Differential Interpretation of Public Signals and Trade in Speculative Markets

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Most models of trade in speculative markets assume that agents interpret public information identically. We provide empirical evidence on the relation between the volume of trade and stock returns around public announcements, and we argue that the evidence is inconsistent with this assumption. We then develop a model of trade around public announcements that incorporates differential interpretations and is consistent with the observed volume-return relation. Then we test the standard model of belief revision underlying most models of trade using stock brokerage research analysts' earnings forecasts. The hypothesis of identical interpretations seems inconsistent with the forecast revisions in these data.

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I. Introduction

Heterogeneity of agents is widely accepted in the economics and finance literature as the explanation for trade. Sources of heterogeneity include differences in preferences (i.e., risk aversion), differences in endowments, and differences in information. The last is the focus of this paper. Most existing models of trade, for example, Pfleiderer (1984), Kyle (1985), and Admati and Pfleiderer (1988), introduce information heterogeneity in a particular fashion: agents observe independent signals drawn from the same distribution, and this distribution is common knowledge (exceptions are Varian [1989] and Harris and Raviv [1993]). This places severe restrictions on the circumstances under which speculative trade can occur. More generally, most models involving explicit information processing are guided by Aumann's (1976) argument that two agents for whom rationality is common knowledge cannot agree to disagree.

Yet on a practical level, it is trivially obvious that people disagree about the probabilities of events even when exposed to seemingly identical evidence. In the economics literature, this view is taken, for example, by Rubinstein (1993), who takes different interpretations of information as a primitive of a model of a monopolist without troubling with whether there exists empirical evidence on the issue. Rather, he seems to think it obvious that agents have different interpretations of information: "In almost all models of economic theory, behavioral differences among consumers are attributed to differences in preferences or in the information they possess. In real life, differences in consumer behavior are often attributed to varying intelligence and ability to process information. Agents reading the same morning newspapers with the same stock price lists will interpret the information differently" (p. 473). It is well known that the assumptions that agents have identical interpretations of information and common knowledge have important consequences for the equilibria of economic models. This paper addresses the issue of whether this assumption is appropriate by looking at a simple model of trading volume in speculative markets. We show that relaxing the assumption that agents interpret public information identically changes predictions about the volume of trade. More important, we provide empirical evidence that the assumption that agents interpret information identically is overly restrictive.

Specifically, we study the volume of trade in common stocks and the forecast revisions of equity research analysts employed by stock brokerage firms around the dates of public announcements of quarterly (interim) earnings. We study differential interpretation of public signals in the context of a financial market because this is the ideal setting in which to study the issue. First, many investors and money managers, and the equity research analysts whose forecasts we study, are intelligent, well-trained, sophisticated individuals with a great deal at stake. There are substantial rewards for success and few barriers to entry, and agents have tremendous incentives to take account of others' information: "If she wants to trade with me, why should I trade with her?" Also, "If her forecast is different from mine, shouldn't I update mine to reflect the information in hers?" If we find that agents fail to take full account of others' information in a well-developed financial market, we can reasonably expect that other agents in other settings will also fail to do so. Second, stock returns, volumes, and analysts' forecast revisions around earnings announcements constitute an excellent data set to examine our hypotheses. We have a large number of observations of prices, volumes of trade, and explicit, though of course imperfect, measures of the beliefs of an important group of agents (brokerage firm equity research analysts). It is rare to have a large data set that includes prices, volumes of trade, and explicit measures of beliefs. We exploit these data to shed some light on the general issue of whether agents can be assumed to interpret information identically.

We begin by providing empirical evidence on the relation between the volume of trade and stock returns around anticipated public announcements. Using the announcement dates of quarterly (interim) earnings from the Compustat quarterly files and daily data on the returns and volumes of common stocks, we find that there are economically and statistically significant positive abnormal volumes associated with quarterly earnings announcements even when prices do not change in response to the announcements. It is notable that there appear to be abnormal volumes that are unrelated to the magnitudes of the price changes. This is inconsistent with most existing models of volume around public announcements in which agents have identical interpretations of public signals.

We discuss a number of candidate explanations for this finding, including both models in which agents have identical interpretations of public signals and other explanations. They include the models of Kim and Verrecchia (1991*a*, 1991*b*) and Harris and Raviv (1993); the possibility that "life cycle" or liquidity trading might be concentrated around the earnings announcement dates; the possibility that the measured announcement period returns do not accurately capture the value effects of the earnings announcements because of the arrival of other information during the announcement window; the possibility that the announcement is accompanied by a switch from a partially to a fully revealing rational expectations equilibrium; and the possibility that the abnormal volume around the announcement might be explained by patterns in the arrival or production of private information, trade due to wealth changes, and trade due to risk shifts around the earnings announcements. None of these candidates seems likely to explain our empirical finding. This leaves the hypothesis that the abnormal volume during the announcement periods occurs because investors have differential interpretations of the announcements. We explore this hypothesis.

Specifically, we construct a model in which trade occurs because agents use different likelihood functions to interpret the public announcement. We take the different likelihoods as a primitive of the model and assume that agents hold their interpretations even though other agents have different ones. Throughout the paper we maintain the assumption that agents optimize within the framework of their beliefs. We think of different likelihoods as corresponding to different "models" used by agents to interpret the world.[1] For example, if investors think that earnings are a linear function of firm value plus noise, different likelihoods correspond to different coefficients or intercepts in the linear relation. The model we construct yields the observed volume-return relation.

Allowing agents to use different likelihood functions to interpret public signals permits agents to revise their beliefs in ways that would be impossible were they to possess the same likelihoods. We go on to present a simple model of belief revision that underlies conventional models of trade and use data on individual brokerage analysts' earnings forecasts around quarterly earnings announcements from the Institutional Brokers Estimate System (I/B/E/S) Detail Tape to determine whether forecast revisions inconsistent with the hypothesis of identical likelihoods occur. We find that such revisions are both reasonably frequent and sizable.

One reason for dropping the assumption that investors possess identical likelihood functions is that it is obviously not literally true.

[1] An example appears in the *New York Times* article by Fisher (1993, p. D4), who observes that "after Apple Computer Inc. announced a decline in earnings for its second fiscal quarter, analysts rushed to revise their estimates for the year. Some revised them downward, as one might expect, but some raised their estimates and others even issued new buy recommendations." It appears that the analysts disagreed because they used different models of the computer industry to interpret the public announcement, for the *Times* goes on to state that "the Apple bulls contend that the pricing pressure on the company will abate in the second half of the year as new products become more available, and sales will continue to grow. The bears say that Apple has been promising earnings growth for some time now, and that maintaining margins will get harder, not easier." In another example, Sease (1991, p. C1) discusses analysts' disagreements about the effect of inflation on corporate earnings and stock prices in the *Wall Street Journal.* These disagreements appear not to be about the level of current and expected future inflation but rather about the manner in which a given level of inflation will affect corporate profitability, i.e., about the model of the economy that should be used to interpret new information about inflation.

This alone provides some justification for exploring the implications of dropping it. In addition, obtaining different likelihoods through the device of having the agents observe different signals about the parameters of the likelihood function generates trade only if "life cycle" or "noise" traders are also added to the model, and the total volume of trade depends on the volume of life cycle trading. We think it unlikely that the volume of life cycle trade is large enough to support the total volume of trade observed in speculative markets. More important, the implications of the model with different likelihoods are consistent with the empirical evidence in this paper, but we argue that the implications of other models are not.

Related papers include Harris and Raviv (1993) and Kim and Verrecchia (1994). Harris and Raviv also assume that agents use different likelihood functions to interpret public signals and motivate this with arguments similar to ours. However, their modeling strategy differs significantly from ours, and some of their results differ as well. Also, they focus on the time-series characteristics of volume and prices, whereas we concentrate on those implications of our model that are different from the implications of alternative models and hypotheses. Kim and Verrecchia (1994) focus on explaining why information asymmetry and bid-offer spreads might be greater after a public announcement. In modeling this, they assume that investors observe the sum of the public signal and idiosyncratic signals that coincide with the public announcement. Although the interpretation is different, their model in which agents receive idiosyncratic private signals that cannot be disentangled from the public announcement has the same implications for the volume-return relation as our model with different likelihoods. However, it seems unlikely that their model can explain the revisions of analysts' forecasts, which we examine in Section IV.

In Section II, we provide empirical evidence on the relation between the volume of trade and stock returns around quarterly earnings announcements, and we argue that it cannot be explained by most existing models and hypotheses. We present our model of trade around public signals in Section III. The model is consistent with the evidence on the volume-return relation presented in Section II. In Section IV, we analyze the restrictions on agents' prior and posterior beliefs surrounding the release of a public signal imposed by the assumption of identical likelihoods, and we use data from the I/B/E/S Detail Tape on stock brokerage analysts' earnings forecasts around quarterly earnings announcements to examine empirically the restrictions on revisions of beliefs. Section V argues that simply endowing agents with different likelihoods is reasonable, and it concludes the paper.

II. The Volume-Return Relation

A. *Empirical Evidence*

In this section we provide empirical evidence on the relation between the volume of trade and stock returns around a large sample of quarterly earnings announcements. Models of trade around public announcements focus on trade due to the public information release and do not model trading due to "life cycle" considerations or trading to exploit private information in the round of trading following the announcement. While clearly some trade is due to these factors, there is no a priori reason to believe that such trade is more frequent around public announcements. We allow for "normal" volume due to life cycle trading or trading to exploit other private information unrelated to the earnings announcement by defining abnormal volume to be the difference between volume in a 3-day period around the announcement and the mean 3-day volume in preannouncement periods with the same stock return.

The announcement dates are taken from the 1992 and 1993 Compustat Primary, Supplementary, Tertiary, and Full Coverage quarterly files, and they cover the period from June 1981 through June 1992. The data on returns, trading volume, stock splits, and stock dividends were taken from the New York Stock Exchange/American Stock Exchange (NYSE/ASE) Daily Return and Master files prepared by the Center for Research in Security Prices (CRSP). For a firm/announcement pair to be included in the sample, volume and return data must be available for the announcement day (day 0), the previous day (-1), and the following day ($+1$), and the firm must have nonmissing return and volume observations for at least 21 of the 30 (trading) days from days -46 through -17.[2] Applying these criteria, we obtained a sample of 2,483 firms and 69,067 individual firm/announcements. The results reported below use the volume data adjusted for stock splits and dividends.

For each firm/announcement pair, we standardize the volume data by dividing the volumes by the average volume over the 30 days from day -46 through day -17. This rescaling prevents the results from being dominated by the largest firms. We then measure the announcement period volume as the mean (standardized and adjusted) volume over days -1, 0, and 1. Our measure of normal volume is also a 3-day volume. Specifically, we partition the 15 trading days

[2] The preannouncement volumes are used to construct estimates of "normal" volumes. One issue is the possibility of trade in the preannouncement period due to information leakages. The evidence in table 2 and fig. 2 discussed below suggests that this is not a problem. Moreover, volume due to information leakage during the preannouncement period will only bias downward the estimates of abnormal volume.

preceding day -1 into five 3-day periods and compute the mean volume over each of these 3-day periods. For each 3-day period we compute a 3-day return, partition the data based on returns, and use the mean (by return cell) of the preannouncement mean 3-day volumes as the estimate of normal volume. We use a 3-day announcement period because there is evidence (Stickel and Verrecchia 1993) that the reactions to some announcements occur on day -1 or $+1$, and this suggests that the date in the Compustat files is not always the date on which the information is first available to investors. In this case, a 1-day announcement period will miss some events. We partition the data based on returns in order to control for a well-documented (see, e.g., Karpoff 1987) relation between volume and returns.

Specifically, we form 23 cells, with the first cell including all returns less than $-.05$ and the twenty-third cell including all returns greater than .05. The other cells have equal sizes, with the exception that the twelfth cell includes the zero returns.[3] Table 1 presents the means and medians of the announcement and nonannouncement volumes calculated separately for each cell, along with p-values of nonparametric Wilcoxon rank-sum tests for differences in the location parameters of the distributions.[4] This test requires only that one be able to rank the data ordinally, and it is not sensitive to the presence of outliers in volume data. The announcement volume-return relation is clearly different from the nonannouncement relation, with both the mean and median volume higher in every cell. The results for the medians indicate that the differences in means are not driven by the presence of a few outliers. The p-values of the nonparametric Wilcoxon rank-sum tests of the hypothesis that the location parameters are the same for the announcement and nonannouncement days are less than .0001 for every cell. Similar results (not presented) were obtained for a NASDAQ sample of 3,978 firms and 60,550 individual firm/announcements beginning in November 1982, the earliest date for which volume data are available on the NASDAQ files. We also repeated the analysis using 1-day announcement periods for both the NYSE/ASE and NASDAQ samples and obtained similar results.

[3] Cell 12 actually contains the 3-day returns R that satisfy $-.00001 < R < .00001$. We use this as the operational definition of a zero return because the truncation of reported returns in the CRSP files means that 3-day returns computed by compounding the reported daily returns will often be less than zero when the actual price change over the 3-day period was exactly zero.

[4] Ideally we would compare the announcement period volumes to estimates of normal volume that condition on other information such as past volume. The results in Gallant, Rossi, and Tauchen (1992) suggest that developing a parametric model of expected volume would be a major endeavor, and certainly beyond the scope of this paper.

TABLE 1

MEAN AND MEDIAN ANNOUNCEMENT AND NONANNOUNCEMENT VOLUMES FOR NYSE/ASE STOCKS

		Nonannouncement Sample				Announcement Sample				
Cell	Return Range	Observations	Mean Volume	Standard Error	Median Volume	Observations	Mean Volume	Standard Error	Median Volume	Wilcoxon p-Value
1	$R < -.05$	29,319	1.562	.022	.991	8,849	2.213	.039	1.430	<.0001
2	$-.045 > R \geq -.05$	5,916	1.186	.030	.839	1,286	1.526	.086	1.050	<.0001
3	$-.040 > R \geq -.045$	6,325	1.127	.021	.807	1,478	1.456	.056	1.058	<.0001
4	$-.035 > R \geq -.040$	8,481	1.198	.115	.801	1,734	1.655	.103	1.062	<.0001
5	$-.030 > R \geq -.035$	10,127	1.059	.014	.783	1,952	1.392	.040	.975	<.0001
6	$-.025 > R \geq -.030$	11,586	1.036	.012	.765	2,196	1.370	.061	.959	<.0001
7	$-.020 > R \geq -.025$	14,013	1.005	.015	.732	2,455	1.332	.036	.942	<.0001
8	$-.015 > R \geq -.020$	16,943	1.031	.018	.739	2,929	1.235	.023	.933	<.0001
9	$-.010 > R \geq -.015$	19,062	.981	.012	.719	3,111	1.251	.026	.904	<.0001
10	$-.005 > R \geq -.010$	20,386	.981	.015	.720	3,404	1.207	.027	.892	<.0001
11	$-.00001 \geq R \geq -.005$	9,034	.968	.016	.733	1,414	1.219	.045	.915	<.0001
12	$.00001 > R > -.00001$	45,489	.942	.008	.655	7,209	1.302	.035	.831	<.0001
13	$.005 \geq R \geq .00001$	8,848	1.016	.021	.746	1,440	1.326	.071	.922	<.0001
14	$.010 \geq R > .005$	19,757	1.008	.014	.749	3,348	1.456	.128	.921	<.0001
15	$.015 \geq R > .010$	17,827	1.030	.010	.784	3,061	1.305	.026	.949	<.0001
16	$.020 \geq R > .015$	15,870	1.103	.014	.820	2,859	1.409	.039	1.015	<.0001
17	$.025 \geq R > .020$	12,871	1.126	.016	.852	2,316	1.432	.032	1.065	<.0001
18	$.030 \geq R > .025$	10,703	1.228	.024	.904	2,116	1.619	.046	1.136	<.0001
19	$.035 \geq R > .030$	9,110	1.257	.035	.905	1,895	1.568	.038	1.188	<.0001
20	$.040 \geq R > .035$	7,783	1.303	.019	.969	1,664	2.063	.476	1.163	<.0001
21	$.045 \geq R > .040$	6,253	1.352	.020	1.017	1,371	1.704	.045	1.270	<.0001
22	$.050 \geq R > .045$	5,708	1.539	.134	1.015	1,337	1.742	.046	1.272	<.0001
23	$R > .05$	33,981	2.011	.021	1.315	9,643	3.042	.094	1.797	<.0001
Total		345,392				69,067				

NOTE.—Events for which volume data are available for fewer than seven of the 10 3-day periods before day −16 are excluded.

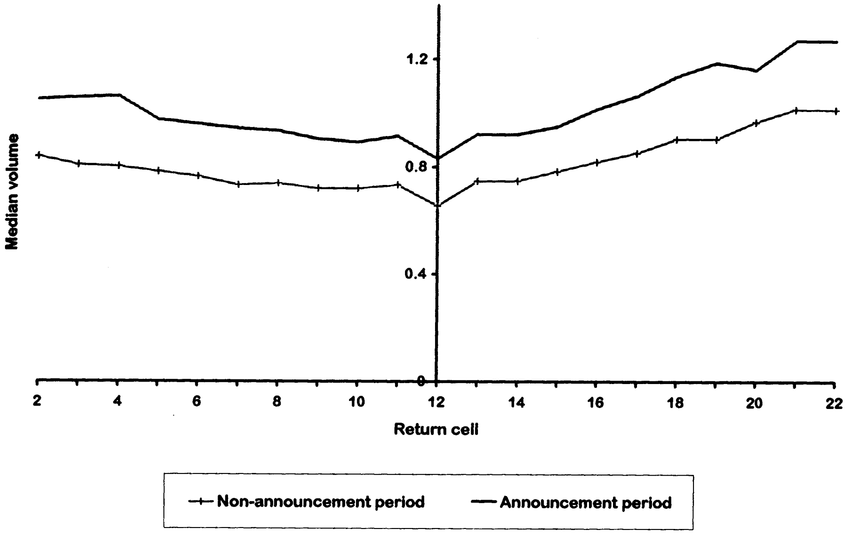

FIG. 1.—Median announcement and nonannouncement period volumes for NYSE/ASE stocks, by return cell. Events for which volume data are available for fewer than seven of the 10 3-day periods before day −16 are excluded.

Using the medians, figure 1 graphs the volume-return relations for announcement and nonannouncement periods. This figure and table 1 indicate that unusual volume around the announcement is not simply due to the combination of higher variability of announcement period returns and the correlation of volume and the absolute magnitudes of returns. Rather, there is significant abnormal volume even when the announcement period return is zero or close to zero. As discussed more fully immediately below, most existing models and hypotheses, for example the models of Kim and Verrecchia (1991*a*, 1991*b*) and Harris and Raviv (1993), imply that the amount of information-related trade is increasing in the absolute magnitude of the price change, and that it should be zero when the price change is zero. Our finding of a positive relation between the volume of trade and the absolute value of the return confirms the conjectured relation in Karpoff (1987).

B. Candidate Explanations

In Section III, we present a model that is consistent with the observed volume-return relation. The key feature of the model is that investors have differential interpretations of the public signal or different likelihood functions. We are interested in this model partly because of our belief that alternative candidate explanations are unable to account for our empirical results. Here we discuss these alternative explanations.

The Kim and Verrecchia (1991*a*, 1991*b*) Models

Kim and Verrecchia derive a specific volume-return relation around public information releases, namely

$$V_{it} = k_{it}|\Delta P_{it}|, \tag{1}$$

where V_{it} and ΔP_{it} are the volume of trade and price change of security i due to the public announcement at time t. The coefficient k_{it} measures the dispersion of risk tolerance coefficients and prior precisions in the population of investors. This result says that the volume of trade due to the earnings announcement is proportional to the absolute value of the price change, with the coefficient of proportionality potentially varying both across firms and over time. One specific implication of this proportionality is that the volume must be zero if the price change due to the announcement is zero, or that the mean announcement and nonannouncement volumes should be equal in the twelfth cell (the zero return cell) in table 1 and that the announcement and nonannouncement relations should coincide in the twelfth cell in figure 1. This is not what we find.

The Kim and Verrecchia models make strong assumptions about the form of investors' preferences and the probability distributions of the signals, and the absence of wealth effects due to the negative exponential utility function plays an important role in (1).[5] There is little reason to expect that the exact proportionality in equation (1) is robust to violations of the simplifying assumptions. However, below we argue that wealth effects and other factors are unlikely to explain the observed volume-return relation.

[5] In the Kim and Verrecchia models, agents' beliefs differ prior to the announcement because they receive (different) private signals. An announcement that causes no price change is one that does not change average beliefs about the liquidating value of the risky asset and has two effects. First, it moves all agents' beliefs toward the mean of their beliefs. Agents whose preannouncement beliefs were above (below) the mean of all agents' beliefs continue to have postannouncement beliefs above (below) the mean but closer to it. Second, it increases the precision of the agents' beliefs; i.e., it causes them to hold their beliefs more strongly. The first effect, taken alone, suggests that agents would adjust their holdings of the risky asset toward the mean holding of the asset. The second effect, taken alone, suggests that agents would adjust their holdings of the risky asset away from the mean holding of the asset. In the Kim and Verrecchia models, these two effects exactly offset one another because investors have constant absolute risk tolerance through the negative exponential utility function; in the absence of negative exponential utility, the exact offsetting will not occur. While these two effects will not *exactly* offset one another in more general models, both offsetting effects will exist, and we conjecture that in more general models the volume effect of an announcement that causes a small price change will be small.

The Harris and Raviv (1993) Model

The Harris and Raviv model, although similar in spirit to ours, does not have the implication that trade occurs in the absence of a price change. Their model assumes two groups of risk-neutral investors, one of which places more weight on public signals than the other. Beliefs depend on the sum of the public signals received through the current date, and trade can occur only when the sum of the signals changes sign. In the model, this trade must be accompanied by a price change, which is inconsistent with what we find in the data.

Life Cycle or Liquidity Trading

It seems unlikely that investors' desires to transact for life cycle or liquidity reasons are concentrated around the dates of quarterly earnings announcements. We can think of no a priori reason why they would be.

However, it is conceivable that liquidity traders might shift trade from the preannouncement period to the immediate postannouncement period if their informational disadvantage was greater during the preannouncement period, and the results in table 1 and figure 1 are consistent with the hypothesis that they do so. However, this hypothesis suggests that bid-ask spreads should be larger during the preannouncement period, which is inconsistent with the evidence in Morse and Ushman (1983), Skinner (1991), Lee, Mucklow, and Ready (1993), and Barclay and Dunbar (1994).

We can look directly for evidence of such shifting of liquidity trade by examining the volume in periods immediately before the earnings announcements. The hypothesis that the unusual announcement period volume is explained by a shift of liquidity trade from the immediate preannouncement period to the immediate postannouncement period implies that volume immediately before the announcement should be less than normal. Table 2 presents the mean volume for each cell for the two 3-day periods (days -4 through -2 and days -7 through -5) immediately before the announcement period, along with the nonannouncement period mean volumes estimated using the five 3-day periods before the announcement period (these include days -7 through -2). The table also presents p-values for Wilcoxon rank-sum tests of the hypotheses that the location parameters of the volume distributions for the two 3-day periods immediately before the announcement period are identical to the location parameter of the volume distribution for the other three 3-day periods before the announcement period.

Figure 2 displays the volume-return relation (using medians) for

TABLE 2

MEAN VOLUMES FOR NYSE/ASE STOCKS FOR THE NONANNOUNCEMENT PERIOD AND TWO 3-DAY PERIODS JUST BEFORE THE ANNOUNCEMENT

		Nonannouncement Sample		Days −4 through −2			Days −7 through −5		
Cell	Return Range	Observations	Mean Volume	Observations	Mean Volume	Wilcoxon p-Value	Observations	Mean Volume	Wilcoxon p-Value
1	$R < -.05$	29,319	1.562	6,142	1.655	.0001	5,963	1.550	.0003
2	$-.045 > R \geq -.05$	5,916	1.186	1,183	1.193	.2050	1,210	1.129	.9526
3	$-.040 > R \geq -.045$	6,325	1.127	1,271	1.188	.1150	1,287	1.115	.4037
4	$-.035 > R \geq -.040$	8,481	1.198	1,703	1.156	.2632	1,670	1.085	.4292
5	$-.030 > R \geq -.035$	10,127	1.059	1,957	1.092	.1032	2,039	1.044	.8400
6	$-.025 > R \geq -.030$	11,586	1.036	2,308	1.099	.0411	2,272	1.067	.0570
7	$-.020 > R \geq -.025$	14,013	1.005	2,814	.996	.1118	2,869	1.033	.5813
8	$-.015 > R \geq -.020$	16,943	1.031	3,386	1.025	.4050	3,317	1.054	.0205
9	$-.010 > R \geq -.015$	19,062	.981	3,732	.982	.3632	3,789	.999	.9544
10	$-.005 > R \geq -.010$	20,386	.981	3,941	.974	.0449	4,025	.990	.0844
11	$-.00001 \geq R \geq -.005$	9,034	.968	1,759	.958	.4180	1,780	.980	.5228
12	$.00001 > R > -.00001$	45,489	.942	9,105	.979	.0089	9,125	.983	.1149
13	$.005 \geq R \geq .00001$	8,848	1.016	1,724	.996	.6901	1,827	1.058	.4455
14	$.010 \geq R > .005$	19,757	1.008	3,890	1.062	.0841	3,938	.984	.8671
15	$.015 \geq R > .010$	17,827	1.030	3,456	1.067	.3459	3,536	1.027	.8108
16	$.020 \geq R > .015$	15,870	1.103	3,164	1.103	.9845	3,138	1.145	.7133
17	$.025 \geq R > .020$	12,871	1.126	2,580	1.153	.0142	2,585	1.180	.1781
18	$.030 \geq R > .025$	10,703	1.228	2,105	1.264	.2820	2,156	1.229	.3054
19	$.035 \geq R > .030$	9,110	1.257	1,854	1.305	.3210	1,837	1.348	.9103
20	$.040 \geq R > .035$	7,783	1.303	1,646	1.358	.1889	1,597	1.332	.6449
21	$.045 \geq R > .040$	6,253	1.352	1,224	1.404	.2255	1,266	1.477	.0016
22	$.050 \geq R > .045$	5,708	1.539	1,093	1.394	.7139	1,159	1.542	.3377
23	$R > .05$	33,981	2.011	7,048	2.177	.0003	6,699	2.110	.0005
Total		345,392		69,085			69,084		

NOTE.—Events for which volume data are available for fewer than seven of the 10 3-day periods before day −16 are excluded.

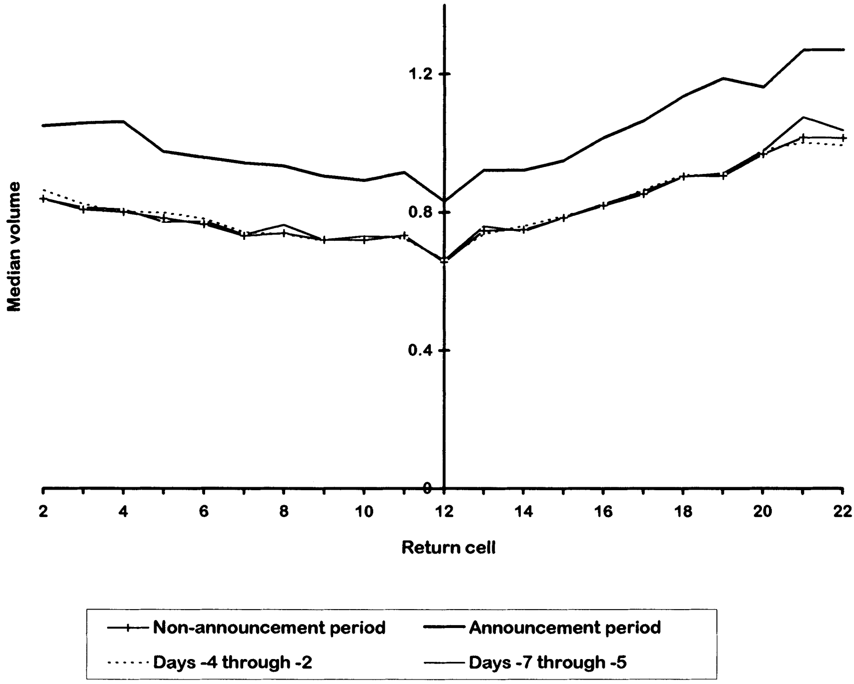

FIG. 2.—Median volumes for NYSE/ASE stocks by return cell for the announcement period, the nonannouncement period, and two 3-day periods just before the announcement. Events for which volume data are available for fewer than seven of the 10 3-day periods before day -16 are excluded.

the two 3-day periods (days -4 through -2 and days -7 through -5) immediately before the announcement period, along with the announcement period relation and the nonannouncement period relation estimated using the five 3-day periods before the announcement period. In table 2 and figure 2, the relation for days -4 through -2 is generally only slightly above that for the five nonannouncement periods, and that for days -7 through -5 seems indistinguishable from that for the five nonannouncement periods. The p-values for cells 1 and 23 are significant at conventional levels, perhaps because the absolute magnitudes of the returns in these cells are larger for days -4 through -2 and -7 through -5 than for the other nonannouncement days. Only a few of the other p-values are significant at conventional levels, and the differences in the point estimates are small. Moreover, when the differences are statistically significant, it is generally the case that the volume for days -4 through -2 is greater than the volume for the other nonannouncement days. The hypothesis that the abnormal volume on the announcement day can be explained by shifting of liquidity trade from the immediate preannouncement period to immediately after the announcement implies that the volumes for days −4 through −2 should be less than the volumes for the other nonannouncement days, so the results in table 2 are inconsistent with shifting of liquidity trade. Again, similar results were obtained for the NASDAQ sample.

Incorrectly Measuring the Announcement Return

The 3-day returns may include some events for which large (in absolute value) daily or intraday returns net to 3-day returns of zero, and the Kim and Verrecchia (1991*a*, 1991*b*) and Harris and Raviv (1993) models predict large volumes for these events. Large volumes conditional on 3-day returns of zero conceivably could lead one to reject the models. However, the hypothesis that the results are due to mismeasuring the announcement returns implies that more precise measurement of the announcement period return should reduce differences between the announcement and nonannouncement volumes. We repeated the analysis using a 1-day announcement period (day 0) and obtained results similar to those obtained with the 3-day window.[6]

Private Information Production Concentrated around Earnings Announcement Dates

An alternative hypothesis is that the production or acquisition of private information is concentrated around earnings announcement dates and that the abnormal announcement period volume we observe is the result of informed investors' exploitation of private information. However, table 2 and figure 2 indicate that there is very little abnormal volume during days −4 through −2 and essentially none during days −7 through −5. With the exception of the argument offered in Kim and Verrecchia (1994), even if the production of private information is concentrated around earnings announcement dates, it seems unlikely that the production and exploitation of private information are concentrated *exclusively* in the 3-day window around the announcement. This makes it unlikely that the abnormal announcement period volume we observe is due to the concentration of private information production around earnings announcements.

The model of Kim and Verrecchia (1994), which offers an explana-

[6] As mentioned above, there is evidence that the reactions to some announcements occur on day −1 or +1, suggesting that the Compustat announcement date is not always the date on which the information is first available to investors. If this is the case, then a 1-day announcement period misses some events, which is likely to bias downward the results obtained with the shorter announcement period.

tion of why information asymmetry and bid-offer spreads might be greater after a public announcement, also has implications for the volume of trade. The authors assume that investors observe the sum of the public signal and idiosyncratic signals that coincide with the public announcement. They interpret the public signal as providing the opportunity for traders who specialize in making expert judgments about public information to do so. As indicated above, this model in which agents receive idiosyncratic private signals that cannot be disentangled from the public announcement has the same implications for the volume of trade as our model with different likelihoods. However, it seems unlikely that the Kim and Verrecchia model can explain the revisions of analysts' forecasts that we examine in Section IV.

Switch from a Partially Revealing to a Fully Revealing Rational Expectations Equilibrium

Rational expectations sequential trade models typically have two equilibria, one of which is partially and the other fully revealing of the agents' information. Most authors appear to think the fully revealing equilibrium implausible. Nonetheless, it is conceivable that there could be a partially revealing equilibrium prior to the earnings announcement and a fully revealing equilibrium after, with trade around the earnings announcement occurring as investors adjust their portfolios to the new equilibrium holdings. Grundy and McNichols (1989) have raised this possibility and argued that this is a possible explanation for the volume of trade around public announcements. While Grundy and McNichols do not discuss the volume-return relation, in their model, volume in the postannouncement round of trade is unrelated to the price change. This is consistent with the results in table 1 and figure 1.

However, if the equilibrium following the earnings announcement is fully revealing, then agents should all have the same beliefs. We possess direct measures of the beliefs of one important group of economic agents, namely the earnings forecasts of brokerage firm research analysts, and the analysts following a firm clearly do not share the same beliefs following a quarterly earnings announcement (see, e.g., Stickel 1991). While the evidence in Stickel indicates that the dispersion of analysts' forecasts declines around earnings announcements, the reduction in the dispersion of beliefs following earnings announcements is small. This is inconsistent with the hypothesis that the equilibrium switches from partially to fully revealing around the earnings announcement.

Wealth Changes

Wealth changes can cause investors to trade. However, wealth changes occur every day, not just on the days of earnings announcements, so it seems difficult to attribute the abnormal volume to wealth changes. While earnings announcements are associated with more variable returns and therefore larger wealth changes, we find abnormal volume at every level of return.

Permanent Risk Changes Due to the Announcement

In addition to changes in wealth, changes in the riskiness of assets can induce trade. When there is only one risky asset, increases in its risk lead to trade in which agents less tolerant of risk sell the risky asset to more risk-tolerant agents. Similarly, decreases in the risk of the asset lead to trade in which the less risk-tolerant agents end up holding more of the asset. Brown, Harlow, and Tinic (1993) find that large abnormal returns are associated with changes in risk. If it is assumed that earnings announcements are associated with changes in the risk characteristics of the announcing firms, at first glance, risk shifts seem like a possible explanation of unusually high volume following earnings announcements.

However, we find abnormal volume for every level of return, not simply for the large returns for which risk shifts have been documented. In addition, the hypothesis that risk shifts lead to trade is less compelling when agents hold portfolios that contain a number of assets, for a change in the risk of a single security is unlikely to have much of an impact on an investor who holds a reasonably well-diversified portfolio. Even if it does, the optimal portfolio rebalancing will involve all the securities in the portfolio, not simply the shares of the announcing firm.

Temporary Risk Changes around the Announcement

There is evidence (see Kalay and Loewenstein 1985) that common stocks have higher return variances and beta coefficients around the dates of earnings announcements, and it might be argued that these temporary risk changes induce trade. The hypothesis would be that agents who do not want to bear this risk would sell stocks to agents better able to bear it just prior to the dates of quarterly earnings announcements and repurchase them just after the announcement.

As discussed above, earnings announcements will lead to significant abnormal volume only if they are associated with risk changes large enough to materially affect the risk of the portfolios that investors actually hold. Even in that case, the implication is that trade will occur in large numbers of securities, not just the securities of the announcing firm. Furthermore, this story implies that there will be high volume both before and after the announcement. The previously discussed results in table 2 and figure 2 indicate that there is not unusual volume before the announcement.

Clientele Shifts

The finance literature contains the idea that trade might occur because of clientele shifts.[7] However, aside from the potential risk shifts previously discussed, we can think of no reason why earnings announcements should be associated with clientele shifts. Most of the literature on clientele shifts involves tax clienteles, which do not seem relevant here.

This reading of the evidence indicates that existing models do not explain the observed volume-return relation, and we are left with the hypothesis that agents have different interpretations of public signals. This idea appears in Varian (1989), and a model of trade in which agents have different likelihoods is studied in the paper by Harris and Raviv (1993) discussed above. In the next section we develop a simple model of trade around public announcements, focusing on explaining the empirical result above.

III. The Different Likelihoods Model

We endow traders with different likelihood functions, which they use to interpret public signals. We make the following specific assumptions: (1) There are only two assets: a riskless asset with a zero rate of return and a risky security with an uncertain payoff X. (2) There are three time periods indexed by t: at time 1, agents have prior beliefs about the value of the asset; at time 2, they observe a public signal and update their beliefs; and at time 3, the value of the risky asset is realized and the agents consume their wealth. (3) There are two types of agents indexed by $i = 1, 2$. Before any signals are observed, the prior beliefs of the agents are represented by normal

[7] Asquith and Krasker (1985) and Richardson, Sefcik, and Thompson (1986) investigate the implication of tax clientele theories that announcements of changes in dividend policies should be followed by trade between investors with different marginal tax rates. Others, e.g., Karpoff and Walkling (1988) and Michaely and Vila (in press), study short-term tax-motivated trading around ex-dividend days.

densities with means X_i and precisions Z_i. A proportion α of traders are of type 1; without loss of generality, they are assumed to be initially more optimistic, $X_1 > X_2$. (4) The public signal at time 2 is given by $L = X + \epsilon$, where ϵ is normally distributed. All the agents observe the same signal L but do not interpret it identically. Specifically, the ith agent believes that $\epsilon \sim N(\mu_i, b_i)$, where $i = 1, 2$ indexes the agents and μ_i and b_i denote the mean and precision of the ith agent, respectively. This assumption is what differentiates this model from most other models of trade. (5) All agents maximize the expected utility of third-period wealth. Preferences are represented by a negative exponential utility function $U(W) = -\exp(-\lambda W)$, where λ is the coefficient of absolute risk aversion.

We use m_{it} to denote the position held by a type i investor in period t and M_{it} to denote the corresponding aggregate position. The risky asset is assumed to be in zero net supply, so that in equilibrium $M_{1t} + M_{2t} = 0$. For convenience, we introduce the notation

$$\hat{Z} \equiv \alpha Z_1 + (1 - \alpha) Z_2,$$

$$\hat{b} \equiv \alpha b_1 + (1 - \alpha) b_2,$$

$$\hat{X} \equiv \alpha Z_1 X_1 + (1 - \alpha) Z_2 X_2,$$

$$\hat{\mu} \equiv \alpha b_1 \mu_1 + (1 - \alpha) b_2 \mu_2.$$

Discussion of the reasonableness of permitting different likelihoods is deferred until Section V. At this point we note that the specification of the different likelihoods in assumption 4 introduces differential interpretation in a very simple fashion. The public signal is given by the liquidating value of the asset plus a random error, but agents disagree about the mean of the error. This causes agents to have different interpretations: one can interpret the signal more positively or negatively, or more optimistically or pessimistically, than the other. With the likelihoods in the model, the difference in mean beliefs given only the public signal is simply $\mu_1 - \mu_2$.

We assume that traders are naive in the sense that at time 1 they do not take account of the fact that at time 2 prices will be "wrong" because the other agents are updating their beliefs and trading using "incorrect" (different) likelihood functions. This can be interpreted as similar to a noisy rational expectations equilibrium in which the supply shock has infinite variance. In such a case, no inference from the first-period price takes place, and during the first period, traders have diffuse beliefs about the second-period price.[8]

[8] There is a second case in which each type knows the others' beliefs and likelihood functions and believes them to be incorrect. Each type can predict the future price conditional on the signal observed, believes that this price will typically be "incorrect"

A. First-Period Trade

Each trader maximizes the expected utility of third-period wealth with respect to the current trading strategy. Type i traders solve the problem

$$\max_{m_{i1}} E_{i1} \exp[-\lambda m_{i1}(X - P_1)],$$

where $E_{i1}(\cdot)$ denotes expectation with respect to X of a type i trader at time 1. The resulting demand is

$$m_{i1}(P_1) = (X_i - P_1)\frac{Z_i}{\lambda}.$$

The aggregate demands are $M_{11}(P_1) = \alpha m_{11}(P_1)$ and $M_{21}(P_1) = (1 - \alpha)m_{21}(P_1)$, respectively. The market-clearing price is

$$P_1^* = \frac{\hat{X}}{\hat{Z}}, \tag{2}$$

and the equilibrium holdings are

$$m_{11}^* = \frac{Z_1 Z_2 (1 - \alpha)(X_1 - X_2)}{\lambda \hat{Z}},$$

$$m_{21}^* = \frac{Z_1 Z_2 \alpha (X_2 - X_1)}{\lambda \hat{Z}}.$$

By assumption, $X_1 > X_2$ and the aggregate supply of securities is zero; therefore, the second type holds a short position, $m_{21}^* < 0$.

because agents of the other type are using the "wrong" likelihood, and chooses time 1 demands to exploit this "mispricing." If each agent knows the beliefs of the other type, trading in the second period is not affected, and the second-period demands and prices are the previously obtained functions of the beliefs about X. However, given their first-period beliefs and interpretation of the signal, traders can predict the second-period outcomes, and their first-period demand functions must reflect these predictions. The problem faced by the type i trader is

$$\max_{m_{i1}} E_{i1} \exp\{-\lambda[m_{i1}(P_2 - P_1) + m_{i2}(X - P_2)]\},$$

where $E_{i1}(\cdot)$ denotes the expectation with respect to X and ϵ of a type i agent at time 1. The first term is the profit realized in the second period from the sale of the first-period position; the second term is the profit from holding the second-period position until the third period. Notice that both X and $L = X + \epsilon$ are "integrated out," and the first-period demand is not conditioned on the second- and third-period realizations. It follows that the difference between the second- and first-period holdings can be represented as a linear function of the price change and that the volume equation can be written as $V = |\beta_0 + \beta_1 \Delta P^*|$, which has the same form as eq. (5) below. Tedious algebra shows that in this case both β_0 and β_1 are nonlinear functions of the underlying parameters and therefore are difficult to interpret. Nevertheless, it can be shown that β_0 is identically zero if the agents interpret information in the same way and therefore that the basic result still holds.

B. Second-Period Trade

After the signal L is observed, the beliefs are updated and trading resumes. The posterior beliefs of type i traders are represented by a normal density with mean Y_i:

$$Y_i \equiv E_{i2}(X|L) = \rho_i X_i + (1 - \rho_i)(L - \mu_i),$$

where $\rho_i \equiv Z_i/(Z_i + b_i)$ denotes the relative weight placed on the prior beliefs by the type i trader. The equilibrium is similar to that in the first period, with

$$P_2^* = \frac{\hat{X} + L\hat{b} - \hat{\mu}}{\hat{Z} + \hat{b}}. \tag{3}$$

When (2) and (3) are compared, it is clear that the price change between the two periods depends linearly on the signal L:

$$\Delta P^* \equiv P_2^* - P_1^* = \frac{\hat{b}(L - P_1^*) - \hat{\mu}}{\hat{Z} + \hat{b}}. \tag{4}$$

Calculating the change in the equilibrium holdings, we can show that it is linearly related to the price change. Recall that the aggregate supply of the risky security is assumed to be zero. Then the absolute value of the change in holdings by the type i traders represents the volume of trade following the announcement. Using V to denote the volume of trade, we get

$$V = |\beta_0 + \beta_1 \Delta P^*|, \tag{5}$$

where

$$\beta_0 \equiv \frac{\alpha(1 - \alpha)}{\lambda\hat{b}} b_1 b_2(\mu_2 - \mu_1)$$

and

$$\beta_1 \equiv \frac{\alpha(1 - \alpha)}{\lambda\hat{b}} (Z_2 b_1 - Z_1 b_2).$$

The first term inside the absolute value in equation (5) is proportional to the differences in means of the likelihood functions of the traders, and the coefficient β_1 depends on the precisions of the priors and the signals. If the likelihood functions were the same, then $\beta_0 = 0$ and $\beta_1 = [\alpha(1 - \alpha)/\lambda](Z_2 - Z_1)$, and the volume would be directly proportional to the absolute value of the price change and to the difference in the precisions of the priors. As long as β_0 is not identically zero, that is, as long as $\mu_1 \neq \mu_2$, our model implies that there will be positive volume around the announcement even when the value effect of the announcement is zero. This is inconsistent with the Kim and Verrecchia (1991*a*, 1991*b*) and Harris and Raviv (1993) models but is observed in the data.[9]

IV. Forecast Revisions

A. *Restrictions on Forecast Revisions*

The preceding model is consistent with the empirical evidence on the volume-return relation presented in Section II*A*. The key feature of the model is that agents are assumed to have different likelihood functions for interpreting public signals. For one group of agents, research analysts employed by stock brokerage firms, there exist direct measures of beliefs both before and after quarterly earnings announcements. A Bayesian model with identical likelihood functions across agents places certain constraints on the relation between the prior and posterior beliefs. This section examines the nature of these constraints in a simple model. The setup is almost identical to the previous model with the exception that there are $N \geq 2$ agents.

We use X_i and Y_i to denote the prior and posterior means of the ith agent. With the normality assumptions the posterior mean is

$$Y_i = \rho_i X_i + (1 - \rho_i)(L - \mu_i),$$

where $\rho_i \equiv Z_i/(Z_i + b_i)$ is the weight placed on the prior mean in the updating process, L is the signal, and μ_i is the agent-specific interpretation of the signal. The same is true for any other agent j:

$$Y_j = \rho_j X_j + (1 - \rho_j)(L - \mu_j).$$

In what follows we assume that the only observable variables are the prior and the posterior means of the agents' beliefs, X_i and Y_i. We focus on the difference between our model and the standard one in which $\mu_i = \mu_j = 0$ for all agents, and we proceed by identifying differences between the models.

The following propositions are obvious if we assume that agents have identical likelihood functions, that is, that $\mu_i = \mu_j \equiv \mu$.[10] Without loss of generality, we assume that $X_i > X_j$.

[9] The coefficient in the Kim and Verrecchia models corresponds to the difference in the precisions of initial beliefs. Their result would be obtained if both types believed that L has the same bias, i.e., $\mu_1 = \mu_2$, even though the precisions might be different. Therefore, we cannot distinguish between the model with identical likelihood functions and the model in which the likelihood functions differ only in the precisions. In both cases, the relative weight put on the prior, ρ_i, is agent specific.

[10] These propositions depend on the normality assumption. However, the mode and the median satisfy the stated conditions for a large class of distributions. We are grateful to Marshall Freimer for pointing this out to us.

PROPOSITION 1. If $\mu_i = \mu_j = \mu$ and the difference between the public signal and its mean μ falls between the means of prior beliefs, then the following restrictions must be satisfied. (i) The distance between the posterior means cannot exceed that between the priors. That is, if $X_i > L - \mu > X_j$, then $Y_i - Y_j < X_i - X_j$. (ii) The relative pessimist cannot turn into the optimist. That is, if $X_i > L - \mu > X_j$, then $X_i > X_j \Rightarrow Y_i > Y_j$.

Application of this result depends crucially on the observability of the mean μ of the public signal. Below we use stock brokerage analysts' forecasts of annual earnings and interpret quarterly earnings as public signals about annual earnings. In this context it does not seem reasonable to assume that the econometrician knows the means of the biases in these forecasts. We therefore require a result that does not depend on the value of μ.

PROPOSITION 2. If $\mu_i = \mu_j = \mu$, then the posterior means either must be revised in the same direction or must get closer together with the sign of the difference in means unchanged. That is, either $\mathrm{sgn}(Y_i - X_i) = \mathrm{sgn}(Y_j - X_j)$ or $X_i > Y_i > Y_j > X_j$.

This proposition implies that for pairs of forecast revisions in which agents' means move in different directions, a "flip" or "divergence" will never be observed if agents have identical likelihoods. More precisely, we use "flip" to refer to a pair of forecasts such that

$$\mathrm{sgn}(Y_i - X_i) \neq \mathrm{sgn}(Y_j - X_j), \quad X_i > X_j, Y_i < Y_j, \tag{6}$$

and use "divergence" to refer to a pair of forecasts such that

$$\mathrm{sgn}(Y_i - X_i) \neq \mathrm{sgn}(Y_j - X_j), \quad |Y_j - Y_i| > |X_j - X_i|. \tag{7}$$

Propositions 1 and 2 need not hold if $\mu_i \neq \mu_j$ for $i \neq j$. That is, flips and divergences may be observed if agents have different likelihood functions. In the remainder of this section we use data on analyst forecasts to look for flips and divergences around quarterly earnings announcements.[11]

B. *Data Description*

Our empirical analysis requires that we observe the changes in expectations of individual economic agents around public information releases. We examine changes in the earnings forecasts of individual stock brokerage research analysts taken from the Institutional Brokers Estimate System maintained by I/B/E/S Inc. (the I/B/E/S Detail Tape) around quarterly earnings announcements. The version of the

[11] The revisions of analysts' forecasts of Apple's earnings discussed in n. 1 are examples of divergences.

data set we use contains the earnings forecasts of most analysts employed by major U.S. brokerage firms, the name (and CUSIP) of the firm whose earnings are being forecast, the date I/B/E/S received the forecast, coded analyst identifiers that allow the forecasts of individual analysts to be tracked over time, and other information for the period 1983–91. With these data it is possible to follow changes in individual analysts' forecasts around quarterly earnings announcements. We take the quarterly earnings announcement dates from the Compustat Industrial quarterly file.

We restrict attention to firms that are followed by 10 or more analysts at some time during the period 1983–91 and use only forecasts of the current fiscal year's earnings around the quarterly earnings announcements for the first, second, and third quarters. Many of the firms for which earnings forecasts appear on the tape are followed by only one or a few analysts. While these firms constitute a large fraction of the followed firms, they account for a much smaller fraction of the forecasts on the tape. Moreover, they are less actively followed. Our analysis requires that we observe updates in relatively short windows before the earnings announcement. Partly for this reason and partly because data storage limitations make it extremely costly to analyze the entire I/B/E/S Detail Tape, we use only firms that are followed by 10 or more analysts at some time. There are 2,131 such firms, of which 988 have quarterly earnings announcement dates in the Compustat file we use.

We examine only forecasts of the current fiscal year's earnings because these forecasts seem to be the focus of analysts' attention. While many analysts also report forecasts of the next fiscal year's earnings and long-term (5-year) earnings growth forecasts, these additional forecasts are updated less frequently. Finally, following O'Brien (1988), we screen for obvious data entry errors by deleting newly entered forecasts for which the absolute value of the change from the previous forecast was greater than or equal to $10.

Our procedure involves comparing the frequency of inconsistencies in the forecast revisions of analysts around quarterly earnings announcements to the frequency of inconsistencies in the forecast revisions of analysts during other (nonannouncement) periods. To accomplish this, for each firm/announcement pair we define four windows. The preannouncement window (A1) consists of a short period of time just before a quarterly earnings announcement, and the postannouncement window (A2) is a period of time immediately following the announcement. The first nonannouncement window (NA1) consists of a relatively short period of time beginning either immediately or somewhat after a quarterly earnings announcement, and the second nonannouncement window (NA2) follows the first

TABLE 3

WINDOWS USED IN COMPARISONS OF THE FREQUENCIES OF FLIPS AND DIVERGENCES

COMPARISON	ANNOUNCEMENT SAMPLES		NONANNOUNCEMENT SAMPLES	
	Window 1 (A1)	Window 2 (A2)	Window 1 (NA1)	Window 2 (NA2)
1	days −13–0	days 1–28	days 29–42	days 43–70
2	days −27–0	days 1–56	days 1–28	days 29–84
3	days −41–0	days 1–42	days 1–42	days 43–84

NOTE.—Days are measured in calendar days relative to the interim earnings announcement date, which is day 0.

nonannouncement window. The lengths of the two nonannouncement windows NA1 and NA2 are equal to the lengths of the pre- and postannouncement windows A1 and A2, respectively. The "announcement sample" consists of those firm/announcement pairs for which at least two analysts provide new forecasts in the preannouncement window (A1), and the "nonannouncement sample" consists of those firm/announcement pairs for which at least two analysts provide new forecasts in the first nonannouncement window (NA1). We require that at least two analysts provide forecasts in these windows because there can be no inconsistencies if only one analyst reports a forecast. We consider only new forecasts because we are interested in forecast revisions due to the quarterly earnings announcement and not those due to information that has accumulated during a long period prior to the announcement. For both samples we require that there be at least one subsequent forecast by the analyst on the tape for this firm.

There is an element of arbitrariness in selecting the windows, and we perform three different comparisons using windows of different lengths to examine the robustness of the results to the choice of windows. The windows for the three comparisons are shown in table 3. Most of the results presented pertain to the first comparison, in which the preannouncement window A1 is the 2 weeks before the quarterly earnings announcement date, the postannouncement window A2 is the 4 weeks following the announcement, the first nonannouncement window NA1 is the 2 weeks beginning 4 weeks after the announcement, and the second nonannouncement window is the next 4 weeks. These windows are short enough that changes in forecasts between A1 and A2 can plausibly be attributed to the quarterly earnings announcement. A drawback is that relatively few analysts report new updates in the 2 weeks just prior to the earnings announcement, or for that matter in any 2-week period. For this reason we also report results for comparisons 2 and 3 in which we include all analysts who report new forecasts in 4- and 6-week windows prior to the quarterly earnings announcement and use 8- and 6-week postannouncement windows, respectively. With the choices of windows in comparisons 2 and 3, the nonannouncement windows cover 12-week periods between announcements, including the dates immediately following one announcement and (almost) immediately preceding the next.[12]

C. *Procedure*

We showed above in proposition 2 that if agents have the same likelihoods, their beliefs following a public signal must be revised in such a way that there are no flips or divergences, as defined by proposition 2. Flips and divergences may be found if agents have different likelihoods. One difficulty we face is that while differential interpretation of the public signal will cause some inconsistencies, we expect that typically the public signal will cause forecast revisions to move in the same direction. This prevents us from basing our conclusions on the "average" or "typical" forecast revision. Instead, we focus on events that models with identical likelihoods say will never occur.

For the ith firm/announcement pair with at least two analysts reporting new forecasts in window A1 (hereafter, the ith announcement sample event), let Θ_i^a denote the set of analysts who provide new forecasts in window A1, and let A_i^a denote the number of analysts in Θ_i^a. For each analyst in Θ_i^a, we examine the last forecast available in window A1 and the first forecast available in A2, and we assume that the first forecast in A2 reflects the content of the announcement. If the analyst does not report a new forecast during A2, we assume that the quarterly announcement gave the analyst no reason to update his forecast, and we use the last forecast prior to the announcement as the postannouncement forecast.[13] Below we refer to these as

[12] We use windows that cover 12- rather than 13-week periods because the quarterly earnings announcement dates are not always exactly 13 weeks apart, and nonannouncement windows that covered 13 weeks would often include the next announcement. One cost of this choice is that the windows we use typically miss 1 week prior to the next announcement.

[13] This seems to be the interpretation that the analysts intend, for they do not explicitly confirm previous forecasts but report new forecasts to I/B/E/S only when their forecasts have changed. (While we do not know that they *never* explicitly confirm previous forecasts, we have not discovered a case.) Certainly vacations and other periods of inattention can exceed the length of windows A2 and NA2 (4 weeks in comparison 1), so that an analyst's failure to revise his forecast does not guarantee that the mean or mode of his beliefs has not changed. However, because we identify inconsistencies in the forecast revisions of a pair of analysts only when both analysts revise their forecasts, this implies that we shall identify fewer inconsistencies than actually occur. This bias is ameliorated by the longer windows we use in comparisons 2 and 3.

"implicit" updates. Then for each possible pair of analysts that can be formed from analysts in Θ_i^a, we check whether the pre- and postannouncement forecasts involve flips or divergences and divide the number of flips, divergences, and inconsistencies (flips or divergences) by the number of possible pairs to obtain the proportion of pairwise comparisons that are inconsistent with proposition 2. Specifically, let $P(\Theta_i^a) = \binom{A_i^a}{2}$ denote the number of pairs of analysts that can be formed from the analysts in Θ_i^a, F_i^a the number of flips, D_i^a the number of divergences, and G_i^a the number of inconsistencies. For each firm in the announcement sample we compute the ratios

$$f_i^a = \frac{F_i^a}{P(\Theta_i^a)}, \quad d_i^a = \frac{D_i^a}{P(\Theta_i^a)}, \quad g_i^a = \frac{G_i^a}{P(\Theta_i^a)}$$

and compute the averages across announcement sample events:

$$\bar{f}^a = \frac{\sum_{i=1}^{N^a} f_i^a}{N^a}, \quad \bar{d}^a = \frac{\sum_{i=1}^{N^a} d_i^a}{N^a}, \quad \bar{g}^a = \frac{\sum_{i=1}^{N^a} g_i^a}{N^a},$$

where N^a is the number of announcement sample events. A literal interpretation of our model together with the restriction $\mu_i = \mu_j$ for all i, j says that the proportions of pairwise comparisons with flips, divergences, and inconsistencies, and therefore the averages, are all zero.

Even if the restriction $\mu_i = \mu_j$ is satisfied, the ratios may not all be zero because some of the analysts may acquire private information during the period surrounding the quarterly earnings announcement. Therefore, some of the forecast revisions reflect private information in addition to the public signal, which can account for pairs of analysts with forecasts that violate proposition 2. In addition, some of the apparent inconsistencies may be due to data entry errors.

Therefore, we compare these ratios for the announcement sample to similar ratios for the nonannouncement sample and base our conclusions on the differences in the ratios. Using nonannouncement periods to control for private information acquisition in the announcement period is justified if the rate of private information acquisition is the same during the announcement and nonannouncement periods. For the ith firm/announcement pair for which at least two analysts report new forecasts in the first nonannouncement window NA1, we compute ratios

$$f_i^{na} = \frac{F_i^{na}}{P(\Theta_i^{na})}, \quad d_i^{na} = \frac{D_i^{na}}{P(\Theta_i^{na})}, \quad g_i^{na} = \frac{G_i^{na}}{P(\Theta_i^{na})},$$

analogous to the ratios for the announcement sample events, and compute the averages across events:

$$\bar{f}^{na} = \frac{\sum_{i=1}^{N^{na}} f_i^{na}}{N^{na}}, \quad \bar{d}^{na} = \frac{\sum_{i=1}^{N^{na}} d_i^{na}}{N^{na}}, \quad \bar{g}^{na} = \frac{\sum_{i=1}^{N^{na}} g_i^{na}}{N^{na}},$$

where N^{na} is the number of nonannouncement sample events. The differences $\bar{f}^a - \bar{f}^{na}$, $\bar{d}^a - \bar{d}^{na}$, and $\bar{g}^a - \bar{g}^{na}$ are measures of the extent to which the changes in beliefs are consistent with the restrictions presented above.

The announcement and postannouncement averages $\bar{f}^a$, $\bar{d}^a$, $\bar{g}^a$ and $\bar{f}^{na}$, $\bar{d}^{na}$, $\bar{g}^{na}$ are averages over different sets of events because the availability of two new forecasts during the announcement window A1 does not guarantee the availability of two new forecasts during the first nonannouncement window NA1, and vice versa. It is conceivable that the firms for which analysts report new forecasts during the window A1 differ systematically from those firms for which analysts report new forecasts during the window NA1. To eliminate the possibility that this biases our conclusions, we compute the differences in proportions $f_i^a - f_i^{na}$, $d_i^a - d_i^{na}$, $g_i^a - g_i^{na}$ for all events that appear in both the announcement and postannouncement samples, and we compute the average differences:

$$\bar{f}^d = \frac{\sum_{i=1}^{N} f_i^a - f_i^{na}}{N}, \quad \bar{d}^d = \frac{\sum_{i=1}^{N} d_i^a - d_i^{na}}{N}, \quad \bar{g}^d = \frac{\sum_{i=1}^{N} g_i^a - g_i^{na}}{N}.$$

Here N is the number of events that appear in both the announcement and nonannouncement samples.

The interpretation of the results in this section turns on the assumption that analysts who do not change their forecasts have implicitly confirmed them. As discussed in note 13, this seems to be the interpretation that the analysts intend, and this assumption underlies our interpretation of the results.[14] A limitation of interpreting ana-

[14] We considered, and rejected, using only the forecasts of analysts who provided new forecasts in both A1 and A2 and NA1 and NA2. The problem with this subsample is that new forecasts in NA2 will tend to come from analysts who acquired nontrivial private information. For example, suppose that there are no public information releases during the nonannouncement period. Then analysts will report new forecasts only if they acquire new private information. If the new forecasts are due to the acquisition of new private information, there is no reason to expect that forecast revisions will be in the same direction. In contrast, analysts will report a new forecast during the second announcement window A2 if they acquire new private information or if the quarterly earnings announcement causes them to revise their beliefs. While

lysts' failure to provide new forecasts during the windows A2 and NA2 as implicit confirmation of their previous forecasts is that some analysts may fail to change their forecasts because they incur costs in recalculating and restating their beliefs. This causes the average proportion of inconsistencies to be biased downward for both the announcement and nonannouncement samples. The differences will be biased upward (i.e., in favor of finding inconsistencies) if these costs are larger during the nonannouncement periods, though there is no reason to think that this is the case. Regardless, any problem is reduced by computing the mean proportions and mean differences in proportions, using the longer windows in comparisons 2 and 3.

The ratios described measure the proportions of pairwise comparisons with inconsistencies out of all pairwise comparisons that can be formed from analysts who provided forecasts in A1 or NA1, regardless of whether the analysts provided new forecasts in A2 or NA2. Most analysts who provide new forecasts in A1 or NA1 do not provide new forecasts in A2 or NA2, and we are able to identify inconsistencies only among those who do. An alternative measure of the frequency of inconsistencies is to restrict attention to the subsamples of analysts who provide forecasts in both A1 and A2 and NA1 and NA2. Below we refer to this as the sample of "explicit" updates.

Specifically, for the ith announcement sample event, let Λ_i^a denote the set of analysts who provide new forecasts in both A1 and A2, let B_i^a denote the number of analysts in Λ_i^a, and let $P(\Lambda_i^a) = \binom{B_i^a}{2}$ denote the number of pairs of analysts that can be formed from the analysts in Λ_i^a. For each firm in the explicit subsample, we compute the ratios $F_i^a/P(\Lambda_i^a)$, $D_i^a/P(\Lambda_i^a)$, and $G_i^a/P(\Lambda_i^a)$ and then compute the averages across explicit subsample events as above. These ratios are alternative measures of the frequency of inconsistencies.

D. *Results*

Explicit Updates

The first comparison in table 4 shows averages of the proportions of pairwise comparisons with flips, divergences, and inconsistencies of either type using only analysts who provide new forecasts in both A1 and A2 (explicit updates). For the first comparison, A1 is the 2-week window just prior to the announcement, and A2 is the 4-week window following the announcement. Standard errors and the numbers of

differential interpretation of the public signal will cause some inconsistencies, we expect that typically the public signal will cause forecast revisions to move in the same direction. This implies that the average proportion of inconsistencies will be lower in the announcement sample even if analysts interpret public signals differently.

TABLE 4

AVERAGE PROPORTIONS OF EXPLICIT FORECAST REVISIONS

Sample	All Analyst Pairs			Analyst Pairs with Small Changes in Average Forecast			Analyst Pairs with Forecast Revisions of Opposite Sign		
	Flips (1)	Divergences (2)	Inconsistencies (3)	Flips (4)	Divergences (5)	Inconsistencies (6)	Flips (7)	Divergences (8)	Inconsistencies (9)
	Comparison 1								
Announcement	.0758	.0917	.1335	.1129	.2055	.2821	.3490	.4098	.6007
	(.0053)	(.0058)	(.0070)	(.0107)	(.0133)	(.0150)	(.0183)	(.0177)	(.0188)
	[1,822]	[1,822]	[1,822]	[740]	[740]	[740]	[537]	[537]	[537]
Nonannouncement	.1206	.1379	.1995	.1849	.2846	.3837	.4350	.4692	.6997
	(.0142)	(.0153)	(.0176)	(.0285)	(.0336)	(.0360)	(.0384)	(.0380)	(.0356)
	[447]	[447]	[447]	[169]	[169]	[169]	[149]	[149]	[149]
	Comparison 2								
Announcement	.0982	.1209	.1707	.1298	.2525	.3281	.3776	.4555	.6505
	(.0032)	(.0035)	(.0041)	(.0056)	(.0071)	(.0077)	(.0087)	(.0082)	(.0084)
	[5,629]	[5,629]	[5,629]	[2,780]	[2,780]	[2,780]	[2,235]	[2,235]	[2,235]
Nonannouncement	.1274	.1417	.2072	.1483	.2603	.3467	.4224	.4606	.6764
	(.0032)	(.0034)	(.0040)	(.0051)	(.0062)	(.0068)	(.0074)	(.0070)	(.0069)
	[6,493]	[6,493]	[6,493]	[3,626]	[3,626]	[3,626]	[3,103]	[3,103]	[3,103]
	Comparison 3								
Announcement	.0924	.1139	.1605	.1187	.2389	.3081	.3645	.4457	.6305
	(.0028)	(.0030)	(.0035)	(.0047)	(.0060)	(.0066)	(.0076)	(.0073)	(.0075)
	[6,879]	[6,879]	[6,879]	[3,508]	[3,508]	[3,508]	[2,761]	[2,761]	[2,761]
Nonannouncement	.1211	.1350	.2003	.1399	.2557	.3392	.4095	.4476	.6656
	(.0032)	(.0033)	(.0039)	(.0049)	(.0062)	(.0068)	(.0075)	(.0071)	(.0071)
	[6,388]	[6,388]	[6,388]	[3,569]	[3,569]	[3,569]	[2,997]	[2,997]	[2,997]

NOTE.—The table displays the means (across events) of the proportions of pairwise comparisons that involve inconsistencies of the indicated type. Standard errors are in parentheses, and the numbers of observations (events) are in brackets.

observations are in parentheses and brackets, respectively. The proportion of events that are either flips or divergences is not equal to the sum of the proportions of flips and divergences because a single observation may be both a flip and a divergence.

For the sample of all analyst pairs (cols. 1–3), the mean proportions of flips, divergences, and inconsistencies are .0758, .0917, and .1335, respectively. These mean proportions, and in fact all the mean proportions in the table, are large relative to their standard errors. These sample means are likely to be upward-biased estimates of the proportions of pairwise comparisons with inconsistencies due to differences in interpretation of the public signal because some of the inconsistencies will be due to analysts' acquisition of private information between the dates of their two forecasts. While the magnitude of the bias is limited by the short lengths of the windows used in this comparison, some bias almost surely exists. Regardless, these mean proportions seem large.

The results for the subsample of analyst pairs with small changes in average forecast (cols. 4–6) show the average proportions of pairwise comparisons that are flips, divergences, and inconsistencies for subsamples consisting of analyst pairs for which the mean of the two analysts' forecasts for A2 is within the range of the two analysts' forecasts for A1. This subsample therefore excludes public announcements that were major surprises and also screens out certain data entry errors. We are interested in the subsample that excludes major surprises because analysts' forecasts will typically all move in the same direction following a major surprise, and we would fail to observe any inconsistencies even though there may be important differences in interpretation. The mean proportions of flips, divergences, and inconsistencies can be interpreted as the proportions of inconsistencies among those comparisons for which we are likely to find inconsistencies. Again, the mean proportions of flips, divergences, and inconsistencies of .1129, .2055, and .2821 seem large. Finally, the results for analyst pairs with forecast revisions of opposite sign (cols. 7–9) show the average proportions of pairwise comparisons that are flips, divergences, and inconsistencies for the subsample consisting of analyst pairs for which the analysts revised their forecasts in different directions.

For completeness, we also provide the mean proportions of pairwise comparisons with inconsistent revisions for the nonannouncement sample using only analysts who provide new forecasts in both NA1 and NA2. Unfortunately, this explicit nonannouncement sample does not provide a control because new forecasts in NA2 will tend to come from analysts who acquired nontrivial private information. For example, suppose that there are no public information releases during the nonannouncement period. Then analysts will report new forecasts only if they acquire new private information. If the new forecasts are due to the acquisition of new private information, there is no reason to expect that forecast revisions will be in the same direction. In contrast, analysts will report a new forecast during the second announcement window A2 if they acquire new private information or if the quarterly earnings announcement causes them to revise their beliefs. While differential interpretation of the public signal will cause some inconsistencies, we expect that typically the public signal will cause forecast revisions to move in the same direction. This implies that the average proportion of inconsistencies will be lower in the announcement sample even if analysts do interpret public signals differently.

Implicit Updates

The first comparison in table 5 shows the averages (across events) of the proportions of pairwise comparisons with inconsistent revisions of the sample of implicit updates. The results for the sample of all analyst pairs (cols. 1–3) show the mean proportions of pairwise comparisons that are flips, divergences, and inconsistencies of either type for both the announcement and nonannouncement samples and also the average difference in proportions when only events that appear in both samples are used. Again, the proportion of events that are either flips or divergences is not equal to the sum of the proportions of flips and divergences because a single observation may be both a flip and a divergence. The mean proportions of flips, divergences, and inconsistencies of either type for the sample are between four and five times greater than the corresponding means for the nonannouncement sample, and the mean differences in proportions are positive and between $4\frac{2}{3}$ and six times as large as their standard errors. While the differences in proportions are not normally distributed, the mean differences are asymptotically normal, and the sample sizes seem large enough that we may safely rely on the asymptotic theory and standard errors.

Columns 4–6 of table 5 show the average proportions of pairwise comparisons that are flips, divergences, and inconsistencies for subsamples consisting of analyst pairs for which the mean of the two analysts' forecasts for A2 (NA2) is within the range of the two analysts' forecasts for A1 (NA1). These subsamples therefore exclude public announcements that were major surprises and also screen out certain data entry errors. Again, the mean proportions of flips, divergences, and inconsistencies of either type for the sample are between four and five times greater than the corresponding means for the control,

TABLE 5

AVERAGE PROPORTIONS OF IMPLICIT FORECAST REVISIONS

Sample	All Analyst Pairs			Analyst Pairs with Small Changes in Average Forecast		
	Flips (1)	Divergences (2)	Inconsistencies (3)	Flips (4)	Divergences (5)	Inconsistencies (6)
	Comparison 1					
Announcement	.0049	.0060	.0086	.0034	.0077	.0100
	(.0005)	(.0006)	(.0007)	(.0005)	(.0008)	(.0009)
	[7,864]	[7,864]	[7,864]	[6,641]	[6,641]	[6,641]
Nonannouncement	.0012	.0014	.0020	.0009	.0016	.0021
	(.0002)	(.0003)	(.0003)	(.0002)	(.0004)	(.0004)
	[8,306]	[8,306]	[8,306]	[7,545]	[7,545]	[7,545]
Difference	.0048	.0042	.0069	.0027	.0059	.0079
	(.0009)	(.0009)	(.0011)	(.0009)	(.0013)	(.0015)
	[4,147]	[4,147]	[4,147]	[3,291]	[3,291]	[3,291]

	Comparison 2					
Announcement	.0154	.0180	.0258	.0134	.0254	.0326
	(.0007)	(.0007)	(.0009)	(.0008)	(.0011)	(.0013)
	[11,973]	[11,973]	[11,973]	[10,032]	[10,032]	[10,032]
Nonannouncement	.0094	.0099	.0152	.0067	.0126	.0163
	(.0004)	(.0004)	(.0005)	(.0004)	(.0006)	(.0007)
	[14,906]	[14,906]	[14,906]	[13,866]	[13,866]	[13,866]
Difference	.0076	.0103	.0139	.0093	.0172	.0224
	(.0010)	(.0010)	(.0012)	(.0012)	(.0016)	(.0018)
	[9,926]	[9,926]	[9,926]	[7,790]	[7,790]	[7,790]
	Comparison 3					
Announcement	.0096	.0111	.0162	.0074	.0139	.0183
	(.0004)	(.0005)	(.0006)	(.0005)	(.0007)	(.0008)
	[15,103]	[15,103]	[15,103]	[13,608]	[13,608]	[13,608]
Nonannouncement	.0053	.0059	.0086	.0033	.0070	.0087
	(.0002)	(.0003)	(.0003)	(.0002)	(.0004)	(.0004)
	[16,323]	[16,323]	[16,323]	[15,614]	[15,614]	[15,614]
Difference	.0068	.0078	.0115	.0069	.0115	.0159
	(.0013)	(.0012)	(.0016)	(.0013)	(.0017)	(.0021)
	[3,868]	[3,868]	[3,868]	[3,239]	[3,239]	[3,239]

Note.—The table displays the means (across events) of the proportions of pairwise comparisons that involve inconsistencies of the indicated type and the mean differences in proportions. Standard errors are in parentheses, and the numbers of observations (events) are in brackets.

and the mean differences in proportions are positive and between three and five times as large as their standard errors.

It may be that the 4-week windows A2 and NA2 used in the first comparison are too short to justify the assumption that an analyst who does not explicitly update his forecast has implicitly confirmed it. Accordingly, we report results using longer windows in the second and third comparisons in table 5. Not surprisingly, the mean proportions of inconsistencies are larger for both the announcement and nonannouncement windows, presumably because the longer windows both allow busy or inattentive analysts more time in which to revise their forecasts and allow for more private information acquisition. More interesting, the mean differences in proportions are larger in comparisons 2 and 3 than in comparison 1; they are largest in comparison 2, in which the windows A2 and NA2 are longest (8 weeks). These results are highly significant, with the mean difference in proportions being between 7.6 and 12.4 times as large as the standard errors. The mean proportions for the announcement and nonannouncement samples are also highly significant, with each of the means at least 16 times as large as the corresponding standard error.

The mean proportions and mean differences in proportions all appear to be rather small. The mean proportions and mean differences in proportions are largest for comparison 2, but even here only 2.58 percent of the pairwise comparisons in the announcement sample involve inconsistencies of either type, and only 3.26 percent of the pairwise comparisons in the subsample of analyst pairs with small changes in the average forecast involve inconsistencies. In interpreting these numbers, one needs to keep in mind that these are events that models with identical likelihoods say are impossible. Even a relatively small number of such events is evidence against such models. Moreover, the small proportions are perfectly consistent with our model. Our model with differences in interpretation of public signals does not imply that public signals will always lead to flips and divergences, but only that they can. Even in our model with differences in interpretation, flips and divergences are extreme events. In fact, our model suggests that analysts' forecast revisions will typically have the same sign, and the mean proportions reported in the table seem reasonable.

Events with at Least One Inconsistency

An alternative measure of the frequency of inconsistencies is the proportion of events with at least one inconsistency. While we do not present detailed results, we found that the proportion of events with at least one inconsistency is about 20 percent for the events in which eight or more analysts provide forecasts (36 or more pairwise comparisons).

E. Magnitudes of the Inconsistencies

Tables 4 and 5 and the analysis of events with at least one inconsistency provide evidence that the number of inconsistencies in the analysts' forecast updates around the earnings announcements is large. Here we present evidence that the magnitude of the inconsistencies is also large.

We use the phrase "prior distance" to refer to the absolute value of the difference between the preannouncement (A1) forecasts of two analysts i and j; that is, the prior distance is $|X_i - X_j|$. Conditional on a divergence, the size of the divergence is defined to be the distance between the postannouncement forecasts made by the same pair of analysts less the prior distance, or $|Y_i - Y_j| - |X_i - X_j|$. The size of a flip is defined to be the distance between the posterior forecasts conditional on a flip. This measure understates the economic significance of the flip, because the larger the prior distance, the more significant a flip of a given size. An alternative measure of a flip is the sum of the prior distance and the size defined above.

Table 6 presents the means of the prior distances and size measures across events in which the respective inconsistencies took place for the three sets of windows (three comparisons) we use. The average size of a divergence is between 40 and 56 cents, depending on the choice of windows; the size of the flips is between 31 and 39 cents. The alternative measure of the size of a flip, which is the sum of the size and the prior distance, ranges between 72 and 85 cents. These measures indicate that the average inconsistency is more than 10 percent of the average earnings forecast in our sample, which is $3.55.

We also measure the size of a divergence or flip relative to the analysts' forecasts by dividing the size measures by the averages of the two analysts' preannouncement forecasts. The averages of these relative size measures are also presented in table 5 in columns 3 and 6. When the average of the preannouncement forecasts is negative, we use its absolute value; when the absolute value of the average of the preannouncement forecasts is less than one, we set the denominator in the ratio calculation equal to one. This causes the mean ratios reported in table 6 to understate the magnitudes of the inconsistencies. The mean ratios reported in the table are between 12 and 22 percent of the average preannouncement earnings forecasts. Our interpretation of the mean ratios and the means of the size measures discussed above is that the inconsistencies are large.

TABLE 6

MEANS OF THE ABSOLUTE VALUES OF FLIPS AND DIVERGENCES

Sample	Divergences: Prior Difference (1)	Divergences: Size (2)	Divergences: Ratio (3)	Flips: Prior Difference (4)	Flips: Size (5)	Flips: Ratio (6)
	Comparison 1					
Announcement	.24	.4	.16	.41	.31	.12
	(.03)	(.03)	(.02)	(.03)	(.02)	(.01)
	[247]	[247]	[247]	[309]	[309]	[309]
Nonannouncement	.23	.4	.16	.37	.38	.15
	(.04)	(.09)	(.03)	(.05)	(.09)	(.04)
	[73]	[73]	[73]	[80]	[80]	[80]
	Comparison 2					
Announcement	.21	.43	.18	.4	.34	.14
	(.01)	(.02)	(.01)	(.01)	(.01)	(.01)
	[1,231]	[1,231]	[1,231]	[1,528]	[1,528]	[1,528]
Nonannouncement	.22	.46	.18	.38	.39	.15
	(.01)	(.02)	(.01)	(.01)	(.01)	(.01)
	[1,882]	[1,882]	[1,882]	[2,149]	[2,149]	[2,149]
	Comparison 3					
Announcement	.24	.56	.22	.46	.39	.15
	(.01)	(.01)	(.01)	(.01)	(.02)	(.004)
	[4,561]	[4,561]	[4,561]	[5,445]	[5,445]	[5,445]
Nonannouncement	.23	.54	.20	.42	.43	.17
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
	[5,340]	[5,340]	[5,340]	[5,631]	[5,631]	[5,631]

NOTE.—Standard errors are in parentheses, and numbers of observations are in brackets.

F. Possible Alternative Explanations

Trends in Information Production

One possible explanation of our results is that the rate of private information production declines throughout the firms' fiscal years. If this effect is sufficiently large, it could explain our results because the nonannouncement samples we use as comparisons are drawn from dates after the firms' quarterly earnings announcements and consist of forecast revisions that occur later in the firms' fiscal years. To check this, we disaggregated the sample by quarters and repeated the analysis for the quarterly subsamples. We found that the minimum (across quarters) of the mean proportions for the announcement sample is greater than the maximum (across quarters) of the mean proportions for the nonannouncement sample, indicating that our results cannot be explained by the hypothesis that the rate of production of private information declines throughout the firms' fiscal years.

Information Production Concentrated around Earnings Announcements

Another possibility is that private information production is concentrated around the earnings announcement dates, and the larger number of inconsistencies around the announcement dates reflects the greater likelihood of private information production during these periods. This potential problem is to some extent mitigated, but not eliminated, by using the longer windows in comparisons 2 and 3. Unfortunately, there seems to be no way to control completely for this potential problem. Specifically, imagine that private information production activities are concentrated in the period surrounding the quarterly earnings announcement. We assume that the first postannouncement forecast supplied by an analyst reflects the information in the announcement, the analyst's interpretation of that information, and any private information she discovered in the period since her last forecast. Under the hypothesis that private information production activities are concentrated in a short period surrounding the quarterly earnings announcement, there is no way to separate out the effects of the announcement and private information production using the analysts' forecast revisions alone, because there are no other forecasts that contain as much private information as the first forecast after the earnings announcement.

However, in Section II*B*, we discussed evidence that the unusual volume seems to be restricted to the 3-day announcement period. With the exception of the story advanced by Kim and Verrecchia (1994), it seems extremely unlikely that the production and exploitation of private information would be restricted to this 3-day period. As indicated above, Kim and Verrecchia assume that investors observe sums of the public signal and idiosyncratic signals that coincide with the public announcement, and they interpret the public signal as providing the opportunity for traders who specialize in making expert judgments about public information to do so. A difference between Kim and Verrecchia's model and ours is that in their model, "differential interpretation" is modeled by having agents receive these idiosyncratic private signals rather than by endowing them with different likelihoods. It is essential in Kim and Verrecchia that agents not know the idiosyncratic interpretations of others. This makes us think it unlikely that this model can explain the inconsistencies in analysts' forecast revisions that we find. News of analysts' forecast revisions on the Broad Tape is accompanied by brief summaries of their reasoning, and the reasons for forecast revisions are sometimes discussed in the financial press (see, e.g., Sease 1991; Fisher 1993). More detailed discussions of analysts' reasoning are available in their written reports, and brokerage firm research analysts are available for telephone conversations with large institutional investors. Our interpretation of this is that, to the extent that brokerage firm analyst activity consists of interpreting public information, their reasoning is generally available to other analysts.

"Mechanical" Flips

The fact that analysts forecast over several quarters implies that it is possible that "mechanical" flips might account for some of the inconsistencies we find. For example, suppose that one analyst forecasts annual earnings to be $5.00 per share, at quarterly intervals of $4.00, $0.50, $0.25, and $0.25, and a second analyst forecasts four quarterly intervals each of $0.50, for an annual forecast of $2.00. Now suppose that the first-quarter earnings come in at $1.00. A flip is possible even if both analysts interpret the information identically. For example, if they both agree that the first-quarter earnings will not affect future quarters' earnings, the first analyst will revise his forecast to $2.00 and the second will revise to $2.50, resulting in a flip. Examples of mechanical divergences can also be constructed. We think it unlikely that these mechanical flips explain our results, but we cannot rule out the possibility.

V. Conclusion

We have presented evidence on the volume-return relation around earnings announcements and argued that it is inconsistent with most existing models in which agents have identical interpretations of the public announcement. We argue that additional evidence on revisions of analysts' forecasts is also inconsistent with identical interpretations, though this argument is weaker in that our conclusions depend on our interpretation of "implicit" updates. One way to reconcile the evidence on the volume-return relation with identical interpretations of public signals is to argue that private information production is concentrated around the announcement, and we cannot rule out this possibility. In addition, we cannot rule out "mechanical" flips and divergences and the possibility that the flips and divergences we find are due to data errors. Regardless, our evidence does rule out a number of models and potential explanations and suggests that agents have differential interpretations of the public signals, that is, different likelihoods. The model in Section III yields the observed volume-return relation.

In our model we simply endow agents with different likelihoods. Is this reasonable? Most models with explicit information processing are guided by Aumann's (1976) argument that two agents for whom rationality is common knowledge cannot agree to disagree.[15] Several recent theoretical papers challenge this result. Kurz (1994) shows that even when the stochastic process generating the data is stationary, agents may disagree provided that they do not know *for sure* that the process is stationary. It seems unlikely that earnings, prices, and other relevant observables are jointly stationary, and even more unlikely that agents know for sure that they are. Neeman (1993, 1994) finds that agents can disagree when each assigns a small probability to the event that others are not rational. On a practical level, each individual is exposed to a different learning experience, including different home environments, schools, friends, mentors, colleagues, and other factors. The accumulated stock of information is too complex and diverse to be conveyed to others. This makes it impossible for agents to take full account of the information held by others.

We intend the different likelihood functions to correspond to different long-term beliefs about the way the economy operates. We refer to certain beliefs as "long-term" because models of the way the world works depend on certain paradigms (e.g., whether markets are best viewed as being typically in equilibrium or disequilibrium) that seem to change very slowly over time, if at all. An example of a short-term belief is the probability distribution of gross national product growth next year, whereas the long-term beliefs correspond to the model of the economy used to interpret the available data and determine the probability distribution of GNP growth. Because agents both possess different likelihood functions and have access to different information, short-term beliefs, or beliefs about the probabilities of particular events, also differ across agents.

An example that should be familiar follows. Consider two imaginary economists, one from Chicago and the other from Cambridge, Massachusetts. They are both well trained and know the same theories, the same econometric techniques, and Bayes's rule. They both

[15] The result depends crucially on the common knowledge assumption. "Intuitively, if player i did not know how player j arrived at his posterior beliefs, player i would not know how to evaluate the fact that player j's beliefs differed from his own" (Fudenberg and Tirole 1991, p. 549). Seemingly minor changes in this assumption can alter the equilibrium dramatically. For example, Geanakoplos and Polemarchakis (1982) assume that players can communicate only their posterior beliefs and are not able to convey their entire information sets. In this case, Aumann's result does not hold, though the posteriors eventually converge.

seek the "truth" and have access to the same data. Yet their posteriors are drastically different, and if asked to comment on the consequences of current economic policies, they would almost certainly offer different predictions. Such an outcome is impossible if they start with common priors and use the same likelihood functions to interpret the available data. We interpret their failure to agree as evidence that they interpret information using different models or that they have different likelihood functions. As long as there is no substantial evidence that would dramatically "surprise" one of them,[16] conversion is unlikely. If it does occur, it is likely to happen over a long period of time.[17]

In thinking about why our economists disagree, note that the problem they face is not to estimate a finite (though perhaps large) number of parameters that appear in a parametric representation of the economy, but rather to pick the model of the economy from the set of all possible functional relations that might characterize the world. In this setting, even with infinitely long time series of data, the beliefs of rational agents do not necessarily converge to a common posterior (see Diaconis and Freedman 1986). These results suggest that other factors may play a large role in our economists' models of the world and that data may cause little or no changes in the long-term beliefs.[18] Other reasons why agents' beliefs may be fixed over time are that the nature of uncertainty is such that information used to update these beliefs arrives very infrequently, and changing one's beliefs is costly. New beliefs must be understood to be compared to the old ones. This requires potentially significant, and sometimes infinite, investment.

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[16] This amounts to being able to reject one of the theories. In fact, Kuhn (1990) has argued that paradigmatic change often comes about only through the death or retirement of the adherents of the unsuccessful paradigm.

[17] An alternative interpretation of the economists' failure to agree is that they receive private benefits from disagreeing and that the views they express do not reflect their beliefs. We prefer to believe that our economists are intellectually honest.

[18] Note the multitude of explanations put forward to explain the stock market break of October 1987. The market break has been blamed on speculative bubbles, changes in fundamental values and in the regulatory environment, and computer-generated trading.

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