 0.29%.

The third column, *CAR on Insider Trade Days*, sums the abnormal returns across trading days to calculate a cumulative abnormal return (CAR) for each insider trading episode. Specifically,

$$\text{(2)} \qquad \textit{Insider Trading CAR}_i = \hat{\delta}_i \cdot N_i$$

where $\hat{\delta}_i$ is the OLS estimate of the coefficient on the insider trading dummy, and N_i is the number of days insider trading occurs. The average cumulative abnormal return on insider trading days for takeover related events is 6.01%, with a standard error of 0.68%.

The *Z-stat of CAR*, column four, lists another statistic that tests whether the mean insider trading CAR differs from zero. The z-stat equals the sum of the individual regression t-statistics for δ_i divided by the square root of the number of regression coefficients.[31]

$$\text{(3)} \qquad Z\text{-}stat = \frac{1}{\sqrt{K}} \sum_{i=1}^{K} \frac{\hat{\delta}_i}{s_i}$$

[31] This approach assumes the individual regression t-statistics are random variables distributed asymptotically N(0,1). Each t-statistic has at least 147 degrees of freedom.

where s_i is the standard error of the estimated coefficient $\hat{\delta}_i$ and K is the number of insider trading episodes or individual regressions. The z-stat for the takeover related insider trading episodes is 19.35. The z-statistic approach to testing whether the mean insider trading CAR is zero incorporates the information contained in the standard deviation of the individual $\hat{\delta}i$ coefficients.[32] The table reports cross-sectional standard errors in addition to the z-statistics because if volatility increases during the event period, the cross-sectional t-test may be more appropriate. In this sample, the two test statistics lead to similar inferences.[33]

Column five of Table 4, *Insider Trading Run-up*, shows whether insider trading increases stock price accuracy. *Run-up*, defined as the insider trading CAR divided by the announcement day abnormal return, measures both the direction and size of the insider trading induced stock price movement.[34]

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[32] This information is especially important when considering the mean insider trading CAR because the number of days of insider trading varies by insider trading episode. A cumulative abnormal return of 8% accumulated over eight insider trading days is not equally as likely as an 8% cumulative abnormal return accumulated over one trading day, and the individual regression t-statistics reflect this fact. The z-statistic therefore captures some information that the cross-sectional t-statistic misses.

[33] See Dodd and Warner (1983) and Warner, Watts, and Wruck (1988) for the derivation of the z-statistic test of abnormal performance.

[34] An alternative run-up index compares the insider trading CAR to the sum of insider trading CAR and the announcement day abnormal return (AR). This alternate measure of run-up has the undesirable property that it may be large and positive when

$$(4) \qquad \text{Run-up}_i = \frac{\hat{\delta}_i \cdot N_i}{\hat{\gamma}_i}$$

Run-up is positive if insider trading moves stock prices in the same direction as the subsequent public announcement of the inside information; run-up is larger the more the insider trading moves prices relative to the public information announcement. Run-up averages 44.16% for the insider trading episodes related to takeovers.

The final column, *Sign Test on Run-up > 0*, displays the probability level of a non-parametric sign test for the sign of run-up. A positive sign for run-up indicates that insider trading improves stock price accuracy; for takeover related events, the probability of observing as many values of run-up with a positive sign as occur in this subsample, given that the probability of a positive sign is 0.5, is approximately zero (<0.0001).

The *total* row in Table 4 consolidates the results across types of inside information after first standardizing the abnormal returns on insider trading days by multiplying the insider trading abnormal returns by -1 if the public announcement day abnormal return is negative. Negative abnormal returns and cumulative abnormal returns observed for insider trading episodes involving negative inside information reflect losses avoided by the

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the insider trading CAR and the announcement day AR have opposite signs, that is, when insider trading moves stock prices in the "wrong" direction.

inside trader. That is, a trader with negative inside information anticipates the stock price drop and sells to avoid a loss.[35]

Table 4 shows that insider trading does move stock prices significantly, measured both by cross-sectional t-tests and z-tests. Insider trading results in an abnormal return of 3.06% on average on the day of the insider trade (see *total* line). Dividing by its standard error of 0.36 yields a t-statistic of 8.50, statistically different from zero at conventional levels. The mean CAR for an insider trading episode is 6.85%, which is also significantly different from zero when compared to its cross-sectional standard error (t-stat=8.90).

To increase stock price accuracy, however, insider trading must move stock prices in the same direction as the subsequent public announcement of the inside information. The non-parametric sign test on run-up suggests that insider trading is likely to move stock prices in the correct direction. Almost 81% of all insider trading episodes have run-up values greater than zero; that is, insider trading increases stock price accuracy 81% of the time, resulting in a probability level of less than 0.0001 for the sign test. Moreover, insider trading moves stock prices, on average, by almost half of the amount (47.56%) that the subsequent public announce-

[35] Of course, the negative (but insignificant) abnormal return on insider trading days calculated for the three positive earnings announcements do not reflect a loss avoided, but a plain loss, since the inside trader had a long position in the stock.

ment of the inside information does.[36] The mean run-up value of 47.56% differs significantly from zero, with a t-statistic of 7.87. Hence, insider trading does increase stock price accuracy in the sample.

Although takeovers comprise most of the sample, the results are robust to the type of inside information, confirming the conclusion that insider trading increases the accuracy of stock prices significantly. In spite of the small number of observations in each category, four of the six event classifications in Panel A exhibit z-statistics significantly different from zero. Positive earnings and miscellaneous bad news announcements have insignificant z-statistics, but there are only three positive earnings and two miscellaneous bad news episodes. Four of six categories have positive run-up values, indicating that stock price accuracy increases; three of six categories experience positive run-up values statistically greater than zero. This robustness of the results to the type of inside information implies that takeovers alone do not drive the results.

Panel B of Table 4 shows that insider trading increases stock price

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[36] Note that the mean run-up index is greater than the ratio of the mean insider trading CAR to the mean announcement day AR. Specifically, the mean ratio is 48%; the ratio of the means is 37%. One explanation for this difference between the two measures is that some large announcement day ARs may not have correspondingly large insider trading CARs. As a result, some individual run-up observations may be low, but the mean ratio does not weight these observations very heavily. The ratio of the means, however, weights these observations relatively heavily. A simple check of the 10 run-up values between 0 and 5% confirms this explanation: the mean insider trading CAR for these observations is .58%, while the mean announcement day AR is 31.67%.

accuracy for different types of takeover related events. The z-statistics for the cumulative abnormal returns due to insider trading are significant for five of seven types of takeover related events; run-up is positive for all of the seven types of takeover related events, statistically significant for four of the seven event types. Again, the categories with few observations have insignificant run-up indices.

Table 4 docs not report the coefficients on the news dummies, which control for interim news announcements preceding the public announcement of the inside information. The average abnormal return on a news day is 6.43%, with a standard error of 0.91%.[37]

Inside traders also earn large abnormal returns over short time periods. Table 5 indicates that the excess return from the insider's first trade through the public information announcement averages more than

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[37] The basic specification controls for interim news announcements which may occur prior to the public announcement of the inside information by including a separate news dummy for each interim news announcement. An alternative approach excludes insider trading dates on which news announcements also occur. Using this approach leaves 165 insider trading episodes in the sample; 18 insider trading episodes are left with no insider trading dates on which to estimate abnormal returns and hence exit the sample. The results are robust to this specification and look very similar to the dummy variable approach of the original specification. Run-up under the alternative approach is 45% versus 48% under the original specification; the CAR on insider trading days is 6.4% versus 6.8% under the original specification. Further analysis uses the news dummy approach of the original specification, but replicating the tests with the alternative specification produces inferences that are robust to the method of controlling for interim news announcements.

30% over a 14 day period, equivalent to an annualized, continuously compounded return of 592%.[38] These results contribute further evidence contradicting the strong-form of the efficient markets hypothesis. The defendants are able to earn large excess returns using inside information.

The results from the basic specification displayed in table 4 suggest that insider trading leads to quick price discovery. The insider induced price movement is large relative to the subsequent price reaction upon the release of the inside information and is statistically significant. The additional specifications to follow investigate the robustness of these results. These specifications address sample selection problems and try to distinguish between the insider trading effect and the price run-up created by rumors and bidder accumulation of target stock prior to takeover announcements.

B. Testing for Sample Selection Bias

Using only insider trading detected by the SEC introduces potential sample selection bias into the estimation of the price effects of insider trading. If unusual price or volume movements trigger an investigation, then the sample consists precisely of the insider trading which moves prices the most. In this case, the sample of detected insider trading provides an

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[38] (32.2% CAR / 13.7 trading days) x (252 trading days per year) = 592% per year, continuously compounded.

upper bound on the size of the insider trading induced price movement, detected and undetected.

On the other hand, the legal elements required to establish an insider trading violation may generate a bias in the opposite direction. The inside information on which a defendant bases his trade must be *material* to establish a violation. Information is material if a reasonable investor considers it important to his/her investment decision. In practice, the courts consider information material if it produces an abnormal price response when it is publicly released. Proving materiality is easier if the price response is large; accordingly, the SEC decides, in part, to pursue an insider trading case based upon the stock price reaction upon the announcement of the inside information.

This materiality requirement may bias the estimates of the insider induced price movements downward. Large price reactions to announcements of inside information can occur when the insider has not moved the price very much prior to the announcement. In the extreme, inside traders who move the price so that it incorporates <u>all</u> of the inside information preclude a significant stock price reaction when the information is subsequently announced publicly. Thus, the traders who move prices the most may be excluded from the sample.

The source of the SEC investigation provides information about the sign and magnitude of the potential upward bias. Table 6 displays the distribution of insider trading episodes by source of the SEC case investigation. The most frequent source of an investigation of an insider trading episode is a public complaint (41% of episodes), followed by an exchange referral (31%). The stock exchanges use two types of surveillance. First, unusual price or volume movements cause an investigation. Second, news announcements which create large price movements produce a search for unusual trading activity prior to the announcements. These surveillance methods could create an upward bias in the sample estimates.

While insider trading cases referred to the SEC by the stock exchanges could bias upwards the estimation of the insider induced price movements, cases with other sources may not produce this bias. The most frequent source of an insider trading case is a public complaint. The Dennis Levine case, for example, began with an anonymous letter from two brokers who were profiting by following Levine's trading, but were subsequently cut off from the information network. They retaliated by informing the SEC. In other instances, the SEC may investigate a company for reasons unrelated to insider trading, e.g. financial fraud, and discover that a corporate insider traded before an earnings announcement. No obvious bias exists in these cases.

One may be able to infer whether the estimated CAR on insider trading days is biased by examining the source of the case investigation. More specifically, cases which begin as stock exchange referrals, SEC investigations, press stories, issuer referrals, or bidder referrals are more likely to introduce an upward bias in the insider induced price movement and run-up index than cases which begin as public complaints, SEC investigations of the issuer for unrelated violations, broker referrals, and other referrals.[39] Most cases in the former category, labelled as the *Exchange Referral* category, begin when someone notices a price run-up prior to a takeover and asks whether insider trading could have created the movement. As argued above, cases in the later category, labelled the *Public Complaint* category, have no such obvious bias. Many public complaints arise when the informant knows the inside trader.[40] Brokers tend to refer clients who trade prior to a news announcement that results in a large price response. Other referrals are typically referrals from another government agency, such as a state banking agency investigating a merger for reasons unrelated to insider trading.

Table 7 segments the announcement day abnormal return, the insider

[39] A SEC investigation refers to cases where the SEC investigates without an outside referral. An issuer referral occurs when the firm in which the inside trader transacts contacts the SEC. A bidder referral arises when the bidding firm in a takeover contacts the SEC about insider trading in the target stock.

[40] Individuals may be more likely to inform the SEC of suspected insider trading if the inside information creates a large price reaction upon announcement. This tendency implies that cases beginning as public complaints might have large announcement day abnormal returns, but does not imply that these cases will have large CARs on insider trading days.

trading cumulative abnormal return, and the insider induced run-up into two groups based upon the case source. As discussed above, the *Exchange Referral* column should contain less bias than the *Public Complaint* column. The results in table 7 indicate that sample selection problems do not significantly bias the earlier findings. Although the insider trading CAR is somewhat greater for exchange referrals than for public complaints, the difference in the means is not significant. The insider trading CAR is 7.9% for exchange referrals and 5.5% for public complaints. The t-statistic for the difference in means is 1.53. The sample means for the announcement price reaction and the run-up also do not differ significantly by source of case. The mean announcement day abnormal return is 18.6% for exchange referrals, 16.7% for public complaints (t-stat=1.01), and the run-up is 54.0% for exchange referrals, 39.9% for public complaints (t-stat=1.12).[41] Even if the exchanges refer cases based upon the size of the insider trading CAR, these results indicate that the estimated insider trading CAR is still large and significantly different from zero for insider trading episodes which do not have this potential bias.

Another way to detect sample selection bias is to divide the sample by the number of stocks the insider trades. When the SEC uncovers

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41 The results from an alternative specification which includes only public complaints in the *Public Complaint* category are almost identical to those reported in Table 8.

insider trading in one stock, it also checks the past trading history of the defendant for suspicious trading around the time of information announcements. Even if a large insider induced price movement leads to the defendants initial detection, any insider trading discovered later in the investigation may be less likely to be selected based upon the size of the insider induced price movement.

Table 8 divides the sample into two groups by whether the inside trader transacted in only one stock or transacted in more than one stock.[42] Again, the insider trading effect and the run-up do not differ by the number of stocks traded.[43] The insider trading CAR for one-stock traders is 7.8%, whereas the mean insider trading CAR for multi-stock traders is 5.9%. Therefore, the upward bias in the insider trading effect created by sample selection is likely to be small. In fact, table 8 suggests instead that information announcements that create large price movements lead to investigations and detection. The mean announcement day abnormal return is significantly larger for one-stock traders (22.7%) than the announcement day abnormal return for multi-stock traders (15.0%, t-stat for difference in means=3.75). These results reinforce the conclusions from table 7: even

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[42] Although ideally one would like to exclude the insider trading episodes which lead to the multi-stock trader's detection from the analysis, the identity of these stocks are unknown.

[43] A similar segmentation for inside traders that transact in less than 5 stocks versus inside traders than transact in 5 or more stocks yields almost identical results.

if the analysis eliminates the insider trading episodes most likely to be contaminated from sample selection problems, the estimates of the insider trading CAR are very similar to the estimates for the entire sample, and are still significantly different from zero.

C. Other Reasons for Price Run-up

Abnormal price and volume volatility and price run-up characterize the period immediately preceding a takeover. One explanation for the relatively large estimates of the total insider effect and insider trading induced run-up is that rumors not reported in the media and purchases by risk arbitragers and bidders create stock price run-up apart from insider trading. The insider trading dummy may capture the effects of events occurring concomitantly with insider trading instead of the effects of the insider trading itself.

To test this hypothesis, I examine the excess returns on days without insider trading and without identifiable news announcements. If rumors, risk arbitragers, and bidders do run-up the stock price, one should be able to observe this run-up on non-insider trading days as well as on insider trading days. I then investigate the insider trading effect net of the run-up observed on other days. The OLS regressions estimate the following equation:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \delta_i Inside_{it} + \sum_{j=1}^{J} \mu_{it} News_{ijt} + \zeta_i OtherDays_{it} + e_{it}$$

(5)

where *OtherDays* is a dummy variable that equals one on day t if there is no insider trading or news announcements on day t, and if day t falls within a specified window before the public announcement day, for example, twenty trading days before the public announcement. The estimated coefficient, $\hat{\zeta}_i$, is a measure of stock price run-up that occurs prior to a takeover for reasons unrelated to insider trading.

Assuming that rumors and risk arbitrageur and bidder accumulation run-up the stock price by roughly the same amount on average whether or not insider trading occurs, one can attribute the portion of the abnormal return on insider trading days that exceeds the average abnormal return on non-insider, non-news days to the effect of insider trading. To measure the insider trading CAR net of any price run-up that occurs in the absence of insider trading, an adjusted insider trading CAR is calculated by subtracting the mean abnormal return on non-insider non-news days from the mean abnormal return on insider trading days, and multiplying by the number of insider trading days in the episode. The adjusted run-up variable divides the adjusted insider trading CAR by the announcement day abnormal return.

$$(6) \qquad \textit{Adjusted Insider Trading } CAR_i = (\hat{\delta}_i - \hat{\zeta}_i) \cdot N_i$$

$$(7) \qquad \textit{Adjusted Run-up}_i = \frac{\textit{Adjusted Insider Trading } CAR_i}{\hat{\gamma}_i}$$

Table 9 displays two approaches to estimating the returns on days without insider trading or news announcements. The approaches differ in the windows used to estimate the abnormal returns on days without insider trading or news. The first method in Table 9, labelled *20 Day Window*, estimates the abnormal returns on non-insider, non-news days over a twenty trading day window prior to the public announcement of the inside information. The second method, labelled *Variable Window*, estimates the cumulative abnormal return from the day of the first insider trade (or the first related news announcement if it occurs earlier) until the date of the public announcement of the inside information for each insider trading episode. These estimates also exclude insider trading days, days of related news announcements, and the public announcement day. For fifty-two episodes of insider trading, all of the days between the first insider trading date and the public announcement day have insider trading or news announcements. The *Variable Window* approach therefore excludes these 52 observations.

If leakage of the inside information increases as the public announcement day approaches, then run-up on days without insider trading or news announcements might be higher for the *Variable Window* than for the *20 Day Window*. More precisely, the *20 Day Window* starts before the first insider trading date, on average.[44] Days in the *Variable Window* are likely to be nearer to the public announcement day, on average, than days in the *20 Day Window*.

Table 9 reports the mean abnormal return on insider trading days, on news days, and on days without news or insider trading. Table 9 also presents the *Adjusted Insider Trade CAR* and the *Adjusted Run-up*. The calculations reveal that stock prices move significantly more on insider trading days than either days with news announcements or days without insider trading or news. For the 20 day window approach, the mean abnormal return on insider trading days is 3.06%, the mean abnormal return on news days is 1.69%, and the mean abnormal return on non-news, non-insider trading days is 0.00%. The abnormal returns for the variable window approach are similar: 2.31% for insider trading days, 1.96% for news days, and 0.72% for non-news, non-insider trading days.

The adjusted insider trade CAR and the adjusted run-up are close to the unadjusted insider trading CAR and run-up results from Table 4, reflecting the small price movements that occur on non-insider trading,

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[44] The mean number of days from the first insider trade to the public announcement date is 13.7, from Table 5.

non-news days. For the 20 day window, the adjusted insider trade CAR is 6.81% versus 6.85% from Table 4. The adjusted run-up is also similar to the unadjusted run-up: 46.50% versus 47.56%. The variable window estimates are somewhat less than the 20 day window estimates, but are still large and significant: the adjusted insider trading CAR is 5.23% (t-stat=4.18) and the adjusted run-up is 33.81% (t-stat=3.35). Again, the results are robust to adjustments for price run-up that may occur in the absence of insider trading. Bidder and risk arbitrager accumulation of target stock, and rumors not reported in the media, do not drive the results.

D. How Much Price Run-up before Takeovers is Attributable to Insider Trading?

Many observers cite the stock price gain in the target firm's stock prior to a takeover announcement as evidence of widespread insider trading. This sample allows one to calculate how much run-up to attribute to insider trading. Such a comparison of the price run-up before takeovers in the insider trading sample and the price run-up before takeovers documented by prior studies is suggestive. Table 10 compares the price run-up before takeover announcements in the insider trading sample to the price run-ups before takeovers documented in Keown and Pinkerton (1981) and Jarrell and Poulsen (1989). Keown and Pinkerton examine 194 successful takeovers during 1975-1978. They find that the cumulative abnormal return (CAR) from 20 trading days before the takeover announcement through the takeover announcement average 25%. The pre-announcement CAR over this time period (day -20 to day -1, relative to announcement day) is 13%, so the price run-up in their sample is 52%.

Jarrell and Poulsen's study of 172 successful cash tender offers during 1981-1985 permits similar calculations. The CAR from day -20 through the announcement day is 25%; the pre-announcement CAR is 11%. The price run-up in the Jarrell and Poulsen sample is therefore 44%. In my sample of detected insider trading, the CAR for the 141 takeover related events from day -20 through the announcement day is 30%, the pre-announcement CAR is 13%, and the price run-up is 44%. Of the 13% pre-announcement CAR in the insider trading sample, 6% occurs on days with insider trading. Hence, one can attribute approximately 44% of the price run-up to days with insider trading.[45] Moreover, of the remaining pre-announcement price run-up, 3% occurs on days with news announcements and 4% occurs on days without insider trading or news announcements. Therefore, insider trading accounts for almost twice as much of the pre-announcement price run-up as news announcements.[46] These simple

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[45] Similar calculations using the adjusted insider trading measurement discussed in section C imply that 41% of the run-up is attributable to insider trading.

[46] Twenty-four percent of the pre-announcement price run-up occurs on days with news announcements in the *Wall Street Journal* or on the *Broadtape*.

calculations suggest that insider trading may be a significant source of stock price run-up before takeover announcements.

E. The Relation of Trading Volume to Price Movements Observed on Insider Trading Days

The abnormal return observed on an insider trading day is 3% on average. Does insider trading volume alone create this price response, or do market participants deduce the presence of an informed trader? To investigate how inside information becomes incorporated into price, this section examines the trading volume of inside traders.

The first step in explaining the relation between insider trading and price movements is to ask whether trading volume on insider trading days is unusual compared to historical trading volume and overall market trading volume. Abnormal trading volume could reflect that inside traders transact a large amount relative to the expected volume; prices adjust because abnormal volume signals the presence of an informed trader. Alternatively, some market participants may infer the presence of an informed trader, perhaps through order characteristics such as size, frequency, or urgency, and complete additional trades. This secondary wave of trading may create abnormal volume. Examining volume on insider trading days net of the insider trading volume should reveal whether any abnormal volume results directly from insider trading volume, or indirectly from secondary trading.

Table 11 displays descriptive statistics concerning the trading volume on insider trading days. Firm volume data come from Interactive Data Services' *Investment Statistical Listing (ISL) Tapes* and from Standard and Poor's *Daily Stock Price Record*. Insider trading volume data come from public and non-public SEC documents. Table 11 reveals that the inside trader purchases (or sells) 9,819 shares, worth $300,023, on an average insider trading day. The total firm trading volume on an insider trading day is 113,909 shares, worth $4,121,533. Relative to the value of the firm, the inside traders transact 0.12% of the firm's equity value; the total firm trading volume is 1.04% of the firm's equity value. The mean ratio of the insider trading volume to the firm trading volume is 41.72%, although the median ratio is considerably less, 11.32%. The explanation for this difference is that insider trading comprises almost 100% of firm volume for a few observations where firm volume is relatively low.

The finding that firm trading volume of approximately 1% of the firm's equity is associated with an abnormal price movement of 3% is consistent with previous related work. Petersen and Umlauf (1990) analyze NYSE specialist's quotations. They conclude that a trade the size of 1% of the firm's equity causes the specialist to revise his quoted price by a mean amount of 2.9% (median=2.2%). Holthausen, Leftwich and Mayers (1987) investigate block trading and determine that a 1% equity trade results in a price movement of at most 0.4%. This much lower value, however, is not necessarily inconsistent with the above results. Holthausen, Leftwich and Mayers look exclusively at block trades, classify the trade as a buy or sell by whether it is followed by an uptick or a downtick, and find that the equity size of the trade does not, in most cases, significantly affect the price response to the trade.

Although these studies allow rough order of magnitude calculations, their data differ in at least one critical respect from the insider trading data. Both Peterson and Umlauf and Holthausen, Leftwich and Mayers consider single trades worth 1% of the firm's equity. In contrast, the 1% of firm equity that changes hands on insider trading days is the *total* firm trading volume. Moreover, the insider traders transact only 0.12% of firm equity on insider trading days, considerably less than the total firm volume of 1% of equity. How does insider trading affect total firm volume and associated price movements?

Without a model for volume behavior, estimating abnormal volume is less straightforward than estimating abnormal returns. The standard approach, outlined by Ajinkya and Jain (1989), employs a log market model similar to the market model for returns.

$$\log(\nu_{it}) = \alpha_i + \beta_i \log(\nu_{mt}) + \epsilon_{it} \tag{8}$$

where ν represents trading volume in shares, i subscripts the individual firm, m represents the market (represented by the total shares traded on the

NYSE, AMEX, or NASDAQ),[47] and t subscripts days. With this specification, using the 150 day estimation period for the 183 firms in the insider trading sample yields a mean adjusted R^2 of 5%.[48] In addition, the Durbin-Watson statistics indicate positive serial correlation in the errors. To adjust for this serial correlation, I add lagged firm volume to the model. Also, to account for known day-of-the-week patterns in volume (see Mulherin and Gerety(1988)), the specification includes day-of-the-week dummies:

$$\text{(9)} \quad \log(v_{it}) = \alpha_i + \beta_i \log(v_{mt}) + \lambda_1 \log(v_{it-1}) + \lambda_2 \log(v_{it-2}) + \delta_1(mon_{it}) + \delta_2(tues_{it}) + \delta_3(weds_{it}) + \delta_4(thurs_{it}) + e_{it}$$

where mon_{it} is a dummy that equals one on Mondays and zero otherwise, etc..

Table 12 lists summary statistics for this specification for the 183 firms in the insider trading sample, using the 150 day estimation period. The mean adjusted R^2 for the modified specification in equation (9) increases to 15% from the 5% of equation (8)'s specification. Durbin h-statistic tests on the new specification (not reported in Table 12) affirm that the addition of lagged firm volume eliminates the serial correlation in the

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[47] J. Harold Mulherin provided market trading volume data.

[48] To avoid taking the log of zero when firm volume equals zero, 1 is added to the firm trading volume.

error terms.[49]

The specification used to detect abnormal volume on insider trading days parallels the OLS regressions used to detect abnormal returns (see equation (1)), with the appropriate modifications as discussed above:

$$\text{(10)} \qquad \begin{aligned} \log(v_{it}) &= \alpha_i + \beta_i \log(v_{mt}) + \lambda_1 \log(v_{it-1}) + \lambda_2 \log(v_{it-2}) + \\ &\quad \delta_1(\mathit{mon}) + \delta_2(\mathit{tues}) + \delta_3(\mathit{weds}) + \delta_4(\mathit{thurs}) + \\ &\quad \gamma_i \mathit{Announce}_{it} + \eta_i \mathit{Inside}_{it} + \sum_{j=1}^{J} \mu_{ij} \mathit{News}_{ijt} + e_{it} \end{aligned}$$

where variables are defined as in equation (9) and η_i directly tests for abnormal volume on insider trading days.

Table 13 presents the results for the 183 OLS regressions. The top row, labelled *Abnormal Return*, repeats the abnormal return results from Table 9. The second row, *Abnormal Volume*, shows the mean $\hat{\eta}_i$ from the OLS regressions. Table 13 shows that the mean abnormal volume on insider trading days is 1.07, that is, volume is 107% higher than expected on insider trading days. *Abnormal Volume Net of Inside Volume* in row three shows the abnormal volume results when the regressions use firm volume minus insider trading volume instead of firm volume. After subtracting shares traded by inside traders, firm volume is still 79% higher than expected on insider trading days. The cross-sectional standard errors, in parentheses, indicate that abnormal volume differs significantly from

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[49] Judge, et al. (1985), p. 326, describes the Durbin h-statistic test for serial correlation in the presence of lagged dependent variables.

zero for both abnormal volume and abnormal volume net of inside volume.

Table 13 also adjusts the abnormal volume estimates for abnormal volume that may occur absent any insider trading. These adjustments are equivalent to the corrections described in Section 4(C) for abnormal returns. The column labelled *Days w/o Insider Trading or News* shows that although abnormal returns are small on non-news, non-insider trading days preceding the public announcement, volume is significantly higher on these days. Specifically, Panel A of Table 13 shows that abnormal volume of 26% (standard error=4%) accompanies an abnormal return of 0.06% on non-news, non-insider trading days during a twenty day window preceding the public announcement. For the variable window, abnormal volume of 65% (standard error =8%) accompanies an abnormal return of 0.90% on an average non-news, non-insider trading day (see Panel B).

Therefore, to separate abnormal volume that may occur even in the absence of insider trading from abnormal volume attributable to insider trading, *adjusted abnormal volume* is calculated. More precisely, this adjustment subtracts the abnormal volume observed on non-news, non-insider trading days from the abnormal volume observed on insider trading days. The *Adjusted Insider Trade Days* column reveals that abnormal volume on insider trading days significantly exceeds that of surrounding days without insider trading or news. For the twenty day window, the mean adjusted abnormal volume is 81% (standard error=7%); for the variable window, the mean adjusted abnormal volume is 28% (standard error=9%).

The interpretation of the *Adjusted Insider Trade Days* calculations for *Abnormal Volume Net of Inside Volume* differs by the window used to estimate abnormal volume on non-news, non-insider trading days. Firm volume net of insider trading volume is on average 54% higher (standard error=8%) on insider trading days than on non-news, non-insider trading days within the 20 day window. For the variable window, however, firm volume net of insider trading volume is on average 1% lower (standard error=9%) on insider trading days than on non-news, non-insider trading days.

The change in sample size from 183 in the 20 day window to 131 in the variable window results in a decline in abnormal volume net of inside volume from 79% to 64%. This decrease, combined with the higher estimate of 65% for abnormal volume on non-news, non-insider trading days in the variable window versus 26% for the 20 day window, creates a negative, but insignificant, variable window estimate for adjusted firm volume net of inside volume.

The construction of the variable window suggests that the variable window estimates are more relevant than the 20 day window estimates. The fixed length of the 20 day window dictates that the 20 day window includes days with little price or volume activity. The 20 day window will sometimes underestimate the magnitude of the increased price and volume activity that precedes the public announcement of the inside information. This underestimation will be most serious for the 52 observations which appear in the 20 day window sample but not the variable window sample. In fact, repeating the analysis using the same reduced variable window sample of 131 firms, but with the 20 day window calculations of abnormal volume on non-news, non-insider trading days, confirms this suspicion. Adjusted abnormal volume net of inside volume is now 32% (standard error=9%). This estimate falls between the 54% value of the larger 20 day window sample and the -1% value of the reduced sample with the variable window calculations.

The implications of Table 13 are therefore that abnormal volume accompanies abnormal returns on insider trading days. The abnormal volume on insider trading days is higher than the abnormal volume on surrounding days, measured with either a 20 day window or a variable window. Most, but not all, of the abnormal volume seems the result of insider trading volume. Net of insider trading volume, abnormal firm volume ranges from -1% to 32% higher on insider trading days than on surrounding days; including insider trading volume, abnormal firm volume is 28% to 81% higher on insider trading days than on surrounding days.

Inside trading volume comprises most of the abnormal firm volume on insider trading days, implying that the insider trading volume creates the price movements on insider trading days. Does the price-volume relation observed on insider trading days differ from that of other days? That is, does insider trading volume affect prices differently than other volume, perhaps because of trade size or frequency? One possibility is that price is more responsive to trading volume on insider trading days than on other days, for reasons suggested above, but may be less responsive to price than on days with public news announcements. To test this hypothesis, I run the following regression:

$$\text{(11)} \quad |AR_{it}| = \alpha + \beta_i AV_{it} + \gamma(Inside_{it} \cdot AV_{it}) + \delta(News_{it} \cdot AV_{it}) + \eta(OtherDays_{it} \cdot AV_{it}) + \mu(Announce_{it} \cdot AV_{it}) + \epsilon_{it}$$

where	*AR*	=	abnormal return
	AV	=	abnormal volume, calculated as residual from equation (9)
	Inside	=	dummy that equals one on insider trading days
	News	=	dummy that equals one on news days
	OtherDays	=	dummy that equals one on non-news, non-insider trading days surrounding the insider trading day, defined as either a 20 day window or a variable window
	Announce	=	dummy that equals one on the final public announcement day

and i subscripts firms (1,...,183), t subscripts days.

Table 14 displays the results from this regression, using both a 20 day window and a variable window for the *OtherDays* variable. The mean coefficient on abnormal volume for the 20 day window is 0.62% (standard error=0.05%), which implies that a 100% increase in abnormal volume is associated with an abnormal price movement of 0.62% on average. The slope dummy for insider trading days, γ, equals 1.09% (standard error=0.06%), indicating that a 100% abnormal volume increase is accompanied by a 1.71% (= 0.62% + 1.09%) abnormal return. Days with news announcements display a price-volume relation with a still greater slope: on news days, a doubling of abnormal volume is associated with a 2.79% abnormal return. The slope on days surrounding the insider trading days is 0.22%, so a doubling of abnormal volume occurs with a 0.84% abnormal price movement. This slope is less than the coefficient on insider trading or news days, but greater than on non-insider trading, non-news days outside the 20 day window. Finally, on the public announcement day of the inside information, a 100% abnormal volume movement is associated with a price reaction of 5.67%.

The extent to which market participants differ or agree in their interpretations of the information revealed on each type of day may help explain the differences in slope parameters. Jain (1988) and Holthausen and Verrecchia (1990) discuss trading volume as a measure of the consensus among market participants about information. Disagreement leads to increased trading; consensus may create price changes even without abnormal trading. A pattern of relative consensus on public news dates and relative disagreement on insider trading days and days without news announcements could explain the magnitudes of the regression coefficients. On news days and the final announcement day, the regression coefficients are greater than on inside trading days or surrounding days. Prices move without much trading volume on public news days; equivalent price movements on insider trading days and surrounding days are accompanied by greater trading volume. The relative disagreement on insider trading days could occur because market participants differ substantially about the presence of informed traders, the content of the inside information, and the potential effects of the information. On the other hand, public news announcements may generate fundamental agreement among market participants.

5. Conclusions and Future Research

The analysis suggests that insider trading does increase stock price accuracy by moving stock prices significantly. The abnormal price movement on insider trading days is 40-50% of the subsequent price reaction to the public announcement of the inside information. Abnormal volume accompanies the abnormal returns on insider trading days; insider trading volume comprises most of this abnormal firm volume. The findings are robust after controlling for pre-announcement news leakage and for adjusting for price run-up that may occur in the absence of any insider trading. An investigation of potential sample selection bias reveals that insider trading detection methods do not appear to screen based upon the size of the insider trading induced price movements, implying that the estimates of these price movements are not severely biased. Moreover, restricting the sample to observations unlikely to create biased estimates still produces large and significant cumulative abnormal returns on insider trading days.

These results have immediate public policy implications to both supporters and opponents of insider trading regulation. First, there are price discovery benefits of insider trading which should be considered when evaluating future legislation concerning insider trading penalties. Second, the current method of calculating insider trading penalties insider trading is incorrect. Specifically, the courts and the SEC ignore insider trading induced price run-up when calculating the defendant's profits. The usual method of determining the amount a defendant repays is to compute the abnormal return on the public announcement day. The defendant then must repay the abnormal return, plus any additional penalties assessed under the Insider Trading Sanctions Act. These additional penalties are commonly calculated as a multiple of the estimated announcement day abnormal return. In many cases, however, the insider's trading creates a significant price movement before the public announcement day, so that the total excess return associated with the inside information equals the insider induced price movement plus the announcement day return. Since the estimates of the run-up index average 50%, the amount of penalty underpayment averages 33%.

The results also provide a foundation for the argument that stock price run-ups before takeover announcements reflect widespread insider trading. Calculations of price run-up over the twenty days preceding the takeover announcement reveal that almost half of this stock price run-up occurs on insider trading days, yet the amount of total price run-up over the twenty days is similar to the amount of price run-up documented by Keown and Pinkerton (1981) and Jarrell and Poulsen (1989).

The preliminary empirical analysis establishes the existence and the size of the insider induced price movement. The results suggest two areas of immediate interest. The first question concerns the magnitude of the insider trading induced run-up and the strategy employed by the inside trader. Why does the inside trader stop trading before prices incorporate all of the inside information? A broader area for future research is the effect of insider trading on the probability of takeover. Insider trading could decrease the probability of a takeover by reducing the profitability of the raider's foothold stake (the amount the raider may acquired before being required to file a 13D to disclose ownership). Insider trading could also raise the cost of takeover by increasing the premium offered to stockholders. If the premium is a fixed mark-up over the stock price, insider trading could increase the required premium by driving up the stock price. The welfare loss resulting from such an increase in the cost of takeovers could be substantial. An empirical investigation of takeover premia and their relation to insider trading may help resolve these issues.

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TABLE 1

Number of Defendants in Insider Trading Cases
Brought by the SEC in the Years 1980-1989
and Number of Insider Trading Episodes by Year in Which Trading Occurs

Year	Number of Civil Defendants[a b]	Number of Defendants in Sample	Percent in Sample	Number of Insider Trading Episodes[c]	Percent of Total Episodes	Cumulative Percent
1974	-	-	-	3	2	2
1977	-	-	-	3	2	3
1978	-	-	-	5	3	6
1979	-	-	-	8	4	10
1980	23	18	78	13	7	17
1981	27	18	67	22	12	30
1982	42	33	79	29	16	46
1983	79	60	76	19	10	56
1984	30	13	43	27	15	71
1985	32	27	84	23	12	83
1986	73	48	66	15	8	91
1987	79	49	62	9	5	96
1988	59	34	58	7	4	100
1989	20	20	100	-	-	-
Total[d]	464	320	69	183		100

[a]*Number of Civil Defendants* lists the number of defendants in SEC civil cases with at least one defendant charged with insider trading.
[b]Listed by year in which the SEC charged the defendants with insider trading. The actual insider trading violations, reported in the *Number of Insider Trading Episodes* column, usually occur in an earlier year.
[c]An insider trading episode involves one or more defendants using inside information to trade in the stock of a specific firm. The count includes only the insider trading associated with the defendants included in the sample.
[d]The 1989 count excludes pending cases.

TABLE 2

Trading Activity, Holding Period Profits, and Penalties of Illegal Inside Trading Defendants

	Median	Standard Deviation	Minimum	Maximum	N
Profit Gained	24,673	2,286,736	(1,517)	23,832,480	211
Loss Avoided[a]	23,575	100,480	(21,741)	471,365	25
# Securities Traded	1.00	6.52	1.00	54.00	255
Profit per Security[b]	17,628	394,414	(10,871)	3,972,080	198
# Immediate Tippees[c]	1.00	2.47	1.00	17.00	95
# Total Tippees[d]	2.00	4.01	1.00	22.00	92
Immediate Tippee Profit	39,103	2,109,121	0	19,030,000	89
Remote Tippee Profit	158,113	410,108	0	1,794,717	31
Total Penalty	21,000	1,872,902	0	25,150,020	255
Penalty per Security	19,349	418,902	0	5,100,000	210
Penalty to Profit Ratio[e]	1.00	0.95	0.00	9.29	169

[a] *Loss Avoided* occurs when defendants sell before a drop in the stock price to avoid a loss.
[b] *Profit per Security* = (Profit Gained + Loss Avoided)/Number of Securities Traded.
[c] *Immediate Tippees* are people to whom defendant disclosed inside information directly.
[d] *Total Tippees* includes immediate tippees plus remote tippees, who receive inside information indirectly.
eTotal Penalty/(Total Profit Gained + Loss Avoided).

TABLE 3

Number of Insider Trading Episodes by Listing Exchange and Type of Inside Information Traded On

Type of Inside Information	Total	Percent
All Takeover Related	145	79
Friendly Tender	38	21
Hostile Tender	34	18
Friendly Merger	50	27
Hostile Merger	3	2
LBO	10	5
Restructuring	5	2
Major Share Acq	5	2
Negative Earnings	12	6
Positive Earnings	3	2
Bankruptcy or Financial Fraud	10	5
Misc Good News[a]	11	6
Misc Bad News[b]	2	1
All Positive News	159	87
All Negative News	24	13
Total	183	100

[a]*Misc Good News* includes a management change, announcements of casino ventures, a patent announcement, a liquidation, a subsidiary divestiture, a Navy contract award, a license for Interferon, and Canadian Minoxidal approval.
[b]*Misc Bad News* includes a lost arbitration suit and failed merger talks.

TABLE 4

Means and Standard Errors of OLS Regressions by Type of Inside Information: Abnormal Returns (ARs) Associated with Insider Trading and Run-up

$$R_{it} = \alpha_i + \beta_i R_{it} + \gamma_i Announce_{it} + \delta_i Inside_{it} + \sum_{j=1}^{J} \mu_{it} News_{ijt} + \epsilon_{it}$$

Panel A:

Type of Inside Info.	Public Ann. Day AR (γ_i)[a]	AR on Inside Trade Day (δ_i)[b]	CAR on Insider Trade Days[c]	Z-Stat of CAR	Insider Trading Run-up[d]	Sign Test on Run-up>0 (prob level)[e]
Takeover Related *(N=145)*	17.46% (0.92)	2.55% (0.29)	6.01% (0.68)	19.35	44.16% (6.23)	0.00
Negative Earnings *(N=12)*	-18.62 (2.96)	-1.94 (0.69)	-2.20 (2.43)	-3.06	14.22 (16.31)	0.19
Positive Earnings *(N=3)*	6.78 (1.24)	0.74 (1.77)	-2.72 (3.58)	-0.48	-45.95 (53.52)	0.50
Bankruptcy or Fraud *(N=10)*	-40.96 (9.17)	-5.65 (2.73)	-16.55 (6.88)	-4.36	87.93 (40.34)	0.17
Misc Good News *(N=11)*	14.43 (3.06)	9.47 (3.39)	16.54 (4.73)	10.36	117.41 (28.71)	0.00
Misc Bad News *(N=2)*	-20.96 (4.43)	-1.65 (1.54)	-8.10 (7.88)	-0.99	48.91 (48.05)	0.50
Total[f] *(N=183)*	18.50 (1.00)	3.06 (0.36)	6.85 (0.77)	21.61	47.56 (6.04)	0.00

[a]Abnormal return on the day the inside information becomes public.

TABLE 4 (cont'd)

Panel B:

Type of Inside Info.	Public Ann. Day AR (γ_i)	AR on Inside Trade Day (δ_i)	CAR on Insider Trade Days	Z-Stat of CAR	Insider Trading Run-up	Sign Test on Run-up>0 (prob level)
			Takeover Related Event			
Friendly Tender	18.45%	2.50%	5.15%	9.49	34.45%	0.00
(N=38)	(1.82)	(0.61)	(1.04)		(8.54)	
Hostile Tender	17.03	2.62	8.15	11.25	58.78	0.00
(N=34)	(1.79)	(0.64)	(1.92)		(13.48)	
Friendly Merger	17.64	3.12	6.78	13.48	49.39	0.00
(N=50)	(1.55)	(0.48)	(1.10)		(12.78)	
Hostile Merger	21.52	2.31	4.46	2.14	58.36	0.12
(N=3)	(10.71)	(1.09)	(2.52)		(52.43)	
LBO	13.92	1.06	1.22	1.88	6.60	0.38
(N=10)	(2.64)	(0.67)	(0.85)		(6.89)	
Restruct.	22.57	0.07	2.06	-0.63	5.12	0.50
(N=5)	(8.45)	(1.43)	(3.42)		(27.51)	
Major Share Acq	10.69	2.50	4.90	2.38	71.78	0.19
(N=5)	(2.04)	(1.24)	(1.72)		(38.43)	

[b]Daily average abnormal return on insider trading days.
[c]Cumulative abnormal return over all insider trading days in a given stock.
[d]Equals *CAR on Insider Trade Days/Public Ann Day AR.*
[e]Probability level of a one-tailed binomial sign test that run-up>0.
[e]CARs standardized by multipling by -1 if the announcement day AR is negative.

TABLE 5

Means and Standard Errors of Cumulative Abnormal Returns (CAR) over Insider Trading Holding Period by Type of Inside Information Traded On

Type of Inside Information	N	Insider Holding Period[a] (# Trading Days)	CAR over Holding Period (%)
Takeover Related	145	12.5	29.9
		(1.4)	(1.5)
Negative Earnings	12	18.4	-30.0
		(7.6)	(4.7)
Positive Earnings	3	21.3	3.3
		(11.2)	(4.2)
Bankruptcy or Financial Fraud	10	26.4	-73.9
		(14.6)	(12.0)
Misc Good News	11	11.2	34.8
		(7.7)	(6.9)
Misc Bad News	2	10.0	-28.1
		(7.0)	(2.5)
Total[b]	183	13.7	32.2
		(1.6)	(1.7)

[a]The insider holding period begins with the first insider purchase or sale and ends when the inside information becomes public, e.g. with a merger announcement.
[b]To standardize the insider trading CAR across insider trading episodes involving both positive and negative news announcements, the CARs are multiplied by -1 if the AR on announcement day is negative.

TABLE 6

Source of SEC Case Investigation by Number and Percent of Insider Trading Episodes

Source of Case	Number of Insider Trading Episodes[a]	Percent of Total
Public Complaint	71	41
All Exchange Referrals	55	31
NASD	19	11
AMEX	15	9
NYSE	15	9
Regional	3	2
CBOT	3	2
SEC Investigation[b]	16	9
Press Story	16	9
Issuer[c]	5	3
Another SEC Case[d]	1	1
Broker	3	2
Bidder	1	1
Other	5	3
Total[e]	174	100

[a]An *insider trading episode* is one or more defendants trading in a given stock using specific information, such as information about an impending takeover.
[b]*SEC Investigation* is the case origin when the SEC begins an investigation without an outside referral.
[c]*Issuer* refers to the firm that is the subject of the inside information.
[d]*Another SEC Case* is the case origin when the SEC investigates a company for another reason, for example, financial fraud, and discovers insider trading violations in addition to the other securities violations.
[e]The source of the SEC case investigation is missing for some insider trading episodes.

TABLE 7

Detecting Sample Selection Bias by Examining Means and Standard Errors of Insider Trading Days CARs, Announcement Day ARs, and Insider Induced Run-up by Source of the SEC Investigation

	Exchange Referrals[a]	Public Complaints[b]	T-stat for the Difference in Means
CAR on Insider Trading Days	7.9% (1.2)[c]	5.5% (1.0)	1.53
Announcement Day AR[d]	18.6% (1.3)	16.7% (1.3)	1.01
Run-up	54.0% (9.4)	39.9% (7.8)	1.12
Number of Insider Trading Episodes	94	80	

[a]*Exchange Referrals* include cases originating as exchange referrals, SEC investigations, press stories, or referrals from the issuer of the stock or the bidding firm.
[b]*Public Complaints* include cases originating from public complaints, SEC investigations of the issuer for unrelated violations, broker referrals, and other referrals.
[c]Standard Errors of the Means are in parentheses.
[d]*Announcement Day AR* is the abnormal return on the day the inside information becomes public, e.g., on the day of a merger announcement.

TABLE 8

Detecting Sample Selection Bias by Examining Means and Standard Errors of Insider Trading Days CARs, Announcement Day ARs, and Insider Induced Run-up by the Number of Stocks the Insider Trades In

	Trade in More Than One Stock[a]	Trade in Exactly One Stock[b]	T-stat for the Difference in Means
CAR on Insider Trading Days	5.9% (0.8)[c]	7.8% (1.4)	1.16
Announcement Day AR[d]	15.0% (0.9)	22.7% (1.8)	3.75
Run-up	44.1% (7.0)	51.3% (10.5)	0.57
Number of Insider Trading Episodes	99	82	

[a]*Trade in More Than One Stock* includes all insider trading episodes where the inside trader participated in other insider trading episodes.
[b]*Trade in Exactly One Stock* consists of all insider trading episodes where the inside trader transacted in only one stock and was not involved in any other insider trading episodes.
[c]Standard Errors of the Means are in parentheses.
[d]*Announcement Day AR* is the abnormal return on the day the inside information becomes public, e.g., on the day of a merger announcement.

TABLE 9

Adjusting Estimates of Abnormal Returns Attributable to Insider Trading for Stock Price Reactions that Occur Absent any Insider Trading Activity

Window to find AR on Days w/o IT or news[a]	Ave AR on Insider Trade Days	Ave AR on News Days	Announcement Day AR	CAR on Insider Trade Days	Insider Trading Run-up	Ave AR on Days w/o Insider Trading or News	Adjusted Insider Trade CAR[b]	Adjusted Run-up[c]
20 Day Window	3.06	6.43	18.50	6.85	47.56	0.06	6.81	46.50
(N=183)	(0.36)	(0.91)	(1.00)	(0.77)	(6.04)	(0.07)	(0.98)	(7.68)
Variable Window	2.31	6.49	16.86	6.94	50.26	0.90	5.23	33.81
(N=131)	(0.32)	(0.94)	(1.01)	(0.99)	(8.00)	(0.28)	(1.25)	(10.09)

[a] *Window to find AR on days w/o IT or news* is the window over which the CAR on non-news, non-insider trading days is calculated. The 20 day window begins 20 trade days before announcement, ends with announcement day. Variable window begins with earlier of first insider trade or news announcement, ends with announcement day.

[b] *Adjusted Insider Trade CAR* = (Average AR per day of insider trading - Average AR on days in specified window without insider trading or interim news announcements) x number of insider trading days.

[c] *Adjusted Run-up* = Adjusted Insider Trade CAR / Announcement Day AR

TABLE 10

The Effect of Insider Trading on the Stock Price Run-up of Target Firms before Takeover Announcements using Cumulative Abnormal Returns (CARs) over a 20 Trading Day Window Preceding the Takeover Announcement

	K&P Sample[a] Acquisitions 1975-78	J&P Sample[b] Tender Offers 1981-85	Takeovers with SEC Insider Trading Charges 1974-88
Target Firm CAR over Long Window (CAR [-20,0])[c]	25.3%	24.9%	31.0%
Pre-Announcement CAR (CAR [-20,-1])	13.2	11.0	13.5
Announcement Day Abnormal Return (CAR [-1,0])	12.0	13.9	17.5
Pre-Announcement Price Run-up (CAR [-20,-1] / CAR [-20,0])	52.1	44.2	43.6
CAR on Insider Trading Days[d]	-	-	5.9
% of Pre-Announcement CAR Attributable to Insider Trading[e]	-	-	43.6
Number of Observations	194	172	141

[a]K&P results from Keown and Pinkerton (1981) sample of successful acquisitions.
[b]J&P results from Jarrell and Poulsen (1989) sample of successful cash tender offers.
[c]CAR [-i,-j] is the cumulative abnormal return from the -ith day (relative to day 0, the announcement day) through the -jth day.
[d]CAR on Insider Trading Days is the mean CAR of target firms on days the inside traders transact in the target firms' stock.
[e]CAR on Insider Trading Days/Pre-Announcement CAR.

TABLE 11

Trading Volume of Inside Traders and Total Firm Volume on Days Insiders Trade: Means and Standard Errors

	Volume Traded by Inside Traders	Total Firm Volume On Days Insiders Trade
Number of Shares	9,819	113,909
	(991)	(10,246)
Dollar Value	300,023	4,121,533
	(35,603)	(594,327)
Percent of Equity	0.12	1.04
	(0.01)	(0.07)
Percent of Firm Volume Traded By Insiders[a]	41.72	-
	(19.01)	

[a] Mean ratio of insider trading volume to firm volume on insider trading days (in percent)

TABLE 12

Summary Statistics of Log Market Model for Trading Volume Using Lagged Firm Volume and Day-of-the-Week Dummies

$$\log(v_{it}) = \alpha_i + \beta_i \log(v_{mt}) + \lambda_1 \log(v_{it-1}) + \lambda_2 \log(v_{it-2}) + \delta_1(mon_{it}) + \delta_2(tues_{it}) + \delta_3(weds_{it}) + \delta_4(thurs_{it}) + \epsilon_{it}$$

	α	β	λ_1	λ_2	δ_1	δ_2	δ_3	δ_4
Mean Coeff	-8.04	0.79	0.26	0.10	0.04	0.08	0.05	-0.04
(std err)	(1.05)	(0.06)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.03)
Mean T-Stat[a]	-0.95	1.96	3.39	1.42	0.12	0.33	0.25	-0.02
Mean R^2	0.19							
Mean Adj R^2	0.15							

[a] Mean of individual t-stats for all regressions.

TABLE 13

Abnormal Returns and Volume on Insider Trading Days, News Days, and Non-News, Non-Insider Trading Days

Panel A:

Window[a]	Insider Trade Days	News Days	Days w/o Insider Trading or News	Announce-ment Day	Cumulative on IT Days	Adjusted Inside Trade Days	Adjusted Cumulative on IT Days
20 Day Window (N=183)							
Abnormal Return	3.06	6.43	0.06	18.50	6.89	3.00	6.85
(percent)	(0.36)	(0.91)	(0.07)	(1.00)	(0.77)	(0.38)	(0.98)
Abnormal Volume	1.07	1.46	0.26	2.46	3.94	0.81	2.05
(percent x 10^{-2})	(0.08)	(0.20)	(0.04)	(0.09)	(0.39)	(0.07)	(0.28)
Abnormal Volume	0.79	1.46	0.26	2.46	2.14	0.54	1.25
Net of Inside Volume	(0.08)	(0.20)	(0.04)	(0.09)	(0.37)	(0.08)	(0.28)
(percent x 10^{-2})							

TABLE 13 (cont'd)

Panel B:

Window	Insider Trade Days	News Days	Days w/o Insider Trading or News	Announcement Day	Cumulative on IT Days	Adjusted Inside Trade Days	Adjusted Cumulative on IT Days
Variable Window *(N=131)*							
Abnormal Return	2.31	6.49	0.90	16.86	6.99	1.42	5.27
(percent)	(0.32)	(0.94)	(0.28)	(1.01)	(1.00)	(0.44)	(1.26)
Abnormal Volume	0.93	1.46	0.65	2.44	3.30	0.28	1.67
(percent x 10^{-2})	(0.09)	(0.20)	(0.08)	(0.10)	(0.54)	(0.09)	(0.61)
Abnormal Volume	0.64	1.46	0.65	2.45	2.33	-0.01	0.70
Net of Inside Volume	(0.10)	(0.20)	(0.08)	(0.10)	(0.51)	(0.09)	(0.58)
(percent x 10^{-2})							

[a]Window used to calculate abnormal returns (or abnormal volume) on non-news, non-insider trading days. The *20 Day Window* begins 20 trade days before announcement day and ends with the announcement day. The *Variable Window* begins with the earlier of the first insider trade or news announcement and ends with the announcement day.

TABLE 14

Relation between Abnormal Returns (AR) and Abnormal Volume (AV): OLS Regressions

$$|AR_{it}| = \alpha + \beta_i AV_{it} + \gamma(Inside_{it} \cdot AV_{it}) + \delta(News_{it} \cdot AV_{it}) + \eta(OtherDays_{it} \cdot AV_{it}) + \mu(Announce_{it} \cdot AV_{it}) + e_{it}$$

	α	$\bar{\beta}_i$	γ	δ	η	μ
			All coefficients x 10^2			
20 Day Window[a]	1.81	0.62	1.09	2.17	0.22	5.05
(std err)	(0.01)	(0.05)	(0.06)	(0.09)	(0.03)	(0.07)
Variable Window[b]	1.81	0.66	1.12	2.22	0.37	5.06
(std err)	(0.01)	(0.05)	(0.06)	(0.09)	(0.04)	(0.07)

[a] *OtherDays* variable consists of all non-news, non-insider trading days in a 20 day window preceding the public announcement

[b] *OtherDays* variable consists of all non-news, non-insider trading days in a variable window from the first insider trade or news announcement to the public announcement

APPENDIX A

Information Collected for Each Defendant

Name
Whether civil case filed
Whether administrative action filed
Whether criminal case filed
Type of Employer
Occupation
Number of violative transactions
Number of stocks traded in
Profit gained or loss avoided
Whether nominee accounts used
Whether foreign bank accounts/brokerages used
Whether defendant traded, tipped, or both
Whether defendant was an original source of the non-public information or a tippee
Number and type of tippees
Direct and remote tippee profit
Whether defendant helped produce non-public information
Whether defendant could influence announcement timing
Litigation history
Violations alleged in initial complaint
Legal theory used
Penalties
Case origin
Trading information:
- Name of stock(s)
- Exchange traded on
- Type of security (option or stock)
- Transaction date
- Transaction volume ($ and share)
- Transaction price
- Transaction profit
- Type of information traded on
- Source of information
- Date information became public

CHAPTER 2

A COMPARISON OF FORWARD AND FUTURES PRICES OF AN INTEREST RATE SENSITIVE FINANCIAL ASSET

1. Introduction

Despite the conceptual similarity of futures and forward contracts, different tax treatments, transactions costs, market structures, default risk, and contractual specifications create divergence between futures and forward prices.[1] Cox, Ingersoll, and Ross (CIR) (1981), incorporating results of Jarrow and Oldfield (1981), Sundaresan (1980), Richard and Sundaresan (1981), and French (1982), describe the theoretical price difference in futures and forward prices generated by the marking-to-market of futures contracts. The empirical evidence for the CIR model, however, is mixed. Prior research supports the weak CIR implication predicting the sign of the average price difference, but fails to support the stronger CIR prediction that specific covariances are important explanatory variables for this price difference (see French (1983)).

The CIR model implies that the price divergence increases with the futures-riskless bond covariance, so using an interest rate sensitive instrument to test the CIR model increases the power of the tests. Using previously unavailable data, this paper tests the weak and strong implications of the CIR model using an interest rate sensitive financial asset,

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[1] Rendleman and Carabini (1979), Elton, Gruber and Rentzler (1984), Kawaller and Koch (1984), Gendreau (1985), Kolb and Gay (1985), and Allen and Thurston (1988) focus on the T-bill market and the role of transactions costs in the price differences. Capozza and Cornell (1979) study both taxes and transactions costs as sources of price divergence. Kamara (1988) examines how market structure affects T-bill futures and forward price differences.

specifically, Eurodollars. Unlike prior empirical studies, test results support both the weak and strong model predictions, successfully explaining intra-sample price difference variations.

The paper consists of seven sections. Section 2 describes the CIR propositions. The data and its construction constitute Section 3. Section 4 provides summary data statistics, and sections 5 and 6 present test results. Section 7 reviews the results in the context of related work and concludes.

2. The CIR Model of Futures and Forwards Prices

CIR focus exclusively on the forward-futures price difference created by the daily resettlement of futures contracts. They outline arbitrage strategies to replicate the payoff of forward and futures contracts at maturity. To reproduce the payoff of a forward contract, the investor takes a long position in forward contracts (requiring zero investment), and buys riskless bonds that mature at the forward contract expiration date. At maturity, the investor exchanges the bonds, which by design are now equal in value to the previously agreed upon forward price, for the commodity. Today's forward price must therefore depend on the riskless bond price and the present value of the commodity at maturity.

In contrast to the by-and-hold strategy required to replicate the forward contract payoff, duplication of a futures contract payoff requires continual reinvestment of a principal amount and accumulated interest in one-period bonds. In addition, the investor must take a long position in futures contracts where the size of his position depends upon past one-period interest rates. The investor must also liquidate his futures contracts position each period and reinvest the proceeds in riskless bonds.

CIR compare the difference between forward and futures contracts to the difference between buying and holding a long bond, and continually rolling over short term bonds. This distinction, CIR argue, leads futures prices to depend upon the correlation between spot prices and interest rates, while forward prices do not.

Similar arbitrage arguments lead to the following CIR propositions (assuming no transactions costs, taxes, or default, and that individuals can borrow or lend at the same nominal rate).[2] Using French's (1983) notation:

$$F(t,T) - f(t,T) = \frac{-PV_{t,T}\{ \int_t^T F(w,T)\ cov[F(w,T),B(w,T)]\ dw\}}{B(t,T)} \qquad (1)$$

[2] Proposition numbering in this paper differs from CIR's numbering.

$$F(t,T) - f(t,T) =$$

$$PV_{t,T} \{ e^{\int_t^T r(w)dw} \int_t^T \frac{P(w)}{B(w,T)} \; var\{B(w,T)\} - cov\{P(w),B(w,T)\} \, dw \} \tag{2}$$

$$F(t,T) - f(t,T) = -PV_{t,T} \{ e^{\int_t^T r(w)dw} \int_t^T f(w,T) \; cov\{f(w,T),B(w,T)\} \; dw \} \tag{3}$$

where **F(t,T)** is the futures price at date t of a contract that matures at date T,

f(t,T) is the forward price at date t of a contract that matures at date T,

$PV_{t,T}$(.) is the present value at time t of a payment received at time T,

B(t,T) is the price at date t of a riskless bond paying one dollar at date T,

P(w) is the spot price at time w,

cov {F(w,T),B(w,T)} is the local covariance at time w between the percentage change in the futures price and the percentage change in the bond price,

var {B(w,T)} is the local variance of the percentage change in the bond price,

cov {P(w),B(w,T)} is the local covariance between the percentage change in the spot price and the percentage change in the bond price,

cov $\{$**f(w,T),B(w,T)**$\}$ is the local covariance at time w between the percentage change in the forward price and the percentage change in the bond price and **r(w)** is the instantaneous interest rate at time w.

These three propositions have several testable implications (for each proposition, the weak implication precedes the strong implication):

Proposition (1) implies that if the covariance between the futures and riskless bond prices is always positive, then the forward price exceeds the futures price. In addition, the futures-forward price difference is a decreasing function of the market's expectation of the futures-bond covariance;

Proposition (2) implies that if the difference in the bond price variance and the covariance between the spot price and the bond price is always negative, then the futures price is less than the forward price. Also, if the correlation between spot and bond prices is positive, then the futures-forward price difference is a decreasing function of the market's expectation of the spot price variance; and

Proposition (3) implies that if the forward-bond covariance is positive, the futures price is less than the forward price, and the futures-forward price difference is a decreasing function of the expected forward-bond covariance.[3]

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[3] The PV operator in propositions (1) - (3) takes the present value of random variables, and must therefore account for uncertainty. An exact test of the CIR propositions requires an intertemporal valuation model. The above testable implications circumvent introducing a joint test by predicting the sign of the average futures-forward difference (the weak tests) or by predicting whether the futures-forward difference increases or decreases with covariances and variances (the strong tests). These tests are

3. Eurodollar Futures and Implied Forwards Data

A Eurodollar deposit is a dollar denominated deposit in a bank outside the U.S.[4] The London Interbank Offer Rate (LIBOR) is the rate at which London banks offer Eurodollars to their most creditworthy customers (other large banks). The Eurodollar futures trades in a quarterly contract cycle with cash settlement based upon the LIBOR rate at expiration.[5]

The lack of exchange-traded Eurodollar forwards complicates the futures-forward comparison. The tests compare Eurodollar futures to the forward prices implied by the cash LIBOR rates.[6] For example, the following equation derives an implied forward for comparison with a

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also more appropriate than an exact test because they allow taxes, transactions costs, default risk, etc. to contribute to the futures-forward price differential.

[4] Further institutional details are available in the CME's Inside Eurodollar Futures, Kolb's Understanding Futures Markets, and Stigum's The Money Market.

[5] Goldman Sachs provided daily data for Eurodollar futures prices for the first three nearby contracts, the 1, 2, 3, 6, and 12 month LIBOR rates, and the 1, 2, 3, 4, 5, 6, 9 and 12 month T-bill rates from March 1982 - June 1987 (similar data are also publicly available). The futures prices are the daily settlement prices that determine the variation margin posted by futures holders as positions are marked to market. The LIBOR rates are the latest available from Telerate at the close of the futures market.

[6] Banks commonly create forward positions from cash rates in the Eurodollar market. For examples comparing T-bill futures with implied forward prices, see Capozza and Cornell (1979), Rendleman and Carabini (1979), Elton, Gruber, and Rentzler (1984), Kawaller and Koch (1984), Gendreau (1985), Kolb and Gay (1985), and Kamara (1988).

nearby Eurofuture contract with exactly three months remaining until expiration:

$$(1 + \frac{183}{360} r_6) = (1 + \frac{91}{360} r_3)(1 + \frac{92}{360} r_{for}) \qquad (4)$$

where r_6 is the six-month LIBOR rate (matures in 183 days)
r_3 is the three-month LIBOR rate (matures in 91 days)
r_{for} is the forward rate (the current rate for a 92 day maturity deposit beginning three months from today).[7]

Thus, to create a long forward position, an investor buys (lends) the six-month LIBOR and sells (borrows) the three-month LIBOR.

Although the Eurodollar placements market is a flexible-date market in which participants can trade any length maturity, historical data for LIBOR rates of odd maturities are not readily available. Indeed, only the actively traded 1, 2, 3, 6, and 12 month rates are publicly available. Using only these LIBOR rates limits the ability to construct an implied forward rate to one implied forward rate every three months (i.e., the three-month, three months forward rate) for a total of 22 data points.

An alternative approach is to construct a LIBOR yield curve on a

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[7] The maturity length of the interest rates in the above equation change with time to expiration.

daily basis. The available LIBOR data is used to fit a yield curve (quadratic in time to maturity) for maturities of 1 to 360 days.[8] The fitted values of this regression then determine the daily implied forward rates. Summary statistics for the 1232 OLS regressions are below:

$$\text{Yield}_i = \alpha + i \cdot \beta_1 + i^2 \cdot \beta_2 + \epsilon_i \qquad \text{where } i = \text{days to maturity } (1,\ldots,360)$$

	Mean	Std Error
α	9.0697	0.0624
(t-stat)	(681.1370)	(19.5789)
β_1	0.0030	0.0001
(t-stat)	(7.2122)	(0.4758)
β_2 (x 10^5)	-0.2565	0.0236
(t-stat)	(-1.2910)	(0.2189)
Standard Error of Estimate	0.0498	0.0022
Adjusted R^2	0.8759	0.0076
R^2	0.9739	0.0038

1 Daily LIBOR Yield Curve Regressions

[8] Using a cubic failed to reduce the regression average standard error significantly. Rendelman and Carabini (1979) use a linear function to approximate the yield curve, but do not provide any information on the fit of this model.

The measurement unit for the above regressions is percentage points, so that the average standard error is approximately 5 basis points.[9]

This approach implicitly assumes that the errors have zero mean; if customers pay a higher rate for Eurodollar deposits with odd length maturities, the fitted values are downwardly biased estimates of the true values. To check for this bias, I use less commonly quoted 4 and 5 month rates available for a limited period of time during the sample period (10/86-7/87) from DRI.[10] I compute the daily yield curve as above (using only the 1, 2, 3, and 6 month maturities) and compare the actual four (or five) month rates with the fitted four (or five) month rates. If a liquidity preference for the common 1, 2, 3, and 6 month maturities exists, then the mean difference should be positive. The standard errors are adjusted for serial correlation induced by overlapping observations (see Hansen and Hodrick (1980)). The test results follow.

The t-statistics are insignificant for both one-tailed and two-tailed tests. Although the DRI data covers only a nine month period, rather than the complete sample period, this period does not seem to possess any unique characteristics which would limit drawing general inferences based

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[9] The daily yield curve regressions potentially could add considerable noise to the data, reducing the power of the tests. In spite of the potential reduction in power, most of the following tests reject the null hypothesis, further strengthening the evidence supporting the CIR model.

[10] DRI began collecting the four and five month rates in October, 1986.

upon it. While this test may convey little direct information about maturities of even less common length (e.g. 43 days), it does suggest that if a bias exists, it is quite small (less than one basis point as measured by the sample means above).

Actual Rate - Fitted Rate	$r_4 - \hat{r}_4$	$r_5 - \hat{r}_5$
Sample Mean	0.0063	0.0060
Standard Error	0.0063	0.0050
T-stat for Mean=0	1.0002	1.2096
N	183	183

2 Tests For Liquidity Preference Bias

4. A preliminary look at the data

Table 1 summarizes the daily percentage changes in the forward and futures rates, measured as the log of the price relatives, log[r(t)/r(t-1)] (where r represents the appropriate yield). The nearby contract has three or fewer months remaining until expiration, the second nearby has three to six months until expiration, etc..

In general, the implied forwards yields are somewhat more volatile than the futures yields. Also, the third nearby forwards are more volatile than the second nearby, which are more volatile than the first nearby, a pattern not repeated in the futures. The measurement errors of forward yields introduced by the yield curve regressions may increase the estimated yield volatilities, and may do so the further the expiration date. (The

LIBOR yield curve regressions use 1, 2, 3, 6, and 12 months rates; 9 month rates are unavailable. The second and third nearby implied forward rates rely upon the 6 to 12 month maturity portion of the LIBOR yield curve, and hence the volatilities of these rates should reflect the relative imprecision of the LIBOR yield curve estimation over these maturity lengths.) The yearly volatility patterns do not suggest that any specific segmentation of the data is necessary. Further analysis includes the entire period. Finally, the kurtosis estimates reflect the absence of extreme outliers in the data.

Table 2 shows the autocorrelations for the daily Eurodollar futures and forwards yield changes. The first-order autocorrelations for the forward rates (all contracts) are negative and significantly different from zero; the first-order autocorrelations for the futures contract are not significant at the 5% level. Again, measurement errors could create a negative first-order autocorrelation. If the measurement error in today's forward rate is positive, then today's yield change will be upwardly biased, while tomorrow's change will be negatively biased.

5. Testing the weak predictions of the CIR model

The weak prediction of **proposition (1)** of the CIR model is that if the futures-riskless bond covariance is positive, then the futures price should be less than the forward price. To ascertain the sign of this covariance, I estimate the sample covariances and cross-correlations of the daily percentage change in the futures price and the t-bill price over the March 1982-June 1987 sample period.[11] The standard errors of the sample cross-correlations are adjusted for the second-degree autocorrelation observed in the data. Table 3 indicates that the cross-correlations, and therefore the covariances, are positive and significantly different from zero for all three contracts, so the CIR model implies that the futures price should be less than the forward price.

To determine whether the futures-forward price difference is negative, the most straightforward approach is to examine the sample mean of the daily percentage difference in prices, measured as the log of the futures-forward price ratio. Table 4 displays the sample means of the daily price differences computed over the March 1982-June 1987 period. The standard error and t-statistics for the cross-correlation coefficient are adjusted as suggested by Hansen-Hodrick (1980) for serial correlation induced by overlapping observations.

These preliminary results are consistent with the weak predictions of the CIR model: the mean differences for the second and third nearby contracts are negative and significantly different from zero; the first nearby has the predicted negative sign, but fails to differ significantly from zero.

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[11] The exchange's formulas were used to convert the quoted interest rates into prices.

Measurement errors may prevent the relatively small value of the first nearby futures-forward difference from differing significantly from zero. Notice that the CIR model also implies that the absolute value of futures-forward price difference increases with the time to expiration.[12] The mean difference between futures and forward prices is .05% for the first nearby contract, .29% for the second nearby, and .58% for the third nearby, which translates to basis point differences of roughly 4, 26, and 51, consistent with the additional prediction of the CIR model.[13]

The next tests of the weak predictions of the CIR model use monthly price differences and covariances instead of the daily difference averaged over the entire sample period. If the covariances change over the March 1982-June 1987 period, this test may be more appropriate than the previous

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[12] Without an intertemporal valuation model, the CIR propositions do not predict the size of this increase.

[13] The instability of the mean futures/forward price difference predicted by the CIR clouds the interpretation of the previous test. I conduct Wald tests for stability of the mean by dividing the sample into 3 parts, based upon time to expiration (the tests employ variance/covariance matrices adjusted for serial correlation induced by overlapping observations). The test results indicate that the mean of the portion of the sample with the greatest time to expiration differs significantly from the means of the sample with the next greatest and the least time to expiration. To correct for this instability, I divide each futures/forward price difference by time remaining to expiration. Wald tests on the transformed price difference fail to reject the null of equality of the means, so I repeat the tests performed in Table 4 for the transformed variable. The results are almost identical to those in Table 4 and are hence not reported, but are available on request.

test. I sample the forward-futures price difference on the third Monday of every month (five days prior to this date if the month is an expiration month, as the futures and forward prices must be equal on the expiration day)[14], and estimate the sample covariances and variances monthly.[15] Thus, approximately 21 (the average number of trading days in a month) observations contribute to the estimate of each sample covariance.

Table 5 displays the sample means, variances, and covariances for the monthly observations.[16] The standard errors and t-statistics of the monthly covariances and variances are corrected for the third-order serial correlation detected in the sample. Knowledge of how the futures-forward

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[14] Tests (both the tests using means and the variance/covariance regressions) omitting the days close to contract maturity produce results very similar to tests that included these days.

[15] The riskless bond price used in calculating the covariances should mature as the contract expires. The first nearby covariance estimates use the 2 month T-bill price, the second nearby covariance estimates use the 4 month T-bill price, and the third nearby covariance estimates use the 6 month T-bill price.

[16] I conduct Wald tests for stability (with respect to time to expiration) of the monthly futures-forward price difference and the monthly covariances and variances (adjusting the asymptotic covariance matrix for overlapping observations). The covariances and the variances fail to reject the null hypothesis of equal means for each subsample; the futures-forward price differences fail to reject the null of equal means for the first and second nearby contracts, and reject the null for the third nearby contract. After adjusting the futures-forward price difference by dividing by days remaining to expiration, Wald tests for the price differences fail to reject the null of equal means for all contracts. Again, tests using the adjusted price difference produce results similar to those in Table 5, and are therefore not reported.

price differences overlap allows a more structured approach to adjusting the standard errors and t-statistics of the differences. Specifically, we know that the first observation is correlated with the second and third observations, but not the fourth or higher observations. For example, the first observation is the March futures-forward price difference; for the nearby contract, this price difference refers to the time period three months forward, that is, June through August. The second (April) and third (May) observations also measure the price difference over this same June through August period, and are therefore correlated with the first observation. The fourth observation, however, switches to the September through November period, as do the fifth and sixth observations. Thus, the March, April, and May observations are correlated with each other, but not with the observations for June or later.

This overlapping structure is similar, but not identical, to the structure outlined in Hansen-Hodrick (1980). Instead of a band-diagonal covariance matrix, the futures-forward price difference covariance matrix is block diagonal, with block size of three (monthly observations, contract expiration every 4 months). The appropriate modification of the asymptotic covariance matrix constrains the off-block elements to be zero, and estimates the block using Hansen-Hodrick's procedure. Not all of the OLS residuals, however, contribute to this estimation. For instance, to estimate the covariance component for first-order correlation, one includes all $\hat{u}_t\hat{u}_{t+1}$

(where the $\hat{u}$ represent the OLS residuals) except the residuals for those observations which one knows are uncorrelated. In the above example, $\hat{u}_1\hat{u}_2$ and $\hat{u}_2\hat{u}_3$ are included, but $\hat{u}_3\hat{u}_4$ is not. Mathematically,

$$\hat{\sigma}_{12} = \frac{1}{N}\sum_{t=1}^{N} \hat{u}_t \hat{u}_{t+1} \qquad t = 1,2,4,5,7,8,10\ldots \tag{5}$$

The evidence shown in Table 5 supports two of three of the CIR propositions (in weak form). If the covariance between the futures price changes and the bond price changes is positive, then **proposition (1)** implies that the futures-forward price difference should be negative. This covariance is positive and significant for all three contracts; the price difference is negative for all three contracts and significantly different from zero for the second and third nearby contracts. The forwards-riskless bond covariance results are similar to the futures-riskless bond covariance results, and are consistent with **proposition (3)**. (Note that since the CIR propositions predict the signs of the variables, one-sided tests are appropriate.) The evidence supporting **proposition (2)**, however, is mixed. **Proposition (2)** suggests that if the difference between the riskless bond variance and the covariance of the spot and bond prices is always negative, then the futures-forward difference should also be negative. While the mean of the variance-covariance difference is significantly different from zero in the predicted direction for the third nearby contract, it is significantly different from zero in the other direction for the second nearby contract.[17] Finally, note that the absolute value of the futures-forward difference increases with time to maturity, as the CIR model predicts.[18]

If the covariances and variance-covariance differences are not constant over time, under the CIR propositions the futures-forward price difference should vary in response to the covariance and variance changes. A rough way to measure the response of the futures-forward price differences is to sort the differences by the sign of the forward-bond covariance and by the sign of the bond variance and the spot-bond covariance difference.[19] Under the second and third CIR propositions, the mean futures-forward price difference should be negative for the subsample of observations for which the forward-bond covariance is positive and for the subsample for which the variance-covariance difference is negative. As

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[17] The mean of the variance-covariance difference is also positive and significantly different from zero for the first nearby contract, but since the mean futures-forward price difference is not significantly different for the first nearby contract, one should not interpret this result as failure to support CIR proposition (2).

[18] Park and Chen (1985) test and find support for the weaker implication of the CIR model that predicts the sign of the average futures-forward price difference. They conduct these tests using futures and forward prices for six physical commodities and four foreign currencies. French's (1983) tests using copper and silver data also support the weaker CIR predictions.

[19] Although one would also like to sort by the sign of the futures-bond covariance, the small number of negative futures-bond covariance observations prevents such a segmentation from providing meaningful information.

Table 6 reflects, this segmentation of the data does not provide much further support for the CIR propositions. The futures-forward price difference is significant in the predicted direction for four out of five contracts when the forward-bond covariance is positive and when the variance-covariance difference is negative, but is significant in the other direction in four out of the ten remaining tests.[20]

One reason for the general lack of support displayed in Table 6 is that the CIR propositions relate the futures-forward price difference observed today to covariances or variances expected over the remainder of the contract. The next tests explicitly account for the evolution of covariance and variance expectations. Also, since the CIR propositions imply that the futures-forward price difference should vary as the covariances and variances change, the next tests, which regress these differences against the covariances and variances, might provide a sharper test of the CIR model.[21]

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[20] One problem with the segmentation tests is that the negative covariance and variance-covariance difference observations are interspersed with the positive observations. Therefore, some of the corresponding futures-forward difference observations will overlap, and some will not, leaving one unable to adjust the standard errors for serial correlation since one can no longer use Hansen and Hodrick's method to estimate the asymptotic covariance matrix. It is not obvious how to interpret the reported OLS standard errors.

[21] Another advantage of the tests of the strong predictions is that while different tax treatments, transactions costs, market structure, default risk, and restrictions to international capital mobility can contribute to Eurodollar futures-forward price

6. Testing the Strong Predictions of the CIR model

The CIR propositions indicate that the futures-forward price difference is a decreasing function of the market's expectation of the futures-riskless bond covariance, the forward-riskless bond covariance, and the spot price variance over the remaining life of the contract. The expected covariances and variances are not directly observable; I try three alternate methods of estimating the expectations. The first method (termed the "no change method") uses the last month's realized covariance as an estimate of the covariance over the remaining contract life. The second method (the "rational expectations method") uses the realized covariances and variances as proxies for the expected covariances and variances and requires that the forecasting errors are orthogonal to the sample covariances and variances. The final method (the "ARIMA method") fits an ARIMA model to each monthly series of sample covariances or variance, and forecasts the market's expectation using this time series model.[22]

To test the strong predictions of the CIR model, I regress the futures-forward price difference against the various series of expected covariances

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divergence, these factors do not affect the validity or the interpretation of the regression tests unless the factors vary systematically with the covariances and variances.

[22] French (1983) tests the strong predictions of the CIR model using the futures price variance, the forward price variance, the riskless bond price variance, and the spot price variance. Using the variances rather than the futures-bond covariance and the forward-bond price variance imposes the additional assumption that the local correlation between the futures or forward prices and the bond remains constant.

and variances. The dependent variable in each OLS regression is the futures-forward price difference. The explanatory variable is the covariance of the daily percent changes in the futures and the riskless bond prices, the variance of the daily percent change in the LIBOR price, or the covariance of the daily percent changes in the forward and the riskless bond prices.[23] While the CIR propositions include the integral of the local covariance from time t (the measurement date of the futures-forward price difference) to maturity, I estimate the series of sample covariances from daily price changes, so the sample covariances are daily covariances.[24] To approximate the increase in the right-hand side of the proposition with time to maturity, I convert each covariance from a daily measure by multiplying by the number of trading days remaining until contract expiration. Thus, a covariance calculated with two months remaining until maturity is multiplied by 42, whereas a covariance estimated with one

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[23] Some would argue that the measurement error in the series of expected covariances or variances is greater than the measurement error in the futures-forward price difference, and hence the price difference should be the explanatory rather than the dependent variable to minimize the error-in-variables problem. French (1983) uses this methodology. I have repeated each test in Tables 7, 8, and 11 running the reverse regression with the price difference as the explanatory variable. The results, available on request, reveal that the t-statistics in each case are almost identical to the t-statistics reported in Tables 7,8, and 11; no results switch from significant to insignificant, or vice-versa.

[24] The CIR propositions include the local covariance; using a sample covariance assumes that the local covariance does not change much over the estimation period.

month left until maturity is multiplied by 21.[25] Finally, I adjust the OLS standard errors for the serial correlation induced by overlapping futures-forward price difference observations, using the modified Hansen-Hodrick approach described earlier.

Table 7 displays the regression results for the no change method of estimating the expected covariance, which suggest that the CIR model is useful in explaining the individual price difference observations. Under the no change method, the market participants form expectations about the expected covariances based upon the past month's sample covariance. The CIR propositions predict that the slope coefficients of the regressions are negative; the evidence displayed in Table 7 strongly supports these predictions. Under the no change method, eight out of nine coefficients are significantly less than zero using a one-tailed t-test and a 5% level of significance. The remaining coefficient (the third nearby forward-bond covariance) is negative, but not significantly less than zero. Recall from Section 4 that the third nearby forward rates are the most volatile, possibly due to measurement errors.

Note also that R^2 increases with time to maturity (first nearby R^2 < second nearby R^2 < third nearby R^2) for each covariance or variance,

[25] This method is not strictly correct unless the price diffusion process has the property that the variance over a period of 2t days is twice the variance over a t-day period. Although the log-normal diffusion process has this property, CIR do not restrict the diffusion process in their propositions.

except for the only regression with an insignificant slope coefficient. This observation is consistent with the CIR model. The propositions imply that the absolute value of the futures-forward price difference increases with time to expiration. If the other factors that create a disparity between futures and forward prices (taxes, market structure, etc.) do not change with time to maturity, marking-to-market should generate a greater proportion of the disparity, leading to a higher R^2, as time to maturity increases.[26]

The evidence supporting the CIR propositions decreases under the rational expectations method of estimating the expected covariances. As Table 8 indicates, although all coefficients have the predicted negative sign, only four out of nine coefficients are significantly less than zero at the 5% level. With the exception of the third nearby forward-bond covariance, however, the regression R^2's do increase with time to maturity.[27]

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[26] One reason for the low R^2's in Tables 7, 8, and 11 is that propositions (1)-(3) are non-linear relations, while the regression tests are linear.

[27] All of the regressions suffer from measurement error problems. The expected covariances are measured with error, as are the implied forward prices. One could reasonably argue that $\hat{\beta}$ overestimates β (if β is negative), and hence the t-statistics are biased towards zero. If the covariance or variance (denoted x) is measured with error u, and the futures-forward price difference (y) is measured with error v, then plim $\hat{\beta} = \beta/\{1 + (\sigma_u^2/\sigma_x^2)\}$ if cov(u,x) = cov(u,y) = cov(v,x) = cov(v,y) = 0. Since the measurement error in the futures-bond variance is probably uncorrelated with the forward price measurement error, such a conclusion seems justified for the case of the futures-bond variance. The measurement error is also uncorrelated with the forward-bond covariance and the spot price variance if one assumes that the forward price

The sensitivity of the results to the method of estimating market expectations may be due to imperfect approximation of the true process by which market participants form expectations. The rational expectations approach makes the strong assumption that market participants have perfect knowledge about the future. If, however, they are not very good at predicting the future, forecast errors will tend to be large and the large forecast errors may prevent the coefficients in Table 8 from attaining significance. Unlike the rational expectations approach, the no change method does not assume the market has knowledge of the future, and may therefore be the more plausible candidate for the underlying true expectations process.

The final method of estimating the expected covariances steers a middle course between the no change method and the rational expectations method. For the ARIMA method, I use Box-Jenkins techniques to fit ARIMA models to each monthly variance and covariance series. Although ideally one would like to employ past data to fit a model, and then use the model parameters to forecast future values, the small number of observa-

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measurement error results from the inadequacy of a quadratic to describe the LIBOR yield curve. More precisely, if the quadratic does not fail in a systematic manner, then today's measurement error should be uncorrelated with past or future errors. Therefore, today's forward price error should be uncorrelated with the spot price variance error and with the forward-bond covariance error (since estimation of these expectations employs past or future observations), and the t-statistics will be biased towards zero.

tions prevents using such a method.[28] Instead, I estimate the ARIMA model using all 63 monthly observations, and use the estimated parameters to forecast. Thus, the ARIMA method assumes that the time series model that appropriately described a series in past continues to describe the series in the future.

Table 9 displays the relevant autocorrelations and partial autocorrelations for the monthly covariance and variance series, and Table 10 describes the ARIMA models eventually selected, which are AR1 or AR2 models. As evident from the autocorrelations and partial correlations in Table 9, the third nearby forward-bond covariance appears to be a white noise series, and in fact, no time series model seems to adequately fit it. I then use the estimated model to forecast the expected covariance or variance. For example, if there are three months remaining until expiration, I take an average of the one-step ahead, two-step ahead, and three-step ahead forecasts to arrive at the expected covariance for the next three months. Similarly, with two months remaining until expiration, the expected covariance is the average of the one-step ahead and the two-step ahead

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[28] Using past data to fit an ARIMA model is also problematic because of the asymptotic justification for the consistency of the modified covariance matrix. Modifying Hansen and Hodrick's approach imposes additional structure on the asymptotic covariance matrix, slightly reducing the number of OLS residuals used for estimation. By dividing the sample in two parts, approximately 31 observations are available to estimate the ARIMA model, and 31 observations are available to test the CIR propositions, probably too few observations to justify asymptotic methods.

forecasts, and so on.

The ARIMA results support the CIR propositions in general, although not as quite as strongly as the no change results. All nine coefficients shown in Table 11 have the predicted sign, and six of the nine are significantly less than zero at the 5% level. One might want to exclude the white-noise third nearby forward-bond covariance series from consideration, in which case six of eight coefficients are significantly less than zero at the 5% level.

7. Conclusions

Unlike prior empirical studies, the above test results support both the weak and strong predictions of the CIR model.[29] French (1983) finds support only for the weak predictions using copper and silver futures and

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[29] Rendleman and Carabini (1979) document futures-forward price differences in the T-bill market, but their derivation of the theoretical futures price treats the futures price as a forward contract. They do not directly test the CIR model, but on page 897 (footnote 3), they argue that ignoring the CIR effect will not bias their results. They use the Cox-Ingersoll-Ross term structure model and input the instantaneous interest rate (which they estimate using the Federal Funds rate), the natural rate, the variance of percentage changes in the interest rate, the mean reverting diffusion process speed of adjustment coefficient, the covariance of changes in interest rates with percentage changes in optimally invested wealth, and the time to maturity of the bond. They compute futures prices for "input parameters that give rise to 'reasonable' unit discount bond prices," but since they fit, rather than estimate the parameters, use the Federal Funds rate to estimate the instantaneous interest rate, and do not reveal the maturity lengths of the bond prices they fit, it is not clear how to interpret their results.

forward data. He finds that two of twenty coefficients in the regression tests are significantly different from zero, and only one of the two coefficients is significant in the predicted direction. The small value of the covariance between commodity prices and riskless bond prices results in estimates that are very sensitive to measurement errors; in fact, French ascribes the failure of the CIR model to explain variation in the futures-forward price differences to these errors.

Choosing Eurodollar futures to examine the empirical validity of the CIR model eliminates many of French's sources of measurement error. French's forward contracts trade in London, the futures contracts in the U.S.; Eurodollars are dollar-denominated, eliminating the exchange rate and transportation/trade restrictions considerations that French faced. Unlike copper and silver futures, the settlement date for Eurodollar futures is precisely defined. Finally, the CME removed the daily price movement limits for Eurodollar futures; prior to this removal, limit moves rarely occurred.[30] French removes the dates on which limit moves occurred, reducing but not eliminating the effects of this measurement error. Using an interest rate sensitive asset increases the power of the tests; using Eurodollar futures data reduces measurement errors. The new evidence

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[30] The CME imposed daily price movement limits from December 1981-December 1985. During 1984 and 1985, no limit moves occurred. While the CME was unable to provide me with similar data for the 1982 and 1983 period, the 1984-85 experience suggests that the imposition of pricing movements should not seriously affect the data.

presented in this paper provides stronger empirical support for the CIR model.

In addition, the results call for a re-examination of Cornell and Reinganum's conclusion that differential tax treatment and low transactions costs rather than daily resettlement drive the observed price differences in the T-bill market. Comparing futures-forwards price differences in the T-bill market and the foreign exchange market, Cornell and Reinganum observe that the futures-forward price discrepancies in the T-bill market differ significantly from zero, while most futures-forward price discrepancies in the foreign exchange market are insignificantly different from zero. They argue that high transactions costs, differential tax treatment of futures and forwards, and daily resettlement potentially contribute to the futures-forward price differences in the T-bill market, but daily resettlement is the only source for futures-forward price differences in the foreign exchange market. Since daily resettlement does not create significant price differences in the foreign exchange market, its contribution to the price discrepancies in the T-bill market must also be small.

In contrast to Cornell and Reinganum's results, this paper shows that despite low transactions costs relative to the T-bill market and no differential tax treatment of futures and forwards, Eurodollar futures-forwards price differences are still significant.[31] Hence, either previously unexplored market conditions contribute to the T-bill's price differential[32], or the daily resettlement effect modeled by CIR is a more important determinant of the differences than prior research suggested.

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[31] The bid-ask spread on a Eurodollar futures contract is typically one basis point ($25); round trip trading costs are $10 for major players. Capozza and Cornell estimate that the T-bill futures bid-ask spread is $30-$100, and round trip trading costs are $25 or less per contract. Also, the spread between going long and short on the LIBOR rate is about 6 basis points; Capozza and Cornell report that shorting T-bills requires a 50 basis point premium.

[32] Kamara (1988) suggests that different futures and forward market structures contribute to the T-bill differential.

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TABLE 1

Means, Standard Deviations, and T-Statistics of the Daily Percentage Change in Futures and Forward Yields of Eurodollars

Panel A: Futures

Year	Mean	Std Dev	T-stat	Skewness	Kurtosis	N
			Nearby Contract			
1982	-0.196	1.726	-1.560	-0.231	0.593	188
1983	-0.006	1.108	-0.086	0.428	0.914	240
1984	-0.091	1.004	-1.335	-0.080	3.982	218
1985	-0.022	1.255	-1.246	0.027	1.922	200
1986	-0.128	1.062	-1.718	-0.472	1.130	202
1987	0.165	1.010	1.736	0.714	3.278	113
1982-87	-0.060	1.223	-1.676	-0.119	2.120	1161
			Second Nearby			
1982	-0.139	1.586	-1.205	0.151	0.706	188
1983	0.022	1.180	0.287	0.280	0.412	237
1984	-0.020	1.160	-0.153	0.495	2.958	217
1985	0.051	1.486	0.486	0.323	2.296	199
1986	-0.140	1.236	-1.606	-0.027	0.606	202
1987	0.179	1.237	1.520	0.376	1.086	110
1982-87	-0.019	1.322	-0.488	0.232	1.572	1153
			Third Nearby			
1982	-0.119	1.451	-1.118	0.538	1.190	185
1983	0.027	1.068	0.388	0.186	0.364	238
1984	-0.014	1.128	-0.176	0.158	1.650	211
1985	0.037	1.405	0.369	0.078	1.712	196
1986	-0.131	1.256	-1.472	0.187	0.874	198
1987	0.220	1.270	1.770	0.407	1.114	104
1982-87	-0.013	1.261	-0.339	0.252	1.353	1132

Panel B: Forwards TABLE 1 (cont'd)

Year	Mean	Std Dev	T-stat	Skewness	Kurtosis	N
			Nearby Contract			
1982	-0.104	1.651	-0.867	-0.688	1.622	188
1983	0.028	1.395	0.317	0.175	2.362	240
1984	-0.100	1.135	-1.307	-0.707	6.168	218
1985	-0.074	1.368	-0.765	-0.285	2.194	200
1986	-0.023	1.094	-0.298	-0.210	2.173	202
1987	0.004	1.729	0.025	0.174	1.780	113
1982-87	-0.046	1.379	-1.142	-0.256	2.788	1161
			Second Nearby			
1982	0.013	1.733	0.100	-0.197	1.146	188
1983	0.046	1.487	0.482	-0.184	2.334	237
1984	-0.011	1.357	-0.118	0.244	4.736	217
1985	-0.008	1.561	-0.069	0.241	1.100	199
1986	-0.067	1.296	-0.405	-0.416	3.282	202
1987	0.066	1.988	0.350	0.329	1.415	110
1982-87	0.008	1.540	0.175	0.032	2.257	1153
			Third Nearby			
1982	-0.060	2.094	-0.388	-0.270	0.659	185
1983	0.067	1.654	0.623	0.125	0.974	238
1984	0.005	1.395	0.051	-0.163	2.574	211
1985	0.097	1.598	0.851	0.161	0.806	196
1986	-0.105	1.404	-1.0547	-0.136	2.602	198
1987	0.196	1.990	1.006	0.088	1.208	104
1982-87	0.022	1.673	0.435	-0.045	1.520	1132

Percentage change in yields defined as $\log(r_t/r_{t-1})*100$. The statistics only include dates when observations for both futures and forwards exist. When an observation is missing, the appropriate root of the ratio of the endpoints of the missing string is used.

TABLE 2

Autocorrelations of the Daily Percent Changes in Eurodollar Futures and Fowards Rates

	N	$S(r_1)$[a]	r_1[b]	r_2	r_3	r_4	r_5	r_6
				Nearby				
Futures	1274	0.028	0.051	0.047	0.007	-0.032	-0.013	0.046
Forwards	1212	0.029	-0.065	-0.067	-0.032	-0.002	-0.020	-0.036
				Second Nearby				
Futures	1276	0.028	0.044	-0.001	0.035	-0.025	-0.052	0.033
Forwards	1202	0.029	-0.105	-0.045	-0.030	0.011	-0.009	0.020
				Third Nearby				
Futures	1276	0.028	0.054	-0.004	0.026	-0.040	-0.037	0.042
Forward	1177	0.029	-0.134	-0.013	-0.027	-0.020	-0.006	-0.034

[a] $S(r_1)$ is the standard error of the first order autocorrelation

[b] r_k is the autocorrelation at lag k

TABLE 3

The Sample Cross-Correlation Coefficient of Futures and Riskless Bond Series

	Nearby Futures, 3-month t-bill	Second Nearby, 6-month t-bill	Third Nearby, 9-month t-bill
Covariance	0.0086	0.0095	0.0083
Cross-Correlation Coefficient [r_{fb}]	0.5021	0.5788	0.4908
Standard Error of r_{fb}	0.0284	0.0288	0.0287
T-Stat for $r_{fb} = 0$	17.6627	20.1742	17.1119
N	1225	1213	1197

r_{fb} is the sample cross-correlation coefficient between the daily percentage change in the futures price and the daily percentage change in the bond price. The variance of the cross-correlation coefficient is adjusted for second-order serial correlation and calculated as:

$$var[r_{fb}] = \frac{1}{N}\ [1 + 2\{(r_{ff}(1) \cdot r_{bb}(1)) + (r_{ff}(2) \cdot r_{bb}(2))\}\ ]$$

where $r_{ff}(i)$ and $r_{bb}(i)$ are the ith order autocorrelations of the daily percentage change in the futures price and the daily percentage change in the bond price, respectively. See Fuller (1976).

TABLE 4

Futures-Forwards Price Differences in Percent and Basis Points

	Percent Difference	Basis Points Difference
	First Nearby	
Mean	-0.051	-4.545
DW Stat	0.516	0.520
Q(102)	5165.240	5115.270
Sign Level Q(102)	0.000	0.000
Std Error of Mean	0.039	3.420
T-Stat	-1.314	-1.329
N	1176	1176
	Second Nearby	
Mean	-0.293	-26.128
DW Stat	0.422	0.429
Q(102)	9111.480	9017.130
Sign Level Q(102)	0.000	0.000
Std Error of Mean	0.050	4.416
T-Stat	-5.810	-5.916
N	1174	1174
	Third Nearby	
Mean	-0.578	-51.297
DW Stat	0.237	0.248
Q(102)	28442.500	27977.400
Sign Level Q(102)	0.000	0.000
Std Error of Mean	0.094	8.110
T-Stat	-6.172	-6.325
N	1156	1156

Q(102) is the Ljung-Box Q-statistic for 102 lags of autocorrelation, which tests for higher order serial correlation. The standard errors and t-statistics are corrected for serial correlation induced by overlapping observations.

TABLE 5

Monthly Futures-Forward Price Differences and Covariances of Futures and Forward Price Changes with Riskless Bond Price Changes:

Tests of the Weak CIR Predictions

	Price Diff. log(F/f)	cov(F,B)	var(B)- cov(P,B)	cov(f,B)
		First Nearby		
Mean	-0.022	0.008	0.022	0.003
DW Stat	1.262	0.749	1.690	0.984
Q(4)	11.458	68.328	7.375	27.495
Sign Level Q(4)	0.022	0.000	0.117	0.000
Std Error of Mean	0.037	0.004	0.004	0.002
T-Stat for Mean=0	-0.579	2.297	5.114	1.950
N	63	63	63	63
		Second Nearby		
Mean	-0.237	0.009	0.148	0.004
DW Stat	1.409	0.576	1.520	0.737
Q(4)	14.115	89.285	14.366	30.184
Sign Level Q(4)	0.007	0.000	0.006	0.000
Std Error of Mean	0.038	0.003	0.063	0.002
T-Stat for Mean=0	-6.176	3.226	2.346	2.354
N	63	63	63	63

	Price Diff. log(F/f)	cov(F,B)	var(B)-cov(P,B)	cov(f,B)
		Third Nearby		
Mean	-0.478	0.008	-0.012	0.002
DW Stat	0.632	0.530	1.648	1.728
Q(4)	77.235	78.913	2.481	1.742
Sign Level Q(4)	0.000	0.000	0.648	0.783
Std Error of Mean	0.078	0.002	0.003	0.000
T-Stat for Mean=0	-6.135	4.074	-3.827	3.146
N	63	63	63	63

log(F/f) is the percent difference in the futures and forward prices.
cov(F,B) is the sample covariance of the daily percentage change in the futures and t-bill prices over the month prior to the matching log(F/f) observation.
var(B)-cov(P,B) is the sample variance of the daily percentage change in the t-bill prices minus the sample covariance of the daily percentage change in the LIBOR and t-bill prices over the month prior to the matching log(F/f) observation. First and second nearby means and std errors are x 10^2.
cov(f,B) is the sample covariance of the daily percentage change in the forward and t-bill prices over the month prior to the matching log(F/f) observation.
Q(4) is the Ljung-Box Q-statistic for 4 lags of autocorrelation.

All t-statistics and standard errors are adjusted for serial correlation.

TABLE 6

Monthly Futures-Forward Price Differences
Sorted by Covariances and Variance-Covariance Differences

	cov (f,B)<0	cov (f,B)>0	Diff	var(B)> cov(P,B)	var(B)< cov(P,B)	Diff
			First Nearby			
Mean	0.021	-0.036	0.059			
Std Error	0.052	0.033	0.062			
T-stat	0.396	-1.164	0.957			
N	18	45	63			
			Second Nearby			
Mean	-0.177	-0.253	0.076	-0.215	-0.301	0.086
Std Error	0.071	0.036	0.080	0.039	0.056	0.074
T-stat	-2.481	-6.959	0.950	-5.559	-5.372	1.161
N	13	50	63	47	16	63
			Third Nearby			
Mean	-0.555	-0.433	-0.122	-0.525	-0.457	-0.068
Std Error	0.086	0.065	0.107	0.124	0.052	0.114
T-stat	-6.484	-6.675	-1.133	-4.251	-8.707	-0.597
N	23	40	63	19	44	63

All means and standard errors multiplied by 10^2.

TABLE 7

OLS Regressions of Futures-Forward Price Differences against Expected Covariances and Variances:

No Change Method

$$\log(F/f)_t = \alpha + \beta \cdot cov(\cdot)_t + \epsilon_t$$

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		First Nearby	
$\hat{\alpha}$	0.003	0.006	0.001
t-stat	(0.086)	(0.164)	(0.042)
$\hat{\beta}$	-0.119	-0.070	-0.290
t-stat	(-1.695)	(-1.753)	(-1.943)
R^2	0.053	0.052	0.066
N	63	63	63
		Second Nearby	
$\hat{\alpha}$	-0.172	-0.153	-0.203
t-stat	(-3.965)	(-3.224)	(-5.334)
$\hat{\beta}$	-0.078	-0.054	-0.098
t-stat	(-2.296)	(-2.538)	(-2.180)
R^2	0.087	0.102	0.078
N	63	63	63

TABLE 7 (cont'd)

No Change Method

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		Third Nearby	
$\hat{\alpha}$	-0.239	-0.291	-0.468
t-stat	(-2.964)	(-3.529)	(-6.596)
$\hat{\beta}$	-0.187	-0.054	-0.037
t-stat	(-4.172)	(-3.312)	(-0.462)
R^2	0.290	0.191	0.003
N	63	63	63

All standard deviations and t-statistics are adjusted for serial correlation using a variant of Hansen/Hodrick (1980). The no change method of calculating the expected covariances and variances assumes that the covariance or variance will remain unchanged from the past month's value for the remaining contract life.

var(P) is the variance of the daily percent change in the spot price variance (based upon the LIBOR rate).

TABLE 8

OLS Regressions of Futures-Forward Price Differences against Expected Covariances and Variances:

Rational Expectations Method

$$\log(F/f)_t = \alpha + \beta \cdot cov(\cdot)_t + \epsilon_t$$

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		First Nearby	
$\hat{\alpha}$	-0.043	-0.043	-0.044
t-stat	(-1.011)	(-0.985)	(-1.086)
$\hat{\beta}$	-0.044	-0.021	-0.009
t-stat	(-0.863)	(-0.762)	(-1.050)
R^2	0.016	0.012	0.023
N	63	63	63
		Second Nearby	
$\hat{\alpha}$	-0.156	-0.218	-0.214
t-stat	(-2.935)	(-3.859)	(-5.097)
$\hat{\beta}$	-0.099	-0.015	-0.161
t-stat	(-2.952)	(-1.203)	(-3.160)
R^2	0.151	0.030	0.156
N	63	63	63

TABLE 8 (cont'd)

Rational Expectations Method

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		Third Nearby	
$\hat{\alpha}$	-0.146	-0.278	-0.437
t-stat	(-1.271)	(-2.832)	(-4.607)
$\hat{\beta}$	-0.220	-0.034	-0.115
t-stat	(-3.777)	(-3.157)	(-1.061)
R^2	0.276	0.200	0.025
N	63	63	63

All standard deviations and t-statistics are adjusted for serial correlation using a variant of Hansen/Hodrick (1980). The rational expectations method of calculating the expected covariances and variances assumes that the expected covariances or variances are the actual covariances and variances that occur over the remaining contract life.

TABLE 9

Sample Autocorrelations and Partial Correlations for Monthly Variances and Covariances

	Lag 1	2	3	4	5	6	7	8	9	10	11	12
					First Nearby							
cov(F,B): r_k	0.60	0.59	0.38	0.41	0.12	0.14	0.06	0.09	0.01	0.04	0.03	0.02
std err [r_k]	(0.21)	(0.24)	(0.26)	(0.27)	(0.28)	(0.28)	(0.28)	(0.28)	(0.28)	(0.28)	(0.28)	(0.28)
$\hat{\phi}_{kk}$	0.60	0.36	-0.11	0.14	-0.32	0.01	0.13	0.00	0.04	-0.05	0.00	-0.01
var(P): r_k	0.59	0.53	0.39	0.28	0.22	0.16	0.11	0.19	0.16	0.15	0.14	0.12
std err [r_k]	(0.21)	(0.24)	(0.25)	(0.26)	(0.27)	(0.27)	(0.27)	(0.27)	(0.28)	(0.28)	(0.28)	(0.28)
$\hat{\phi}_{kk}$	0.59	0.28	0.00	-0.06	0.01	0.01	-0.02	0.17	0.02	-0.03	0.00	0.01
cov(f,B): r_k	0.47	0.23	0.30	0.21	0.10	0.09	0.08	0.11	0.11	0.02	0.02	0.06
std err [r_k]	(0.21)	(0.23)	(0.23)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)
$\hat{\phi}_{kk}$	0.47	0.01	0.24	-0.03	-0.01	0.00	0.01	0.08	0.02	-0.07	0.00	0.03
					Second Nearby							
cov(F,B): r_k	0.69	0.68	0.48	0.41	0.17	0.16	0.04	0.04	0.00	0.02	0.03	-0.01
std err [r_k]	(0.21)	(0.25)	(0.27)	(0.29)	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)
$\hat{\phi}_{kk}$	0.69	0.39	-0.20	-0.03	-0.28	0.09	0.08	0.03	0.05	-0.07	0.07	-0.16
var(P): r_k	0.40	0.27	0.18	0.22	0.10	0.00	-0.04	0.00	-0.04	0.03	0.05	0.06
std err [r_k]	(0.21)	(0.22)	(0.23)	(0.23)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)
$\hat{\phi}_{kk}$	0.40	0.13	0.04	0.13	-0.06	-0.09	-0.04	0.02	-0.04	0.10	0.06	0.01
cov(f,B): r_k	0.62	0.23	0.10	-0.06	-0.15	-0.03	0.06	0.02	0.00	0.02	-0.04	-0.05
std err [r_k]	(0.21)	(0.24)	(0.24)	(0.24)	(0.24)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)
$\hat{\phi}_{kk}$	0.62	-0.24	0.12	-0.24	0.03	0.14	0.00	0.00	0.00	0.02	-0.04	0.04

TABLE 9 (cont'd)

	Lag 1	2	3	4	5	6	7	8	9	10	11	12
					Third Nearby							
cov(F,B): r_k	0.73	0.56	0.49	0.31	0.18	0.11	0.08	0.00	-0.04	-0.04	-0.02	-0.06
std err $[r_k]$	(0.21)	(0.25)	(0.27)	(0.28)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)
$\hat{\phi}_{kk}$	0.73	0.05	0.14	-0.23	-0.05	-0.01	0.11	-0.13	0.00	0.00	0.12	0.14
var(P): r_k	0.50	0.37	0.22	0.09	0.16	0.12	0.07	0.06	0.00	-0.03	0.00	-0.05
std err $[r_k]$	(0.21)	(0.23)	(0.24)	(0.24)	(0.24)	(0.24)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)
$\hat{\phi}_{kk}$	0.50	0.15	-0.02	-0.08	0.16	0.01	-0.06	0.01	0.03	-0.05	0.03	-0.04
cov(f,B): r_k	0.06	0.05	-0.12	-0.04	-0.18	-0.15	-0.10	0.05	0.13	0.05	0.16	-0.06
std err $[r_k]$	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
$\hat{\phi}_{kk}$	0.06	0.05	-0.13	-0.03	-0.16	-0.15	-0.08	0.02	0.10	-0.01	0.12	-0.10

r_k is the sample autocorrelation and $\hat{\phi}_{kk}$ is the sample partial correlation of the series. Standard errors for autocorrelations use Bartlett's formula (Box-Jenkins, p.177). Standard error for partial correlation $\approx .13 = 1/N^{½}$ (Box-Jenkins, p.178).

TABLE 10

Arima Models for Monthly Covariances and Variances:

$$(1-\phi_1 L-\phi_2 L^2)y_t = \phi_o + u_t \quad \text{or} \quad (1-\phi_1 L)y_t = \phi_o + u_t$$

	AR Coefficients					Sign	Autocorrelation of residuals at lag					
	ϕ_o	ϕ_1	ϕ_2	R^2	Q(21)	Level	1	2	3	4	5	6
						First Nearby						
cov(F,B)	1.048	0.479	0.520	0.404	6.714	0.999	0.013	-0.196	-0.166	0.072	-0.087	-0.062
	(0.002)	(3.759)	(4.201)				(0.103)	(-1.554)	(-1.317)	(0.575)	(-0.693)	(-0.490)
var(P)	0.015	0.424	0.280	0.399	8.679	0.992	-0.001	0.007	-0.023	-0.068	0.009	-0.056
	(2.323)	(3.361)	(2.222)				(-0.012)	(0.056)	(-0.180)	(-0.539)	(0.070)	(-0.446)
cov(f,B)	0.003	0.476		0.241	12.515	0.925	-0.050	-0.161	0.218	-0.150	-0.035	0.138
	(1.684)	(4.370)					(-0.400)	(-1.279)	(1.733)	(-1.189)	(-0.279)	(1.092)

T-stats are in parentheses. T-stats for autocorrelation coefficients of residuals calculated using standard error estimate = $1/N^{½}$ (see Box-Jenkins, p.290). There are 63 monthly observations. Q(21) is the Ljung-Box Q-statistic for 21 lags of the residuals.

TABLE 10 (cont'd)

	AR Coefficients					Sign	Autocorrelation of residuals at lag					
	ϕ_o	ϕ_1	ϕ_2	R^2	Q(21)	Level	1	2	3	4	5	6
						Second Nearby						
cov(F,B)	0.006	0.350	0.449	0.600	10.648	0.969	0.005	0.024	-0.078	-0.116	-0.250	0.013
	(1.379)	(3.022)	(3.982)				(0.038)	(0.194)	(-0.616)	(-0.923)	(-1.986)	(0.111)
var(P)	0.019	0.442		0.177	11.688	0.948	-0.055	0.083	0.013	0.160	0.009	-0.035
	(5.155)	(3.598)					(-0.437)	(0.662)	(0.105)	(1.267)	(0.073)	(-0.276)
cov(f,B)	0.003	0.616		0.389	13.960	0.871	0.116	-0.229	-0.073	-0.127	-0.163	0.055
	(1.751)	(6.184)					(0.924)	(-1.819)	(-0.577)	(-1.007)	(-1.297)	(0.435)
						Third Nearby						
cov(F,B)	0.008	0.734		0.547	11.152	0.960	-0.077	-0.102	0.178	-0.024	-0.101	-0.049
	(3.210)	(8.519)					(-0.611)	(-0.814)	(1.409)	(-0.191)	(-0.800)	(-0.388)
var(P)	0.002	0.512		0.257	10.338	0.974	-0.090	0.158	0.022	-0.123	0.108	0.034
	(4.608)	(4.554)					(-0.712)	(1.256)	(0.178)	(-0.976)	(0.860)	(0.267)
cov(f,B)	0.002	0.066		0.004	15.923	0.774	-0.018	0.012	-0.118	-0.006	-0.117	-0.194
	(2.544)	(0.506)					(-0.139)	(0.098)	(-0.940)	(-0.046)	(-0.929)	(-1.543)

TABLE 11

OLS Regressions of Futures-Forward Price Differences against Expected Covariances and Variances:

ARIMA Method

$$\log(F/f)_t = \alpha + \beta \cdot cov(\cdot)_t + \epsilon_t$$

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		First Nearby	
$\hat{\alpha}$	-0.056	-0.012	0.002
t-stat	(-1.283)	(-0.205)	(0.031)
$\hat{\beta}$	-0.013	-0.075	-0.525
t-stat	(-0.188)	(-1.132)	(-1.832)
R^2	0.001	0.026	0.055
N	61	61	62
		Second Nearby	
$\hat{\alpha}$	-0.159	0.074	-0.166
t-stat	(-2.609)	(0.684)	(-3.081)
$\hat{\beta}$	-0.094	-0.167	-0.217
t-stat	(-1.971)	(-3.229)	(-2.327)
R^2	0.074	0.163	0.097
N	61	62	62

TABLE 11 (cont'd)

ARIMA Method

	Explanatory Variable		
	cov(F,B)	var(P)	cov(f,B)
		Third Nearby	
$\hat{\alpha}$	-0.088	-0.079	-0.367
t-stat	(-0.636)	(-0.392)	(-0.666)
$\hat{\beta}$	-0.273	-0.102	-0.385
t-stat	(-3.287)	(-2.140)	(-0.184)
R^2	0.188	0.086	0.001
N	62	62	62

All standard deviations and t-statistics are adjusted for serial correlation using a variant of Hansen/Hodrick (1980). The ARIMA method fits an ARIMA model to each covariance or variance series, then uses this time series model to forecast expected covariances and variances.