COMMONWEALTH OF KENTUCKY
BEFORE THE PUBLIC SERVICE COMMISSION

IN THE MATTER OF:	**)**	
	)	
THE APPLICATION OF KENTUCKY-AMERICAN WATER COMPANY FOR AN ADJUSTMENT OF RATES ON AND AFTER MARCH 28, 2010	**)))**	**CASE NO. 2010-00036**

NOTICE OF FILING

In accordance with the agreement reached between Kentucky-American Water Company ("KAWC") and the Attorney General relating to the waiver of cross-examination of the cost of capital expert witnesses at the evidentiary hearing in this matter, KAWC states that: (1) the average yield on Moody's A-rated long-term utility bonds for July 2010 was 5.26 percent; (2) the documents attached at Tab 1 are the workpapers and source documents related to Dr. Vander Weide's Rebuttal Testimony Schedule 2; and (3) the documents attached at Tab 2 are copies of the nine articles listed at Table 3 (page 22) of Dr. Vander Weide's Rebuttal Testimony.

Respectfully submitted,

STOLL KEENON OGDEN PLLC

300 West Vine Street, Suite 2100
Lexington, Kentucky 40507-1801
Telephone: (859) 231-3000
L.Ingram@skofirm.com

BY: Lindsey W. Ingram III
Lindsey W. Ingram III
Attorneys for Kentucky-American Water Company

CERTIFICATE

In accordance with Ordering Paragraph No. 6 of the Commission's February 16, 2010 Order, this is to certify that Kentucky-American Water Company's August 24, 2010 electronic filing is a true and accurate copy of the documents to be filed in paper medium; that the electronic filing has been transmitted to the Commission on August 24, 2010; that an original and one copy of the filing will be delivered to the Commission on August 25, 2010; and, that, on August 24, 2010, electronic mail notification of the electronic filing will be provided to the Commission and the following:

David Edward Spenard
Office of the Attorney General
1024 Capital Center Drive, Suite 200
Frankfort, Kentucky 40601
david.spenard@ag.ky.gov
dennis.howard@ag.ky.gov
heather.kash@ag.ky.gov

Iris G. Skidmore
Bates and Skidmore
415 West Main Street, Suite 2
Frankfort, Kentucky 40601
batesandskidmore@gmail.com

David J. Barberie
Leslye M. Bowman
LFUCG Department of Law
200 East Main Street
Lexington, Kentucky 40507
dbarberi@lfucg.com
lbowman@lfucg.com

STOLL KEENON OGDEN PLLC

By Lindsey W. Ingram III

Attorneys for Kentucky-American Water Company

Custom Report - Artesian Resources Corp (ARTNB)

Enter ticker: ARTNE Get Profile

Symbol	Last	Open	Change	% Change	Year High	Year Low	Last Trade
ARTNB	**NA**	NA	+NA	+NA%	NA	NA	2010-07-12 - Closed

Pricing/Earnings

Recent Price	18
P/E Ratio	NMF
P/E (Trailing)	19.355
P/E (Median)	NMF
Rel. P/E Ratio	1.187

Ratings*

Financial Strength	NMF
Stock's Price Stability	10
Price Growth Persistence	65
Earnings Predictability	90

% Annualized Return (EOM)

	This Stock	VL Arith. Index
1 yr	-2.614	29.614
3 yrs	-9.737	-2.954
5 yrs	-0.148	4.398

Value Line Ranks*

Performance:	3	(Raised - 06/11/2010)
Safety:	4	(Raised - 07/16/2010)
Technical:	3	(Lowered - 07/16/2010)
Industry:	91	(Water Utility)
BETA:	0.55	(1.00 = Market)

3 to 5 Year Projections

	Price	Gain	Ann'l Tot. Return
High	N/A	N/A	N/A
Low	N/A	N/A	N/A

**Data based on the latest 07/16/2010 issue.*

Business Profile

BUSINESS: Artesian Resources Corporation, through its subsidiaries, engages in the distribution and sale of water to residential, commercial, industrial, governmental, municipal, and utility customers in the state of Delaware. It also provides water for public and private fire protection to customers in its service territories. In addition, the company offers wastewater services, as well as designs and constructs wastewater facilities and infrastructure. As of December 31, 2006, Artesian Resources had approximately 73,800 metered customers and served a population of approximately 243,000. As of the above date, it served customers through approximately 1,050 miles of transmission and distribution mains. Has 198 employees. Chairman, C.E.O. & President: Dian C. Taylor . Inc.: DE. Address: 664 Churchmans Road, Newwark, DE 19702. Tel.: 302-453-6900. Internet: http://www.artesianwater.com

AMERICAN WATER NYSE-AWK

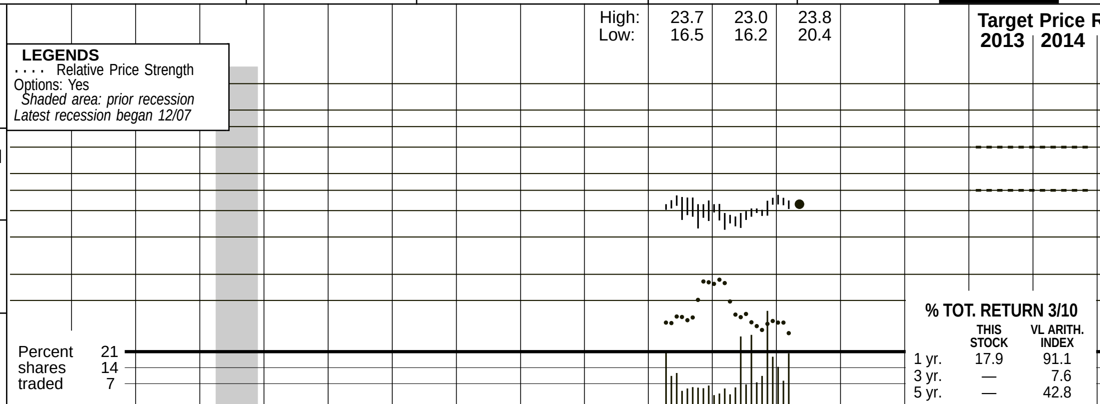
RECENT PRICE **21.48** | P/E RATIO **16.3** (Trailing: 17.3 Median: NMF) | RELATIVE P/E RATIO **0.90** | DIV'D YLD **3.9%** | VALUE LINE

TIMELINESS – E
SAFETY **3** New 7/25/08
TECHNICAL – E
BETA .65 (1.00 = Market)

	2008	2009	2010
High:	23.7	23.0	23.8
Low:	16.5	16.2	20.4

Target Price Range 2013 | 2014 | 2015

LEGENDS
.... Relative Price Strength
Options: Yes
Shaded area: prior recession
Latest recession began 12/07



2013-15 PROJECTIONS

	Price	Gain	Ann'l Total Return
High	40	(+85%)	20%
Low	25	(+15%)	9%

Insider Decisions

	M	J	J	A	S	O	N	D	J
to Buy	0	3	0	0	0	0	2	0	0
Options	0	0	0	0	0	0	0	0	0
to Sell	0	0	0	0	0	0	0	0	0

Institutional Decisions

	2Q2009	3Q2009	4Q2009
to Buy	137	152	178
to Sell	66	72	77
Hld's(000)	82903	119774	157474

Percent shares traded 21 14 7

% TOT. RETURN 3/10

	THIS STOCK	VL ARITH. INDEX
1 yr.	17.9	91.1
3 yr.	—	7.6
5 yr.	—	42.8

1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	© VALUE LINE PUB., INC.	13-15
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	13.08	13.84	14.61	13.98	*14.05*	*13.95*	Revenues per sh	*14.65*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	.65	d.47	2.87	2.89	*2.95*	*3.05*	"Cash Flow" per sh	*3.35*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	d.97	d2.14	1.10	1.25	*1.40*	*1.50*	Earnings per sh A	*1.70*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	.40	.82	*.86*	*.90*	Div'd Decl'd per sh B	*1.00*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	4.31	4.74	6.31	4.50	*4.30*	*4.25*	Cap'l Spending per sh	*4.20*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	23.86	28.39	25.64	22.91	*22.95*	*23.35*	Book Value per sh D	*24.40*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	160.00	160.00	160.00	174.63	*185.00*	*195.00*	Common Shs Outst'g C	*215.00*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	18.9	15.6	*Bold figures are*		Avg Ann'l P/E Ratio	*20.0*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	1.14	1.04	*Value Line*		Relative P/E Ratio	*1.35*
- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	- -	1.9%	4.2%	*estimates*		Avg Ann'l Div'd Yield	*3.1%*

CAPITAL STRUCTURE as of 12/31/09
Total Debt $5342.3 mill. **Due in 5 Yrs** $243.9 mill.
LT Debt $5288.2 mill. **LT Interest** $296.5 mill.
(Total interest coverage: 2.1x) (57% of Cap'l)

Leases, Uncapitalized: Annual rentals $29.0 mill.
Pension Assets-12/09 $695.5 mill
Oblig. $1128.2 mill.
Pfd Stock $24.2 mill. **Pfd Div'd** $2.0 mill.

Common Stock 174,670,026 shs.
as of 2/25/10

MARKET CAP: $3.8 billion (Mid Cap)

2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011		13-15
- -	- -	- -	- -	- -	- -	2093.1	2214.2	2336.9	2440.7	*2600*	*2725*	Revenues ($mill)	*3150*
- -	- -	- -	- -	- -	- -	d155.8	d342.3	187.2	209.9	*250*	*280*	Net Profit ($mill)	*350*
- -	- -	- -	- -	- -	- -	- -	- -	37.4%	37.9%	*38.5%*	*39.0%*	Income Tax Rate	*40.0%*
- -	- -	- -	- -	- -	- -	- -	- -	12.5%	10.0%	*10.0%*	*10.0%*	AFUDC % to Net Profit	*15.0%*
- -	- -	- -	- -	- -	- -	56.1%	50.9%	53.1%	56.9%	*55.5%*	*55.0%*	Long-Term Debt Ratio	*53.0%*
- -	- -	- -	- -	- -	- -	43.9%	49.1%	46.9%	43.1%	*44.5%*	*45.0%*	Common Equity Ratio	*47.0%*
- -	- -	- -	- -	- -	- -	8692.8	9245.7	8750.2	9289.0	*9635*	*10050*	Total Capital ($mill)	*11250*
- -	- -	- -	- -	- -	- -	8720.6	9318.0	9991.8	10524	*11050*	*11550*	Net Plant ($mill)	*13050*
- -	- -	- -	- -	- -	- -	NMF	NMF	3.7%	3.8%	*4.0%*	*4.0%*	Return on Total Cap'l	*4.5%*
- -	- -	- -	- -	- -	- -	NMF	NMF	4.6%	5.2%	*6.0%*	*6.0%*	Return on Shr. Equity	*6.5%*
- -	- -	- -	- -	- -	- -	NMF	NMF	4.6%	5.2%	*6.0%*	*6.0%*	Return on Com Equity	*6.5%*
- -	- -	- -	- -	- -	- -	NMF	NMF	3.0%	1.8%	*2.0%*	*2.0%*	Retained to Com Eq	*2.5%*
- -	- -	- -	- -	- -	- -	- -	- -	34%	65%	*62%*	*63%*	All Div'ds to Net Prof	*62%*

CURRENT POSITION ($MILL.)	2007	2008	12/31/09
Cash Assets	13.5	9.5	22.3
Other	416.9	408.2	476.8
Current Assets	430.4	417.7	499.1
Accts Payable	168.9	149.8	138.6
Debt Due	316.8	654.8	54.1
Other	288.8	300.2	414.7
Current Liab.	774.5	1104.8	607.4
Fix. Chg. Cov.	228%	198%	225%

ANNUAL RATES of change (per sh)	Past 10 Yrs.	Past 5 Yrs.	Est'd '07-'09 to '13-'15
Revenues	- -	- -	1.0%
"Cash Flow"	- -	- -	21.0%
Earnings	- -	- -	NMF
Dividends	- -	- -	39.0%
Book Value	- -	- -	-1.5%

Cal-endar	QUARTERLY REVENUES ($ mill.) A Mar.31	Jun. 30	Sep. 30	Dec. 31	Full Year
2007	468.6	558.7	633.1	553.8	2214.2
2008	506.8	589.4	672.2	568.5	2336.9
2009	550.2	612.7	680.0	597.8	2440.7
2010	*575*	*650*	*725*	*650*	*2600*
2011	*600*	*680*	*760*	*685*	*2725*

Cal-endar	EARNINGS PER SHARE A Mar.31	Jun. 30	Sep. 30	Dec. 31	Full Year
2007	.02	.31	d1.00	d1.47	d2.14
2008	.04	.28	.55	.23	1.10
2009	.19	.32	.52	.21	1.25
2010	*.19*	*.35*	*.57*	*.29*	*1.40*
2011	*.22*	*.37*	*.60*	*.31*	*1.50*

Cal-endar	QUARTERLY DIVIDENDS PAID B■ Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2006	- -	- -	- -	- -	- -
2007	- -	- -	- -	- -	- -
2008	- -	- -	.20	.20	.40
2009	.20	.20	.21	.21	.82
2010	.21				

BUSINESS: American Water Works Company, Inc. is the largest investor-owned water and wastewater utility in the U.S., providing services to over 15 million people in 32 states and Canada. Its non-regulated business assists municipalities and military bases with the maintenance and upkeep as well. Regulated operations made up almost 90% of 2008 revenues. New Jersey is its biggest market accounting for nearly 20% of revenues. Has roughly 7,300 employees. Depreciation rate, 2.1% in '08. RWE AG owns roughly 49% of common stock outstanding. Capital World Investors, 8%. Off. & dir. own less than 1%. President & CEO; Donald L. Correl. Chairman; George Mackenzie Jr. Address: 1025 Laurel Oak Road, Vorhees, NJ 08043. Telephone: 856-346-8200. Internet: www.amwater.com.

American Water Works disappointed in the final quarter of 2009. The water utility reported earnings of $0.21 a share in the December period, 9% short of last year's mark and 16% below our estimate. Favorable rate case rewards lifted revenues 5%, but growth was a little lighter than expected, with inclement weather conditions in most of the company's biggest markets resulting in a sharp volume decline. Meanwhile, operating and interest costs increased as did the share count. On another note, management provided earnings guidance for the first time, but failed to supply specifics about how it expected to achieve 7%-10% earnings growth. Wall Street appeared unsettled and AWK shares have fallen 6%-plus in value since our January review.

We suspect that management is being a bit cautious with its outlook. Weather in the fourth quarter was a significant hurdle, and a return to more normal conditions should be a major boon in 2010. Plus, the company has over $200 million in rate relief pending. Regulatory boards have been fairly favorable in recent memory, and we expect similarly constructive rulings to continue being handed down. Margins should benefit from these improvements too, enabling the company to come in at the high end of guidance and earn $1.40 a share this year.

Increasing infrastructure costs are threatening longer-term growth, however. Despite improved regulatory backing, maintenance expenses are likely to continue to eat away at profitability over time. Indeed, many of the nation's water systems are decaying and require significant investment. However, American does not have the funds on hand to keep up with these costs, and will have to continue looking to outside financiers to make the improvements. These initiatives, although necessary, will keep growth under wraps in 2011 and thereafter.

Most will want to take a pass on this issue. Although the stock's healthy stream of income makes it an appealing total return vehicle, its lack of trading history makes it a speculative selection. Indeed, AWK has yet to be assigned performance indicators, such as a Timeliness rank or Price Stability score.

Andre J. Costanza *April 23, 2010*

(A) Diluted earnings. Excludes nonrecurring gains (losses): '08, ($4.62); '09, ($2.63). Discontinued operations: '06, (4¢). Next earnings report due early May. Quarterly earnings may not sum due to rounding.
(B) Dividends to be paid in January, April, July, and October. ■ Div. reinvestment available.
(C) In millions.
(D) Includes intangibles. In 2009: $1.250 billion, $7.16/share.
(E) The stock has not been trading long enough to generate a Timeliness rank.

Company's Financial Strength	B
Stock's Price Stability	80
Price Growth Persistence	NMF
Earnings Predictability	NMF

AMER. STATES WATER NYSE-AWR

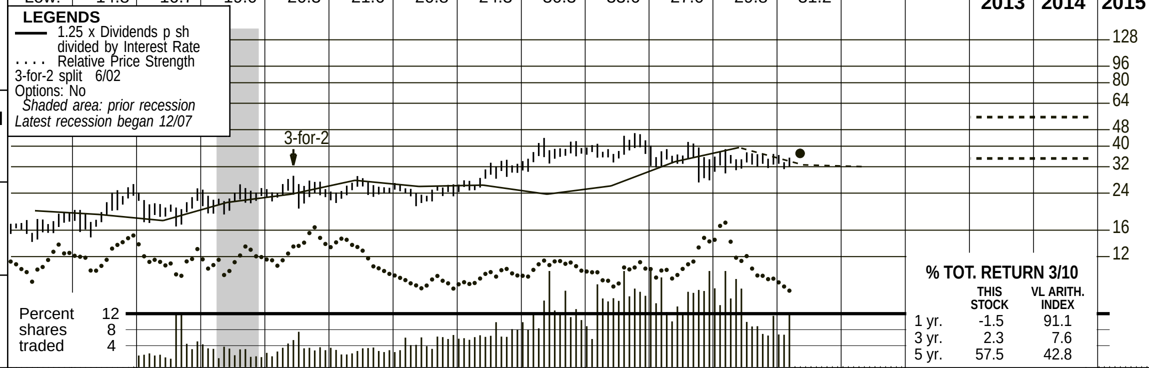
RECENT PRICE	P/E RATIO	RELATIVE P/E RATIO	DIV'D YLD
37.04	23.6 (Trailing: 22.9, Median: 22.0)	1.30	2.8%

VALUE LINE

TIMELINESS **4** Lowered 3/19/10
SAFETY **3** New 2/4/00
TECHNICAL **3** Lowered 4/23/10
BETA .80 (1.00 = Market)

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
High:	26.5	25.3	26.4	29.0	29.0	26.8	34.6	43.8	46.1	42.0	38.8	38.2
Low:	14.8	16.7	19.0	20.3	21.6	20.8	24.3	30.3	33.6	27.0	29.8	31.2

Target Price Range
2013 | 2014 | 2015



2013-15 PROJECTIONS

	Price	Gain	Ann'l Total Return
High	55	(+50%)	13%
Low	35	(-5%)	2%

Insider Decisions

	M	J	J	A	S	O	N	D	J
to Buy	0	0	0	0	0	0	1	1	0
Options	0	0	0	0	0	0	0	4	0
to Sell	0	0	0	1	0	0	0	4	0

Institutional Decisions

	2Q2009	3Q2009	4Q2009
to Buy	66	54	57
to Sell	53	53	39
Hld's(000)	10578	10847	11007

% TOT. RETURN 3/10

	THIS STOCK	VL ARITH. INDEX
1 yr.	-1.5	91.1
3 yr.	2.3	7.6
5 yr.	57.5	42.8

1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	© VALUE LINE PUB., INC.	13-15
10.43	11.03	11.37	11.44	11.02	12.91	12.17	13.06	13.78	13.98	13.61	14.06	15.76	17.49	18.42	19.48	**19.75**	**20.25**	Revenues per sh	**22.10**
1.68	1.75	1.75	1.85	2.04	2.26	2.20	2.53	2.54	2.08	2.23	2.64	2.89	3.31	3.37	3.40	**3.50**	**3.70**	"Cash Flow" per sh	**4.15**
.95	1.03	1.13	1.04	1.08	1.19	1.28	1.35	1.34	.78	1.05	1.32	1.33	1.62	1.55	1.62	**1.75**	**1.90**	Earnings per sh A	**2.35**
.80	.81	.82	.83	.84	.85	.86	.87	.87	.88	.89	.90	.91	.96	1.00	1.01	**1.04**	**1.08**	Div'd Decl'd per sh B■	**1.18**
2.43	2.19	2.40	2.58	3.11	4.30	3.03	3.18	2.68	3.76	5.03	4.24	3.91	2.89	4.45	4.18	**4.15**	**4.10**	Cap'l Spending per sh	**4.20**
10.07	10.29	11.01	11.24	11.48	11.82	12.74	13.22	14.05	13.97	15.01	15.72	16.64	17.53	17.95	19.39	**20.25**	**21.00**	Book Value per sh	**22.35**
11.77	11.77	13.33	13.44	13.44	13.44	15.12	15.12	15.18	15.21	16.75	16.80	17.05	17.23	17.30	18.53	**19.25**	**20.00**	Common Shs Outst'g C	**21.50**
12.8	11.6	12.6	14.5	15.5	17.1	15.9	16.7	18.3	31.9	23.2	21.9	27.7	24.0	22.6	21.2	*Bold figures are*		Avg Ann'l P/E Ratio	**19.0**
.84	.78	.79	.84	.81	.97	1.03	.86	1.00	1.82	1.23	1.17	1.50	1.27	1.36	1.42	*Value Line*		Relative P/E Ratio	**1.25**
6.6%	6.7%	5.8%	5.5%	5.0%	4.2%	4.2%	3.9%	3.6%	3.5%	3.6%	3.1%	2.5%	2.5%	2.9%	2.9%	*estimates*		Avg Ann'l Div'd Yield	**2.6%**

CAPITAL STRUCTURE as of 12/31/09
Total Debt $306.3 mill. **Due in 5 Yrs** $12.3 mill.
LT Debt $305.6 mill. **LT Interest** $22.3 mill.
(LT interest earned: 3.4x: total interest coverage: 3.2x) (56% of Cap'l)

Leases, Uncapitalized: Annual rentals $3.2 mill.

Pension Assets-12/09 $74.0 mill. **Oblig.** $103.1 mill.

Pfd Stock None.

Common Stock 18,554,364 shs. as of 3/10/10

MARKET CAP: $700 million (Small Cap)

2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011		13-15
184.0	197.5	209.2	212.7	228.0	236.2	268.6	301.4	318.7	361.0	**380**	**405**	Revenues ($mill)	**475**
18.0	20.4	20.3	11.9	16.5	22.5	23.1	28.0	26.8	29.5	**33.0**	**38.0**	Net Profit ($mill)	**50.0**
45.7%	43.0%	38.9%	43.5%	37.4%	47.0%	40.5%	42.6%	37.8%	38.9%	**38.5%**	**38.5%**	Income Tax Rate	**38.5%**
--	--	--	--	--	--	12.2%	8.5%	6.9%	3.2%	**5.0%**	**5.0%**	AFUDC % to Net Profit	**5.0%**
47.5%	54.9%	52.0%	52.0%	47.7%	50.4%	48.6%	46.9%	46.2%	45.9%	**47.0%**	**47.0%**	Long-Term Debt Ratio	**49.0%**
51.9%	44.7%	48.0%	48.0%	52.3%	49.6%	51.4%	53.1%	53.8%	54.1%	**53.0%**	**53.0%**	Common Equity Ratio	**51.0%**
371.1	447.6	444.4	442.3	480.4	532.5	551.6	569.4	577.0	665.0	**735**	**795**	Total Capital ($mill)	**940**
509.1	539.8	563.3	602.3	664.2	713.2	750.6	776.4	825.3	866.4	**910**	**955**	Net Plant ($mill)	**1100**
6.4%	6.1%	6.5%	4.6%	5.2%	5.4%	6.0%	6.7%	6.4%	5.9%	**6.0%**	**6.5%**	Return on Total Cap'l	**7.0%**
9.2%	10.1%	9.5%	5.6%	6.6%	8.5%	8.1%	9.3%	8.6%	8.2%	**8.5%**	**9.0%**	Return on Shr. Equity	**10.5%**
9.3%	10.1%	9.5%	5.6%	6.6%	8.5%	8.1%	9.3%	8.6%	8.2%	**8.5%**	**9.0%**	Return on Com Equity	**10.5%**
3.0%	3.6%	3.3%	NMF	1.0%	2.8%	2.7%	3.9%	3.1%	3.2%	**3.5%**	**4.0%**	Retained to Com Eq	**5.0%**
68%	65%	65%	113%	84%	67%	67%	58%	64%	61%	**61%**	**57%**	All Div'ds to Net Prof	**50%**

CURRENT POSITION ($MILL.)	2007	2008	12/31/09
Cash Assets	1.7	7.3	1.7
Other	61.4	83.3	94.3
Current Assets	63.1	90.6	96.0
Accts Payable	29.1	36.6	33.9
Debt Due	37.8	75.3	.7
Other	27.4	25.5	65.1
Current Liab.	94.3	137.4	99.7
Fix. Chg. Cov.	314%	293%	352%

ANNUAL RATES of change (per sh)	Past 10 Yrs.	Past 5 Yrs.	Est'd '07-'09 to '13-'15
Revenues	4.5%	6.0%	*3.0%*
"Cash Flow"	5.0%	8.0%	*3.5%*
Earnings	4.0%	8.5%	*6.5%*
Dividends	1.5%	2.5%	*3.0%*
Book Value	4.5%	5.0%	*3.5%*

Cal-endar	QUARTERLY REVENUES ($ mill.) Mar.31	Jun. 30	Sep. 30	Dec. 31	Full Year
2007	72.3	79.3	75.8	74.0	301.4
2008	68.9	80.3	85.3	84.2	318.7
2009	79.6	93.6	101.5	86.3	361.0
2010	**83.0**	**98.0**	**107**	**92.0**	**380**
2011	**89.0**	**105**	**114**	**97.0**	**405**

Cal-endar	EARNINGS PER SHARE A Mar.31	Jun. 30	Sep. 30	Dec. 31	Full Year
2007	.40	.42	.44	.35	1.62
2008	.30	.53	.26	.43	1.55
2009	.28	.64	.52	.18	1.62
2010	**.27**	**.58**	**.54**	**.36**	**1.75**
2011	**.28**	**.64**	**.57**	**.41**	**1.90**

Cal-endar	QUARTERLY DIVIDENDS PAID B■ Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2006	.225	.225	.225	.235	.91
2007	.235	.235	.235	.250	.96
2008	.250	.250	.250	.250	1.00
2009	.250	.250	.250	.260	1.01
2010	.260				

BUSINESS: American States Water Co. operates as a holding company. Through its principal subsidiary, Golden State Water Company, it supplies water to more than 250,000 customers in 75 communities in 10 counties. Service areas include the greater metropolitan areas of Los Angeles and Orange Counties. The company also provides electric utility services to nearly 23,250 customers in the city of Big Bear Lake and in areas of San Bernardino County. Acquired Chaparral City Water of Arizona (10/00). Has 703 employees. Officers & directors own 2.6% of common stock (4/10 Proxy). Chairman: Lloyd Ross. President & CEO: Robert J. Sprowls. Inc: CA. Addr: 630 East Foothill Boulevard, San Dimas, CA 91773. Tel: 909-394-3600. Internet: www.aswater.com.

The costs of doing business continue to add up for American States Water. Indeed, the water utility saw earnings cut by more than half in the fourth quarter of 2009, despite posting a 3% top-line advance. Higher maintenance and SG&A expenses were the problem, dragging down operating margins a full basis point. Meanwhile, a higher share count shaved a couple of pennies off share earnings.

Operating expenses ought to continue mounting going forward . . . Water infrastructures are growing older and, in many cases, outdated. They require significant repairs and sometimes, complete overhauls. As a result, maintenance costs are expected to remain on an upward trajectory for the foreseeable future. Although the cost structure is likely to benefit from the absence of a $2-plus million legal charge incurred last year, margins will probably show modest improvement in 2010 before stalling in 2011 and eroding thereafter.

. . . and the financial burden remains worrisome. With a fairly leveraged balance sheet and negligible reserve, American is strapped for cash and will need to tap debt and equity markets in order to keep up with the burgeoning infrastructure costs we envision persisting in the years to come. Such endeavors come at a price, however, and the higher interest rate and loftier share count will limit shareholder gains. Against this backdrop, we now look for the company to earn $1.75 a share in 2010 and $1.90 next year.

Prospective investors will probably want to look elsewhere. These shares are ranked 4 (Below Average) for Timeliness, and are likely to trail the broader market for the coming six to 12 months. The issue's longer-term prospects are not any better, with rising costs likely to limit gains over the next 3 to 5 years. The stock is already trading within the 2013-2015 Target Price Range based on our projections. The income component may seem tempting at first blush, but its appeal fades when compared to those of some other stocks in our *Survey*, particularly in the utility space. Although the company has a longstanding history of dividend increases, its financial constraints may well keep growth in check.

Andre J. Costanza *April 23, 2010*

(A) Primary earnings. Excludes nonrecurring gains/(losses): '04, 14¢; '05, 25¢; '06, 6¢; '08, (27¢). Next earnings report due early May. Quarterly egs. may not add due to rounding.
(B) Dividends historically paid in early March, June, September, and December. ■ Div'd reinvestment plan available.
(C) In millions, adjusted for split.

Company's Financial Strength	B++
Stock's Price Stability	85
Price Growth Persistence	70
Earnings Predictability	70

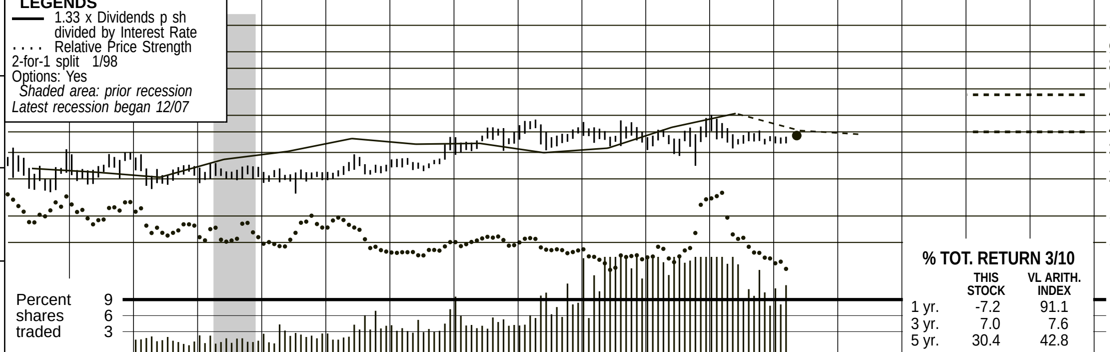
CALIFORNIA WATER NYSE-CWT

RECENT PRICE **38.51** | P/E RATIO **19.2** (Trailing: 19.7 / Median: 22.0) | RELATIVE P/E RATIO **1.06** | DIV'D YLD **3.1%** | VALUE LINE

TIMELINESS 4 Lowered 11/6/09
SAFETY 3 Lowered 7/27/07
TECHNICAL 3 Lowered 4/23/10
BETA .75 (1.00 = Market)

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
High:	32.0	31.4	28.6	26.9	31.4	37.9	42.1	45.8	45.4	46.6	48.3	39.6
Low:	22.6	21.5	22.9	20.5	23.7	26.1	31.2	32.8	34.2	27.7	33.5	35.3

LEGENDS
1.33 x Dividends p sh divided by Interest Rate
.... Relative Price Strength
2-for-1 split 1/98
Options: Yes
Shaded area: prior recession
Latest recession began 12/07



2013-15 PROJECTIONS

	Price	Gain	Ann'l Total Return
High	60	(+55%)	*14%*
Low	40	(+5%)	*4%*

Insider Decisions

	M	J	J	A	S	O	N	D	J
to Buy	0	0	0	0	0	0	0	0	0
Options	0	0	0	0	0	0	2	0	0
to Sell	0	0	0	0	0	0	0	0	0

Institutional Decisions

	2Q2009	3Q2009	4Q2009
to Buy	76	56	51
to Sell	85	75	56
Hld's(000)	10018	9635	10204

% TOT. RETURN 3/10

	THIS STOCK	VL ARITH. INDEX
1 yr.	-7.2	91.1
3 yr.	7.0	7.6
5 yr.	30.4	42.8

1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	© VALUE LINE PUB., INC.	13-15
12.59	13.17	14.48	15.48	14.76	15.96	16.16	16.26	17.33	16.37	17.18	17.44	16.20	17.76	19.80	21.64	*22.40*	*23.70*	Revenues per sh	*25.45*
2.02	2.07	2.50	2.92	2.60	2.75	2.52	2.20	2.65	2.51	2.83	3.03	2.71	3.12	3.72	3.87	*3.95*	*4.15*	"Cash Flow" per sh	*4.50*
1.22	1.17	1.51	1.83	1.45	1.53	1.31	.94	1.25	1.21	1.46	1.47	1.34	1.50	1.90	1.95	*2.05*	*2.25*	Earnings per sh A	*2.60*
.99	1.02	1.04	1.06	1.07	1.09	1.10	1.12	1.12	1.12	1.13	1.14	1.15	1.16	1.17	1.18	*1.19*	*1.20*	Div'd Decl'd per sh B ■	*1.25*
2.26	2.17	2.83	2.61	2.74	3.44	2.45	4.09	5.82	4.39	3.73	4.01	4.28	3.68	4.82	5.33	*5.35*	*5.35*	Cap'l Spending per sh	*5.40*
11.56	11.72	12.22	13.00	13.38	13.43	12.90	12.95	13.12	14.44	15.66	15.79	18.15	18.50	19.44	20.26	*20.70*	*21.40*	Book Value per sh C	*23.25*
12.49	12.54	12.62	12.62	12.62	12.94	15.15	15.18	15.18	16.93	18.37	18.39	20.66	20.67	20.72	20.77	*21.00*	*21.50*	Common Shs Outst'g D	*23.00*
14.1	13.7	11.9	12.6	17.8	17.8	19.6	27.1	19.8	22.1	20.1	24.9	29.2	26.1	19.8	19.7	*Bold figures are*		Avg Ann'l P/E Ratio	*19.0*
.92	.92	.75	.73	.93	1.01	1.27	1.39	1.08	1.26	1.06	1.33	1.58	1.39	1.19	1.32	*Value Line*		Relative P/E Ratio	*1.25*
5.8%	6.4%	5.8%	4.6%	4.2%	4.0%	4.3%	4.4%	4.5%	4.2%	3.9%	3.1%	2.9%	3.0%	3.1%	3.1%	*estimates*		Avg Ann'l Div'd Yield	*2.5%*

2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011		13-15
244.8	246.8	263.2	277.1	315.6	320.7	334.7	367.1	410.3	449.4	*470*	*510*	Revenues ($mill) E	*585*
20.0	14.4	19.1	19.4	26.0	27.2	25.6	31.2	39.8	40.6	*43.0*	*49.0*	Net Profit ($mill)	*60.0*
42.3%	39.4%	39.7%	39.9%	39.6%	42.4%	37.4%	39.9%	37.7%	40.3%	*39.0%*	*39.0%*	Income Tax Rate	*39.0%*
- -	- -	- -	10.3%	3.2%	3.3%	10.6%	8.3%	8.6%	7.6%	*8.5%*	*10.0%*	AFUDC % to Net Profit	*10.0%*
48.9%	50.3%	55.3%	50.2%	48.6%	48.3%	43.5%	42.9%	41.6%	47.1%	*47.0%*	*46.5%*	Long-Term Debt Ratio	*49.0%*
50.2%	48.8%	44.0%	49.1%	50.8%	51.1%	55.9%	56.6%	58.4%	52.9%	*53.0%*	*53.5%*	Common Equity Ratio	*51.0%*
388.8	402.7	453.1	498.4	565.9	568.1	670.1	674.9	690.4	794.9	*855*	*905*	Total Capital ($mill)	*1055*
582.0	624.3	697.0	759.5	800.3	862.7	941.5	1010.2	1112.4	1198.1	*1265*	*1325*	Net Plant ($mill)	*1475*
6.8%	5.3%	5.9%	5.6%	6.1%	6.3%	5.2%	5.9%	7.1%	6.5%	*6.5%*	*7.0%*	Return on Total Cap'l	*7.0%*
10.0%	7.2%	9.4%	7.8%	8.9%	9.3%	6.8%	8.1%	9.9%	9.6%	*10.0%*	*10.5%*	Return on Shr. Equity	*11.0%*
10.1%	7.2%	9.5%	7.9%	9.0%	9.3%	6.8%	8.1%	9.9%	9.6%	*10.0%*	*10.5%*	Return on Com Equity	*11.0%*
1.8%	NMF	1.0%	.7%	2.1%	2.1%	1.0%	1.8%	3.8%	3.8%	*4.0%*	*5.0%*	Retained to Com Eq	*6.0%*
82%	119%	90%	91%	77%	78%	86%	77%	61%	60%	*58%*	*53%*	All Div'ds to Net Prof	*48%*

CAPITAL STRUCTURE as of 12/31/09
Total Debt $399.3 mill. **Due in 5 Yrs** $55.2 mill.
LT Debt $374.3 mill. **LT Interest** $24.4 mill.

(LT interest earned: 4.1x; total int. cov.: 3.8x)

Pension Assets-12/09 $105.6 mill. **Oblig.** $219.7 mill.

Pfd Stock None

Common Stock 20,765,422 shs. **as of 2/24/10**

MARKET CAP: $800 million (Small Cap)

CURRENT POSITION ($MILL.)	2007	2008	12/31/09
Cash Assets	6.7	13.9	9.9
Other	53.3	65.9	82.3
Current Assets	60.0	79.8	92.2
Accts Payable	36.7	45.1	43.7
Debt Due	2.7	42.8	25.0
Other	30.3	35.3	41.7
Current Liab.	69.7	123.2	110.4
Fix. Chg. Cov.	333%	398%	430%

ANNUAL RATES of change (per sh)	Past 10 Yrs.	Past 5 Yrs.	Est'd '07-'09 to '13-'15
Revenues	2.5%	3.0%	*4.5%*
"Cash Flow"	2.5%	6.0%	*4.0%*
Earnings	1.0%	6.5%	*6.5%*
Dividends	1.0%	1.0%	*1.0%*
Book Value	4.0%	6.0%	*3.0%*

Cal-endar	QUARTERLY REVENUES ($ mill.) E Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2007	71.6	95.8	113.8	85.9	367.1
2008	72.9	105.6	131.7	100.1	410.3
2009	86.6	116.7	139.2	106.9	449.4
2010	*93.0*	*122*	*145*	*110*	*470*
2011	*100*	*131*	*157*	*122*	*510*

Cal-endar	EARNINGS PER SHARE A Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2007	.07	.37	.67	.39	1.50
2008	.01	.48	1.06	.35	1.90
2009	.12	.58	.94	.31	1.95
2010	*.11*	*.61*	*.98*	*.35*	*2.05*
2011	*.14*	*.67*	*1.03*	*.41*	*2.25*

Cal-endar	QUARTERLY DIVIDENDS PAID B ■ Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2006	.2875	.2875	.2875	.2875	1.15
2007	.290	.290	.290	.290	1.16
2008	.293	.293	.293	.293	1.17
2009	.295	.295	.295	.295	1.18
2010	.2975				

BUSINESS: California Water Service Group provides regulated and nonregulated water service to roughly 463,600 customers in 83 communities in California, Washington, New Mexico, and Hawaii. Main service areas: San Francisco Bay area, Sacramento Valley, Salinas Valley, San Joaquin Valley & parts of Los Angeles. Acquired Rio Grande Corp; West Hawaii Utilities (9/08). Revenue breakdown, '08: residential, 69%; business, 18%; public authorities, 5%; industrial, 5%; other, 3%. '08 reported depreciation rate: 2.4%. Has roughly 929 employees. Chairman: Robert W. Foy. President & CEO: Peter C. Nelson (4/09 Proxy). Inc.: Delaware. Address: 1720 North First Street, San Jose, California 95112-4598. Telephone: 408-367-8200. Internet: www.calwatergroup.com.

Increased expenses sank California Water Service Group's bottom line in the fourth quarter. The water utility posted share earnings of $0.31, 11% below both last year's mark and our estimate. The top line rose a better-than-anticipated 7%, to roughly $107 million, but expenses grew faster, due to increased water production and SG&A costs, specifically for higher pension and benefit commitments.

We have tempered our 2010 earnings expectations accordingly. Operating costs are likely to continue to rise, as aging infrastructures require greater maintenance and repairs. The company will get little in the way of relief from rate hikes this year, however, because other than potential modest inflationary increases, there is not expected to be any rate increases implemented until 2011. Most of the company's subsidiaries have not been up for general rate case reviews in more than three years, owing to the changeover to a consolidated filing system. As a result, we suspect that earnings growth will be lucky to top 5% this year.

Growth rates ought to pick up next year, however. As mentioned above, the company has filed a rate relief request with the California Public Utilities Commission (CPUC) for more than $70 million. A ruling is likely to be handed down by yearend, with the new rates effective January 1, 2011. Although the proposal may be a bit lofty, we expect a favorable ruling, given the recent regulatory landscape and necessity to maintain current water standards. Therefore, we've pegged CWT to earn $2.25 a share, on revenues of more than $500 million next year.

That said, we think the stock is fully valued at this time. It is ranked 4 (Below Average) for Timeliness and trails the *Value Line* median in terms of 3- to 5-year appreciation potential. Although a more constructive regulatory climate looks to be in place, the greater stock and debt offerings that are likely to be needed to keep up the burgeoning infrastructure costs will probably dilute shareholder gains to 2013-2015. The issue's steady dividend growth adds some appeal for those seeking total return, but investors have better pure-growth and/or income vehicles to choose from elsewhere.

Andre J. Costanza *April 23, 2010*

(A) Basic EPS. Excl. nonrecurring gain (loss): '00, (7¢); '01, 4¢; '02, 8¢. Next earnings report due late July.
(B) Dividends historically paid in mid-Feb., May, Aug., and Nov. ■ Div'd reinvestment plan available.
(C) Incl. deferred charges. In '09: $2.6 mill., $.13/sh.
(D) In millions, adjusted for split.
(E) Excludes non-reg. rev.

Company's Financial Strength	B++
Stock's Price Stability	85
Price Growth Persistence	75
Earnings Predictability	80

Line No.	Company	Jun-10	Jun-10	May-10	May-10	Apr-10	Apr-10	DIV1	DIV2	DIV3	DIV4	d_1	d_2	d_3	d_4	P_0	Dividend	I/B/E/S Growth [1]	Value Line Forecasted Growth	Average Growth	Cost of Equity	1+g	1+k
1	Amer. States Water	35.47	31.41	39.44	32.61	39.61	34.79	0.2631	0.2737	0.2737	0.2737	0.250	0.260	0.260	0.260	35.555	1.1179	4.00%	6.50%	5.25%	8.6%	1.05	1.09
2	Amer. Water Works	21.81	19.78	22.13	19.41	22.22	20.75	0.2315	0.2315	0.2315	0.2315	0.210	0.210	0.210	0.210	21.017	0.9771	10.25%	NA	10.25%	15.1%	1.10	1.15
3	Aqua America	18.10	16.65	18.73	16.52	18.64	17.55	0.1478	0.1588	0.1588	0.1588	0.135	0.145	0.145	0.145	17.698	0.6541	7.50%	11.50%	9.50%	13.4%	1.10	1.13
4	Artesian Res. 'A'	19.33	16.43	19.24	17.28	19.33	17.41	0.1887	0.1982	0.1982	0.1993	0.178	0.187	0.187	0.188	18.170	0.8148	6.00%	NA	6.00%	10.7%	1.06	1.11
5	California Water	37.03	33.81	39.70	34.54	39.55	37.42	0.3128	0.3128	0.3154	0.3154	0.295	0.295	0.298	0.298	37.008	1.3012	5.55%	6.50%	6.03%	9.7%	1.06	1.10
6	Pennichuck	23.57	20.77	24.41	21.12	23.50	22.20	0.1908	0.1908	0.1962	0.1962	0.175	0.175	0.180	0.180	22.595	0.8097	9.00%	NA	9.00%	12.8%	1.09	1.13
7	SJW Corp.	25.10	22.55	28.19	23.17	28.24	24.99	0.1815	0.1815	0.1870	0.1870	0.165	0.165	0.170	0.170	25.373	0.7722	10.00%	NA	10.00%	13.2%	1.10	1.13
8	York Water	15.60	12.96	14.45	12.83	14.24	13.60	0.1336	0.1357	0.1357	0.1357	0.126	0.128	0.128	0.128	13.946	0.5608	6.00%	6.00%	6.00%	10.2%	1.06	1.10
9	Average																				11.7%		

[1] Analysts’ growth forecasts obtained from Thomson Reuters and Yahoo Finance July 2010.

PENNICHUCK CORP NDQ--PNNW

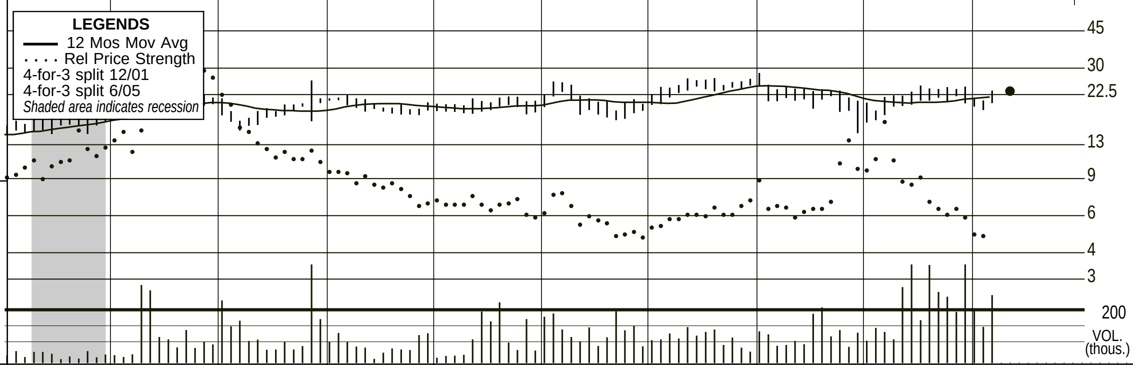
RECENT PRICE	TRAILING P/E RATIO	RELATIVE P/E RATIO	DIV'D YLD	VALUE LINE
23.36	42.5	2.24	3.1%	

RANKS

PERFORMANCE	3	Average
Technical	3	Average
SAFETY	3	Average
BETA .50	(1.00 = Market)	

Financial Strength	B+
Price Stability	90
Price Growth Persistence	50
Earnings Predictability	30

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
High	20.25	24.30	26.25	22.34	22.00	25.90	26.92	28.48	24.80	23.51
Low	14.49	17.63	15.18	17.99	18.10	17.00	20.05	14.75	16.56	19.00



© VALUE LINE PUBLISHING, INC.	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010/2011
SALES PER SH	7.15	7.35	6.69	7.15	5.67	5.81	6.99	7.29	7.05	
"CASH FLOW" PER SH	2.09	2.00	1.53	1.57	.89	.99	1.77	2.10	1.43	
EARNINGS PER SH	1.14	1.13	.62	.60	.13	.14	.84	1.11	.55	***.72** [A,B]**/.79** [C]*
DIV'DS DECL'D PER SH	.57	.59	.63	.65	.66	.66	.66	.66	.70	
CAP'L SPENDING PER SH	2.58	1.65	2.25	1.69	2.60	5.08	4.25	3.45	1.76	
BOOK VALUE PER SH	9.61	9.55	9.44	9.37	10.89	10.57	10.78	11.24	11.87	
COMMON SHS OUTST'G (MILL)	3.18	3.19	3.19	3.22	4.19	4.21	4.23	4.25	4.65	
AVG ANN'L P/E RATIO	14.5	18.1	30.3	32.7	NMF	NMF	29.1	20.0	38.9	***32.4/29.6***
RELATIVE P/E RATIO	.74	.99	1.73	1.73	NMF	NMF	1.54	1.20	2.60	
AVG ANN'L DIV'D YIELD	3.4%	2.9%	3.4%	3.3%	3.3%	3.2%	2.7%	3.0%	3.3%	
SALES ($MILL)	22.8	23.4	21.4	23.0	23.8	24.5	29.5	31.0	32.8	***Bold figures are consensus earnings estimates and, using the recent prices, P/E ratios.***
OPERATING MARGIN	51.0%	44.5%	37.9%	40.7%	34.0%	30.7%	39.3%	47.0%	48.4%	
DEPRECIATION ($MILL)	3.0	2.8	2.9	3.1	3.3	3.6	3.9	4.2	4.3	
NET PROFIT ($MILL)	3.6	3.6	2.0	1.9	.5	.6	3.6	4.7	2.4	
INCOME TAX RATE	39.1%	37.2%	38.9%	38.4%	38.0%	38.0%	39.2%	36.7%	39.6%	
NET PROFIT MARGIN	15.9%	15.4%	9.2%	8.4%	2.0%	2.3%	12.1%	15.2%	7.3%	
WORKING CAP'L ($MILL)	3.5	4.6	.4	d11.0	19.2	3.2	2.9	d1.9	.6	
LONG-TERM DEBT ($MILL)	27.1	26.9	26.9	16.9	41.3	47.7	58.0	59.6	54.3	
SHR. EQUITY ($MILL)	30.6	30.4	30.2	30.2	45.6	44.6	45.6	47.8	55.2	
RETURN ON TOTAL CAP'L	8.0%	8.0%	5.1%	5.9%	1.7%	2.2%	4.8%	6.2%	3.9%	
RETURN ON SHR. EQUITY	11.8%	11.8%	6.5%	6.4%	1.0%	1.3%	7.9%	9.9%	4.3%	
RETAINED TO COM EQ	5.9%	5.5%	NMF	NMF	NMF	NMF	1.7%	4.0%	NMF	
ALL DIV'DS TO NET PROF	50%	54%	102%	107%	NMF	NMF	78%	59%	NMF	

[A]No. of analysts changing earn. est. in last 10 days: 0 up, 1 down, consensus 5-year earnings growth not available. [B]Based upon 3 analysts' estimates. [C]Based upon 3 analysts' estimates.

ANNUAL RATES

of change (per share)	5 Yrs.	1 Yr.
Sales	--	-3.5%
"Cash Flow"	1.0%	-31.5%
Earnings	1.0%	-50.5%
Dividends	1.5%	6.0%
Book Value	3.5%	5.5%

Fiscal Year	QUARTERLY SALES ($mill.) 1Q	2Q	3Q	4Q	Full Year
12/31/07	6.0	7.1	9.4	7.0	29.5
12/31/08	6.8	7.9	8.4	7.9	31.0
12/31/09	7.0	8.5	9.5	7.8	32.8
12/31/10					

Fiscal Year	EARNINGS PER SHARE 1Q	2Q	3Q	4Q	Full Year
12/31/06	d.17	.04	.16	.11	.14
12/31/07	.04	.31	.38	.11	.84
12/31/08	.59	.19	.21	.12	1.11
12/31/09	d.02	.18	.32	.07	.55
12/31/10	***.03***	***.22***	***.39***		

Cal-endar	QUARTERLY DIVIDENDS PAID 1Q	2Q	3Q	4Q	Full Year
2007	.165	.165	.165	.165	.66
2008	.165	.165	.165	.165	.66
2009	.175	.175	.175	.175	.70
2010	.18				

INSTITUTIONAL DECISIONS

	2Q'09	3Q'09	4Q'09
to Buy	28	19	25
to Sell	12	19	10
Hld's(000)	2314	2358	2520

ASSETS ($mill.)	2007	2008	12/31/09
Cash Assets	9.0	1.1	1.6
Receivables	4.7	5.1	4.4
Inventory (Avg cost)	1.1	.9	.7
Other	1.0	1.8	2.8
Current Assets	15.8	8.9	9.5
Property, Plant & Equip, at cost	175.6	187.4	192.6
Accum Depreciation	35.3	36.1	37.8
Net Property	140.3	151.3	154.8
Other	12.5	14.8	13.3
Total Assets	168.6	175.0	177.6
LIABILITIES ($mill.)			
Accts Payable	1.9	.4	1.1
Debt Due	6.7	6.7	5.9
Other	4.3	3.7	1.9
Current Liab	12.9	10.8	8.9

LONG-TERM DEBT AND EQUITY as of 12/31/09

Total Debt $60.2 mill. **Due in 5 Yrs.** $9.5 mill.
LT Debt $54.3 mill.
Including Cap. Leases None
(50% of Cap'l)

Leases, Uncapitalized Annual rentals $.4 mill.

Pension Liability $5.7 mill. in '09 vs. $6.4 mill. in '08

Pfd Stock None **Pfd Div'd Paid** None

Common Stock 4,651,058 shares
(50% of Cap'l)

INDUSTRY: Water Utility

BUSINESS: Pennichuck Corporation, through its subsidiaries, engages in the collection, storage, treatment, and distribution of potable water for domestic, industrial, commercial, and fire protection service in southern and central New Hampshire. The company also provides non-regulated water management services, including monitoring, maintenance, testing, and compliance reporting services for water systems of various towns, businesses, and residential communities. In addition, it engages in real estate planning, development, and management of residential, commercial, industrial, and retail properties. Further, Pennichuck controls approximately 450 acres of developable land in Nashua and Merrimack, New Hampshire. It serves Nashua, New Hampshire and 10 surrounding municipalities in southern New Hampshire with an estimated total population of 110,000. Has 101 employees. C.E.O. & President: Duane C. Montopoli . Inc.: NH. Address: 25 Manchester Street, Merrimack, NH 03054. Tel.: (603) 882-5191. Internet: http://www.pennichuck.com.

W.T.

April 23, 2010

TOTAL SHAREHOLDER RETURN

Dividends plus appreciation as of 3/31/2010

3 Mos.	6 Mos.	1 Yr.	3 Yrs.	5 Yrs.
12.27%	9.86%	18.76%	11.61%	5.56%

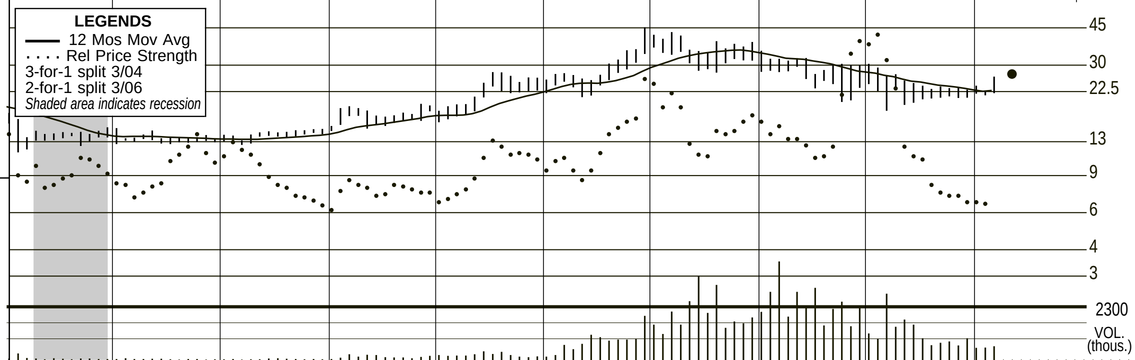
SJW CORP. NYSE--SJW

RECENT PRICE	TRAILING P/E RATIO	RELATIVE P/E RATIO	DIV'D YLD	
27.27	33.7	1.77	2.5%	VALUE LINE

RANKS

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	
High	17.83	15.07	14.95	19.64	27.80	45.33	43.00	35.11	30.44	27.60	High
Low	11.58	12.67	12.57	14.60	16.07	21.16	27.65	20.05	18.22	21.60	Low

PERFORMANCE **3** *Average*
Technical 3 *Average*
SAFETY **3** *Average*
BETA .95 (1.00 = Market)



Financial Strength	B+
Price Stability	65
Price Growth Persistence	75
Earnings Predictability	85

© VALUE LINE PUBLISHING, INC.	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010/2011
SALES PER SH	7.45	7.97	8.20	9.14	9.86	10.35	11.25	12.12	11.68	
"CASH FLOW" PER SH	1.49	1.55	1.75	1.89	2.21	2.38	2.30	2.44	2.21	
EARNINGS PER SH	.77	.78	.91	.87	1.12	1.19	1.04	1.08	.81	***1.04*** [A,B]***/1.13*** [C]
DIV'DS DECL'D PER SH	.43	.46	.49	.51	.53	.57	.61	.65	.66	
CAP'L SPENDING PER SH	2.63	2.06	3.41	2.31	2.83	3.87	6.62	3.79	3.17	
BOOK VALUE PER SH	8.17	8.40	9.11	10.11	10.72	12.48	12.90	13.99	13.66	
COMMON SHS OUTST'G (MILL)	18.27	18.27	18.27	18.27	18.27	18.28	18.36	18.18	18.50	
AVG ANN'L P/E RATIO	18.5	17.3	15.4	19.6	19.7	23.5	33.4	26.2	28.7	***26.2/24.1***
RELATIVE P/E RATIO	.95	.94	.88	1.04	1.04	1.27	1.77	1.58	1.92	
AVG ANN'L DIV'D YIELD	3.0%	3.4%	3.5%	3.0%	2.4%	2.0%	1.7%	2.3%	2.8%	
SALES ($MILL)	136.1	145.7	149.7	166.9	180.1	189.2	206.6	220.3	216.1	***Bold figures***
OPERATING MARGIN	64.4%	63.7%	56.0%	56.4%	55.9%	57.0%	41.8%	42.4%	42.5%	***are consensus***
DEPRECIATION ($MILL)	13.2	14.0	15.2	18.5	19.7	21.3	22.9	24.0	25.6	***earnings***
NET PROFIT ($MILL)	14.0	14.2	16.7	16.0	20.7	22.2	19.3	20.2	15.2	***estimates***
INCOME TAX RATE	34.5%	40.4%	36.2%	42.1%	41.6%	40.8%	39.4%	39.5%	40.4%	***and, using the***
NET PROFIT MARGIN	10.3%	9.8%	11.2%	9.6%	11.5%	11.7%	9.4%	9.2%	7.0%	***recent prices,***
WORKING CAP'L ($MILL)	d3.8	d4.9	12.0	13.0	10.8	22.2	d1.4	d11.3	d4.0	***P/E ratios.***
LONG-TERM DEBT ($MILL)	110.0	110.0	139.6	143.6	145.3	163.6	216.3	216.6	246.9	
SHR. EQUITY ($MILL)	149.4	153.5	166.4	184.7	195.9	228.2	236.9	254.3	252.8	
RETURN ON TOTAL CAP'L	6.7%	6.9%	6.9%	6.5%	7.6%	7.0%	5.7%	5.8%	4.4%	
RETURN ON SHR. EQUITY	9.4%	9.3%	10.0%	8.7%	10.6%	9.7%	8.2%	8.0%	6.0%	
RETAINED TO COM EQ	4.1%	3.8%	4.7%	3.6%	5.6%	5.2%	3.5%	3.3%	1.2%	
ALL DIV'DS TO NET PROF	56%	59%	53%	58%	47%	46%	57%	59%	80%	

[A] *No. of analysts changing earn. est. in last 10 days: 0 up, 0 down, consensus 5-year earnings growth not available.* [B] *Based upon 2 analysts' estimates.* [C] *Based upon 2 analysts' estimates.*

ANNUAL RATES

of change (per share)	5 Yrs.	1 Yr.
Sales	6.5%	-3.5%
"Cash Flow"	6.0%	-9.5%
Earnings	3.0%	-25.5%
Dividends	5.5%	2.5%
Book Value	8.0%	-2.5%

Fiscal Year	QUARTERLY SALES ($mill.) 1Q	2Q	3Q	4Q	Full Year
12/31/07	39.0	55.1	64.9	47.6	206.6
12/31/08	41.3	60.0	69.5	49.5	220.3
12/31/09	40.0	58.2	69.3	48.6	216.1
12/31/10					

Fiscal Year	EARNINGS PER SHARE 1Q	2Q	3Q	4Q	Full Year
12/31/06	.14	.35	.48	.22	1.19
12/31/07	.12	.29	.43	.20	1.04
12/31/08	.15	.34	.44	.15	1.08
12/31/09	.01	.23	.43	.14	.81
12/31/10	***.05***	***.26***	***.48***		

Cal-endar	QUARTERLY DIVIDENDS PAID 1Q	2Q	3Q	4Q	Full Year
2007	.151	.151	.151	.151	.60
2008	.161	.161	.161	.161	.64
2009	.165	.165	.165	.165	.66
2010	.17				

INSTITUTIONAL DECISIONS

	2Q'09	3Q'09	4Q'09
to Buy	43	34	43
to Sell	40	29	24
Hld's(000)	8694	8607	8827

ASSETS ($mill.)	2007	2008	12/31/09
Cash Assets	2.4	3.4	1.4
Receivables	23.0	24.5	23.3
Inventory	.8	.9	1.0
Other	5.4	3.2	2.3
Current Assets	31.6	32.0	28.0
Property, Plant & Equip, at cost	904.3	958.7	1020.7
Accum Depreciation	258.8	274.5	302.2
Net Property	645.5	684.2	718.5
Other	90.2	134.7	132.0
Total Assets	767.3	850.9	878.5
LIABILITIES ($mill.)			
Accts Payable	9.3	5.8	6.6
Debt Due	5.6	19.1	6.9
Other	18.1	18.4	18.5
Current Liab	33.0	43.3	32.0

LONG-TERM DEBT AND EQUITY as of 12/31/09

Total Debt $253.8 mill. **Due in 5 Yrs.** $21.5 mill.
LT Debt $246.9 mill.
Including Cap. Leases None
(49% of Cap'l)
Leases, Uncapitalized Annual rentals None

Pension Liability $47.5 mill. in '09 vs. $42.3 mill. in '08

Pfd Stock None **Pfd Div'd Paid** None

Common Stock 18,499,602 shares
(51% of Cap'l)

INDUSTRY: Water Utility

BUSINESS: SJW Corporation, through its subsidiaries, engages in the production, purchase, storage, purification, distribution, and retail sale of water. The company offers nonregulated water-related services, including water system operations, cash remittances, and maintenance contract services. SJW also owns undeveloped land; a 70% limited partnership interest in 444 West Santa Clara Street, L.P.; and operates commercial buildings in Arizona, California, Connecticut, Florida, Tennessee, and Texas. As of September 30, 2009, SJW provided water service to approximately 226,000 connections that served a population of approximately one million people in the San Jose area. It also provides water service to approximately 8,700 connections that serve approximately 36,000 residents in a service area in the region between San Antonio and Austin, Texas. Has 375 employees. Chairman: Charles J. Toeniskoetter. Inc.: CA. Address: 110 W. Taylor Street, San Jose, CA 95110. Tel.: (408) 279-7800. Internet: http://www.sjwater.com.

W.T.

April 23, 2010

TOTAL SHAREHOLDER RETURN

Dividends plus appreciation as of 3/31/2010

3 Mos.	6 Mos.	1 Yr.	3 Yrs.	5 Yrs.
13.50%	12.94%	3.07%	-32.38%	62.58%

AQUA AMERICA NYSE-WTR

RECENT PRICE **17.97** | P/E RATIO **21.9** (Trailing: 23.3, Median: 25.0) | RELATIVE P/E RATIO **1.21** | DIV'D YLD **3.2%** | **VALUE LINE**

TIMELINESS 3 Lowered 6/26/09
SAFETY 3 Lowered 8/1/03
TECHNICAL 3 Lowered 4/23/10
BETA .65 (1.00 = Market)

	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
High:	11.5	12.0	14.8	15.0	16.8	18.5	29.2	29.8	26.6	22.0	21.5	18.1	
Low:	7.6	6.3	9.4	9.6	11.8	14.2	17.5	20.1	18.9	12.2	15.4	16.5	

2013-15 PROJECTIONS

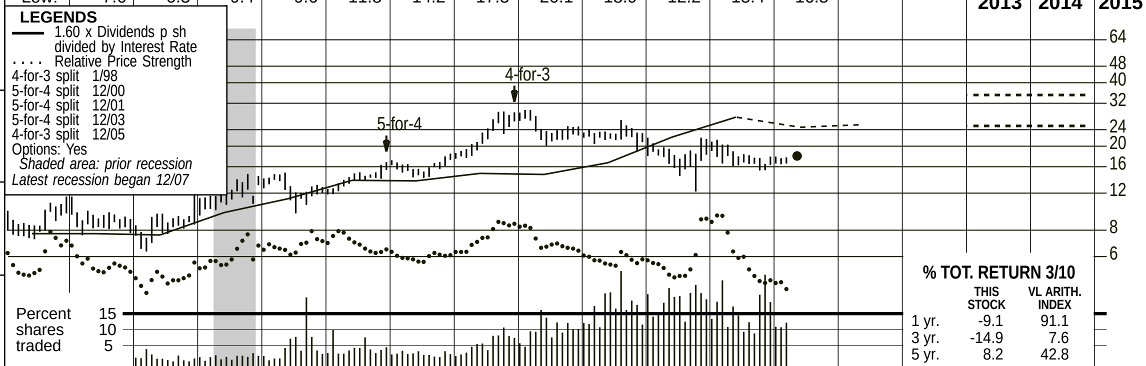
	Price	Gain	Ann'l Total Return
High	35	(+95%)	20%
Low	25	(+40%)	11%

Insider Decisions

	M	J	J	A	S	O	N	D	J
to Buy	0	0	0	0	0	0	2	0	0
Options	0	0	0	0	0	0	0	1	0
to Sell	0	0	0	0	0	0	0	1	0

Institutional Decisions

	2Q2009	3Q2009	4Q2009
to Buy	117	88	127
to Sell	136	118	104
Hld's(000)	61341	60196	60166



1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	© VALUE LINE PUB., INC.	13-15
1.82	1.84	1.86	2.02	2.09	2.41	2.46	2.70	2.85	2.97	3.48	3.85	4.03	4.52	4.63	4.91	*5.30*	*5.70*	Revenues per sh	*6.95*
.42	.47	.50	.56	.61	.72	.76	.86	.94	.96	1.09	1.21	1.26	1.37	1.42	1.61	*1.75*	*1.90*	"Cash Flow" per sh	*2.60*
.26	.29	.30	.34	.40	.42	.47	.51	.54	.57	.64	.71	.70	.71	.73	.77	*.85*	*.95*	Earnings per sh A	*1.40*
.21	.22	.23	.24	.26	.27	.28	.30	.32	.35	.37	.40	.44	.48	.51	.55	*.60*	*.65*	Div'd Decl'd per sh B ■	*.70*
.46	.52	.48	.58	.82	.90	1.16	1.09	1.20	1.32	1.54	1.84	2.05	1.79	1.98	2.08	*2.15*	*2.25*	Cap'l Spending per sh	*2.50*
2.41	2.46	2.69	2.84	3.21	3.42	3.85	4.15	4.36	5.34	5.89	6.30	6.96	7.32	7.82	8.12	*8.30*	*8.60*	Book Value per sh	*10.15*
59.77	63.74	65.75	67.47	72.20	106.80	111.82	113.97	113.19	123.45	127.18	128.97	132.33	133.40	135.37	136.49	*137.50*	*138.00*	Common Shs Outst'g C	*140.00*
13.5	12.0	15.6	17.8	22.5	21.2	18.2	23.6	23.6	24.5	25.1	31.8	34.7	32.0	24.9	23.1	*Bold figures are*		Avg Ann'l P/E Ratio	*21.0*
.89	.80	.98	1.03	1.17	1.21	1.18	1.21	1.29	1.40	1.33	1.69	1.87	1.70	1.50	1.54	*Value Line*		Relative P/E Ratio	*1.40*
6.0%	6.2%	4.9%	3.9%	2.9%	3.0%	3.3%	2.5%	2.5%	2.5%	2.3%	1.8%	1.8%	2.1%	2.8%	3.1%	*estimates*		Avg Ann'l Div'd Yield	*2.0%*

CAPITAL STRUCTURE as of 12/31/09
Total Debt $1473.6 mill. **Due in 5 Yrs** $276.5 mill.
LT Debt $1386.6 mill. **LT Interest** $70.0 mill.
(LT interest earned: 3.5x; total interest coverage: 3.5x) (56% of Cap'l)

Pension Assets-12/09 $135.6 mill. **Oblig.** $217.8 mill.

Pfd Stock None

Common Stock 136,679,644 shares as of 2/12/10

MARKET CAP: $2.4 billion (Mid Cap)

2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011		13-15
275.5	307.3	322.0	367.2	442.0	496.8	533.5	602.5	627.0	670.5	*730*	*785*	Revenues ($mill)	*975*
50.7	58.5	62.7	67.3	80.0	91.2	92.0	95.0	97.9	104.4	*125*	*135*	Net Profit ($mill)	*195*
38.9%	39.3%	38.5%	39.3%	39.4%	38.4%	39.6%	38.9%	39.7%	39.4%	*39.0%*	*39.0%*	Income Tax Rate	*39.0%*
- -	- -	- -	- -	- -	- -	- -	- -	2.9%	3.1%	*2.8%*	*2.5%*	AFUDC % to Net Profit	*1.7%*
52.0%	52.2%	54.2%	51.4%	50.0%	52.0%	51.6%	55.4%	54.1%	55.6%	*55.0%*	*54.0%*	Long-Term Debt Ratio	*49.5%*
47.8%	47.7%	45.8%	48.6%	50.0%	48.0%	48.4%	44.6%	45.9%	44.4%	*45.0%*	*46.0%*	Common Equity Ratio	*50.5%*
901.1	990.4	1076.2	1355.7	1497.3	1690.4	1904.4	2191.4	2306.6	2495.5	*2530*	*2575*	Total Capital ($mill)	*2805*
1251.4	1368.1	1490.8	1824.3	2069.8	2280.0	2506.0	2792.8	2997.4	3227.3	*3300*	*3350*	Net Plant ($mill)	*3600*
7.4%	7.8%	7.6%	6.4%	6.7%	6.9%	6.4%	5.9%	5.7%	5.6%	*6.0%*	*6.5%*	Return on Total Cap'l	*8.0%*
11.7%	12.3%	12.7%	10.2%	10.7%	11.2%	10.0%	9.7%	9.3%	9.4%	*10.0%*	*11.0%*	Return on Shr. Equity	*14.0%*
11.7%	12.4%	12.7%	10.2%	10.7%	11.2%	10.0%	9.7%	9.3%	9.4%	*10.0%*	*11.0%*	Return on Com Equity	*14.0%*
4.7%	5.1%	5.2%	4.2%	4.6%	4.9%	3.7%	3.2%	2.8%	2.7%	*3.0%*	*3.5%*	Retained to Com Eq D	*7.0%*
60%	59%	59%	59%	57%	56%	63%	67%	70%	72%	*70%*	*67%*	All Div'ds to Net Prof	*51%*

CURRENT POSITION ($MILL.)

	2007	2008	12/31/09
Cash Assets	14.5	14.9	21.9
Receivables	82.9	84.5	78.7
Inventory (AvgCst)	8.8	9.8	9.5
Other	9.3	11.8	11.5
Current Assets	115.5	121.0	121.6
Accts Payable	45.8	50.0	57.9
Debt Due	80.8	87.9	87.0
Other	56.6	55.3	56.1
Current Liab.	183.2	193.2	201.0
Fix. Chg. Cov.	323%	329%	346%

ANNUAL RATES

of change (per sh)	Past 10 Yrs.	Past 5 Yrs.	Est'd '07-'09 to '13-'15
Revenues	8.0%	9.0%	*7.0%*
"Cash Flow"	9.5%	8.0%	*10.0%*
Earnings	7.5%	5.5%	*11.5%*
Dividends	7.0%	8.0%	*5.5%*
Book Value	9.5%	10.0%	*4.5%*

QUARTERLY REVENUES ($ mill.)

Cal-endar	Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2007	137.3	150.6	165.5	149.1	602.5
2008	139.3	151.0	177.1	159.6	627.0
2009	154.5	167.3	180.8	167.9	670.5
2010	*165*	*185*	*195*	*185*	*730*
2011	*175*	*195*	*210*	*205*	*785*

EARNINGS PER SHARE A

Cal-endar	Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2007	.13	.17	.22	.19	.71
2008	.11	.17	.26	.19	.73
2009	.14	.19	.25	.20	.77
2010	*.15*	*.20*	*.27*	*.23*	*.85*
2011	*.17*	*.22*	*.30*	*.26*	*.95*

QUARTERLY DIVIDENDS PAID B ■

Cal-endar	Mar.31	Jun.30	Sep.30	Dec.31	Full Year
2006	.107	.107	.115	.115	.44
2007	.115	.115	.125	.125	.48
2008	.125	.125	.125	.135	.51
2009	.135	.135	.135	.145	.55
2010	.145				

BUSINESS: Aqua America, Inc. is the holding company for water and wastewater utilities that serve approximately three million residents in Pennsylvania, Ohio, North Carolina, Illinois, Texas, New Jersey, Florida, Indiana, and five other states. Divested three of four non-water businesses in '91; telemarketing group in '93; and others. Acquired AquaSource, 7/03; Consumers Water, 4/99; and others. Water supply revenues '09: residential, 58.5%; commercial, 14%; industrial & other, 27.5%. Officers and directors own 1.5% of the common stock (4/10 Proxy). Chairman & Chief Executive Officer: Nicholas DeBenedictis. Incorporated: Pennsylvania. Address: 762 West Lancaster Avenue, Bryn Mawr, Pennsylvania 19010. Telephone: 610-525-1400. Internet: www.aquaamerica.com.

Aqua America managed to increase its profits in 2009 despite the weakened economic backdrop. For the full year, revenues advanced 7%, mostly due to benefits from rate-relief cases and gains from acquisitions. This offset unfavorable weather conditions that hurt the top line. The bottom line benefited from cost-cutting efforts, but this was discounted by a 6% increase in capital spending.

The company's customer growth over the next few years will most likely be gained through acquisitions. Toward this end, Aqua America's New Jersey subsidiary completed the purchase of the water system assets of Bloomsbury Borough. This added about 1,000 residential and commercial customers. More acquisitions of smaller water and wastewater companies will be one of the main points of focus for WTR's management.

Earnings gains over the next few years should be bolstered through rate relief cases. During the first two months of 2010, Aqua America has won rate relief cases that should add $6 million per annum to the top line. An additional $65 million in lawsuits should be resolved in the latter half of this year, and management plans to petition for $25 million–$30 million in rate increases and surcharges by yearend.

The dividend payout should continue to be a bright spot for Aqua America. The historical trend of management raising its dividend every year will most likely continue going forward.

This stock is ranked to mirror the broader market over the coming year. Although share earnings were flat year over year in the second half of 2009, we estimate that the top and bottom lines will advance over the next few quarters.

These shares hold above-average appreciation potential over the coming 3 to 5 years. The aforementioned gains from acquisitions should enable revenues and earnings to continue to rise over the pull to 2013–2015. Other points of interest for this equity include its high scores for Stock Price Stability and Earnings Predictability. All told, this stock is best suited for long-term conservative investors.

John D. Burke *April 23, 2010*

(A) Diluted shares. Excl. nonrec. gains (losses): '99, (11¢); '00, 2¢; '01, 2¢; '02, 5¢; '03, 4¢. Excl. gain from disc. operations: '96, 2¢. Earnings may not add due to rounding.
Next earnings report due late April.
(B) Dividends historically paid in early March, June, Sept. & Dec. ■ Div'd. reinvestment plan available (5% discount).
(C) In millions, adjusted for stock splits.

Company's Financial Strength	B+
Stock's Price Stability	95
Price Growth Persistence	70
Earnings Predictability	100

YORK WATER CO NDQ--YORW

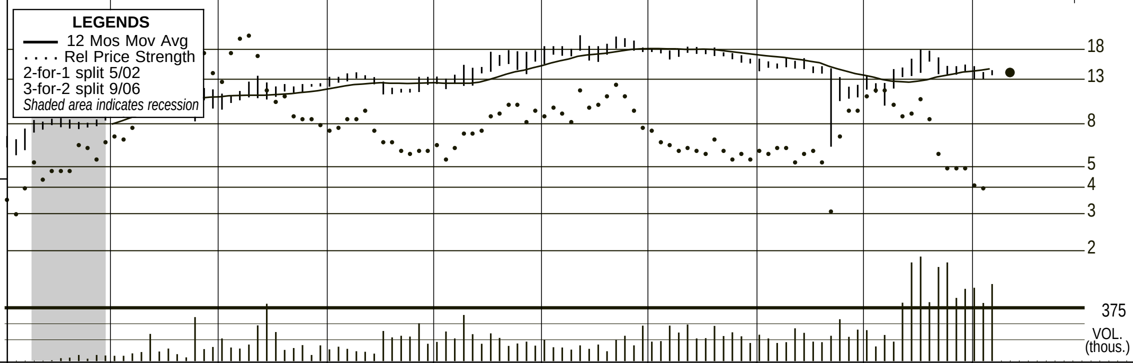
RECENT PRICE	TRAILING P/E RATIO	RELATIVE P/E RATIO	DIV'D YLD	VALUE LINE
13.96	21.8	1.15	3.7%	

RANKS

PERFORMANCE	3	Average
Technical	3	Average
SAFETY	3	Average
BETA .65		(1.00 = Market)

Financial Strength	B+
Price Stability	85
Price Growth Persistence	55
Earnings Predictability	95

	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
High	10.22	13.45	13.49	14.03	17.87	20.99	18.55	16.50	17.95	15.00
Low	5.67	8.20	9.33	11.00	11.67	15.33	15.45	6.23	9.74	13.04



© VALUE LINE PUBLISHING, INC.	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010/2011
REVENUES PER SH	2.05	2.05	2.17	2.18	2.58	2.56	2.79	2.89	2.95	
"CASH FLOW" PER SH	.59	.57	.65	.65	.79	.77	.86	.88	.95	
EARNINGS PER SH	.43	.40	.47	.49	.56	.58	.57	.57	.64	***.68*** [A,B]***/.72*** [C]
DIV'D DECL'D PER SH	.34	.35	.37	.39	.42	.45	.48	.49	.51	
CAP'L SPENDING PER SH	.75	.66	1.07	2.50	1.69	1.85	1.69	2.17	1.18	
BOOK VALUE PER SH	3.79	3.90	4.06	4.65	4.85	5.84	5.97	6.14	6.92	
COMMON SHS OUTST'G (MILL)	9.46	9.55	9.63	10.33	10.40	11.20	11.27	11.37	12.56	
AVG ANN'L P/E RATIO	17.9	26.9	24.5	25.7	26.3	31.2	30.3	24.6	21.9	***20.5/19.4***
RELATIVE P/E RATIO	.92	1.47	1.40	1.36	1.39	1.68	1.61	1.48	1.46	
AVG ANN'L DIV'D YIELD	4.3%	3.3%	3.2%	3.1%	2.9%	2.5%	2.8%	3.5%	3.6%	
REVENUES ($MILL)	19.4	19.6	20.9	22.5	26.8	28.7	31.4	32.8	37.0	***Bold figures are consensus earnings estimates and, using the recent prices, P/E ratios.***
NET PROFIT ($MILL)	4.0	3.8	4.4	4.8	5.8	6.1	6.4	6.4	7.5	
INCOME TAX RATE	35.8%	34.9%	34.8%	36.7%	36.7%	34.4%	36.5%	36.1%	37.9%	
AFUDC % TO NET PROFIT	2.2%	3.7%	--	--	--	7.2%	3.6%	10.1%	--	
LONG-TERM DEBT RATIO	47.7%	46.7%	43.4%	42.5%	44.1%	48.3%	46.5%	54.5%	45.7%	
COMMON EQUITY RATIO	52.3%	53.3%	56.6%	57.5%	55.9%	51.7%	53.5%	45.5%	54.3%	
TOTAL CAPITAL ($MILL)	68.6	69.9	69.0	83.6	90.3	126.5	125.7	153.4	160.1	
NET PLANT ($MILL)	102.3	106.7	116.5	140.0	155.3	174.4	191.6	211.4	222.0	
RETURN ON TOTAL CAP'L	7.9%	7.4%	8.5%	7.6%	8.4%	6.2%	6.7%	5.7%	6.2%	
RETURN ON SHR. EQUITY	11.2%	10.2%	11.4%	10.0%	11.6%	9.3%	9.5%	9.2%	8.6%	
RETURN ON COM EQUITY	11.2%	10.2%	11.4%	10.0%	11.6%	9.3%	9.5%	9.2%	8.6%	
RETAINED TO COM EQ	2.5%	1.3%	2.6%	2.1%	3.0%	2.2%	1.7%	1.4%	1.9%	
ALL DIV'DS TO NET PROF	78%	88%	77%	79%	74%	77%	82%	85%	78%	

[A] No. of analysts changing earn. est. in last 10 days: 0 up, 0 down, consensus 5-year earnings growth 6.0% per year. [B] Based upon 4 analysts' estimates. [C] Based upon 4 analysts' estimates.

ANNUAL RATES

of change (per share)	5 Yrs.	1 Yr.
Revenues	6.0%	2.0%
"Cash Flow"	7.5%	7.5%
Earnings	5.5%	12.5%
Dividends	6.0%	3.5%
Book Value	8.5%	13.0%

Fiscal Year	QUARTERLY SALES ($mill.) 1Q	2Q	3Q	4Q	Full Year
12/31/07	7.4	7.9	8.3	7.8	31.4
12/31/08	7.5	7.8	8.6	8.9	32.8
12/31/09	8.8	9.2	9.8	9.2	37.0
12/31/10					

Fiscal Year	EARNINGS PER SHARE 1Q	2Q	3Q	4Q	Full Year
12/31/06	.12	.14	.17	.15	.58
12/31/07	.12	.15	.15	.15	.57
12/31/08	.11	.13	.15	.18	.57
12/31/09	.13	.17	.18	.16	.64
12/31/10	***.14***	***.18***	***.19***		

Cal-endar	QUARTERLY DIVIDENDS PAID 1Q	2Q	3Q	4Q	Full Year
2007	.118	.118	.118	.118	.47
2008	.121	.121	.121	.121	.48
2009	.126	.126	.126	.126	.50
2010	.128	.128			

INSTITUTIONAL DECISIONS

	2Q'09	3Q'09	4Q'09
to Buy	30	35	28
to Sell	12	16	15
Hld's(000)	2477	2941	2961

ASSETS ($mill.)	2007	2008	12/31/09
Cash Assets	.0	.0	.0
Receivables	5.2	5.9	5.4
Inventory (Avg cost)	.8	.7	.7
Other	.8	.7	1.0
Current Assets	6.8	7.3	7.1
Property, Plant & Equip, at cost	223.1	246.0	260.4
Accum Depreciation	31.5	34.6	38.4
Net Property	191.6	211.4	222.0
Other	12.6	21.7	19.7
Total Assets	211.0	240.4	248.8
LIABILITIES ($mill.)			
Accts Payable	3.2	2.0	1.4
Debt Due	15.0	8.7	9.3
Other	3.2	3.5	3.9
Current Liab	21.4	14.2	14.6

LONG-TERM DEBT AND EQUITY as of 12/31/09

Total Debt $82.6 mill. **Due in 5 Yrs.** $24.6 mill.
LT Debt $73.2 mill.
Including Cap. Leases None
(46% of Cap'l)

Leases, Uncapitalized Annual rentals None

Pension Liability $8.8 mill. in '09 vs. $9.8 mill. in '08

Pfd Stock None **Pfd Div'd Paid** None

Common Stock 12,558,724 shares
(54% of Cap'l)

INDUSTRY: Water Utility

BUSINESS: The York Water Company engages in the impounding, purification, and distribution of water in York County and Adams County, Pennsylvania. The company supplies water for residential, commercial, industrial, and other customers. It has two reservoirs, Lake Williams, which is 700 feet long and 58 feet high, and creates a reservoir covering approximately 165 acres containing about 870 million gallons of water; and Lake Redman, which is 1,000 feet long and 52 feet high and creates a reservoir covering approximately 290 acres containing about 1.3 billion gallons of water. In addition, the company possesses a 15-mile pipeline from the Susquehanna River to Lake Redman that provides access to an additional supply of water. As of December 31, 2009, the company served approximately 180,000 residential, commercial, industrial, and other customers in 39 municipalities in York County and seven municipalities in Adams County. Has 111 employees. C.E.O. & President: Jeffrey R. Hines. Inc.: PA. Address: 130 East Market Street, York, PA 17401. Tel.: (717) 845-3601. Internet: http://www.yorkwater.com.

W.T.

April 23, 2010

TOTAL SHAREHOLDER RETURN

Dividends plus appreciation as of 3/31/2010

3 Mos.	6 Mos.	1 Yr.	3 Yrs.	5 Yrs.
-4.36%	1.00%	15.19%	-10.47%	26.22%

Available online at www.sciencedirect.com
SCIENCE DIRECT®
ELSEVIER
Journal of Accounting and Economics 36 (2003) 105–146
JOURNAL OF Accounting & Economics
www.elsevier.com/locate/econbase

Biased forecasts or biased earnings? The role of reported earnings in explaining apparent bias and over/underreaction in analysts' earnings forecasts ☆

Jeffery Abarbanell[a], Reuven Lehavy[b,*]

[a] *Kenan-Flagler Business School, University of North Carolina at Chapel Hill, Chapel Hill, NC 27599-3490, USA*

[b] *University of Michigan Business School, 701 Tappan Street, Ann Arbor, MI 48109-1234, USA*

Received 21 February 2002; received in revised form 23 October 2003; accepted 11 November 2003

Abstract

The extensive literature that investigates whether analysts' earnings forecasts are biased and/or inefficient has produced conflicting evidence and no definitive answers to either question. This paper shows how two relatively small but statistically influential asymmetries in the tail and the middle of distributions of analysts' forecast errors can exaggerate or obscure evidence consistent with analyst bias and inefficiency, leading to inconsistent inferences. We identify an empirical link between firms' recognition of unexpected accruals and the presence of the two asymmetries in distributions of forecast errors that suggests that firm reporting choices play an important role in determining analysts' forecast errors.

JEL classification: G10; G14; M41

Keywords: Analysts' forecasts; Earnings management; Bias; Inefficiency

☆ We would like to thank workshop participants at Emory University, the University of North Carolina, Hebrew University, the University of Michigan, New York University, Duke University, the University of Illinois (Champaign-Urbana), Tel Aviv University, the Joint Symposium of the Eleventh Annual Conference of Financial Economics and Accounting and the Seventh Mitsui Life Symposium on Global Financial Markets, the October 2000 NBER (Behavioral Finance) conference, the 2002 *Journal of Accounting & Economics* Conference, and George Benston, Peggy Bishop, Alon Brav, Robert Bushman, Francesca Cornelli, John Hand, Cam Harvey, David Hsieh, Mark Lang, Thomas Lys, Ed., Maydew, Jim Ohlson, Jay Patel, Christine Petrovits, the reviewer, Brian Rountree, Paul Simko, Ro Verrecchia, Ross Watts (the editor), Peter Wysocki, and Jerry Zimmerman for their helpful comments.

*Corresponding author. Tel.: +1-734-763-1508; fax: +1-734-936-0282.
E-mail address: rlehavy@umich.edu (R. Lehavy).

0165-4101/$ - see front matter
doi:10.1016/j.jacceco.2003.11.001

1. Introduction

Four decades of research have produced an array of empirical evidence and a set of behavioral and incentive-based theories that address two fundamental questions: Are analysts' forecasts biased? And Do analysts underreact or overreact to information in prior realizations of economic variables? This empirical literature has long offered conflicting conclusions and is not converging to a definitive answer to either question. On the one hand, theories that predict optimism in forecasts are consistent with the persistent statistical finding in the literature of cross-sectional negative (i.e., bad news) mean forecast errors as well as negative intercepts from regressions of forecasts on reported earnings. On the other hand, such theories are inconsistent both with the finding that median forecast errors are most often zero and with the fact that the percentage of apparently pessimistic errors is greater than the percentage of apparently optimistic errors in the cross-section. A similar inconsistency is found in the literature on analyst over/underreaction to prior realizations of economic variables, including prior stock returns, prior earnings changes, and prior analyst forecast errors. Here, again, empirical evidence supports conflicting conclusions that analysts overreact to prior news, underreact to prior news, and both underreact and overreact as a function of the sign of prior economic news. Further reflecting the lack of consensus in the literature, a handful of studies fail to reject unbiasedness and efficiency in analyst forecasts after "correcting" methodological flaws or assuming nonstandard analyst loss functions.[1]

The accumulation of often inconsistent results concerning analyst rationality and incentives makes it difficult for researchers, practitioners, and policy makers to understand what this literature tells us. This motivates us to reexamine the body of evidence with the goal of identifying the extent to which particular theories for apparent errors in analysts' forecasts are supported by the data. Such an exercise is both appropriate and necessary at this juncture as it can, among other things, lead to modified theories that will be tested using the new and unique hypotheses they generate.

We extend our analysis beyond a synthesis and summary of the findings in the literature by identifying the role of two relatively small asymmetries in the cross-sectional distributions of analysts' forecast errors in generating conflicting statistical evidence. We note that the majority of conclusions concerning analyst-forecast rationality in the literature are directly or indirectly drawn from analyses of these distributions. The first asymmetry is a larger number and a greater magnitude of observations that fall in the extreme negative relative to the extreme positive tail of the forecast error distributions (hereafter, the *tail asymmetry*). The second asymmetry is a higher incidence of small positive relative to small negative forecast errors in cross-sectional distributions (hereafter, *the middle asymmetry*). The individual and combined impact of these asymmetries on statistical tests leads to three important observations. First, differences in the manner in which researchers

[1] A representative selection of evidence and theory relevant to both the bias and over/underreaction literatures is discussed in the body of the paper.

implicitly or explicitly weight observations that fall into these asymmetries contribute to inconsistent conclusions concerning analyst bias and inefficiency. Second, a variety of econometric techniques and data adjustments fail to eliminate inconsistencies in inferences across different statistical indicators and conditioning variables. Such techniques include using indicator variables or data partitions in parametric tests, applying nonparametric methods, and performing data truncations and transformations. Third, econometric approaches that choose loss functions that yield consistent inferences—essentially by attenuating the statistical impact of observations that comprise the asymmetries—will not provide definitive answers to the question of whether analysts' forecasts are biased and inefficient. This is because at this stage in the literature too little is known about analysts' actual loss functions, and such methods thus leave unresolved the question of why the asymmetries in forecast error distributions are present.

We present statistical evidence that demonstrates how the two asymmetries in forecast error distributions can indicate analyst optimism, pessimism, or unbiasedness. We also show how observations that comprise the asymmetries can contribute to, as well as obscure, a finding of apparent analyst inefficiency with respect to prior news variables, including prior returns, prior earnings changes, and prior forecast errors. For example, our empirical evidence explains why prior research that relies on parametric statistics always finds evidence of optimistic bias as well as apparent analyst underreaction to prior bad news for all alternative variables chosen to represent prior news. It also explains why evidence of apparent misreaction to good news is *not* robust across parametric statistics or across prior news variables, and why the degree of misreaction to prior bad news is always greater than the degree of misreaction to prior good news, regardless of the statistical approach adopted or the prior information variable examined.

Finally, while our analysis does not lead to an immediately obvious solution to problems of inferences in the literature, it does reveal a link between the reported earnings typically employed to benchmark forecasts and the presence of the two asymmetries in distributions of forecast errors. Specifically, we find that extreme negative unexpected accruals included in reported earnings go hand in hand with observations in the cross-section that generate the tail asymmetry. We also find that the middle asymmetry in distributions of forecast error is eliminated when the reported earnings component of the earnings surprise is stripped of unexpected accruals. This evidence suggests benefits to refining extant cognitive- and incentive-based theories of analyst forecast bias and inefficiency so that they can account for an endogenous relation between forecast errors and manipulation of earnings reports by firms. The evidence also highlights the importance of future research into the question of whether reported earnings are, in fact, the correct benchmark for assessing analyst bias and inefficiency. This is because common motivations for manipulating earnings can give rise to the appearance of analyst forecast errors of exactly the type that comprise the two asymmetries if unbiased and efficient forecasts are benchmarked against manipulated earnings. Thus, it is possible that some evidence previously deemed to reflect the impact of analysts' incentives and cognitive tendencies on forecasts is, after all, attributable to the fact that analysts do not have the motivation or ability to completely anticipate earnings management by firms in their forecasts.

This paper's emphasis is on fleshing out salient characteristics of forecast error distributions with an eye toward ultimately explaining how they arise. The analysis highlights the importance of new research that explains the actual properties of forecast error data and cautions against the application of econometric fixes that either fit the data to specific empirical models or fit specific empirical models to the data without strong a priori grounds for doing so. Our findings also represent a step toward understanding what analysts really aim for when they forecast, which is useful for developing more appropriate null hypotheses in tests of analysts' forecast rationality, and sounder statistical test specifications, as well as the identification of first-order effects that may require control when testing hypotheses that predict analyst forecast errors.

In the next section we describe our data and present evidence of the sensitivity of statistical inferences concerning analyst optimism and pessimism to relatively small numbers of observations that comprise the tail and middle asymmetries. Section 3 extends the analysis to demonstrate the impact of the two forecast error asymmetries on inferences concerning analyst over/underreaction conditional on prior realizations of stock returns and earnings changes, as well as on serial correlation in consecutive-quarter forecast errors. Section 4 presents evidence of a link between biases in reported earnings and the two asymmetries and discusses possible explanations for this link as well as the implications for interpreting evidence from the literature and for the conduct of future research. A summary and conclusions are provided in Section 5.

2. Properties of typical distributions of analysts' forecast errors and inferences concerning analysts' optimism, pessimism, and unbiasedness

2.1. Data

The empirical evidence in this paper is drawn from a large database of consensus quarterly earnings forecasts provided by Zacks Investment Research. The Zacks earnings forecast database contains approximately 180,000 consensus quarterly forecasts for the period 1985–1998. For each firm quarter we calculate forecast errors as the actual earnings per share (as reported in Zacks) minus the consensus earnings forecast outstanding prior to announcement of quarterly earnings, scaled by the stock price at the beginning of the quarter and multiplied by 100. Our results are insensitive to alternative definitions of forecasts such as the last available forecast or average of the last three forecasts issued prior to quarter-end. Inspection of the data revealed a handful of observations that upon further review indicated data errors. These observations had no impact on the basic features of cross-sectional distributions of errors that we describe, but they were nevertheless removed before carrying out the statistical tests reported in this paper. Empirical results obtained after removing these observations were virtually identical to those obtained when the distributions of quarterly forecast errors were winsorized at the 1st and 99th percentiles, a common practice for mitigating the possible effects of data errors followed in the literature. (To enhance comparability with the majority of studies cited below, all test results reported in the paper are based on the winsorized data.)

Lack of available price data reduced the sample size to 123,822 quarterly forecast errors. The data requirements for estimating quarterly accruals further reduced the sample on which our tabled results are based to 33,548 observations.[2] For the sake of brevity we present only results for this reduced sample. We stress, however, that the middle and tail symmetries we document below are present in the full sample of forecast errors and that the proportion of observations that comprise these asymmetries is roughly the same as that for the reduced sample. Moreover, the descriptive evidence and statistical findings relevant to apparent bias and inefficiency in analyst forecasts presented in this section and the next are qualitatively similar when we do not impose the requirement that data be available to calculate unexpected accruals.[3]

2.2. The impact of asymmetries in the distribution of forecast errors on inferences concerning bias

One of the most widely held beliefs among accounting and finance academics is that incentives and/or cognitive biases induce analysts to produce generally optimistic forecasts (see, e.g., reviews by Brown (1993) and Kothari, 2001). This view is repeatedly reinforced when studies that employ analysts' forecasts as a measure of expected earnings present descriptive statistics and refer casually to negative mean forecast errors as evidence of the purportedly "well-documented" phenomenon of optimism in analyst forecasts.[4] The belief is even more common among regulators (see, e.g., Becker, 2001) and the business press (see, e.g., Taylor, 2002). In spite of the prevalent view of analyst forecast optimism, summary statistics associated with forecast error distributions reported in Panel A of Table 1 raise doubts about this conclusion.

[2] As described in Section 4, we use a quarterly version of the modified Jones model to estimate accruals. For the purposes of sensitivity tests, we also examine a measure of unexpected accruals that excludes nonrecurring and special items (see, Hribar and Collins, 2002), and use this adjusted measure in conjunction with *Zacks*' consensus forecast estimates and actual reported earnings, which also exclude such items. All the results involving unexpected accruals reported in the paper are qualitatively unaltered using this alternative measure.

[3] The results are also qualitatively similar when data from alternative forecast providers (I/B/E/S and First Call) are employed, indicating that the findings we revisit in this study are not idiosyncratic to a particular data source (see, Abarbanell and Lehavy, 2002).

[4] The perception is also strengthened in a number of studies that place analyst forecasts and reported earnings numbers (i.e., the two elements that comprise the forecast error) on opposite sides of a regression equation. These studies uniformly find significant intercepts and either casually refer to them as consistent with analyst optimism or emphasize them in supporting their prediction of analyst bias. Evidence presented below, however, indicates a nonlinear relation between forecasts and earnings, which contributes to nonzero intercepts in OLS regressions.

Table 1
Descriptive statistics on quarterly distributions of forecast errors (Panel A), the tail asymmetry (Panel B), and the middle asymmetry (Panel C), 1985–1998

Panel A: Statistics on forecast error distributions

Number of observations	33,548
Mean	−0.126
Median	0.000
% Positive	48%
% Negative	40%
% Zero	12%

Panel B: Statistics on the "tail asymmetry" in forecast error distributions

P5	−1.333
P10	−0.653
P25	−0.149
P75	0.137
P90	0.393
P95	0.684

Panel C: Statistics on the "middle asymmetry" in forecast error distributions

Range of forecast errors (1)	Ratio of positive to negative forecast errors (2)	% of total number of observations (3)
Overall	1.19	100
Forecast errors = 0		12
[−0.1, 0) & (0, 0.1]	1.63*	29
[−0.2, −0.1) & (0.1, 0.2]	1.54*	18
[−0.3, −0.2) & (0.2, 0.3]	1.31*	10
[−0.4, −0.3) & (0.3, 0.4]	1.22*	7
[−0.5, −0.4) & (0.4, 0.5]	1.00	5
[−1, −0.5) & (0.5, 1]	0.83*	11
[Min, −1) & (1, Max]	0.40*	9

This table provides descriptive statistics on quarterly distributions of forecast errors for the period of 1985–1998. Analyst earnings forecasts and actual realized earnings are provided by *Zacks Investment Research*. Panel A provides the mean, median, and frequencies of quarterly forecast errors. Panel B provides percentile values of forecast error distributions. Panel C reports the ratio of positive to negative forecast errors for observations that fall into increasingly larger and nonoverlapping symmetric intervals moving out from zero forecast errors. For example, the forecast error range of [−0.1, 0) & (0, 0.1] includes all observations that are greater than or equal to −0.1 and (strictly) less than zero and observations that are greater than zero and less than or equal to 0.1. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price.

* A test of the difference in the frequency of positive to negative forecast errors is statistically significant at or below a 1% level.

As can be seen in Panel A, the only statistical indication that supports the argument for analyst optimism is a fairly large negative mean forecast error of −0.126. In contrast, the median error is zero, suggesting unbiased forecasts, while the percentage of positive errors is significantly greater than the percentage of negative errors (48% vs. 40%), suggesting apparent analyst pessimism.

To better understand the causes of this inconsistency in the evidence of analyst biases among the summary statistics, we take a closer look at the distribution of forecast errors. Panel A of Fig. 1 presents a plot of the 1st through the 100th percentiles of the pooled quarterly distributions of forecast errors over the sample period. Moving from left to right, forecast errors range from the most negative to the most positive.

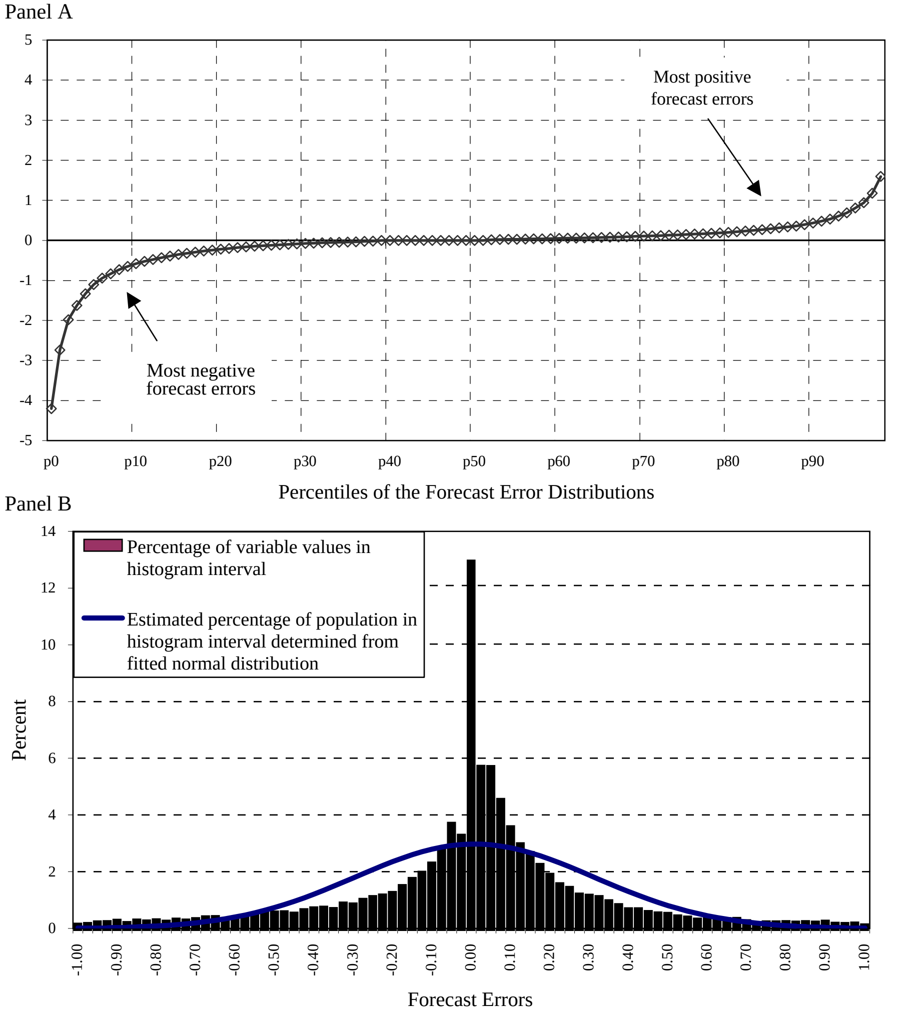


Fig. 1. Percentile values of quarterly distributions of analyst forecast errors (Panel A) and histogram of forecast errors for observations within forecast errors of -1 to $+1$ (Panel B). Panel A depicts percentile values of quarterly distributions of analyst forecast errors. Panel B presents percentage of forecast error values in histogram intervals for observations within a forecast error of -1% to $+1\%$ of the beginning-of-period stock price. Forecast error equals reported earnings minus the consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price ($N = 33,548$).

One distinctive feature of the distribution is that the left tail (ex-post bad news) is longer and fatter than the right tail, i.e., far more extreme forecast errors of greater absolute magnitude are observed in the ex-post "optimistic" tail of the distribution than in the "pessimistic" tail. We refer to this characteristic of the distribution as the *tail asymmetry*. Although Fig. 1 summarizes the distribution of observations over the entire sample period, unreported results indicate that a tail asymmetry is present in each quarter represented in the sample. To get a sense of the magnitude of the asymmetry, we return to Panel B of Table 1, where the 5th percentile (extreme negative forecast errors) is nearly twice the size observed for the 95th percentile (-1.333 vs. 0.684). Alternatively, we find that 13% of the observations fall below a negative forecast error of -0.5, while only 7% fall above a positive error of an equal magnitude (not reported in the table).

Closer visual inspection of the data reveals a second feature of the distribution depicted in Panel B of Fig. 1—a higher frequency of small positive forecast errors versus small negative errors. Specifically, the figure presents the frequencies of forecast errors that fall in fixed subintervals of 0.025 within the range of -1 to $+1$. Clearly, the *incidence* of small positive relative to small negative errors increases as forecast errors become smaller in absolute magnitude. We refer to this property of the distribution as the *middle asymmetry*.[5] Statistics on the magnitude of the middle asymmetry are reported in Panel C of Table 1. This panel presents the ratio of positive (i.e., apparently pessimistic) errors to negative errors for observations that fall into increasingly larger and nonoverlapping symmetric intervals moving out from zero forecast errors. Consistent with the visual evidence in Panel B of Fig. 1, this ratio increases for smaller, symmetric intervals of forecast errors, reaching 1.63 in the smallest interval examined (significantly different from 1, as well as significantly different from the ratios calculated for the larger intervals).[6] Another distinguishing feature of the distribution seen in Panel C of Table 1 and evident in both Panels A and B of Fig. 1 is the large number of exactly zero observations (12%). Depending on one's previous exposure to the data or instincts about the task of forecasting, the magnitude of the clustering at exactly zero may not seem

[5] The visual evidence in Panel B of Fig. 1 is consistent with specific circumstances in which analysts have incentives to produce forecasts that fall slightly short of reported earnings (see, e.g., Degeorge et al., 1999; Matsumoto, 2002; Brown, 2001; Burgstahler and Eames, 2002; Bartov et al., 2000; Dechow et al., 2003; Abarbanell and Lehavy, 2003a, b). However, prior studies have not considered the impact of observations that comprise the middle asymmetry on inferences concerning the *general* tendency of analysts to produce biased and/or inefficient forecasts.

[6] An analysis of unscaled forecast errors confirms that rounding down a greater number of negative than positive forecast errors to a value of zero when errors are scaled by price does not systematically induce the middle asymmetry (see, Degeorge et al., 1999). Similarly, there is no obvious link between the presence of the middle asymmetry and round-off errors induced by the application of stock-split factors to consensus forecast errors discussed in Baber and Kang (2002) and Payne and Thomas (2002). Abarbanell and Lehavy (2002) present evidence confirming the presence of the middle asymmetry in samples confined to firms with stock-split factors of less than 1.

surprising. Nevertheless, the large number of forecasts of exactly zero has important impacts on statistical inferences.[7]

The statistics presented above indicate that the tail asymmetry pulls the mean forecast error toward a negative value, supporting the case for analyst optimism. But, as shown in Panel C of Table 1, the excess of *small* positive over *small* negative errors associated with the middle asymmetry is largely responsible for a significantly higher overall incidence of positive to negative forecast errors in the distribution, thus supporting the case for analyst pessimism. Finally, a zero median forecast error, which supports an inference of analyst unbiasedness, reflects the countervailing effects of the middle asymmetry and tail asymmetries. A rough calculation pertaining to the nonzero forecast errors in the interval between [−0.1, 0) and (0, 0.1] gives a sense of these effects. There are 9662 observations in this region. If nonzero forecast errors were random, we would expect 4831 forecasts to be positive, when in fact 5928 are positive, indicating that small errors in the distribution of absolute magnitude less than or equal to 0.1 contribute 1097 more observations to the right of zero than would be expected if the distribution was symmetric. This region of the forecast error distribution contains 29% of all observations but contributes more than 42% of the total number of pessimistic errors in excess of optimistic errors and represents roughly 3.3% of the entire distribution. Their impact offsets, all else being equal, the contribution of approximately 2.5% of negative observations in excess of what would be expected if the distribution of errors were symmetric, arising from the tail asymmetry (relative to the extreme decile cutoffs of a fitted normal distribution). Because 12% of the forecast error sample has a value of exactly zero, the relative sizes of the tail and middle asymmetries are each sufficiently small (and offsetting) to ensure that the median error remains at zero.

The evidence in Table 1 and Fig. 1 yields two important implications for drawing inferences about the nature and extent of analyst bias. First, depending on which summary statistic the researcher chooses to emphasize, support can found for analyst optimism, pessimism, and even unbiasedness. Second, if a researcher relies on a given summary statistic to draw an inference about analyst bias, a relatively small percentage of observations in the distribution of forecast errors will be responsible for his or her conclusion. This is troublesome because extant hypotheses that predict analyst optimism or pessimism typically do not indicate how often the phenomenon will occur in the cross-section and often convey the impression that

[7] Because many factors can affect the process that generates the typical distribution of forecast errors, there is no reason to expect them to be normally or even symmetrically distributed. Supplemental analyses unreported in the tables reject normality on the basis of skewness and kurtosis. It is interesting to note, however, that kurtosis in the forecast error distribution does not align with the typical descriptions of leptokurtosis (high peak and fat tails) or platykurtosis (flat center and/or shoulders). Relative to decile cutoffs of the fitted normal distribution, we find that the most extreme negative decile of the actual distribution contains only 5% of the observations and the most extreme positive decile contains only 2.5% of the observations. Thus, even though the extreme negative tail is roughly twice the size of the extreme pessimistic tail, extreme observations are actually *underrepresented* in the distribution relative to a normal, especially in the positive tail. The thinner tails and shoulders of the distribution highlight the role of peakedness as a source of deviation from normality, a fact that is relevant to assessing the appropriateness of statistics used by researchers to draw inferences about analyst forecast bias.

bias will be pervasive in the distribution (see, studies suggesting that analysts are hard-wired or motivated to produce optimistic forecasts, e.g., Affleck-Graves et al. (1990), Francis and Philbrick (1993), and Kim and Lustgarten (1998), or that selection biases lead to hubris in analysts' earnings forecasts, e.g., McNichols and O'Brien, 1997).[8]

Some studies have explicitly recognized the disproportional impact of extreme negative forecast errors on conclusions drawn in the literature, but for the most part they have had little influence on general perceptions. For example, Degeorge et al. (1999) predict a tendency for pessimistic errors to occur but recognize the common perception that analyst forecasts are optimistic; they note in passing that extreme negative forecast errors are responsible for an optimistic mean forecast in their sample. Some studies also tend to deal with this feature of the data in an ad hoc manner. Keane and Runkle (1998), for example, recognize the impact of extreme negative forecast errors on statistical inferences concerning analyst forecast rationality and thus eliminate observations from their sample based on whether reported earnings contain large negative special items. However, Abarbanell and Lehavy (2002) show that there is a very high correlation between observations found in the extreme negative tail of forecast error distributions and firms that report large negative special items, even when special items are excluded from the reported earnings benchmark used to calculate the forecast error. Thus, by imposing rules that eliminate observations from their sample based on the size of negative special items, Keane and Runkle (1998) effectively truncate the extreme negative tail of forecast error distributions, and in so doing nearly eliminate evidence of mean optimism in their sample.

Some researchers are less explicit in justifying the removal of observations from the distribution of forecast errors when testing for forecast rationality, or are unaware that they have done so in a manner that results in sample distributions that deviate substantially from the population distribution. For example, many studies implicitly limit observations in their samples to those that are less extreme by choosing ostensibly symmetric rules for eliminating them, such as winsorization or truncations of values greater than a given absolute magnitude.[9] It should be evident from Panel A of Fig. 1 that such rules inherently mitigate the statistical impact of the

[8] A notable exception is the attribution of optimism in analysts' earnings forecasts to incentives to attract and maintain investment banking relationships (see, e.g., Lin and McNichols, 1998; Dugar and Nathan, 1995). Evidence consistent with this argument is based on fairly small samples of firms issuing equity. We emphasize that all the qualitative results in this paper are unaltered after eliminating observations for which an IPO or a seasoned equity offering took place within 1 year of the date of a forecast. Furthermore, the number of observations removed from the sample for this reason represents a very small percentage of those in each of the quarters in our sample period.

[9] For example, Kothari (2001) reports that Lim (2001) excludes absolute forecast errors of $10 per share or more, Degeorge et al. (1999) delete absolute forecast errors greater than 25 cents per share, Richardson et al. (1999) delete price-deflated forecast errors that exceed 10% in absolute value, and Brown (2001) winsorizes absolute forecast errors greater than 25 cents per share (which implies a much larger tail winsorization than typically undertaken to remove possible data errors). While none of these procedures, when applied to our data, completely eliminates the tail asymmetry, all of them substantially attenuate to varying degrees its statistical impact on our tests.

tail asymmetry and arbitrarily transform the distribution, frequently without a theoretical or institutional reason for doing so.[10]

One might justify truncating data on the grounds that the disproportional impact of the extreme tail makes it difficult detect general tendencies, or that such "errors" may not accurately reflect factors relevant to analysts' objective functions (see, e.g., Abarbanell and Lehavy, 2003b; Gu and Wu, 2003; Keane and Runkle, 1998). However, it is possible for researchers to "throw the baby out with the bathwater" if they assume that these observations do not reflect the effects of incentives or cognitive biases, albeit in a more noisy fashion than other observations in the distribution. Another concern that arises from transforming the distribution of errors without justification is that it may suppress one feature of the data (e.g., the tail asymmetry), leaving another unusual but more subtle feature of the distribution (e.g., the middle asymmetry) to dominate an inference that forecasts are generally biased or to offset the other and yield an inference that forecasts are generally unbiased. This is an important issue because there has been a tendency in the literature on forecast rationality for new hypotheses to crop up motivated solely by the goal of explaining "new" empirical results. For example, after truncating large absolute values of forecast errors, Brown (2001) finds that the mean and median forecasts in recent years indicate a shift away from analyst optimism and toward analyst pessimism. Increasing pessimism as a function of market sentiment as reflected in changes in price level or changes in analyst incentives has also been a subject of growing interest in the behavioral finance literature. Clearly, when data inclusion rules that systematically reduce the tail asymmetry are applied, empirical evidence in support of increasing or time-varying analyst pessimism will be affected by the size and magnitude of the remaining middle asymmetry.

Perhaps the most unsatisfying aspect of the evidence presented in Table 1 is the fact that general incentive and behavioral theories of analyst forecast errors are not sufficiently developed at this stage to predict that when forecast errors are extreme they are more likely to be *optimistic* and when forecast errors are small they are more likely to be *pessimistic*. That is, individual behavioral and incentive theories for analyst forecast errors do not account for the simultaneous presence of the two asymmetries that play such an important role in generating evidence consistent with analyst bias and, as we show in the next section, analyst forecast inefficiency with respect to prior information (see Abarbanell and Lehavy, 2003a, for an exception).

3. The effect of the two asymmetries on evidence of apparent analyst misreaction to prior stock returns, prior earnings changes, and prior forecast errors

In this section, we demonstrate how observations that comprise the tail and middle asymmetries in forecast error distributions *conditional on prior realizations of*

[10] For example, in our data an arbitrary symmetric truncation of the distribution at the 10th and the 90th percentiles reduces the measure of skewness in the remainder of the distribution to a level that does not reject normality and results in a mean forecast error near zero among the remaining observations. A similar effect occurs with an arbitrary one-sided truncation of the negative tail at a value as low as the 3rd percentile.

economic variables contribute to inconsistent inferences concerning the efficiency of analysts' forecasts. One important message of the ensuing analysis is that the likelihood that a forecast error observation falls into one or the other asymmetry varies by the sign and magnitude of the prior news. This feature of the data links the empirical literature on analyst inefficiency to the heretofore separate literature on analyst bias. This is because observations that comprise the two asymmetries and lead—depending on the statistic relied on—to inconsistent inferences concerning analyst bias also contribute to conflicting inferences concerning whether analysts underreact, overreact, or react efficiently to prior news.

We consider realizations of three economic variables: prior period stock returns, prior period earnings changes, and prior period analyst forecast errors. These three variables are those most often identified in previous studies of analyst forecast efficiency.[11] Consistent with the previous literature, we define prior abnormal returns (*PrAR*) as equal to the return between 10 days after the last quarterly earnings announcement to 10 days prior to the current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period.[12] Prior earnings changes (*PrEC*) are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the price at the beginning of the period, and prior forecast errors (*PrFE*) are the prior quarter's forecast error.

The remainder of this section proceeds as follows: we first present evidence on the existence of the tail and middle asymmetries in distributions of forecast errors conditional on the sign of prior news variables. We then analyze the role of the asymmetries in producing indications of analyst inefficiency in both summary statistics and regression coefficients and discuss the robustness of these findings. Next, we show the disproportionate impact of observations that comprise the asymmetries in generating evidence of serial correlation in analyst forecast errors. Finally, we discuss the shortcomings of econometric "fixes" that intentionally or unintentionally ameliorate the impact of one or both asymmetries on inferences concerning analyst forecast rationality.

3.1. The tail and middle asymmetries in forecast error distributions conditional on prior news variables

Tests of analyst forecast efficiency typically partition distributions of forecast errors based on the sign of the prior news to capture potential differences in analyst reactions to prior good versus prior bad news. Accordingly, before we review the

[11] Studies that examine the issue of current period forecast efficiency with respect to prior period realization of returns or earnings (e.g., Abarbanell, 1991; Easterwood and Nutt, 1999) commonly frame the question in terms of whether analysts over- or underreact to prior news. In contrast, studies that examine the issue of current period forecast efficiency with respect to analysts' own past forecast errors are generally limited to the question of whether there is significant serial correlation in lagged forecast errors, without regard to how the sign and magnitude of prior forecast errors affect that correlation.

[12] All reported results are qualitatively similar when prior abnormal returns are measured between 10 days after the last quarterly earnings announcement to either 30 days prior or 1 day prior to the current quarter earnings announcement.

statistical evidence, we first examine the features of forecast error distributions conditional on the sign of prior news variables. Panels A–C of Fig. 2, which depict the percentiles of the distributions of forecast errors conditional on the sign of each of the three prior news variables, show that prior bad news partitions are characterized by larger tail asymmetries than prior good news partitions for all prior news variables.

Panels A–C of Fig. 3—which depict the frequencies of forecast errors that fall in fixed subintervals of 0.025 within the range of −0.5 to +0.5 for *PrAR*, *PrEC*, and *PrFE*, respectively—show that prior good news partitions are characterized by larger middle asymmetries than prior bad news partitions for all three prior news variables.[13]

Together, Figs. 2 and 3 suggest that distributions of forecast errors conditional on the sign of prior news retain the characteristic asymmetries found in the unconditional distributions in Section 2. However, the likelihood of a subsequent forecast error falling into the middle asymmetry is greater following prior good news, while the likelihood of a forecast error falling into the tail asymmetry is greater following prior bad news.[14] Below we investigate the impact of the variation in the size of the asymmetries in distributions of forecast errors conditional on the sign of news on inferences about analyst inefficiency that are drawn from summary statistics (Section 3.1.1) and regression coefficients (Section 3.1.2).

3.1.1. Inferences about analyst efficiency from summary statistics

Panel A of Table 2 shows how the two asymmetries impact summary statistics, including means, medians, and the percentages of negative to positive forecast errors in distributions of forecast errors conditional on the sign of prior news. We begin with the case of prior bad news. Prior bad news partitions for all three variables produce significantly negative mean forecast errors (−0.195 for *PrAR*, −0.291 for *PrEC*, and −0.305 for *PrFE*), supporting an inference of analyst underreaction (i.e., the mean forecast is too high following bad news). The higher percentages of negative than positive forecast errors in the bad news partitions of each variable (e.g., 50% vs. 40% for negative *PrEC*) are also consistent with a tendency for analysts to underreact to prior bad news. The charts in Figs. 2 and 3 foreshadow these results. The relatively larger tail asymmetry in prior bad news partitions drives parametric means to large negative values. Similarly, the larger negative relative to

[13] The concentration of small (extreme) errors among positive (negative) prior returns news is not induced by scaling by prices that are systematically higher (lower) following a period of abnormal positive (negative) returns, since the middle and tail asymmetries are still present in distributions of unscaled forecast errors and errors deflated by forecasts.

[14] Abarbanell and Lehavy (2003a) report the same patterns in forecast error distributions conditional on classification of ranked values of stock recommendations, P/E ratio, and market-to-book ratios into high and low categories. It is certainly possible that some form of irrationality or incentive effect leads to different forecast error regimes on either side of a demarcation point of zero, and therefore coincidentally sorts the two asymmetries that are located on either side of a zero. However, the continued presence of relatively small but statistically influential asymmetries in the conditional distributions may overwhelm the researcher's ability to detect these incentive or behavioral factors, or may give the false impression that such a factor is pervasive in the distribution when it is not.

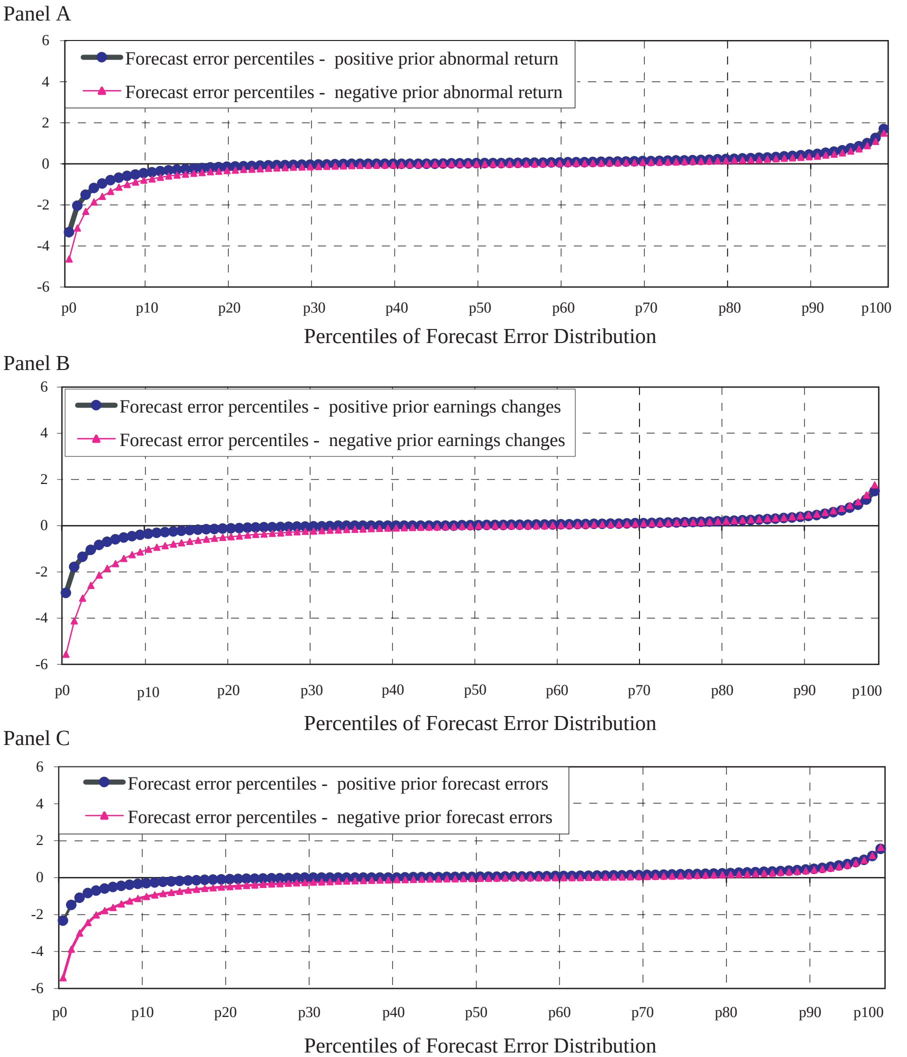


Fig. 2. Forecast error equals reported earnings minus consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price. Prior market-adjusted return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the beginning-of-period price.

positive tails account for greater overall frequencies of negative than positive errors, consistent with underreaction to bad news for all three variables. This is so even though prior bad news distributions of forecast errors for *PrAR* and *PrEC* are characterized by middle asymmetries, which, all else equal, tend to push the ratio of positive to negative errors toward values greater than 1.

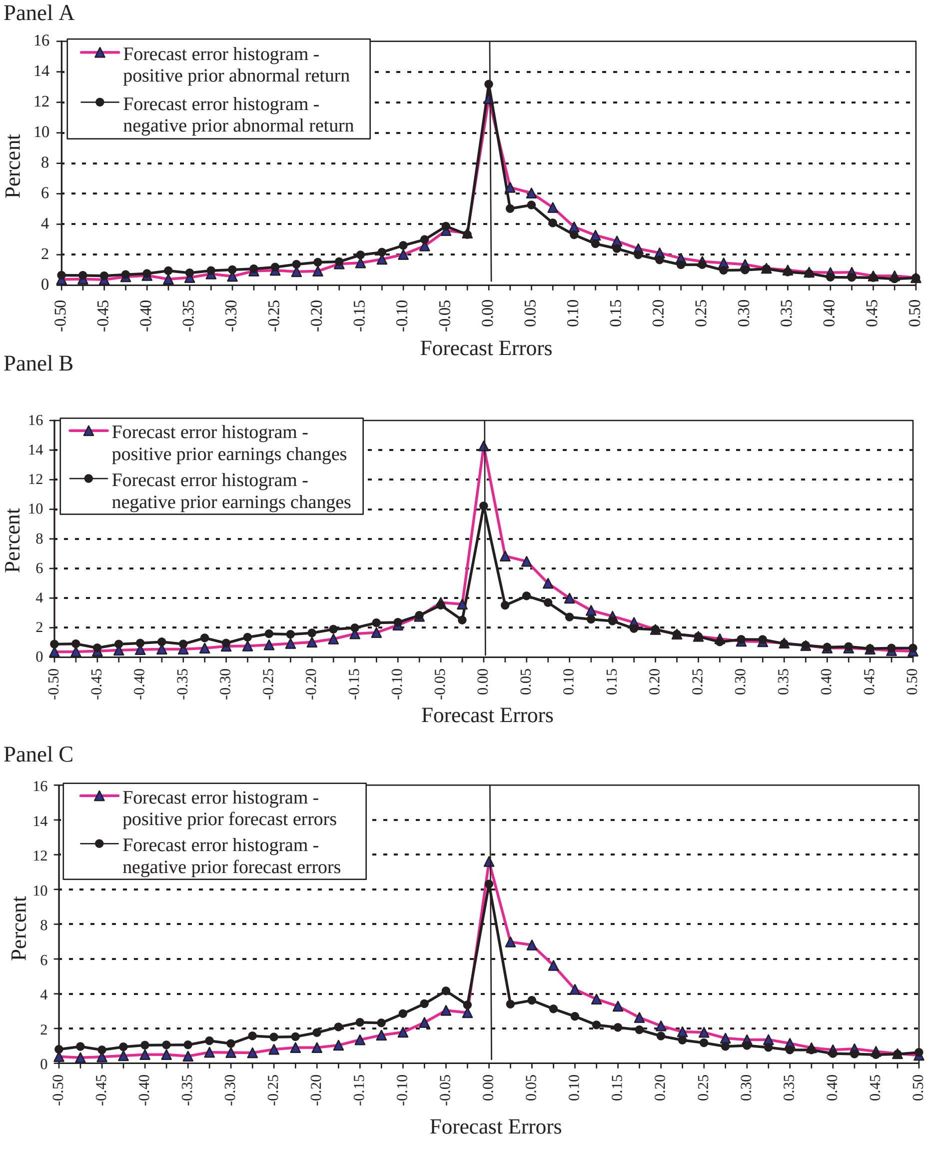


Fig. 3. Histogram of forecast errors by sign of prior abnormal returns (Panel A), prior earnings changes (Panel B), and prior forecast errors (Panel C). This figure presents the percentage of forecast error values in histogram intervals for observations within forecast error of -0.5 to $+0.5$ by sign of prior abnormal return (Panel A), prior earnings changes (Panel B), and prior forecast errors (Panel C). Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price. Prior abnormal return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the beginning-of-period price.

Table 2
Mean, median, and frequency of forecast errors (Panel A), and ratio of positive to negative forecast errors in symmetric regions for bad (Panel B) and good (Panel C) prior news variables

Panel A: Mean, median, and frequency of forecast errors by sign of prior news variables

Statistic	Sign of prior abnormal return		Sign of prior earnings changes		Sign of prior forecast errors	
	Negative (1)	Positive (2)	Negative (3)	Positive (4)	Negative (5)	Positive (6)
Mean	−0.195*	−0.041*,#	−0.291*	−0.036*,#	−0.305*	0.017*,#
Median	0.000	0.028	−0.015	0.020	−0.043	0.042
% Zero forecast errors	13%	12%	10%	14%	10%	11%
% Positive forecast errors	42%	54%	40%	52%	36%	59%
% Negative forecast errors	45%	34%	50%	34%	54%	30%
N	16,940	13,833	11,526	21,062	12,999	15,415

Panel B: Ratio of positive to negative forecast errors for negative *realizations of prior news*

Range of forecast errors	Negative prior abnormal return		Negative prior earnings changes		Negative prior forecast errors	
	Ratio of positive to negative FE (1)	% of total (2)	Ratio of positive to negative FE (3)	% of total (4)	Ratio of positive to negative FE (5)	% of total (6)
Overall	0.94	100	0.81	100	0.66	100
Forecast errors = 0		13		10		10
[−0.1, 0) & (0, 0.1]	1.39	27	1.26	21	0.94	23
[−0.2, −0.1) & (0.1, 0.2]	1.27	17	1.15	17	0.94	17
[−0.3, −0.2) & (0.2, 0.3]	0.99	10	0.93	11	0.75	10
[−0.4, −0.3) & (0.3, 0.4]	0.96	7	0.93	8	0.72	7
[−0.5, −0.4) & (0.4, 0.5]	0.73	5	0.74	6	0.59	5
[−1, −0.5) & (0.5, 1]	0.60	11	0.56	14	0.52	14
[Min, −1) & (1, Max]	0.29	10	0.28	14	0.24	14

Panel C: Ratio of positive to negative forecast errors for positive *realizations of prior news*

Range of forecast errors	Positive prior abnormal return		Positive prior earnings changes		Positive prior forecast errors	
	Ratio of positive to negative FE (1)	% of total (2)	Ratio of positive to negative FE (3)	% of total (4)	Ratio of positive to negative FE (5)	% of total (6)
Overall	1.58	100	1.53	100	1.99	100
Forecast errors = 0		12		14		11
[−0.1, 0) & (0, 0.1]	1.86	31	1.82	33	2.33	33
[−0.2, −0.1) & (0.1, 0.2]	1.89	18	1.85	18	2.42	19
[−0.3, −0.2) & (0.2, 0.3]	1.85	10	1.66	9	2.22	10
[−0.4, −0.3) & (0.3, 0.4]	1.70	6	1.49	6	2.03	7
[−0.5, −0.4) & (0.4, 0.5]	1.52	5	1.28	4	1.70	4
[−1, −0.5) & (0.5, 1]	1.25	10	1.17	9	1.44	10
[Min, −1) & (1, Max]	0.62	8	0.58	7	0.83	6

Panel A provides statistics on forecast errors (FE) by sign of prior abnormal return, prior earnings changes, and prior forecast errors. Panel B (Panel C) reports the ratio of positive to negative forecast errors for observations that fall into increasingly larger and nonoverlapping symmetric intervals moving out from zero forecast errors for negative (positive) prior abnormal returns, prior earnings changes, and prior forecast errors. Prior abnormal return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by beginning-of-period price. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by price.

*Significantly different than zero at a 1% level or better.

#Mean forecast error for positive prior news variables is significantly different than mean forecast error for negative prior news variables at a 1% level or better.

The impact of the tail asymmetry on the inference of underreaction to prior bad news can be seen in Panel B of Table 2, which presents the number of observations in increasingly larger nonoverlapping symmetric intervals starting from zero for the three prior bad news partitions. Even though large errors in the intervals [min, −1) and (1, max] make up a relatively small percentage of the observations in the bad news distributions of *PrAR*, *PrEC*, and *PrFE* (10%, 14%, and 14%, respectively), errors of these absolute magnitudes comprise 3.45 (=1/0.29) 3.57 (=1/0.28), and 4.17 (=1/0.24) bad news observations for every good news observation, respectively.

Apparent consistency across summary statistical indicators of analyst underreaction to prior bad news does not carry over to the case of prior good news. The mean error for the good news partitions of *PrAR* and *PrEC* reported in columns 2 and 4 of Panel A of Table 2 are negative, consistent with analyst *overreaction* (i.e., the mean forecast is too high following good news), but is positive in the case of good news *PrFE*, suggesting *underreaction*. These mixed parametric results are attributable to the fact that tail asymmetries, although relatively small compared to their bad news counterparts, are still sufficiently large to produce negative mean errors for both prior good news partitions of *PrAR* and *PrEC* (see Fig. 2). However, they are not large enough to generate a negative median for these variables because, as seen in Panel C of Table 2, there is an even greater *frequency* of small positive errors associated with middle asymmetries in the good news partitions than for unconditional distributions (e.g., the ratio of positive errors to negative errors is 1.86 in the interval [−0.1, 0), (0, 0.1] of the *PrAR* partition but only 1.63 in that same interval of the unconditional distribution). The middle asymmetries are thus sufficiently large to offset relatively small tail asymmetries in these good news partitions, leading to indications of underreaction to good news in nonparametric statistics.[15]

3.1.2. Inferences about analyst efficiency from regression analysis

While means, medians, and ratios of positive to negative forecast errors are viable statistics from which to draw inferences of analyst inefficiency, most studies rely on slopes of regressions of forecast errors on prior news variables. The most persistent findings from such regressions are significant positive slope coefficients that are consistent with overall analyst *underreaction* to prior news realizations. To examine

[15] In this study, as in any study that partitions prior news variables by sign, we treat all prior variables as if they were interchangeable for the purposes of drawing inferences concerning a general tendency toward analyst inefficiency. Clearly, partitioning on the sign of news is likely to lead to misclassification in the case of prior earnings news, since the average firm is *not* likely to have an expected change of zero. Moreover, both prior earnings changes and prior forecast errors entail the use of an earnings benchmark, which, as discussed in the next section, introduces another potential problem of classification associated with potential time-series correlations induced by earnings management. These are interesting issues worthy of further consideration. However, they do not preclude an analysis of how the tail and middle asymmetries in forecast error distributions have combined to generate inconsistent indications of analyst inefficiency in the existing literature. If anything, these issues further strengthen the case for adopting the approach of identifying salient features of distributions of forecast errors in an effort to develop more precise hypotheses and design more appropriate empirical tests.

Table 3
Slope coefficients from OLS and rank regressions of forecast errors on prior news variables

	Explanatory variable					
	Prior abnormal return		Prior earnings changes		Prior forecast errors	
	OLS	Ranked	OLS	Ranked	OLS	Ranked
Overall	0.744	0.162	0.819	0.160	0.238	0.253
	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Prior bad news	1.602	0.213	2.306	0.130	0.231	0.265
	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Prior good news	0.089	0.199	−0.835	0.157	0.045	0.170
	0.28	<0.01	0.01	<0.01	0.11	<0.01

This table reports slope coefficient estimates from OLS and rank regressions of forecast errors on prior abnormal return, prior earnings changes, and prior forecast errors with the White-corrected p-values. Prior abnormal return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by beginning-of-period price. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by price.

the effect of the two asymmetries on this inference, we first estimate the slope coefficients for separate OLS and rank regressions of forecast errors on *PrAR*, *PrEC*, and *PrFE*. After applying White corrections suggested by the regression diagnostics, the estimates, as shown in the first row of Table 3, confirm that the typical finding reported in the prior literature of overall underreaction holds for all three prior news variables in our sample, inasmuch as all three coefficients are positive and reliably different from zero. Similarly, rank regressions produce significant positive slope coefficients in the case of all three prior news variables.

Next, we compare the inferences from regression slope coefficients estimated by the sign of prior news to assess their consistency with the parametric and nonparametric evidence presented in Panel A of Table 2 and the preceding regression results for the overall samples. These results are presented in Table 3. Consistent with regression results for the overall sample, prior bad news partitions of all three variables produce OLS and rank slope coefficients that are significantly positive, indicating once again analyst underreaction to prior bad news. These results are consistent with indications of underreaction in both the parametric and nonparametric summary statistics associated with all three bad news partitions reported in Panel A of Table 2. In sharp contrast, however, regression results for the prior good news partitions generate inconsistent indications across both OLS and rank regression slope coefficients and across prior news variables. The OLS slope coefficient is positive but insignificant in the case of good news *PrAR* and *PrFE*, resulting in a failure to reject efficiency in these cases, but it is reliably negative for the good news *PrEC* variable, consistent with analyst *overreaction* to prior good earnings news. That is, OLS performed on the prior good news partitions of forecast errors produces *no* evidence of apparent analyst underreaction observed both in the overall samples and in the prior bad news partitions. In contrast, and adding to the ambiguity, rank regressions do produce reliably positive slope coefficients consistent with underreaction for all three prior good news variables. This finding is also consistent with the rank regression results for both the overall samples and the prior bad news partitions for all three prior news variables that suggest analyst underreaction.

It is evident from the foregoing collection of parametric and nonparametric results that it is difficult to draw a clear inference regarding the existence and nature of analyst inefficiency with respect to prior news. These results are a microcosm of similar inconsistencies found in the literature on analyst efficiency with respect to prior news, examples of which are discussed below. In keeping with our goal of assessing the extent, to which theories that predict systematic errors in analysts forecasts are supported by the evidence, we next delve further into the robustness of specific findings concerning analyst-forecast efficiency. As in the case of inferences on bias in analysts' forecasts, we find inconsistencies and a lack of robustness of evidence, which are linked to the relative size of the two asymmetries present in forecast error distributions.

3.2. *How robust is evidence of analyst underreaction to bad news?*

To further isolate the disproportional influence of the asymmetries on statistics, we examine the relation between forecast errors and prior news variables in finer partitions of the prior news variables. Our goal is to demonstrate that while the statistical indications of analyst underreaction to prior bad news are largely consistent in Tables 2 and 3, the phenomenon is not robust in the distribution of forecast errors. Fig. 4 depicts the percentiles of the distributions of forecast errors for the lowest, highest, and the combined distribution of the 2nd through the 9th decile of each prior news variable. One pattern evident in all of the panels is that the most extreme prior bad news decile is always associated with the most extreme negative forecast errors.

The effect of this association is evident in Fig. 5, which summarizes the mean and median forecast errors by decile of prior news for all three variables: The largest negative mean error by far is produced in the 1st decile of all prior news variables. This finding helps explain why overall bad news partitions of prior news yield parametric means that are always consistent with analyst underreaction.[16]

To gauge the effect of observations in the lowest prior news decile (which, as seen in Fig. 4, are associated with extreme negative forecast errors), we reestimate the

[16] Furthermore, in unreported results we find that OLS regressions by individual deciles produce significant positive coefficients in *only* the 1st decile among all deciles associated with prior bad news for all three prior variables. The combination of greater (lower) variation in the independent variable and a strong linear (nonlinear) relation between prior news and forecast errors in the first decile (other deciles) contribute to these results, as we discuss later.

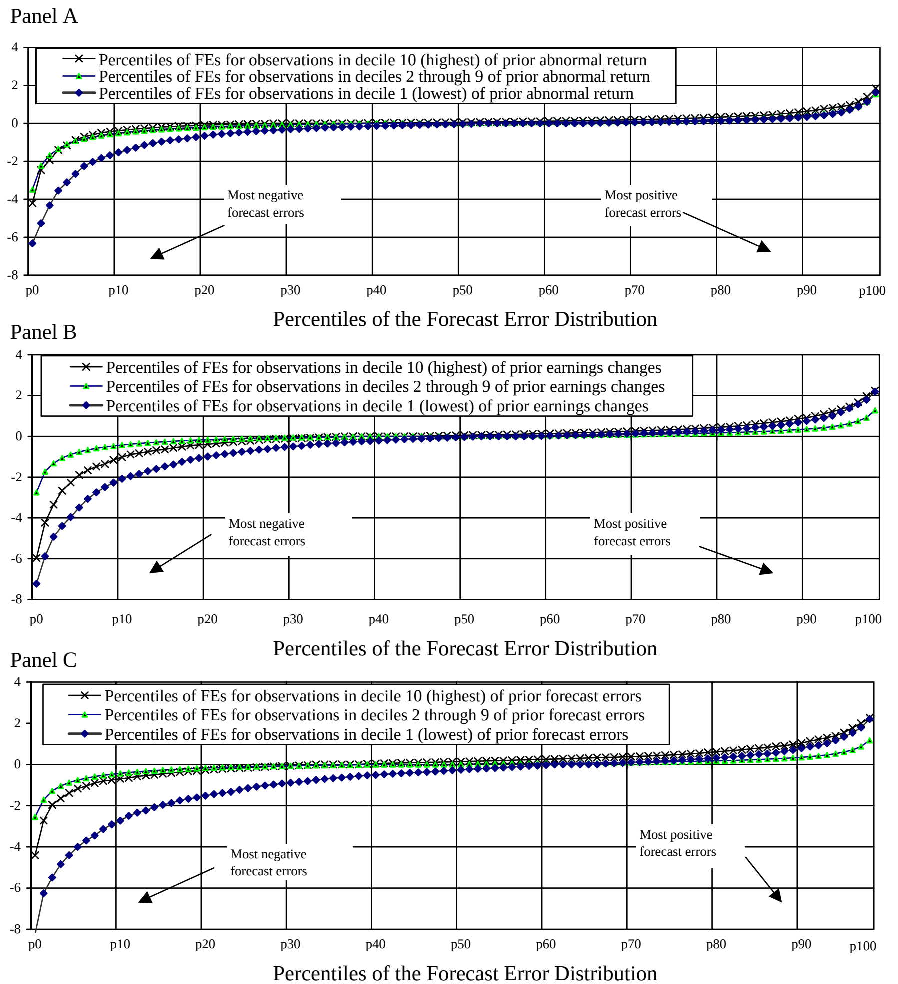


Fig. 4. The tail asymmetry in forecast errors within selected deciles of prior news variables. This figure depicts percentiles of quarterly distributions of analysts' forecast errors that fall in selected deciles (lowest, highest, and the combined distribution of the 2nd through the 9th decile) of prior abnormal returns (Panel A) prior earnings changes (Panel B) and prior forecast errors (Panel C). Forecast error equals reported earnings minus consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price. Prior market-adjusted return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the beginning-of-period price.

OLS regressions for the overall sample after excluding observations in this decile (unreported in the tables). We find that removing the 1st decile of prior news results in declines in the overall coefficients from values of 0.744, 0.819, and 0.238, to values

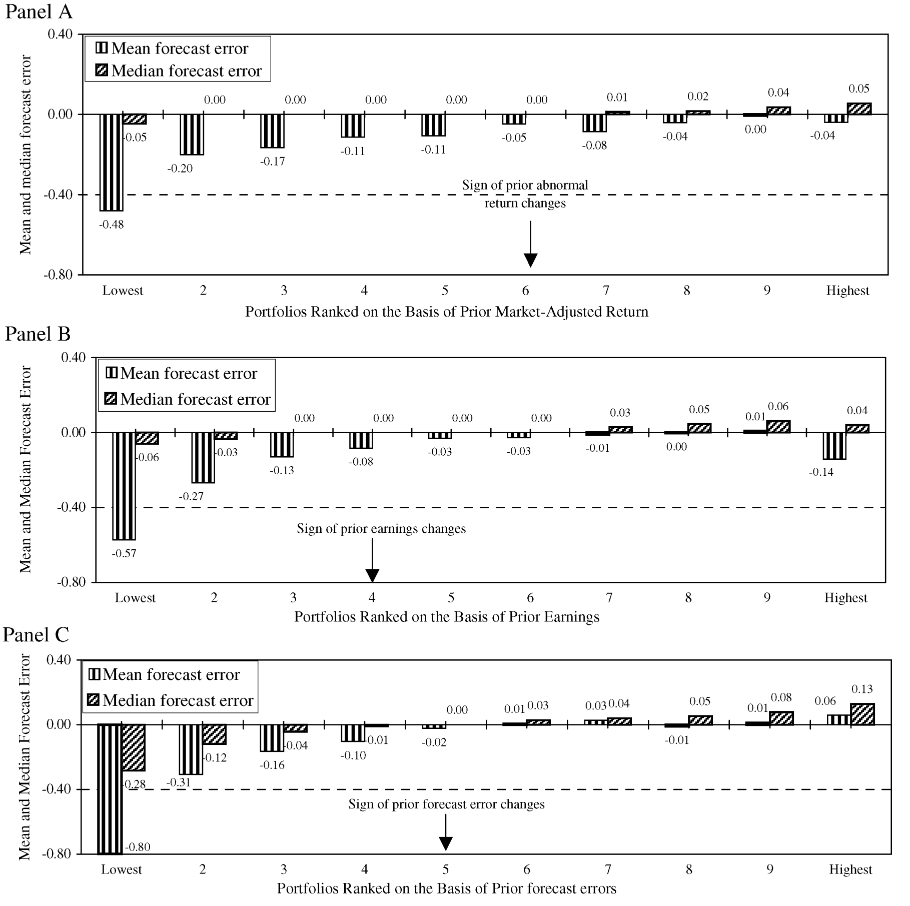


Fig. 5. Mean and median forecast errors by decile ranking of prior abnormal return (Panel A), prior earnings changes (Panel B), and prior forecast errors (Panel C). This figure depicts mean and median forecast errors for portfolios ranked on the basis of prior abnormal return (Panel A), prior earnings changes (Panel B), and prior forecast errors (Panel C). Prior abnormal return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the beginning-of-period price. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by price.

of 0.380, -0.559, and 0.194, for *PrAR*, *PrEC*, and *PrFE*, respectively, and *t*-statistics are significantly reduced in each case. Removal of individual deciles 2–9 before reestimating the regressions leads to virtually no change in the coefficients for all three prior news variables, whereas removal of the 10th decile actually leads to increases in the coefficients for all three variables. Notably, the disproportionate influence of extreme forecast error observations associated with extreme prior news is an effect that is not specifically predicted by extant behavioral or incentive-based theories of analyst inefficiency.[17]

The middle asymmetry also contributes, albeit more subtly than the tail asymmetry, to producing OLS regression coefficients that are consistent with underreaction to bad news. As seen in the first row of Panels A–C of Table 4 ("Overall"), which presents the ratio of positive to negative forecast errors by deciles of all three prior news variables, the percentage of positive errors increases as prior news improves. Consider, for example, in Panel A, the evidence for the first 5 deciles of *PrAR*, which only pertain to prior bad news realizations. The steadily increasing rate of small positive errors as *PrAR* improves will contribute to a positive slope coefficient in OLS regressions of forecast errors on prior bad news, reinforcing an inference of underreaction from this statistic. The concern raised by evidence in the remaining rows of Panel A of Table 4 is that less extreme prior bad news generates increasingly higher incidences of small positive versus small negative forecast errors—that is, observations that represent exactly the opposite of analyst underreaction.

Finally, recall that nonparametric statistics, including percentages of negative errors, rank regression slopes, and medians, also provide consistent indications of analyst underreaction to bad news. The nonparametric evidence in Panel A of Table 4 suggests however that this finding is also not as robust as it first appears. In the case of *PrAR*, for example, only the two most extreme negative deciles are associated with a reliably higher frequency of negative errors, which would not be expected if analyst underreaction to bad news was a pervasive phenomenon. In fact, there is a monotonic increase in the rate of positive to negative errors in the deciles that contain bad news realizations, with the 3rd decile containing a statistically equal number of each, and deciles 4–6 containing a reliably *greater* number of positive than negative errors.[18] Thus, observations that form the tail asymmetry, which is most pronounced in extreme bad news *PrAR*, even have a disproportional impact on some nonparametric evidence of underreaction to bad news, including indications from medians, percentages of negative errors, and rank regressions.[19]

[17] It is not well recognized that the inference of underreaction to prior bad news generated by the parametric tests favored in the literature is common to all prior news variables and is always driven by the concentration of extreme negative errors associated with extreme prior bad news. This conclusion can be drawn from studies investigating over/underreaction to prior returns (see, e.g., Brown et al., 1985; Klein, 1990; Lys and Sohn, 1990; Abarbanell, 1991; Elgers and Murray, 1992; Abarbanell and Bernard, 1992; Chan et al., 1996) and studies investigating over/underreaction to prior earnings changes (see, e.g., De Bondt and Thaler, 1990; Abarbanell and Bernard, 1992; Easterwood and Nutt, 1999).

[18] The 6th decile of *PrAR* includes small negative, small positive, and a limited number of zero observations. The demarcation point of zero occurs in the 4th decile of *PrEC*, reflecting a greater likelihood of positive earnings changes than negative earnings changes. The demarcation occurs in the 5th decile of *PrFE*, reflecting both a high percentage of zero prior forecast errors as well as the higher incidence overall of positive versus negative errors associated with the middle asymmetry. As suggested in footnote 15, simply partitioning prior news at the value of zero (as is done in the literature) may not lead to appropriate comparisons with respect to analyst efficiency across prior news variables in all situations.

[19] Recall that rank regressions of forecast errors and prior news produce large positive and significant slope coefficients, consistent with underreaction to bad news prior returns even though the incidence of positive errors is equal to or greater than the incidence of negative forecast errors in all but the most

Table 4
Ratio of small positive to small negative forecast errors in symmetric regions by decile ranking of prior abnormal return (Panel A), prior earnings changes (Panel B), and prior forecast error (Panel C)

Range of forecast errors	Lowest	2	3	4	5	6	7	8	9	Highest
Panel A: Ratio of small positive to small negative forecast errors and percentage of total decile observations within deciles of prior abnormal return										
Overall	0.66	0.78	0.97	1.08	1.17	1.27	1.33	1.39	1.76	2.12
[−0.1, 0) & (0, 0.1]	1.39	1.12	1.35	1.51	1.53	1.61	1.66	1.75	1.84	2.43
	24%	30%	32%	34%	35%	36%	38%	36%	34%	31%
[−0.2, −0.1) & (0.1, 0.2]	1.11	1.16	1.26	1.24	1.49	1.53	1.46	1.54	2.41	2.60
	18%	19%	21%	19%	20%	21%	20%	20%	21%	21%
[−0.3, −0.2) & (0.2, 0.3]	0.75	0.83	0.99	1.15	1.14	1.31	1.72	1.56	2.02	2.64
	10%	11%	11%	11%	12%	12%	11%	12%	12%	11%
Panel B: Ratio of small positive to small negative forecast errors and percentage of total decile observations within deciles of prior earnings changes										
Overall	0.75	0.77	0.86	0.91	1.16	1.53	1.83	1.87	1.83	1.45
[−0.1, 0) & (0, 0.1]	1.52	1.30	1.18	1.14	1.38	2.10	2.36	2.07	2.00	1.98
	16%	21%	28%	41%	56%	54%	45%	33%	25%	18%
[−0.2, −0.1) & (0.1, 0.2]	1.25	1.15	1.11	1.08	1.29	1.57	2.24	2.54	2.20	1.91
	13%	19%	21%	23%	19%	20%	24%	25%	22%	15%
[−0.3, −0.2) & (0.2, 0.3]	0.97	0.98	0.91	0.79	0.93	1.19	2.03	2.17	1.98	2.19
	9%	12%	13%	12%	7%	9%	11%	13%	13%	11%
Panel C: Ratio of small positive to small negative forecast errors and percentage of total decile observations within deciles of prior forecast errors										
Overall	0.53	0.58	0.70	0.74	1.32	2.25	2.06	1.91	1.95	1.82
[−0.1, 0) & (0, 0.1]	1.10	0.90	0.91	0.87	1.50	3.02	2.22	2.05	2.09	1.65
	8%	15%	24%	37%	65%	58%	46%	33%	24%	13%
[−0.2, −0.1) & (0.1, 0.2]	1.27	0.94	0.88	0.90	1.16	2.17	2.68	2.59	2.75	1.99
	10%	17%	23%	25%	18%	21%	24%	25%	23%	16%
[−0.3, −0.2) & (0.2, 0.3]	0.90	0.71	0.69	0.64	1.28	1.69	2.16	2.66	2.20	2.32
	9%	12%	14%	11%	7%	8%	10%	14%	15%	13%

This table reports the ratio of small positive to small negative forecast errors for observations that fall into increasingly larger and nonoverlapping symmetric intervals moving out from zero forecast errors and the percentage of observations that fall in these intervals of the total nonzero forecast errors in that decile. Prior abnormal return is the return between 10 days after the last quarterly earnings announcement to 10 days prior to current quarterly earnings announcement minus the return on the value-weighted market portfolio for the same period. Prior earnings changes are defined as the prior quarter seasonal earnings change (from quarter $t-5$ to quarter $t-1$) scaled by the beginning-of-period price. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by price.

(*footnote continued*)
extreme deciles of bad news *PrAR*. This occurs because the most negative ranks of *PrAR* are paired with the most negative forecast errors, which when combined with the increasing incidence of pessimistic errors as bad news becomes less extreme (in principle, overreaction), accounts for an overall positive association in the rank slope coefficient that is consistent with apparent underreaction.

3.3. How robust is the evidence of misreaction to prior good news?

As seen in Tables 2 and 3, evidence can be found for either analyst underreaction or overreaction to prior good news, depending on the statistical approach and/or prior variable on which the researcher focuses. Our goal in this section is to examine the robustness of parametric evidence of analyst overreaction and nonparametric evidence of analyst underreaction to good news.

In Panel A of Fig. 4, the most extreme prior good news decile in the case of *PrAR* does not display a tail asymmetry substantially different from the combined deciles 2–9. In contrast, in the case of *PrEC* (in Panel B) the most extreme positive decile actually exhibits the second largest degree of tail asymmetry inasmuch the combined inner decile distribution (deciles 2–9) has a considerably smaller tail asymmetry. In the case of *PrFE*, depicted in Panel C, the most extreme positive decile displays a slightly greater degree of tail asymmetry than the combined deciles 2–9. Thus, although the tail asymmetry is always present in extreme prior good news deciles, there is considerable variation in the degree of tail asymmetry across extreme good news realizations of prior news variables—a phenomenon that once again is not contemplated by general incentive and behavioral theories.

The statistical impact of variation in the degree of tail asymmetries in extreme good news deciles across prior variables is reflected in the mean forecast errors by decile presented in Fig. 5. Notably, as seen in Panel B, the relatively large tail asymmetry associated with extreme good news *PrEC* leads to a negative mean error in the 10th decile (i.e., overreaction), which aligns with the large tail asymmetry observed in Panel B of Fig. 4. In contrast, mean forecast errors for the good news *PrEC* deciles 5–9 are small and in many cases significantly positive (i.e., consistent with underreaction) because the tail asymmetry associated with these observations is small. The disproportional influence of the 10th decile of *PrEC* is also evident in regression results. In addition to being responsible for the only overall prior good news partition that produces a significant OLS slope coefficient, it is the only individual decile comprising good news for any variable that produces a significant slope coefficient (unreported in the tables). We note that removal of the 10th decile from the overall regression of forecast errors on *PrEC* leads to an increase in the slope coefficient from a value of 0.819 to 3.17, with a corresponding increase in the t-statistic. That is, the strong negative association between forecast errors and prior good news in this decile, which contributes disproportionately to the finding of overreaction to good news, also introduces severe nonlinearity in the overall regression.[20]

[20] The increasing rate of small positive errors as good news becomes more extreme contributes to positive slope coefficients in OLS regressions of forecast errors on prior good news. This is analogous to the impact of increasing rates of positive errors as bad news becomes less extreme, an effect more evident when the most extreme decile of good news is removed. The concern here, however, is that more extreme prior news leads to higher incidences of less extreme positive forecast errors—a phenomenon that is not only counterintuitive but is not predicted by extant incentive and behavioral theories of analyst inefficiency.

The most extreme good news *PrEC* decile is, therefore, largely responsible for the negative slope coefficient and the negative mean observed for good news *PrEC* partitions, suggesting the dominant influence of a small number of observations from the left tail of the distribution of forecast errors in producing parametric evidence of overreaction to good news prior earnings changes. Easterwood and Nutt (1999) refer to regression results that indicate a combination of underreaction to bad news and overreaction to good news as *generalized optimism*. From the evidence presented thus far it is clear that a small number of extreme negative forecast error observations associated with both extreme bad and extreme good news *PrEC* realizations are largely responsible for this finding. The question of the robustness of the finding of generalized optimism is magnified in the case of statistical indications of overreaction to good news because, as was reported in Table 2, good news *PrAR* and *PrFE* do not generate consistent parametric evidence of generalized optimism, even in the extreme deciles. This lends a "razor's edge" quality to the result that hinges on whether there is a sufficiently large number of extreme bad and good news realizations associated with extremely negative forecasts.[21] Furthermore, ambiguity in interpreting the evidence is introduced because there is no extant behavioral or incentive theory of analyst inefficiency that predicts that, when overreaction occurs, it will be concentrated among extreme prior news and come in the form of extreme analyst overreaction.

Finally, just as in the case of prior bad news, the presence of asymmetries also raises questions about the robustness of nonparametric evidence of analyst misreaction to prior good news. Recall from Section 3.1.1 that, in contrast to parametric statistics, nonparametric statistics suggested analyst *underreaction* to prior good news for all three prior news variables. The evidence in Tables 2 and 4 indicates that large middle asymmetries reinforce nonparametric indications of underreaction—in particular, the increasing relation between the magnitude of good news and the likelihood of small positive forecast errors, a relation that is monotonic in the case of *PrAR* and *PrFE*. Thus, the middle asymmetry, and its variation with the magnitude of prior good news, has a disproportionate impact on the inference of underreaction to good news from nonparametric statistics, including indications from medians, percentages of negative errors, and rank regressions. Notably, the percentage of positive forecast errors is substantially larger than the percentage of negative errors even in the most extreme *PrEC* decile. That is, the decile largely responsible for producing the only statistical evidence that analysts overreact to good news displays a strong tendency for errors that are consistent with underreaction.

3.4. The tail and middle asymmetries and serial correlation in analysts' forecasts

The preceding results indicate that regression evidence of underreaction is disproportionately influenced by apparent extreme underreaction to extreme bad

[21] Easterwood and Nutt (1999) eliminate the middle third of the prior earnings news distribution before estimating OLS slope coefficients, which provide the statistical support for their conclusion that analysts underreact to bad news and overreact to good news. Clearly, this test design gives even greater weight to observations that comprise the tail asymmetry.

prior news and is also impacted by the increase in the middle asymmetry as prior news improves. The asymmetries have important impacts on alternative (to regression) tests of analyst inefficiency in the literature. For example, as mentioned earlier, the analysis of the relation between current and prior forecast errors is typically not couched in terms of over- or underreaction to signed prior news, but rather in terms of overall serial correlation in lagged analyst forecast errors (see, e.g., Brown and Rozeff, 1979; Mendenhall, 1991; Abarbanell and Bernard, 1992; Ali et al., 1992; Shane and Brous, 2001; Alford and Berger, 1999). These studies focus almost exclusively on parametric measures of serial correlation and primarily on the first lag, or consecutive period errors.

Table 5 presents the Pearson and Spearman correlation between consecutive quarterly forecast errors for the overall sample and within each of the deciles of current forecast errors. The mean correlations for the entire sample are statistically significant, with yearly averages of 0.15 and 0.22, respectively. Note that the first decile, which includes the observations in the extreme left tail that are associated with the tail asymmetry, produces the greatest Pearson and Spearman correlations of 0.17 and 0.19, respectively. In contrast, the correlations in all other deciles are much smaller and most often statistically insignificant in the case of the Pearson measure. It is interesting to note that if distributions of forecast errors were symmetric, then forming deciles on the basis of current forecast errors (a procedure only followed in Table 5) would be expected to attenuate, relative to the overall sample serial correlation, the estimated correlation in every decile. However, the facts that correlation is not attenuated in the most extreme negative forecast error decile (in fact, it is larger than the overall correlation) and that the Pearson correlation is insignificant in the most extreme positive forecast error decile are additional indications of the important role the tail asymmetry plays in the findings of serial correlation. We note that when the deciles are formed based on *prior* forecast errors (that is they are sorted on the independent variable, as is done in all other tests performed in the paper) we still find that Pearson correlations are highest in the most extreme negative forecast error decile.[22]

Finally, we note that the strongest Spearman correlations in the table, other than the most extreme negative decile of current forecast errors, are found in deciles 6 and 7, i.e., those with a high concentration of current and prior small pessimistic forecast errors. The evidence is also inconsistent with what would be expected based on forming deciles on current forecast errors, where correlation in the middle deciles would be driven to zero. The higher correlations in deciles 6 and 7 are found whether deciles are formed on current or prior forecast errors. The evidence suggests the need for further exploration into the role of observations in the middle asymmetry in producing estimated serial correlation consistent with apparent analyst under-reaction to their own forecast errors.

[22] It is also interesting to note from columns 4 and 5 that the first decile is not only associated with the largest mean values for current forecast errors, but is also associated with the largest mean value among the prior (i.e., lagged) forecast error deciles.

Table 5
Serial correlation in consecutive-period forecast errors

Decile ranking of forecast errors (1)	Pearson correlation in consecutive forecast errors (2)	Spearman correlation in consecutive forecast errors (3)	Mean forecast errors (4)	Mean prior quarter forecast errors (5)
Lowest	0.17#	0.19#	−2.08	−0.79
2	0.04&	0.07#	−0.44	−0.26
3	0.03	0.06#	−0.17	−0.12
4	0.06#	0.05&	−0.06	−0.04
5	0.06#	0.03&	0.00	−0.07
6	−0.01	0.09#	0.03	0.04
7	0.01	0.08#	0.08	0.04
8	−0.02	0.04&	0.15	−0.01
9	0.00	0.04&	0.29	0.02
Highest	0.00	0.04&	0.90	−0.12
Overall	0.15#	0.22#	−0.13	−0.13

This table reports the Pearson and Spearman correlation coefficients and means of current and prior quarter forecast errors *within* deciles of the ranked (current) forecast error distribution. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by beginning-of-period price.
#(&) Represents a statistically significant correlation at a 1% (5%) level.

3.5. *Summary and implications of the tail and middle asymmetries on inferences of analyst efficiency*

An important conclusion from the analysis of conditional forecast error distributions is that the sign of prior news variables sorts observations from the tail and middle asymmetries in a manner that (1) reinforces the inference of underreaction found in parametric statistics for all prior *bad* news partitions, an inference that is largely the result of the dominant impact of the tail asymmetry; and (2) can create offsetting or reinforcing effects that contribute to producing conflicting signs of means and regression slope coefficients within and across different prior *good* news partitions of the variables. Thus, the presence of middle and tail asymmetries in conditional distributions of forecast errors helps explain why evidence of underreaction to bad news appears to be so robust in the literature while evidence of under- and overreaction to good news is not. Attenuation of means and slope coefficients due to the relatively greater impact of the middle asymmetry in good news distributions of forecast errors also helps explain why, in every study to date that employs parametric tests and concludes that analysts' forecasts are inefficient, the magnitude of misreaction to bad news is always found to be greater than the magnitude of misreaction to good news.

It is tempting to infer from the insignificance of slope coefficients pertaining to regressions of forecast errors on prior news generated for some good news partitions reported in Table 3 and in all inner deciles of distributions of all prior news variables that, apart from cases of extreme prior news, analysts produce efficient forecasts (see, footnote 16). However, the sensitivity of statistical findings in prior good news partitions documented above suggests that we exercise caution in reaching this conclusion. Results in Fig. 4 and Table 4, along with unreported results, verify that all decile partitions of *PrAR* and *PrEC* are characterized by both middle and tail asymmetries, and that every good (bad) news decile of *PrFE* is characterized by a middle (tail) asymmetry. While it is possible that failure to reject zero slope coefficients in the inner deciles is the result of a general tendency for analyst forecasts to be efficient when prior news is not extreme, we must concede the possibility that the lower variation in the independent variable and small numbers of observations associated with tail and middle asymmetries *within deciles* combine to produce nonlinearities and lower power in a manner that obscures evidence of analyst inefficiency. That is, slicing up the data into greater numbers of partitions does not appear to eliminate the potential impact of both asymmetries in influencing inferences concerning the existence and nature of analyst inefficiency in parametric tests.[23]

The evidence in this section reveals how asymmetries can produce and potentially obscure indications of analyst inefficiency, depending on the statistical approach adopted by the researcher. Next, we describe examples of procedures that (perhaps unintentionally) mitigate the impact of observations that comprise the asymmetries, but may not necessarily shed new light on the question of whether analysts' forecasts are efficient.

3.6. Data transformations, nonlinear statistical methods, and alternative loss functions

Apart from partitioning forecast errors in parametric tests and applying nonparametric tests, some studies implicitly or explicitly adjust the underlying data in order to attenuate the disproportional impacts and nonlinearities induced by the tail asymmetry. Two such approaches are truncating and winsorizing forecast errors. As in the case of inferences concerning bias discussed in Section 2, the effects of arbitrary truncations on inferences concerning analyst under- and overreaction can be significant. Keane and Runkle (1998), for example, argue that evidence of misreaction to prior earnings news is overstated as a result of uncontrolled cross-correlation in forecast errors. However, they explicitly state that their finding of efficiency—after applying GMM to control for bias in standard errors induced by cross-correlation—rests on having first imposed a

[23] Severe heteroscedasticity in the decile regression residuals are consistent with this argument. In addition, while we do not advocate arbitrary truncations of the data to mitigate the impact of the asymmetries we find that small symmetric truncations of tail observations within decile distributions similar to those described in the previous section for the unconditional distribution of forecast errors result in significant slope coefficients in many of the inner deciles of prior returns and prior earnings changes. Because small truncations of extreme observations reduce the number of observations in each decile and further reduce variation in the independent variable, it is possible that the statistical significance of the coefficients after truncation in these cases reflects the presence of analyst inefficiency and/or the elimination of the offsetting impact of the tail asymmetry in a manner that allows the middle asymmetry to dominate an inference of inefficiency.

sample selection criterion that results in the truncation of large forecast error observations in the extreme negative tail of the distribution. Their argument for doing so is that the Compustat reported earnings used to benchmark forecasts for such observations includes large negative transitory items that analysts do not forecast. Abarbanell and Lehavy (2002) show that tail asymmetries also characterize distributions of forecast errors based on the earnings reported by commercial forecast data sources such as I/B/E/S, Zacks, and First Call, which are, in principle, free of such special items. They also report a high correlation between the observations that fall into the extreme negative tail of the distribution of forecast errors calculated with Compustat-reported earnings and those that fall into the extreme negative tail of distributions calculated with earnings provided by forecast data services. Thus, it remains to be seen whether the finding of analyst forecast rationality continues to hold when GMM procedures are applied to untruncated distributions of forecast error based on "cleaned" reported earnings numbers rather than truncated distributions of forecast errors based on Compustat earnings.[24]

An alternative to arbitrarily truncating a subset of observations is to transform the entire distribution of forecasts, a common procedure used to eliminate nonlinearities, stabilize variances, or induce a normal distribution of forecast errors to avoid violating the assumptions of the standard linear model. For example, log and power transformations mitigate skewness and the disproportionate impact of extreme observations when the dependent variable is forecast errors. However, each type of transformation alters the structure of the data in a unique way, and it is possible for different transformations to yield different inferences concerning analyst inefficiency. That is, transformations of distributions of forecast error are not likely to lead to greater consensus in the literature unless strong a priori grounds for preferring one transformation to another can be agreed upon. Such grounds can only be found by gaining a better understanding of what factors are responsible for creating relevant features of the untransformed data—an understanding that in turn would require more exacting theories than have thus far been produced as well as more institutional research into the analysts' actual forecasting task.

Finally, instead of adapting the data to fit the model the researcher may choose to adapt the model to fit the data. Disproportionate variation in the degree of tail asymmetry as a function of the sign and magnitude of prior news suggests, at a minimum, that parametric tests of analyst inefficiency should be adapted to allow for the nonlinear relationship between forecast errors and prior news. For example, after Basu and Markov (2003) replaced the quadratic assumption in their standard OLS regression with a linear loss function assuming that analysts minimize absolute forecast errors, they found little evidence to support analyst inefficiency. Imposing this loss function has an effect similar to truncating extreme observations, since such

[24] We note that although arbitrarily truncating the dependent variable (e.g., Keane and Runkle, 1998) may seem to be a more egregious form of biasing a test, the evidence presented earlier suggests that arbitrarily truncating observations in the middle of the distribution of the prior earnings news (e.g., Easterwood and Nutt, 1999) can also create problems when researchers draw inferences about the tendency for analysts to misreact to prior news, inasmuch as this procedure can further accentuate the already disproportionate impact of the tail asymmetry.

observations are given less weight in the regression (as opposed to being removed outright from the distribution).[25]

Clearly there is something to be learned from examining how inferences change under different assumed loss functions. However, at this stage in the literature, the approach will have limited benefits for a number of reasons. First, while a logical case can be made for one loss function that leads to the failure to reject unbiasedness and efficiency, an equally strong case for a loss function that leads to a rejection of unbiasedness and efficiency can also be made, without either assumption being inconsistent with existing empirical evidence of how analysts are compensated. In such cases, the conclusion about whether analyst forecasts are rational will hinge on which assumption best describes analysts' true loss function—a subject about which we know surprisingly little.[26] Second, it is possible that some errors are actually partially explained by cognitive or incentive factors that are coincidental with or are exacerbated by other factors that give rise to the same errors the researcher underweights by assuming a given loss function. Finally, although assuming a given loss function—like the choice of alternative test statistics or data truncations—may lead to a statistical inference consistent with rationality, such an approach ignores the empirical fact that the two notable asymmetries are present in the distribution. Given their influence on inferences, providing compelling reasons for these asymmetries is a prerequisite for judging whether and in what circumstances incentives or cognitive biases induce analyst forecast errors.

In the next section we take a step toward understanding how the asymmetries in forecast error distributions arise by identifying a link between the presence of observations that comprise the two asymmetries and unexpected accruals included in the reported earnings used to benchmark forecasts. This link suggest the possibility that some "errors" in the distribution of forecast errors may arise only because the forecast was inappropriately benchmarked with *reported* earnings, when in fact the analyst had targeted a different earnings number.

4. Linking bias in reported earnings to apparent bias and inefficiency in analyst forecasts

4.1. Accounting conservatism and unexpected accruals

Abarbanell and Lehavy (2003a) argue that an important factor affecting the recognition of accounting accruals is the conservative bent of GAAP. Because

[25] Note that, as discussed earlier, there may be greater difficulty detecting irrationality (alternatively, a greater likelihood of failing to reject efficiency) using regression analysis once procedures that attenuate the impact of left tail observations are introduced because the middle asymmetry is still present.

[26] The fact that the evidence of misreaction to even extreme good news is mixed for different definitions of prior news and different parametric statistics presents a challenge to adapting behavioral theories to better fit the data. Unless we can identify a common cognitive factor that explains why differences in apparent misreaction depend on the extremeness of prior news, the empirical case for any form of generalized bias or inefficiency will hinge on a relatively small number of observations comprising the tail and middle asymmetries that are not predicted by the theory.

conservative accounting principles facilitate the immediate recognition of economic losses but restrict the recognition of economic gains, the maximum amount of possible income-decreasing accruals that a typical firm can recognize in a given accounting period will be larger than the maximum amount of income-increasing accruals (see, e.g., Watts, 2003). Table 6 provides evidence that supports this intuition.

The table presents selected summary statistics associated with cross-sectional distributions of firms' quarterly unexpected accruals over the sample period.[27] The mean unexpected accrual over the sample period is −0.217. While the distribution is negatively skewed, the median is 0.023 and the percentage of positive and negative unexpected accruals is nearly equal. It is evident from Table 6 that, while the unexpected accrual distribution is relatively symmetric in the middle, it is characterized by a longer negative than positive tail. For example, the magnitude of the average values at the 25th and 75th percentiles is nearly identical. However, symmetric counterpart percentiles outside these values begin to diverge by relatively large amounts, beginning with a comparison of the values at the 10th and 90th percentiles. The differences become progressively larger with comparisons of counterpart percentiles farther out in the tails. For example, the average 5th and 3rd percentile values are approximately 1.17 times larger than the average 95th and 97th percentiles, and the average value of the 1st percentile is 1.30 times larger than the average value of the 99th percentile. We stress that, although the percentile values of unexpected accruals vary from quarter to quarter, the basic shape of the distribution is similar in every quarter.

4.2. Linking unexpected accruals to asymmetry in tails of forecast error distributions

The measure of unexpected accruals we employ is based on historical relations known prior to the quarter for which earnings are forecast. Although the term "unexpected" is used, it is possible—in fact likely—that analysts will acquire new information about changes in the relations between sales and accruals that occurred during the quarter before they issue their last forecast for a quarter. Nevertheless, we can use the measure of unexpected accruals to identify, ex-post, cases in which significant changes in accrual relations did take place, and then assess whether the evidence is consistent with analysts' issuing a final forecast of earnings for the quarter either unaware of some of these changes or unmotivated to forecast them.

If analysts' forecasts do not account for the fact that some firms will recognize accruals placing them in the extreme negative tails of the distribution of unexpected accruals, then there will be a direct link between the negative tail of this distribution and the extreme negative tail of the forecast error distribution. The conjectured link

[27] Unexpected accruals reported in the tables are the measure produced by the modified Jones model applied to quarterly data (see Appendix A for calculations). To facilitate comparison with our forecast error measure, we express unexpected accruals on a per share basis scaled by price and multiplied by 100. As indicated earlier, the qualitative results are unaltered when we employ the unmodified Jones model and other estimation techniques found in the literature, including one that excludes nonrecurring and special items.

Table 6
Descriptive statistics on quarterly distributions of unexpected accrual, 1985–1998

Unexpected accrual	
Number of observations	33,548
Mean	−0.217
Median	0.023
Standard deviation	5.600
Skewness	−1.399
Kurtosis	16.454
% Positive	50.8
% Negative	49.2
% Zero	0.0
P1	−20.820
P3	−11.547
P5	−8.386
P10	−4.574
P25	−1.349
P75	1.350
P90	4.185
P95	7.148
P97	9.891
P99	15.945

This table reports descriptive statistics on quarterly distributions of unexpected accruals. Unexpected accruals are calculated using the modified Jones model as described in the appendix (expressed as unexpected accrual per share scaled by price and multiplied by 100).

is depicted in Fig. 6. The figure shows mean forecast errors in intervals of (+/−) 0.5% centered on the percentiles of unexpected accruals. For example, the mean forecast error corresponding to the Xth percentile of unexpected accruals is computed using observations that fall in the interval of $X-0.5$ to $X+0.5$ percentiles of the unexpected accruals distribution.

It is clear from Fig. 6 that extreme negative forecast errors are associated with extreme negative unexpected accruals. That is, the evidence suggests a direct connection between the tail asymmetry in the forecast error distribution (documented in earlier sections) and an asymmetry in tails of the unexpected accrual measure.[28] This link continues to be observed even when we employ consensus earnings estimates and reported earnings that are, in principle, stripped of

[28] Another example of this link relates to the evidence on serial correlation in forecast errors presented earlier. Recall from Table 5 that the most extreme prior forecast error decile is also associated with the most negative mean current forecast errors. In unreported results we find that this decile is also characterized by the largest negative lagged and current unexpected accruals observed for these deciles (whether forecast error deciles are formed on the current or prior forecast errors). Thus, consecutive quarters of large, negative unexpected accruals go hand-in-hand with consecutive quarters of extreme negative forecast error observations that, in turn, are associated with high levels of estimated serial correlation.

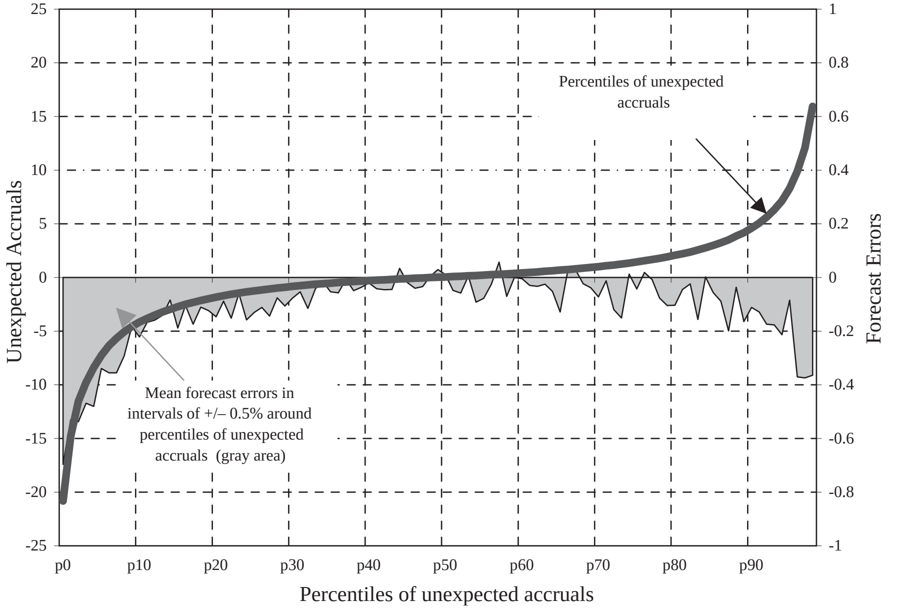


Fig. 6. Linking unexpected accruals and the asymmetry in tails of forecast error distributions. This figure depicts percentiles of unexpected accruals and mean forecast errors (gray area) in intervals of (+/−) 0.5% around unexpected accruals percentiles. For example, the mean forecast errors corresponding to the Xth percentile of unexpected accruals is computed using observations that fall in the interval of $X-0.5$ to $X+0.5$ percentiles of the unexpected accruals distribution. Forecast error equals reported earnings minus consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price. Unexpected accruals are the measure produced by the modified Jones model as described in the appendix (expressed as percentage of unexpected accrual per share scaled by price and multiplied by 100).

nonrecurring items and special charges (because Zacks indicates that analysts do not attempt to forecast these items), and a measure of unexpected accruals that also strips such items (see, Hribar and Collins, 2002). This suggests that an association exists between extreme negative accruals deemed "special or nonrecurring" and extreme negative accruals that do not fit this description. One possible reason for this association is that firms take an "unforecasted earnings bath," recognizing operating expenses larger than justified by the firm's actual performance for the period at the same time as they recognize large discretionary or nondiscretionary negative transitory operating and nonoperating items (see, Abarbanell and Lehavy, 2003b).

A second explanation for the association between large negative unexpected accruals and large negative forecast errors is that all the models of unexpected accruals examined in this study are prone to misclassifying nondiscretionary accruals as discretionary in periods when firms are recognizing large, negative transitory items. Combining the misclassification argument with a cognitive based argument that analysts react too slowly to extreme current performance would account for the observed link between unexpected accruals and forecast errors. While a more detailed analysis is beyond the scope of this paper, the evidence in Fig. 6 sheds additional light on the question of misclassification. It is seen in the figure that the largest percentiles of *positive* unexpected accruals are actually associated with fairly large negative mean forecast errors. The upside down U-shape that characterizes mean forecast errors over the range of unexpected accruals is inconsistent with a straightforward misclassification argument.[29] This is because if extreme positive unexpected accruals reflected misclassification in the case of firms that experience strong current performance, these would be the same cases in which analysts' forecasts would tend to underreact to extreme current good news and issue forecasts that fall short of reported earnings. The association between firm recognition of large negative transitory items and large negative operating items and the association between forecast errors and unexpected accruals are empirical phenomena that clearly deserve further exploration.

4.3. Linking unexpected accruals and the asymmetry in the middle of forecast error distributions

Table 7 provides evidence suggesting that unexpected accruals are also associated with the middle asymmetry in forecast error distributions. Column 2 presents a comparison of the ratio of positive to negative errors in narrow intervals centered on a zero forecast error (as reported in Panel B of Table 1) to the analogous ratio when forecast errors are based on reported earnings after "backing out" the realization of unexpected accruals for the quarter. In sharp contrast to the results reported in Table 1, the results in Table 7 indicate that after controlling for unexpected accruals, the number of small positive forecast errors *never* exceeds the number of small negative forecast errors in any interval. For example, the ratio of good to bad earnings surprises in the interval between $[-0.1, 0)$ and $(0, 0.1]$ is 1.63 (a value reliably different from 1) when errors are computed using earnings as reported by the firm, compared to 0.95 (statistically indistinguishable from 1) when errors are based on reported earnings adjusted for unexpected accruals. Thus, as in the case of the tail asymmetry, there is an empirical link between firms' recognition of unexpected accruals and the middle asymmetry. Given the impact of the tail and middle asymmetries on inferences concerning analyst bias and inefficiency described in Sections 2 and 3, researchers should take into account the role of unexpected accruals in the reported earnings typically used to benchmark forecast.

[29] The plot of *median* forecast errors around unexpected accrual percentiles also displays an upside down U-shape. However, as one might expect from the summary statistics describing the forecast error distributions in Table 1, the magnitude of these median errors is much smaller than mean errors, and large negative median forecast errors are only found in the most extreme positive and negative unexpected accrual percentiles.

Table 7
Linking unexpected accruals and the asymmetry in the middle of forecast error distributions

Range of forecast errors (1)	Ratio of positive to negative forecast errors based on *reported* earnings (2)	Ratio of positive to negative forecast errors based on earnings adjusted for unexpected accruals (3)
Overall	1.19*	0.96*
[−0.1, 0) & (0, 0.1]	1.63*	0.95
[−0.2, −0.1) & (0.1, 0.2]	1.54*	0.97
[−0.3, −0.2) & (0.2, 0.3]	1.31*	1.09
[−0.4, −0.3) & (0.3, 0.4]	1.22*	0.97
[−0.5, −0.4) & (0.4, 0.5]	1.00	0.99
[−1, −0.5) & (0.5, 1]	0.83*	0.95*
[Min, −1) & (1, Max]	0.40*	0.95*

This table provides the ratio of positive to negative forecast errors for observations that fall into increasingly larger and nonoverlapping symmetric intervals moving out from zero forecast errors. For example, the forecast error range of [−0.1, 0) & (0, 0.1] includes all observations that are greater than or equal to −0.1 and (strictly) less than zero and observations that are greater than zero and less than or equal to 0.1. Forecast error is reported earnings minus the last consensus forecast of quarterly earnings issued prior to earnings announcement scaled by the beginning-of-period price. Earnings before unexpected accruals (used to compute the forecast error ratios in column 3) are calculated as the difference between reported earnings and the empirical measure of unexpected accruals.
*A test of the difference in the frequency of positive to negative forecast errors is statistically significant at or below a 1% level.

4.4. Explanations for a link between asymmetries in forecast error distributions and unexpected accruals

One general explanation for the link between unexpected accruals and the presence of asymmetries in forecast error distributions is that incentive or judgment factors that affect analysts' forecasts are exacerbated when estimates of unexpected accruals are likely to be unusual. For example, it is possible that cases of underreaction that appear to be concentrated among firms with the most extreme bad news reflect situations in which analysts have the weakest (strongest) incentives to lower (inflate) forecasts or suffer from cognitive obstacles that prevent them from revising their forecasts downward. At the same time, it has been argued in the accounting literature that unexpected accrual models produce biased downward estimates in exactly the same circumstances, i.e., when firms are experiencing extremely poor performance (see, e.g., Dechow et al., 1995).[30] This combination of

[30] The controversy over bias in unexpected accrual estimates relates to the issue of whether they truly reflect the exercise of discretion on the part of management. The conclusion that such measures are flawed is generally based on results from misclassification tests in which the maintained assumption is that historical data have not been affected by earnings management. This assumption can be challenged on logical grounds and, somewhat circularly, on the grounds that no evidence in the empirical literature supports this assumption.

potentially unrelated factors could account for the fact that extreme negative unexpected accruals accompany analysts' final forecasts for quarters characterized by prior bad news. Analogously, a higher incidence of small positive versus small negative errors as news improves is consistent with a greater likelihood of a *fixed* amount of judgment-related underreaction or incentive-based inflation of forecasts the better the prior news. The fact that unexpected accruals also appear to be related to the presence of the middle asymmetry may be coincidental to a slight tendency for unexpected accrual estimates to be positive in cases of firms experiencing high growth and positive returns (see, e.g., McNichols, 2000).[31]

Clearly there is a long list of possible combinations of unrelated factors that can simultaneously give rise to the two asymmetries in forecast error distributions and their apparent link to unusual unexpected accruals, which makes it difficult to pinpoint their source. Nevertheless, researchers still have good reason to consider these empirical facts when developing empirical test designs, choosing test statistics, and formulating and refining analytical models. One important reason is that if analysts' incentives or errors in judgment are responsible for systematic errors, it should be recognized that these factors appear to frequently produce very specific kinds of errors; i.e., small positive and extreme negative errors. To date, however, individual incentive and cognitive-based theories do not identify the economic conditions, such as extreme good and bad prior performance, that would be more likely to trigger or exacerbate incentive or judgment issues in a manner leading to exactly these types of errors. These explanations are also not easily reconciled with an apparent schizophrenia displayed by analysts who tend to slightly underreact to extreme good prior news with great regularity, but overreact extremely in a limited number of extreme good news cases. Finally, current behavioral and incentive-based theories do not account for actions undertaken by *firms* that produce reported earnings associated with forecast errors of the type found in the tail and middle asymmetries. Until such theories begin to address these issues it is not clear how observations that fall into the observed asymmetries should be treated in statistical tests of general forms of analyst irrationality. The identification of specific types of influential errors and their link to unexpected accruals documented in this paper provides a basis or expanding and refining behavioral and incentive theories of forecast errors.

A second reason for focusing on the empirical properties of forecast error distributions and their link to unexpected accruals is because it supports an alternative perspective on the cause of apparent forecast errors; i.e., the possibility that analysts either lack the ability or motivation to forecast discretionary biases in reported earnings. If so, then earnings manipulations undertaken to beat forecasts or to create reserves (e.g., earnings baths) that *are not* anticipated in analysts' forecasts

[31] McNichols (2000) argues that a positive association between unexpected accruals and growth reflects a bias in unexpected accrual models, but she does not perform tests to distinguish between this hypothesis and the alternative that high-growth firms are more likely to recognize a positive discretionary accrual to meet an earnings target, as argued in Abarbanell and Lehavy (2003a). We note that the presence of the middle asymmetry among firms with prior bad news returns and earnings changes is inconsistent with the misclassification argument.

may in part account for concentrations of small positive and large negative observations in distributions of forecast errors.[32] This suggests that evidence previously inferred to indicate systematic errors in analysts' forecasts might actually reflect the inappropriate benchmarking of forecasts.[33] An important implication of this possibility is that researchers may be formulating and testing new incentive and cognitive theories or turning to more advanced statistical methods and data transformations in order to explain forecast errors that are apparent, not real.

5. Summary and conclusions

In this paper we reexamine the evidence in the literature on analyst-forecast rationality and incentives and assess the extent to which extant theories for analysts' forecast errors are supported by the accumulated empirical evidence. We identify two relatively small asymmetries in cross-sectional distributions of forecast error observations and demonstrate the important role they play in generating statistical results that lack robustness or lead to conflicting conclusions concerning the existence and nature of analyst bias and inefficiency with respect to prior news. We describe how inferences in the literature have been affected, but these examples by no means enumerate all of the potential problems faced by the researcher using earnings surprise data. Our examples do demonstrate how some widely held beliefs about analysts' proclivity to commit systematic errors (e.g., the common belief that analysts generally produce optimistic forecasts) are not well supported by a broader analysis of the distribution of forecast errors. After four decades of research on the rationality of analysts' forecasts it is somewhat disconcerting that the most definitive statements observers and critics of earnings forecasters appear willing to agree on are ones for which there is only tenuous empirical support.

We stress that the evidence presented in this paper is not inconsistent with forecast errors due to analysts' errors in judgment and/or the effects of incentives. However, it does suggest that refinements to extant incentive and cognitive-based theories of systematic errors in analysts' forecasts may be necessary to account for the *joint* existence of both a tail asymmetry and a middle asymmetry in cross-sectional

[32] Abarbanell and Lehavy (2003b) offer theoretical, empirical, and anecdotal support for the assumption that analysts may not be motivated to account for or capable of anticipating earnings management in their forecasts. Based on this assumption they develop a framework in which analysts always forecast unmanaged earnings and firms undertake extreme income-decreasing actions or manipulations that leave reported earnings slightly above outstanding forecasts to inform investors of their private information. They describe a setting in which neither analysts nor managers behave opportunistically and investors are rational, where the two documented asymmetries in forecast error distributions arise and are foreshadowed by the sign and magnitude of stock returns before the announcement of earnings. In their setting, prior news predicts biases in the reported earnings benchmark, not biases in analysts' forecasts.

[33] Gu and Wu (2003) offer a variation on this argument suggesting that the analysts forecast the median earnings of the firm's ex-ante distribution, which also suggests that for some firms ultimate reported earnings (reports that differ from median earnings) are not the correct benchmark to use to assess whether analysts' forecasts are biased.

distributions of forecast errors. At the very least, researchers attempting to assess the descriptiveness of such theories should be mindful of the disproportionate impact of relatively small numbers of observations in the cross-section on statistical inferences.[34]

The evidence we present also highlights an empirical link between unexpected accruals embedded in the reported earnings benchmark to forecasts and the presence of the tail and middle asymmetries in forecast error distributions. Such biases in reported earnings benchmarks may point the way toward expanding and refining incentive and cognitive-based theories of analyst errors in the future. However, these results also raise questions about whether analysts are expected or motivated to forecast discretionary manipulations of reported earnings by firms. Thus, these results also highlight the fact that research to clarify the true target at which analyst forecasts are aimed is a prerequisite to making a compelling case for or against analyst rationality. Organizing our thinking around the salient properties of forecast error distributions and how they arise has the potential to improve the chaotic state of our current understanding of analyst forecasting and the errors analysts may or may not systematically commit.

Appendix A. The calculation of unexpected accruals

Our proxy for firms' earnings management, quarterly unexpected accruals, is calculated using the modified Jones (1991) model (Dechow et al., 1995); see Weiss (1999) and Han and Wang (1998) for recent applications of the Jones model to estimate quarterly unexpected accruals. All required data (as well as earnings realizations) are taken from the 1999 Compustat Industrial, Full Coverage, and Research files.

According to this model, unexpected accruals (scaled by lagged total assets) equal the difference between the predicted value of the scaled expected accruals (*NDAP*) and scaled total accruals (*TA*). Total accruals are defined as

$$TA_t = (\Delta CA_t - \Delta CL_t - \Delta Cash_t + \Delta STD_t - DEP_t)/A_{t-1},$$

where ΔCA_t is the change in current assets between current and prior quarter, ΔCL_t the change in current liabilities between current and prior quarter, $\Delta Cash_t$ the change in cash and cash equivalents between current and prior quarter, ΔSTD_t the change in debt included in current liabilities between current and prior quarter, DEP_t the current-quarter depreciation and amortization expense, and A_t the total assets.

[34] For example, given the recent attention in the literature to incentive factors that give rise to small, apparently pessimistic forecast errors (see footnote 5), it is important that researchers testing general behavioral theories understand that the middle asymmetry has the ability to produce evidence consistent with cognitive failures or, potentially, to obscure it. Similarly, the tail asymmetry has played a role in producing both parametric and nonparametric evidence that supports incentive-based theories of bias and inefficiency. However, such theories identify no role for extreme news or extreme forecast errors in generating predictions and do not acknowledge or recognize their crucial role in providing support for hypotheses.

The predicted value of expected accruals is calculated as

$$NDAP_t = \alpha_1(1/A_{t-1}) + \alpha_2(\Delta REV_t - \Delta REC_t) + \alpha_3 PPE_t,$$

where ΔREV_t is the change in revenues between current and prior quarter scaled by prior quarter total assets, ΔREC_t the change in net receivables between current and prior quarter scaled by prior quarter total assets, and PPE_t the gross property plant and equipment scaled by prior quarter total assets.

We estimate the firm-specific parameters, α_1, α_2, and α_3, from the following regression using firms that have at least ten quarters of data:

$$TA_{t-1} = a_1(1/A_{t-2}) + a_2\Delta REV_{t-1} + a_3 PPE_{t-1} + \varepsilon_{t-1}.$$

The modified Jones model resulted in 35,535 firm-quarter measures of quarterly unexpected accruals with available forecast errors on the Zacks database.

References

Abarbanell, J., 1991. Do analysts' earnings forecasts incorporate information in prior stock price changes? Journal of Accounting and Economics 14, 147–165.

Abarbanell, J., Bernard, V., 1992. Tests of analysts' overreaction/underreaction to earnings information as an explanation for anomalous stock price behavior. Journal of Finance 47, 1181–1207.

Abarbanell, J., Lehavy, R., 2002. Differences in commercial database reported earnings: implications for empirical research. Working paper, University of North Carolina and University of Michigan.

Abarbanell, J., Lehavy, R., 2003a. Can stock recommendations predict earnings management and analysts' earnings forecast errors? Journal of Accounting Research 41, 1–31.

Abarbanell, J., Lehavy, R., 2003b. An explanation for why prior stock returns and analysts' earnings forecast revisions predict earnings management and forecast errors. Working paper, University of North Carolina and University of Michigan.

Affleck-Graves, J., Davis, L., Mendenhall, R., 1990. Forecasts of earnings per share: possible sources of analyst superiority and bias. Contemporary Accounting Research 106, 501–517.

Alford, A., Berger, P., 1999. A simultaneous equations analysis of forecast accuracy, analyst following, and trading volume. Journal of Accounting, Auditing & Finance 14, 219–240.

Ali, A., Klein, A., Rosenfeld, J., 1992. Analysts' use of information about permanent and transitory earnings components in forecasting annual EPS. Accounting Review 67, 183–198.

Baber, W., Kang, S., 2002. The impact of split adjusting and rounding on analysts' forecast error calculations. Accounting Horizons 16, 277–300.

Bartov, E., Givoly, D., Hayn, C., 2000. The rewards to meeting or beating earnings expectations. Working paper, New York University.

Basu, S., Markov, S., 2003. Loss function assumptions in rational expectations tests on financial analysts' earnings forecasts. Working Paper, Emory University.

Becker, D., 2001. Analyzing analysts' remarks" by General Counsel, U.S. Securities & Exchange Commission, Remarks before the Committee on Federal Regulation of Securities of the American Bar Association, August 7.

Brown, L., 1993. Earnings forecasting research: its implications for capital markets research. International Journal of Forecasting 9, 295–320.

Brown, L., 2001. A temporal analysis of earnings surprises: profits versus losses. Journal of Accounting Research 39 (2), 221–241.

Brown, L., Rozeff, M., 1979. Univariate time series models of quarterly accounting earnings per share: a proposed model. Journal of Accounting Research 17, 179–189.

Brown, L., Foster, G., Noreen, E., 1985. Security Analysts' Multi-year Earnings Forecasts and the Capital Markets. Studies in Accounting Research, No. 21, American Accounting Association, Sarasota, FL.

Burgstahler, D., Eames, M., 2002. Management of earnings and analyst forecasts. Working paper, University of Washington.

Chan, L., Jegadeesh, N., Lakonishok, J., 1996. Momentum strategies. Journal of Finance 51, 1681–1713.

De Bondt, W., Thaler, R., 1990. Do security analysts overreact? American Economic Review 80, 52–57.

Dechow, P., Sloan, R., Sweeney, A., 1995. Detecting earnings management. Accounting Review 70, 193–225.

Dechow, P., Richardson, S., Tuna, I., 2003. Why are earnings kinky? an examination of the earnings management explanation. Review of Accounting Studies 8, 355–384.

Degeorge, F., Patel, J., Zeckhauser, R., 1999. Earnings management to exceed thresholds. Journal of Business 72, 1–33.

Dugar, A., Nathan, S., 1995. The effect of investment banking relationships on financial analysts' earnings forecasts and investment recommendations. Contemporary Accounting Research 12, 131–161.

Easterwood, J., Nutt, S., 1999. Inefficiency in analysts' earnings forecasts: systematic misreaction or systematic optimism? Journal of Finance 54 (5), 1777–1797.

Elgers, P., Murray, D., 1992. The relative and complementary performance of analyst and security-price-based measures of expected earnings. Journal of Accounting and Economics 15, 303–316.

Francis, J., Philbrick, D., 1993. Analysts' decisions as products of a multi-task environment. Journal of Accounting Research 31, 216–230.

Gu, Z., Wu, J., 2003. Earnings skewness and analyst forecast bias. Journal of Accounting and Economics 35, 5–30.

Han, J., Wang, S., 1998. Political costs and earnings management of oil companies during the 1990 Persian Gulf crisis. The Accounting Review 73, 103–118.

Hribar, P., Collins, D., 2002. Errors in estimating accruals: implications for empirical research. Journal of Accounting Research 40 (1), 105–134.

Jones, J., 1991. Earnings management during import relief investigations. Journal of Accounting Research 29, 193–228.

Keane, M., Runkle, D., 1998. Are financial analysts' forecasts of corporate profits rational? Journal of Political Economy 106, 768–805.

Kim, C., Lustgarten, S., 1998. Broker-analysts' trade-boosting incentive and their earnings forecast bias. Working paper, Queens College of the City University of New York.

Klein, A., 1990. A direct test of the cognitive bias theory of share price reversals. Journal of Accounting and Economics 13, 155–166.

Kothari, S.P., 2001. Capital markets research in accounting. Journal of Accounting and Economics 31, 105–232.

Lim, T., 2001. Rationality and analysts' forecast bias. Journal of Finance 56, 369–386.

Lin, H., McNichols, M., 1998. Underwriting relationships, analysts' earnings forecasts and investment recommendations. Journal of Accounting and Economics 25 (1), 101–127.

Lys, T., Sohn, S., 1990. The association between revisions of financial analysts' earnings forecasts and security-price changes. Journal of Accounting and Economics 13, 341–363.

Matsumoto, D., 2002. Management's incentives to avoid negative earnings surprises. The Accounting Review 77, 483–515.

McNichols, M., 2000. Research design issues in earnings management studies. Journal of Accounting and Public Policy 19, 313.

McNichols, M., O'Brien, P., 1997. Self-selection and analyst coverage. Journal of Accounting Research 35, 167–208.

Mendenhall, R., 1991. Evidence on the possible underweighting of earnings-related information. Journal of Accounting Research 29 (1), 170–179.

Payne, J., Thomas, W., 2002. The implications of using stock-split adjusted I/B/E/S data in empirical research. Working paper, University of Oklahoma.

Richardson, S., Teoh, S.H., Wysocki, P., 1999. Tracking analysts' forecasts over the annual earnings horizon: are analysts' forecasts optimistic or pessimistic? Working paper, University of Michigan.

Shane, P., Brous, P., 2001. Investor and (value line) analyst underreaction to information about future earnings: the corrective role of non-earnings-surprise information. Journal of Accounting Research 39, 387–415.

Taylor, P., 2002. Global investing—bullish analysts more likely to be promoted. Financial Times, February 1.

Watts, R.L., 2003. Conservatism in accounting, Part I: explanations and implications. Accounting Horizons, September 2003, pp. 207–221.

Weiss, I., 1999. Earnings management in response to a transitory earnings shock. Working paper, University of Chicago.

Analyst Forecasting Errors: Additional Evidence

Lawrence D. Brown

Analyst forecasting errors are approximately as large as Dreman and Berry (1995) documented, and an optimistic bias is evident for all years from 1985 through 1996. In contrast to their findings, I show that analyst forecasting errors and bias have decreased over time. Moreover, the optimistic bias in quarterly forecasts was absent for S&P 500 firms from 1993 through 1996. Analyst forecasting errors are smaller for (1) S&P 500 firms than for other firms; (2) firms with comparatively large amounts of market capitalization, absolute value of earnings forecast, and analyst following; and (3) firms in certain industries.

In recent issues of this journal, David Dreman, Michael Berry, and I have presented alternative views of analysts' earnings forecast errors and their implications for security analysis (Dreman and Berry 1995, Brown 1996, Dreman 1996). The first two papers provided alternative views concerning several issues, including whether (1) analysts' earnings forecast errors are "too large," (2) analysts' earnings forecast errors have increased over time, and (3) analysts' earnings forecasts are optimistically biased.

In the opinion of Dreman and Berry, analysts' earnings forecast errors are too large, and using the deflators the authors suggested (e.g., actual or predicted earnings), analyst forecasting errors do appear large. If analysts' earnings forecast errors are deflated by stock price, however, or compared with forecasts based on extrapolative techniques, they do not appear too large. Dreman–Berry also maintained that analysts' earnings forecasting errors have increased over time. My analysis of their findings, however, suggested that the accuracy of analysts' earnings forecasts has actually improved over time. In addition, Dreman–Berry provided evidence that analysts' earnings forecasts are biased toward optimism. Relying on information provided by I/B/E/S International, I showed that an optimistic bias was absent for S&P 500 firms for the 11 quarters from first-quarter 1993 through third-quarter 1995.

In his letter to the editor, Dreman (1996) responded to the views I expressed in my article, disagreeing with most of them. He correctly observed that much of my analysis was based on the Abel–Noser database, which Dreman–Berry had used but which was inaccessible to me; my analysis relied on summary information provided in the Dreman–Berry article. Moreover, although not stated by Dreman, neither did I examine the I/B/E/S data that I had relied on in my 1996 article. Instead, I relied on summary information provided to me by I/B/E/S.

This article is based on I/B/E/S data for fourth-quarter 1983 through second-quarter 1996. It presents evidence regarding the following issues:

- Is the Dreman–Berry result that analyst forecasting errors are "too large" robust to using a different data source than the Abel–Noser database?
- Is the Dreman–Berry conclusion that analysts' forecasting errors have increased over time robust to using I/B/E/S data? Does it pertain equally to S&P 500 firms and other firms?
- Is the optimistic bias documented by Dreman–Berry robust to using I/B/E/S data? Does this optimism pertain equally to S&P 500 and other firms? Has it been mitigated over time? Is the extent of mitigation similar for both S&P 500 firms and other firms?
- Do analyst forecasting errors and bias differ depending on such firm-specific factors as market capitalization, absolute value of predicted EPS, analyst following, and industry classification?

PRELIMINARY RESULTS

Dreman and Berry relied on the Abel–Noser database, which uses information from Value Line, Zacks Investment Research, I/B/E/S, and First Call. Because different vendors of analyst forecasts define both forecasted and actual earnings numbers differently, mixing data from different vendors introduces error (Philbrick and Ricks 1991), potentially making analysts' earnings forecast errors appear larger than they actually are. For this study, I used the data of a single vendor, I/B/E/S, for the

Lawrence D. Brown is Controllers RoundTable Research Professor at Georgia State University.

time period from fourth-quarter 1983 through second-quarter 1996. The sample consists of all U.S. firms for which analyst earnings forecast errors could be calculated.

Figure 1 provides frequency distributions using the SURPE and SURPF definitions of analyst forecasting errors (earnings surprise), defined as

SURPE = (Actual quarterly earnings – Predicted quarterly earnings)/|Actual quarterly earnings|

SURPF = (Actual quarterly earnings – Predicted quarterly earnings)/|Predicted quarterly earnings|.

Predicted quarterly earnings were obtained from the I/B/E/S summary tape using the last consensus (mean) estimate prior to the firm's quarterly earnings announcement.[1]

SURPE and SURPF are two of the four definitions of earnings surprise Dreman–Berry and I used in our research.[2] My Figure 1 corresponds to their Figure 1 pertaining to SURPE and SURPF, and my results are very similar to theirs. More specifically, the modal and median values of earnings surprise are zero; *small* positive errors are more frequent than negative errors; and *large* negative errors outnumber positive errors. These findings suggest that whereas analysts are more likely to be on target than anywhere else, managers manipulate earnings in a way to generate a considerable number of small positive (relative to small negative) surprises and large negative (relative to large positive) surprises ("big baths").[3]

I/B/E/S VERSUS ABEL–NOSER DATA

Table 1 provides summary statistics on the I/B/E/S and Abel–Noser data. The I/B/E/S results are based on my analysis of these data; the Abel–Noser results are reproduced from Dreman–Berry's Table 1. The average error (mean absolute surprise) using the I/B/E/S data is substantially larger than that using the Abel–Noser data. The I/B/E/S SURPE of 0.590 is approximately one-third greater than the Abel–Noser SURPE of 0.438, and the I/B/E/S SURPF of 0.916 is more than twice as large as the Abel–Noser SURPF of 0.415. Moreover, the mean surprise (bias) using the I/B/E/S data is also substantially larger in absolute value than that documented by Dreman–Berry using the Abel–Noser data. More particularly, the I/B/E/S SURPE and SURPF are –0.316 and –0.414, respectively, compared with the Abel–Noser SURPE and SURPF of –0.250 and –0.111.

My results could differ from Dreman–Berry's because of different sample-selection procedures. Dreman–Berry's sample is confined to firms with fiscal years ending in March, June, September, or December that are followed (after 1981) by at least four analysts. When the I/B/E/S sample is similarly restricted, the results are nearly identical to Dreman–Berry's.[4] More particularly, for the 46,859 I/B/E/S observations that satisfy these criteria, the average absolute surprise of 0.416 (SURPE definition) is similar to Dreman–Berry's 0.438, and the mean SURPE of –0.218 using the I/B/E/S sample closely approximates Dreman–Berry's –0.250.

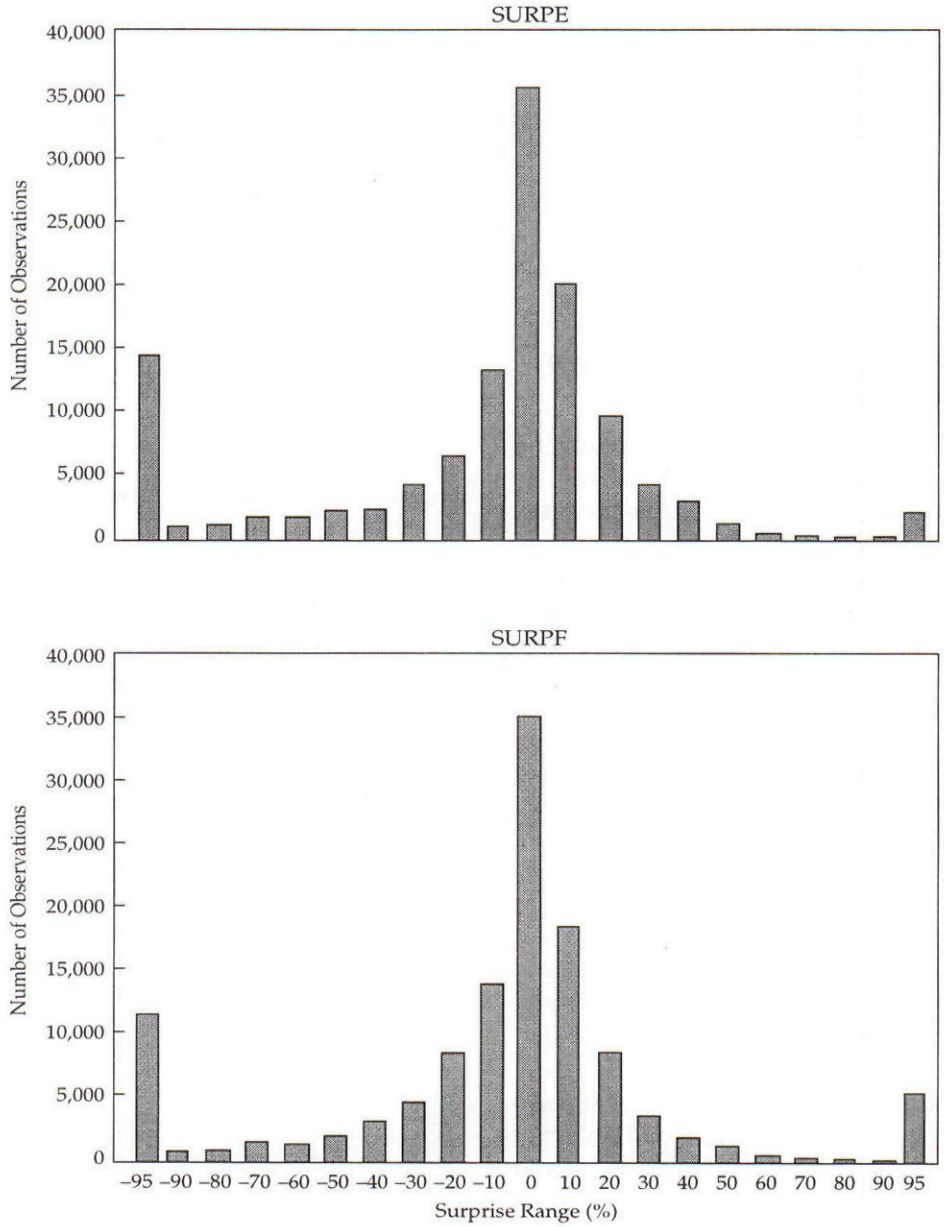
From these results, I conclude that the Dreman–Berry finding of large analyst forecasting errors is robust to using a different data source. Dreman–Berry used Abel–Noser data and examined the first-quarter 1974 through fourth-quarter 1991 time period; I obtained similar results using the I/B/E/S data for fourth-quarter 1983 through second-quarter 1996.

HAVE FORECASTING ERRORS CHANGED?

Evidence regarding five definitions of error—mean absolute surprise, mean surprise (bias), and the proportion of errors outside the +/–10 percent, +10 percent, and –10 percent bandwidths—is presented in Table 2 for all firms, S&P 500 firms, and non-S&P 500 firms.[5] All five error metrics use the SURPF definition of earnings surprise, which has predicted quarterly earnings as its deflator. Dreman–Berry provided evidence pertaining to three +/– bandwidths: 5 percent, 10 percent, and 15 percent. I focused on the second of these bandwidths, +/–10 percent, and considered its plus and minus sides separately.[6]

Dreman–Berry concluded that analyst forecasting errors increase over time. In contrast, Table 2 reveals that both mean absolute surprise and mean surprise (bias) have *decreased* significantly over time. This result is borne out by the rank correlations of analyst forecasting error with year, which are –0.973 and 0.489 for mean absolute surprise and mean surprise, respectively.[7] Nevertheless, the mean surprise is negative and significant in every year from 1985 through 1996, suggesting that, although the optimistic bias has been mitigated, it remains significant. The rank correlations of time with the proportion of errors outside the +/–10 percent, +10 percent, and –10 percent bandwidths are –0.995, –0.038, and –0.945, respectively. The –10 percent bandwidth result is significant, but the +10 percent bandwidth result is not. Thus, the temporal reduction of error results from mitigation of the optimistic bias. Indeed, no temporal reduction in the percentage of large positive errors (i.e., earnings *underestimates*) has occurred.

Figure 1. Histograms of SURPE and SURPF



Comparison of S&P 500 firms with other firms is important because many investors invest exclusively in S&P 500 firms and/or use the S&P 500 Index as a benchmark. Analyst forecasting errors are much smaller for S&P 500 firms than for other firms. More specifically, in *every* year, the mean absolute surprise and the proportion of forecasts outside the +/–10 percent, +10 percent, and –10 percent bandwidths is smaller for the S&P 500 firms than it is for the other firms. Clearly, the earnings of S&P 500 firms are easier to forecast than are those of non-S&P 500 firms.

Although forecasts for S&P 500 firms exhibit a significant optimistic bias for the 1984–96 period as a whole, the optimistic bias in forecasting quarterly earnings of S&P 500 firms disappeared as of 1993. More specifically, for S&P 500 firms, a significant optimistic bias is evident in every year in the 1985–92 period but not in the four most recent years, 1993 through 1996. In contrast, the bottom panel of Table 2 reveals that the optimistic bias in forecasting quarterly earnings of other (non-S&P 500) firms exists in all 12 years, 1985 through 1996. Perhaps the disappearance of the optimistic bias for S&P 500 firms is attributable to mitigation of the big-bath phenomenon or a lessening of the tendency of these firms' managers to manipulate earnings in a way to generate a large number of small positive (relative to small negative) surprises.[8]

Table 1. Descriptive Statistics for Earnings Forecast Errors

	I/B/E/S (4Q 1983–2Q 1996)		Abel–Noser (1Q 1974–4Q 1991)	
Statistic	SURPE	SURPF	SURPE	SURPF
Number of forecasts	129,436		66,100	
Mean absolute surprise	0.590	0.916	0.438	0.415
Mean surprise (bias)	–0.316*	–0.414*	–0.250*	–0.111*
Median	0.000	0.000	0.000	0.000
Maximum	314.000	863.000	49.000	48.000
Minimum	–186.259	–819.000	–216.000	–282.600

Note: SURPE (SURPF) is consensus EPS surprise as a percent of absolute value of actual (forecast) EPS.
*Significant at the 5 percent level, two-tailed test.

DO FORECASTING ERRORS DIFFER BY FIRM-SPECIFIC FACTORS?

Table 3 shows whether errors differ by market capitalization, absolute value of earnings forecast, or analyst following. Such comparisons are relevant because many investors invest primarily in large firms, firms with comparatively large earnings forecasts, or firms with relatively heavy analyst following. For these investors, the average analyst earnings forecast error per se is less relevant than the average forecasting error for these firm-specific subsamples.

The market capitalization results are monotonic for four of the five error measures: mean absolute surprise, mean surprise, and proportion of errors outside the +/–10 percent and –10 percent bandwidths. The highest capitalization group (i.e., firms with market caps in excess of $3 billion) has a smaller proportion of errors outside the +10 percent bandwidth than do any of the other market cap groups. Regarding bias, a significant optimistic bias (negative mean surprise) is evident for all market caps except the largest one.

The absolute value of earnings forecast results is not monotonic for any of the five definitions of error. Nevertheless, the mean absolute surprise and the mean surprise (bias) results are nearly monotonic; the exception occurs when forecasted earnings are at least $1. For this group, the mean absolute surprise and the mean surprise (bias) are approximately halfway between what they are for the [$0.10, $0.25) and [$0.25, $0.50) groups. The bandwidth results are similar to the mean absolute surprise and bias results in that the largest absolute value of earnings forecast group (i.e., ≥ $1) does not have the smallest proportion of errors outside the +/–10 percent, +10 percent, or –10 percent bandwidths.[9]

Similar to the absolute value of earnings forecast results, the analyst-following results are not monotonic for any of the five definitions of error. Nevertheless, the results are monotonic for all five error measures as the number of analysts increases from 1 to 5, and the smallest errors are obtained for the largest analyst following (10 or more) for four of the error measures.[10] Moreover, the rank correlations for the five error measures range from an absolute value of 0.782 to 0.988, and they all are statistically significant. Thus, error generally decreases when analyst following increases.

DO FORECASTING ERRORS DIFFER BY SECTOR?

The five error metrics are provided in Table 4 for each of the 14 industries in the I/B/E/S sample with data pertaining to at least 50 firms. The mean absolute surprise ranges from a low of 0.255 to a high of 1.663. Two industries have a mean absolute surprise below 0.400: food and kindred products (0.255) and holding companies and other investment offices (0.392). At the other extreme, two industries have mean absolute surprises in excess of 1.0: oil and gas extraction (1.663) and primary metal industries (1.267).

Eleven of the 14 industries evidence a significant optimistic bias. Optimistic bias for the other three—transportation equipment, communications, and insurance carriers—is not significant. The mean surprises range from a low of –0.068 to a high of –0.721. Three industries have an optimistic bias below 0.080 in absolute value: food and kindred products (–0.068), transportation equipment (–0.070), and communications (–0.076). At the other extreme, two industries have an optimistic bias above 0.500 in absolute value: oil and gas extraction (–0.721) and primary metal industries (–0.532).

The proportion of analyst forecasting errors outside the +/–10 percent bandwidth ranges from a low of 0.361 to a high of 0.780. Two industries have less than 40 percent of their observations outside the +/–10 percent bandwidth: food and kindred products (0.361) and depository institutions (0.369). At the other extreme, two industries have more than two-thirds of their observations outside the +/–10 percent bandwidth: oil and gas extraction (0.780) and primary metal industries (0.683). Twelve of the 14 industries have more errors outside the –10 percent than outside the +10 percent

Table 2. Forecast Errors by Year: All Firms, S&P 500 Firms, and Other Firms

Year/Statistic	Number of Firms	Number of Forecasts	Mean Absolute Surprise	Mean Surprise	+/–10 Percent[a]	+10 Percent[a]	–10 Percent[a]
All firms							
1984	2,109	2,246	2.525	0.795	0.697	0.311	0.386
1985	2,525	8,608	1.593	–0.667*	0.651	0.226	0.426
1986	2,580	8,506	1.773	–1.007*	0.656	0.245	0.412
1987	2,829	8,856	1.362	–0.700*	0.650	0.264	0.386
1988	2,804	9,041	1.067	–0.468*	0.620	0.269	0.351
1989	2,874	9,461	0.959	–0.537*	0.615	0.240	0.374
1990	2,890	9,627	1.034	–0.685*	0.600	0.215	0.384
1991	2,875	9,583	0.802	–0.444*	0.598	0.242	0.356
1992	3,195	10,702	0.688	–0.330*	0.557	0.261	0.296
1993	3,630	12,563	0.583	–0.230*	0.544	0.258	0.286
1994	4,193	14,213	0.494	–0.189*	0.514	0.258	0.256
1995	4,476	15,013	0.541	–0.244*	0.510	0.256	0.255
1996	4,593	11,008	0.527	–0.173*	0.501	0.260	0.241
Mean			0.916	–0.414*	0.577	0.252	0.326
Rank Correlation			–0.973*	0.489*	–0.995*	–0.038	–0.945*
S&P 500 firms							
1984	431	452	0.701	0.237	0.593	0.305	0.288
1985	443	1,743	0.748	–0.474*	0.503	0.186	0.317
1986	453	1,714	0.620	–0.250*	0.496	0.225	0.271
1987	463	1,791	0.487	–0.137*	0.487	0.245	0.243
1988	466	1,852	0.382	–0.143*	0.470	0.259	0.211
1989	473	1,842	0.427	–0.166*	0.447	0.203	0.245
1990	476	1,896	0.331	–0.113*	0.441	0.191	0.249
1991	481	1,892	0.442	–0.267*	0.467	0.189	0.277
1992	485	1,887	0.467	–0.148*	0.420	0.205	0.215
1993	486	1,983	0.345	0.027	0.409	0.220	0.189
1994	492	1,993	0.233	0.027	0.335	0.208	0.126
1995	492	1,936	0.190	–0.008	0.335	0.196	0.139
1996	494	1,314	0.310	0.002	0.318	0.177	0.141
Mean			0.418	–0.129*	0.431	0.211	0.220
Rank Correlation			–0.868*	0.357	–0.978*	–0.462	–0.819*
Other firms							
1984	1,678	1,794	2.985	0.935	0.724	0.312	0.411
1985	2,082	6,865	1.807	–0.716*	0.689	0.236	0.453
1986	2,127	6,792	2.064	–1.198*	0.697	0.250	0.447
1987	2,366	7,074	1.583	–0.843*	0.692	0.269	0.422
1988	2,338	7,189	1.244	–0.552*	0.659	0.272	0.387
1989	2,401	7,619	1.087	–0.626*	0.655	0.250	0.406
1990	2,414	7,731	1.206	–0.825*	0.639	0.221	0.417
1991	2,394	7,691	0.890	–0.488*	0.630	0.255	0.376
1992	2,710	8,815	0.735	–0.369*	0.586	0.274	0.313
1993	3,144	10,580	0.628	–0.278*	0.569	0.265	0.305
1994	3,701	12,220	0.537	–0.225*	0.543	0.266	0.277
1995	3,984	13,077	0.593	–0.279*	0.536	0.264	0.272
1996	4,099	9,694	0.557	–0.197*	0.526	0.272	0.254
Mean			1.019	–0.473*	0.608	0.260	0.348
Rank Correlation			–0.973*	0.489*	–0.984*	0.088	–0.912*

Note: Mean absolute surprise, mean surprise, and the percentage of surprises outside the three bandwidths use absolute value of earnings forecast as the deflator.

[a]Proportion of surprises outside bandwidth.

*Significant at the 5 percent level, two-tailed test.

Table 3. Forecast Errors Classified by Market Capitalization, Absolute Value of Earnings Forecast, and Analyst Following

	Number of Firms	Number of Forecasts	Mean Absolute Surprise	Mean Surprise	+/–10 Percent[d]	+10 Percent[d]	–10 Percent[d]
Market capitalization ($ millions)[a]							
<50	3,137	18,247	2.198	–1.445*	0.774	0.242	0.532
[50–100)	3,316	17,572	1.228	–0.616*	0.679	0.266	0.412
[100–500)	4,529	46,349	0.749	–0.271*	0.585	0.267	0.318
[500–3,000)	2,350	33,777	0.511	–0.096*	0.481	0.246	0.234
≥3,000	652	12,445	0.278	–0.019	0.370	0.203	0.167
Rank correlation			–1.000*	1.000*	–1.000*	–0.300	–1.000*
Absolute value of earnings forecast (cents)[b]							
<5	2,731	8,588	5.407	–2.564*	0.819	0.348	0.471
[5–10)	3,750	13,796	1.528	–0.681*	0.827	0.363	0.464
[10–25)	5,863	40,552	0.644	–0.300*	0.598	0.258	0.340
[25–50)	5,210	37,857	0.380	–0.159*	0.499	0.218	0.282
[50–100)	2,957	22,100	0.297	–0.105*	0.444	0.199	0.245
≥100	1,094	6,544	0.607	–0.250*	0.507	0.277	0.281
Rank correlation			–0.829*	0.829*	–0.771	–0.771	–0.943*
Analyst following (number of analysts)[c]							
1	6,189	35,979	1.421	–0.593*	0.707	0.293	0.414
2	5,011	22,983	1.035	–0.578*	0.629	0.272	0.358
3	3,913	15,728	0.790	–0.364*	0.581	0.251	0.330
4	3,077	11,411	0.674	–0.294*	0.544	0.246	0.298
5	2,384	8,532	0.581	–0.225*	0.519	0.241	0.278
6	1,898	6,775	0.762	–0.460*	0.482	0.217	0.266
7	1,555	5,354	0.553	–0.285*	0.465	0.207	0.258
8	1,296	4,356	0.795	–0.135	0.449	0.191	0.258
9	1,090	3,664	0.486	–0.233*	0.452	0.208	0.244
≥10	1,023	14,654	0.354	–0.126*	0.387	0.192	0.195
Rank correlation			–0.782*	0.842*	–0.988*	–0.939*	–0.988*

Note: Mean absolute surprise, mean surprise, and the percentage of surprises outside the three bandwidths use absolute value of earnings forecast as the deflator.

[a]Stock price multiplied by number of common stocks outstanding.

[b]Earnings forecast is the I/B/E/S mean forecast.

[c]Number of analysts whose forecast is included in the calculation of the I/B/E/S mean forecast.

[d]Proportion of surprises outside bandwidth.

*Significant at the 5 percent level, two-tailed test.

bandwidth, indicating that when large errors occur, analysts are more likely to overestimate earnings (optimistic bias) than to underestimate them (pessimistic bias). The two exceptions are depository institutions and insurance carriers. Perhaps these two industries are less likely than the other 12 to take big baths, which induce large negative errors and give the appearance of analyst optimism.

CONCLUSION

Using the Abel–Noser database for 1974 through 1991, Dreman and Berry argued that analyst forecasting errors are too large. Based on the I/B/E/S database for 1983 through 1996, I show that analysts' earnings forecast errors are approximately as large as Dreman–Berry documented. Thus, their results appear to have external validity.

Dreman–Berry maintained that analyst forecasting errors have increased over time. In a 1996 article, I argued that the Abel–Noser data, as summarized by Dreman–Berry, suggest precisely the opposite. In his critique of my analysis, David Dreman correctly pointed out that I did not access the data Dreman–Berry used to reach their conclusions. In this study, I used I/B/E/S data to examine five error metrics to determine whether analyst forecasting accuracy has deteriorated over time. I found that analyst forecasting errors have *decreased* significantly over time, especially for mean absolute surprise and the proportion of errors outside the +/–10 percent and –10 percent bandwidths.[11] My finding that analysts' earnings forecast errors have decreased over time is robust to firms included in as opposed to those excluded from the S&P 500.

I examined whether analyst forecasting errors differ according to certain firm-specific factors:

Table 4. Forecast Errors by Industry

SIC Code	Industry Name	Number of Firms	Number of Forecasts	Mean Absolute Surprise	Mean Surprise	+/−10 Percent[a]	+10 Percent[a]	−10 Percent[a]
13	Oil and gas extraction	73	1,681	1.663	−0.721*	0.780	0.338	0.442
20	Food and kindred products	55	1,644	0.255	−0.068*	0.361	0.166	0.195
28	Chemicals and allied products	128	3,910	0.454	−0.159*	0.422	0.189	0.233
33	Primary metal industries	63	1,619	1.267	−0.532*	0.683	0.298	0.385
35	Industrial, commercial machinery and computer equipment	128	3,958	0.794	−0.243*	0.596	0.274	0.322
36	Electronics and other equipment companies	104	2,824	0.856	−0.370*	0.556	0.237	0.319
37	Transportation equipment	66	2,096	0.820	−0.070	0.553	0.249	0.305
38	Measurement instruments; photo goods; watches	76	1,991	0.445	−0.186*	0.425	0.186	0.239
48	Communications	56	1,292	0.455	−0.076	0.429	0.202	0.227
49	Electric, gas, and sanitary services	190	6,766	0.436	−0.130*	0.560	0.261	0.299
60	Depository institutions	421	7,298	0.543	−0.336*	0.369	0.197	0.171
63	Insurance carriers	189	4,453	0.512	−0.142	0.517	0.285	0.232
67	Holding; other investment offices	82	777	0.392	−0.151*	0.539	0.175	0.364
73	Business services	78	2,111	0.540	−0.263*	0.448	0.182	0.266

Notes: Mean absolute surprise, mean surprise, and the percentage of surprises outside the three bandwidths use absolute value of earnings forecast as the deflator. To be included in Table 4, an industry must have more than 50 firms in the sample.

[a]Proportion of forecast errors (using absolute value of earnings forecast as a deflator) outside bandwidth.

*Significant at the 5 percent level, two-tailed test.

inclusion in the S&P 500, market capitalization, absolute value of earnings forecast, analyst following, and industry membership. I showed that: (1) analyst forecasting errors for S&P 500 firms are smaller than for other firms; (2) analyst forecasting errors are relatively small for firms with comparatively large market cap, absolute value of earnings forecast, and analyst following; and (3) analyst forecasting errors for firms in certain industries are substantially larger than those in other industries. Thus, depending on the nature of the firms followed by investors, analysts' earnings forecast errors may be considerably larger or smaller than average.

Dreman and Berry showed that analysts' earnings forecasts exhibit an optimistic bias. I had argued in my 1996 paper that the optimistic bias was not evident for S&P 500 firms for the period from first-quarter 1993 through third-quarter 1995. Moreover, according to I/B/E/S, the optimistic bias has not been evident for S&P 500 firms for the subsequent period, fourth-quarter 1995 through second-quarter 1997.[12]

Based on the I/B/E/S data, which include both S&P 500 and other firms, I documented an optimistic bias in analysts' quarterly earnings forecasts for all years, 1985 through 1996, and in 11 of 14 industries. I also showed that the optimistic bias in quarterly forecasts has diminished significantly over time for both S&P 500 and other firms and that it was absent for S&P 500 firms for each year from 1993 through 1996. The optimistic bias in quarterly forecasts for non-S&P 500 firms remains.[13]

NOTES

1. Because earnings forecast errors cannot be calculated when the actual or quarterly earnings forecast equals zero, these observations were omitted from the analysis. To be consistent with Dreman–Berry, I did not adjust outliers in any manner.
2. The other two definitions of earnings surprise are SURP8 and SURPC7, which respectively use the standard deviation of trailing eight-quarter actual earnings per share and the standard deviation of trailing seven-quarter changes in earnings per share.
3. Other studies have documented that managers manipulate earnings in order to report positive earnings, positive earnings growth, and/or earnings that exceed analyst expectations. When managers cannot succeed in these goals, they are likely to take a "big bath." See Lowenstein (1997).
4. For simplicity, I do not provide these results in a table.
5. These results and those that follow are based on the full I/B/E/S sample of 129,436 observations described in Table 1.
6. This suggestion was made when I presented an earlier version of this article at the 1997 Prudential Securities Quantitative Research Seminar for Institutional Investors.
7. The positive rank correlation for mean surprise indicates that the bias has become less negative (i.e., there has been a temporal reduction in the optimistic bias).
8. Such an analysis is beyond the scope of this study but is on the author's research agenda.
9. When I presented results at the 1997 Prudential Securities

Quantitative Research Seminar for Institutional Investors, I used the actual EPS as a deflator. It was suggested to me that the aberrant results for the largest EPS group may be attributable to large random shocks in the actuals. When I substituted forecasted EPS for actual EPS (as in this article), the tenor of my results was unchanged.

10. The exception is the proportion of errors outside the +10 percent bandwidth, for which the proportion of 19.2 percent for the analyst following of ≥10 slightly exceeds the proportion of 19.1 percent for the analyst following of 8.
11. The exception is that the percentage of errors outside the +10 percent bandwidth has not decreased significantly for either the entire I/B/E/S sample or the non-S&P 500 subsample.
12. According to information provided to me by I/B/E/S, the mean surprises for S&P 500 firms for these seven quarters (sample sizes are in parentheses) are 1.7 percent (488), 2.4 percent (492), 2.6 percent (490), 2.4 percent (490), 1.9 percent (481), 3.3 percent (492), and 2.2 percent (491). The optimistic bias is still present for S&P 500 firms for annual forecasts.
13. I am grateful to Deres Tegenaw for providing me with excellent research assistance.

REFERENCES

Brown, L.D. 1996. "Forecasting Errors and Their Implications for Security Analysis: An Alternative Perspective." *Financial Analysts Journal*, vol. 52, no. 1 (January/February):40–47.

Dreman, D.N. 1996. "Analyst Forecasting Errors." *Financial Analysts Journal*, vol. 52, no. 3 (May/June):77–80.

Dreman, D.N., and M.A. Berry. 1995. "Forecasting Errors and Their Implications for Security Analysis." *Financial Analysts Journal*, vol. 51, no. 3 (May/June):30–41.

Lowenstein, R. 1997. "How To Be a Winner in the Profits Game." *Wall Street Journal* (April 3):C1.

Philbrick, D.R., and W.E. Ricks. 1991. "Using Value Line and I/B/E/S Analyst Forecasts in Accounting Research." *Journal of Accounting Research*, vol. 29, no. 2 (Autumn):397–417.

Available online at www.sciencedirect.com
SCIENCE @ DIRECT®
International Review of Financial Analysis
14 (2005) 1–22

ELSEVIER

IRFA
INTERNATIONAL REVIEW OF
Financial Analysis

Trends in analyst earnings forecast properties

Stephen J. Ciccone*

Whittemore School of Business and Economics, University of New Hampshire, McConnell Hall, 15 College Road, Durham, NH 03824, USA

Abstract

Forecast dispersion, error, and optimism are computed using 120,022 quarterly observations from 1990 to 2001. Forecast dispersion, error, and optimism all decrease steadily over the sample period, with loss firms showing an especially striking decrease. By the end of the sample period, dispersion and error differences between profit and loss firms are relatively minor, optimism for loss firms is around an unbiased 50%, and pessimism dominates profit firms. Additionally, loss firm earnings appear more difficult to forecast. The reduction in dispersion, error, and optimism does not appear fully attributable to earnings management, earnings guidance, or earnings smoothing. The trends are consistent with increased litigation concerns.

JEL classification: D84; G14; G24
Keywords: Earnings forecasts; Analysts; Earnings management; Forecast dispersion; Forecast accuracy

1. Introduction

A major responsibility of analysts is to make earnings forecasts. Professionals, such as investment bankers, financial advisors, and stockbrokers, rely on these forecasts to make their decisions, as do many individual investors. The forecasts serve as critical inputs into stock valuation models. Earnings announcement period returns are influenced by the forecasts (e.g., Imhoff & Lobo, 1992), and forecast dispersion is even related to monthly or annual stock returns (Ang & Ciccone, 2001; Diether, Malloy, & Scherbina, 2002; Dische, 2002). Forecasts are now publicly available on many investment-related web sites, providing free access to millions of investors all over the world.

* Tel.: +1-603-862-3343; fax: +1-603-862-3383.
E-mail address: stephen.ciccone@unh.edu (S.J. Ciccone).

1057-5219/$ - see front matter
doi:10.1016/j.irfa.2004.06.001

For a long period of time, the ability of analysts to forecast earnings was questioned. Analysts were biased some argued, optimistic and unresponsive to earnings changes (Abarbanell & Bernard, 1992; DeBondt & Thaler, 1990). They tended to herd, making forecasts or recommendations similar to other analysts (Hong, Kubik, & Solomon, 2000; Olsen, 1996; Stickel, 1990; Trueman, 1994; Welch, 2000). They were better than time-series earnings estimates, but only slightly (Fried & Givoly, 1982; O'Brien, 1988).

Recent studies have found that analyst forecasts have changed, perhaps even improved. Analysts have reduced both the size of their forecast errors and their optimism (Brown, 1997; Matsumoto, 2002; Richardson, Teoh, & Wysocki, 2001). Unfortunately for the analysts, many attribute this trend, not to better forecast accuracy, but to increases in earnings guidance, management, or smoothing (e.g., Degeorge, Patel, & Zeckhauser, 1999; Matsumoto, 2002).

The purpose of this study is twofold, both to document trends in forecast properties and to differentiate among theories as to why the trends exist. Several trends are investigated; some revisited, some new: (1) the trends of dispersion, error, and optimism; (2) the trend of wrongly forecasted profits or losses; (3) the trend of naïve forecast performance versus analyst forecast performance; (4) the trend of earnings volatility; and (5) the trend of Street versus GAAP earning differences. In addition, the influence of Regulation FD on the trends is examined. Quarterly data is used during a 1990 to 2001 sample period. As previous research has shown that analysts have greater difficulty forecasting the earnings of firms with losses (Brown, 2001; Butler & Saraoglu, 1999; Ciccone, 2001; Dowen, 1996; Dreman & Berry, 1995), firms with profits and losses are separated and examined independently in much of the testing.[1]

There are several possible explanations for changes in forecast properties: legal liability (e.g., Skinner, 1994), earnings guidance (e.g., Matsumoto, 2002), earnings management (e.g., Degeorge et al., 1999), earnings smoothing (consistent with Bartov, 1993), or information flow improvements (consistent with Asthana, 2003). The testing investigates the validity of these reasons.

The results are quite remarkable. Forecast properties have undergone an extraordinary change, perhaps best called a transformation, during the sample period. Forecast dispersion and error both decrease throughout the sample period, with most of the decrease due to loss firm forecasts. Although analysts still do not forecast loss firms with the same degree of accuracy as profit firms, the differences in forecasting performance are steadily eroding.

Optimism also decreases as analysts moved from being optimistically biased to being pessimistically biased during the sample period. The pessimism associated with profit firms is astonishing. Near the end of the sample period, almost three quarters of the

[1] Several related studies exist. Brown (1997), Richardson et al. (2001), and Matsumoto (2002) all show a decreasing trend in signed earnings surprise or optimism, although they do not separate firms by profitability. Gu and Wu (2003) evaluate forecast differences between profit and loss firms but do not examine trends in performance. Dreman and Berry (1995) and Butler and Saraoglu (1999) do separate firms by profitability while examining trends, but both rely on sample periods ending in 1991. Brown (2001) uses the signed, earnings surprise of the last forecast made prior to the earnings release date to examine shifts in the trend of the median surprise for profit and loss subsamples.

quarterly forecasts for profit firms are pessimistic. Analysts still tend to be optimistic toward loss firms, but this optimism has decreased dramatically over the sample period, hovering around an unbiased 50% at the end of the period. The decrease in the optimistic biases is so pronounced that the still-lingering legend of analyst earnings optimism (e.g., Easterwood & Nutt, 1999; Gu & Wu, 2003) is clearly no longer true, even for loss firms. If anything, analysts have a new concern: earnings pessimism for profit firms.

Additional results show that analysts have gotten much better at predicting the sign of earnings when firms report losses. Moreover, forecasting loss firm earnings appears to be much more difficult than forecasting profit firm earnings. Given this difficulty, analysts actually seem to provide greater value to the market when forecasting for loss firms.

Finally, the results suggest that the trends in forecast properties are unlikely to be fully attributable to earnings guidance, management, or smoothing. Firms unlikely to manage earnings—those with negative surprises, earnings declines, and losses—experience similar reductions in dispersion and error as the sample of all firms. So do firms considered unlikely to be guiding firms toward a specific earnings target, those with high dispersion. Furthermore, Street versus GAAP earnings differences and earnings volatility do not affect the results. The trends in forecast properties are consistent with litigation concerns, especially those surrounding loss reporting. In addition, although not specifically tested, analysts, aided by new information technology, may have simply improved in their forecasting abilities.

2. Forecast property changes

One of the most prominent explanations for the changing trends in forecast properties centers on earnings management. In the financial press, managers are often thought to play an "earnings game," manipulating reported earnings (and hence the surprise) to reap various benefits: increased stock prices, favorable publicity, and bonuses (Vickers, 1999). Fox (1997) tells of a Microsoft 1997 quarterly earnings release in January, the 41st time in 42 consecutive quarters that Microsoft met or beat the Wall Street consensus. The earnings game is often considered dangerous: when played long-term prospects are sacrificed by concern with short-term profits. Corporate decisions are altered, accounting rules are stretched, and investors lose faith in both financial statements and stock prices (Collingwood, 2001).

Academics have intensively investigated the issue of earnings management. Burgstahler and Dichev (1997) and Degeorge et al. (1999) find that firms manage earnings to meet analyst expectations, avoid losses, and avoid earnings declines. These studies mention several reasons why executives manage earnings, including increased job security, increased bonuses, and bolstered investor interest. Furthermore, anecdotal evidence suggests that firms like the favorable publicity of positive surprises, profits, and earnings increases. Of the three objectives identified by Degeorge, Patel, and Zeckhauser, the positive profit objective proves predominant. However, missing a consensus earnings estimate can be very costly to a firm. For example, Skinner and Sloan (2002) find that, all else equal, the price decline after a negative surprise is greater than the price increase following a positive surprise.

Another way of managing earnings entails "smoothing" or making earnings less volatile through time (e.g., Bartov, 1993). There are several theories that attempt to explain this behavior. Healy (1985) and Holthausen, Larcker, and Sloan (1995) find smoothed earnings are related to management bonus arrangements. Degeorge et al. (1999) use these findings to argue that managers may reduce high earnings levels to make future earnings objectives easier to meet. Fudenberg and Tirole (1995) argue that managers will boost earnings in bad times to increase the probability of retaining their jobs. Trueman and Titman (1988) believe that firms smooth earnings to lower their perceived bankruptcy risk and thus lower their cost of debt.

A cheaper way of playing the earnings game involves forecast guidance. Firms guide analysts toward a pessimistic target and then beat that target (Matsumoto, 2002), an easy way to garner favorable publicity.

An additional perspective on earnings guidance is rooted in legal liability issues. Firms face scrutiny when reporting large, unexpected losses. The consequent stock price decrease angers investors, who then might sue the firm for damages, consistent with Skinner (1994, 1997). Kasznik and Lev (1995) provide support for this argument by showing that firms increased their tendency to warn investors of impending losses. By warning of losses, firms are not necessarily playing an earnings game. As such, guiding analysts toward pessimistic targets and warning analysts of losses, although related, are considered two distinct concepts in this study.

Simpler explanations also exist to explain forecasting trends. For example, an alternative viewpoint looks at data availability and the information revolution, consistent with Asthana (2003). Forecasting techniques might be improving, aided in part by more precise and timelier economic information. Communications channels between firm managers and analysts may be better. Perhaps even the recent proliferation of freely available financial information on the Internet makes analysts more careful as they strive to add value and provide information above and beyond what is known by individual investors.

3. Data and methodology

The First Call summary database is used to obtain the forecast properties. Quarterly forecasts are used to present all results. The results using annual forecasts are similar to the quarterly results and do not require separate analysis. The last mean forecast available prior to the fiscal period end is used as the consensus forecast. All conclusions are similar if median forecasts are used instead of the mean forecasts or if the last mean forecasts prior to the earnings release are used instead of the last mean forecasts prior to fiscal period end.

Forecast dispersion is defined as the standard deviation of the forecasts divided by the absolute value of the mean forecast. This measure requires at least two forecasts.[2] Forecast error is defined as the difference between the actual earnings and the mean forecasted

[2] Although the procedure sharply reduces the sample size, the results for dispersion are similar if only companies with five or more analysts are included.

earnings, divided by the actual earnings. The absolute value is taken to obtain the final error number. A "raw error" is also computed as the absolute value of the difference between actual and forecasted earnings (i.e., the error is not deflated).[3] A forecast is considered optimistic if the mean forecast is greater than the corresponding actual earnings. The error and optimism measures require at least one forecast.

Many studies deflate the forecast properties by the stock price rather than the deflators described above. Thus, as a check, trends in dispersion and error are reexamined using price at the beginning of the fiscal year as the deflator. These results are qualitatively similar to the presented results, although the trends are not quite as obvious.[4]

Forecast dispersion is sometimes thought to signify herding. With this interpretation, low dispersion would be undesirable as it suggests greater herding. However, in this study, low dispersion is considered a desirable property. At least two reasons suggest this is true: (1) firms with losses or earnings declines, potential candidates to hide bad information, tend to have highly dispersed forecasts in previous studies (Ciccone, 2001), and (2) the high positive correlation between dispersion and error.[5]

An important component of this research is the separation of firms with losses and profits. A loss is defined as when the actual earnings per First Call are less than zero. A profit is defined as when actual earnings are greater than or equal to zero. First Call earnings, frequently referred to as "Street" or "operating" earnings (among other names), are often different from earnings under generally accepted accounting principles or GAAP (Abarbanell & Lehavy, 2000; Bradshaw & Sloan, 2002). The results are similar if GAAP earnings are used to determine profitability. The Compustat database is used to obtain GAAP earnings.

To alleviate problems with small denominators, a firm with a divisor less than US$0.02 in absolute value terms has the problem divisor set to US$0.02. Two procedures are used to reduce the influence of large observations. Firms with dispersion or error numbers greater than 10 and firms with earnings per share greater than an absolute value of US$20 are eliminated from their respective sample. Combined, the two procedures eliminate a total of 220 quarterly observations with no effect on the conclusions.

The final sample includes the years 1990 through 2001, a 12-year or 48-quarter period.[6] The total sample includes 120,022 firm quarters: 94,194 with profits and 25,828 (21.5%) with losses. The number of observations varies by the forecast property being examined.

---

[3] The raw error, often called the "earnings surprise" (although usually with the sign or direction of the error), is important because this number is often reported by the news media. It is important to note that "error" and "raw error" have two distinct meanings in this study.

[4] Using price as a deflator, average profit firm dispersion decreases from 0.0027 in the early (1990–1995) sample period to 0.0015 in the later sample period (1996–2001). Loss firm dispersion decreases from 0.0128 to 0.0069. Profit firm error decreases from 0.0052 to 0.0041, while loss firm error decreases from 0.0409 to 0.0333. All differences are significant with 99% confidence.

[5] To illustrate the latter point, the correlation between the dispersion and error is computed as 0.22 (0.24 if a log transform is performed). In a related test, every quarter each firm is placed into 1 of 10 portfolios based on its ranking of dispersion and 1 of 10 portfolios based on its ranking of error. The correlation between the group placement (1–10) is then computed. The correlation between the dispersion and error groupings is .47.

[6] The year 1990 contains considerably less sample firms than the other 11 years. Caution is thus recommended when evaluating the 1990 data.

The dispersion measure has the fewest number of observations: 84,919 quarterly observations.

Portfolio analyses are used to communicate the results in an easily accessible manner. The included tables present the results year-by-year and also during two sample periods: an "early" sample period from 1990 through 1995 and a "later" sample period from 1996 through 2001. Each period contains half the sample years. In addition, regression models controlling for size and book-to-market ratio are used to support the major conclusions reached.

4. Forecasting trends

Table 1 presents, by year, the forecast properties and maximum number of observations (recall there are sample size differences among the various properties). Dispersion, error, raw error, and optimism all steadily decrease throughout the sample period. The trend for optimism is interesting as the forecasts changed from being optimistic more than 50% of the time in the first couple of sample years to being optimistic less than 50% of the time after 1992. The amount of optimism continues to decrease during the sample period, reaching a low of 34.27% in 2000.

Table 1
Forecast dispersion, error, and optimism

	Quarterly forecasts				
	Maximum number of observations	Dispersion	Error	Raw error	Percent optimistic
All years	120,022	0.22	0.44	0.09	40.27
1990–1995	40,949	0.27	0.48	0.11	45.90
1996–2001	79,073	0.20	0.42	0.09	37.36
Difference		0.07*	0.06*	0.02*	8.54*
1990	1373	0.31	0.58	0.16	57.70
1991	2929	0.38	0.59	0.15	53.77
1992	6497	0.30	0.46	0.11	46.36
1993	8411	0.26	0.46	0.12	46.64
1994	10,249	0.25	0.46	0.10	43.33
1995	11,490	0.24	0.47	0.09	43.88
1996	14,002	0.23	0.44	0.09	39.27
1997	14,942	0.19	0.41	0.08	38.86
1998	15,184	0.20	0.41	0.08	38.71
1999	13,638	0.20	0.43	0.09	34.95
2000	12,314	0.17	0.42	0.10	34.27
2001	8993	0.21	0.42	0.09	37.46

This table reports mean analyst quarterly forecast properties over the sample period 1990 through 2001. Dispersion is defined as the standard deviation of the quarterly forecasts divided by the absolute mean forecast. Raw error is defined as the absolute value of the actual earnings less the forecasted earnings. Error is defined as the absolute value of the actual earnings less the forecasted earnings, divided by the absolute actual earnings. A firm's forecast is considered optimistic if the mean forecast is greater than the corresponding actual earnings. As the sample size varies by the forecast property in question, the maximum number of observations is reported.

* Difference is significantly different from zero with 99% confidence.

Table 2 shows the same forecast properties after separating firms by profitability. The dispersion and error of loss firms is considerably greater than the dispersion and error of profit firms. This occurs in every sample year and, although not tabulated, in every sample quarter. However, loss firms show greater reductions in dispersion and error throughout the sample period. The average dispersion of loss firms decreases from a high of 1.12 in 1990 to 0.30 in 2000 and 0.33 in 2001. Thus, the typical forecast dispersion of a loss firm today is roughly a quarter of what it was just 10 years ago. The story is similar for forecast error. The mean forecast error of loss firms decreases from a high of 1.16 in 1990 to 0.63 in 2000 and 0.55 in 2001. The error reduction for profit firms is not nearly as large, decreasing from a high of 0.48 in 1991 to 0.33 in 2000 and 0.35 in 2001.

The first two charts in Fig. 1 show the forecast dispersion and error by year and profitability. The figure provides a nice illustration of the eroding dichotomous forecasting ability of analysts. Clearly, analysts are narrowing the gap in their performance between profit and loss firms.

Table 2 also presents statistics for the mean raw error. Similar to the previous results, improvement in the raw error numbers occurs regardless of profitability, but the improvement is especially large for loss firms. For example, the raw error of loss firms decreases by more than half, from an average of US$0.48 in 1991 to US$0.21 in 2000 and US$0.16 in 2001.

The last columns of Table 2 show the percentage of optimistic forecasts. In the early sample period, analysts are overwhelmingly optimistic toward loss firms, more than 75% of time. The optimism remains above 70% until 1997 when it drops to 67.66%. From

Table 2
Forecast dispersion, error, raw error, and optimism by profitability

	Dispersion		Error		Raw error		Percent optimistic (negative surprise)	
	Profit	Loss	Profit	Loss	Profit	Loss	Profit	Loss
All quarters	0.15	0.53	0.35	0.78	0.06	0.23	33.63	64.48
1990–1995	0.18	0.88	0.37	1.02	0.07	0.33	40.32	75.93
1996–2001	0.13	0.43	0.33	0.70	0.05	0.20	29.76	60.70
Difference	0.05*	0.45*	0.04*	0.32*	0.02*	0.13*	10.56*	15.23*
1990	0.19	1.12	0.47	1.16	0.10	0.49	52.97	85.42
1991	0.24	1.11	0.48	1.09	0.08	0.48	48.40	78.44
1992	0.21	0.94	0.37	0.95	0.07	0.34	40.91	76.43
1993	0.17	0.91	0.37	0.96	0.08	0.34	41.67	74.80
1994	0.17	0.80	0.36	0.99	0.06	0.30	37.82	73.54
1995	0.16	0.81	0.35	1.11	0.06	0.28	37.54	76.75
1996	0.15	0.70	0.34	0.86	0.05	0.26	32.06	70.90
1997	0.12	0.50	0.32	0.78	0.05	0.22	31.58	67.66
1998	0.13	0.47	0.32	0.71	0.04	0.19	30.68	65.21
1999	0.14	0.39	0.33	0.70	0.05	0.20	26.84	58.42
2000	0.13	0.30	0.33	0.63	0.05	0.21	26.63	51.97
2001	0.15	0.33	0.35	0.55	0.05	0.16	29.44	53.12

This table reports mean analyst quarterly forecast properties sorted by profitability over the sample period 1990 through 2001. A profit occurs when actual quarterly earnings are greater than or equal to zero. A loss occurs when actual quarterly earnings are less than zero. See Table 1 for variable definitions.

* Difference is significantly different from zero with 99% confidence.

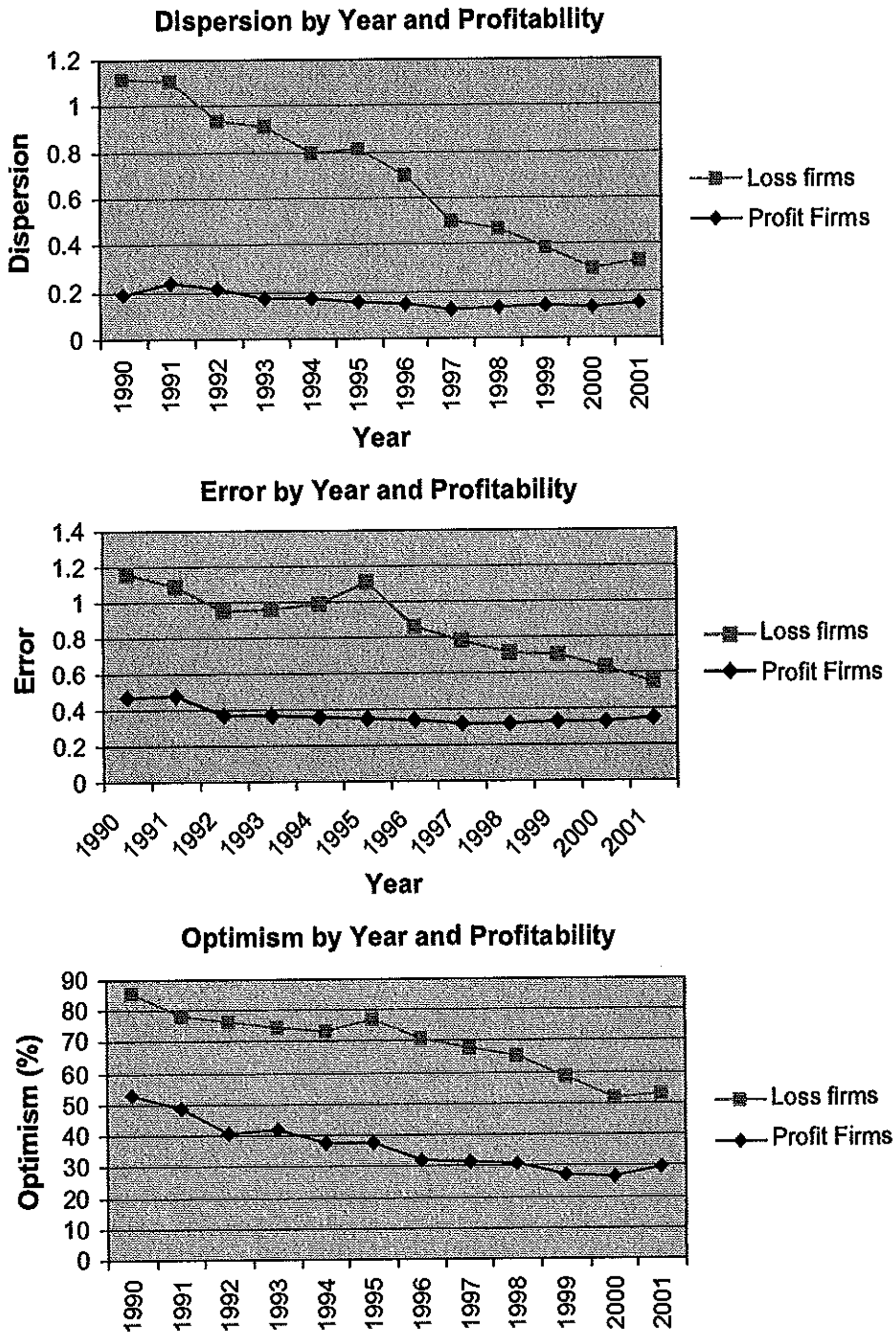


Fig. 1. Forecast properties by year and profitability.

there, the optimism continues to decrease, dropping to an almost unbiased 51.97% in 2000 and 53.12% in the 2001. For profit firms, optimism on average vanishes in 1991 and continues to decrease steadily throughout the sample period. By the end of the sample period, optimism is under 30%. The last chart in Fig. 1 illustrates this trend of decreasing optimism for both profit and loss firms.

Although the testing focuses on realized actual earnings to determine profitability, the results from Table 2 are repeated using expected earnings to determine profitability. Firms are resorted into profit and loss portfolios based on the mean forecast at fiscal year end. These results (not tabulated) are qualitatively similar to the Table 2 results, although average dispersion, error, and optimism are higher for expected profit firms (versus actual profit firms) and lower for expected loss firms. Optimism actually drops below 50% for expected loss firms during the last three sample years: 1999, 2000, and 2001. Related testing is performed on Table 6.

Regression models are utilized next to control for variables aside from profitability that influence forecasts. Previous studies have shown that size and growth prospects (growth indicated by book-to-market ratio) affect the information environment (e.g., Atiase, 1985; Ciccone, 2001).[7]

To test, two sets of regression models are used. The first set of regressions is employed to confirm the trend of lower dispersion and error during the sample period. These models use dispersion and error as the dependent variables and size, book-to-market ratio, a loss dummy variable, and year dummy variables as the independent variables. The Compustat database is used to gather the size and book-to-market ratio data. Size is defined as price times shares, computed at the beginning of the fiscal year. Book-to-market ratio is defined as beginning of fiscal year equity (Compustat item A216) divided by size. Logarithms of size and book-to-market ratio are used in the regressions. The loss dummy variable equals one if the actual First Call earnings are negative and zero otherwise. The year dummy variables equal one if the forecast is from the corresponding year and zero otherwise. The first year dummy variable corresponds to 1991, leaving 1990 as the base year. This specification is as follows for firm i during year t, quarter q.

$$\begin{aligned}\text{Forecast property}_{i,t,q} = a &+ b_1 \log(\text{size})_{i,t} + b_2 \log(\text{b/m})_{i,t} \\ &+ b_3 \text{ loss dummy}_{i,t,q} + b_4 \text{ year 1991 dummy}_{i,t} + \ldots \\ &+ b_{14} \text{ year 2001 dummy}_{i,t} + e_{i,t,q} \qquad (1)\end{aligned}$$

Table 3 presents the results of these regressions. Although size, book-to-market ratio, and especially losses affect the forecasts, the significant, negative values on the year dummy variables tend to increase in magnitude over the sample period. For example, using error as the dependent variable, the coefficient of the 1992 year dummy is -0.11 (indicating an average decrease of -0.11 relative to the 1990 base year), while that of the 2001 year dummy is -0.23 (indicating an average decrease of -0.23 relative to the 1990 base year). These results confirm the trends revealed in the portfolio results.

In the second set of regressions, models are employed annually from 1990 through 2001 to confirm the erosion of differences between profit and loss firm forecasts.

[7] The size of the analyst following is also included in separate regressions with no effect on the conclusions. Analyst following is not included in the presented results because of its strong correlation to size, thus blurring the relation between size and the forecast properties.

Table 3
Regression results using year dummy variables

	Dispersion		Error	
	Coefficient	*t* Value	Coefficient	*t* Value
Intercept	0.24	9.21	1.09	30.61
log (size)	0.01	2.17	−0.04	−22.61
log (book/market)	0.06	21.55	0.06	15.95
Loss dummy	0.42	82.48	0.43	61.21
1991	0.07	2.78	−0.02	−0.60
1992	0.00	0.21	−0.11	−3.71
1993	−0.03	−1.21	−0.13	−4.42
1994	−0.04	−1.99	−0.13	−4.47
1995	−0.05	−2.33	−0.12	−4.33
1996	−0.05	−2.45	−0.15	−5.34
1997	−0.11	−5.40	−0.19	−6.86
1998	−0.11	−5.44	−0.19	−6.82
1999	−0.13	−6.23	−0.19	−6.67
2000	−0.15	−7.61	−0.20	−7.31
2001	−0.17	−8.27	−0.23	−8.29
N	75,337		105,287	

This table reports the results of a regression model. Either forecast dispersion or error is the dependent variable. The independent variables are the logarithm of size (price times shares) in thousands, the logarithm of book-to-market value (equity/size), a loss dummy equal to one if the actual quarterly First Call earnings are below zero and equal to zero otherwise, and year dummy variables spanning 1991 through 2001 equal to one if the quarterly forecast is from the corresponding year. The regression model is below:

$$\text{Forecast property}_{i,t} = a + b_1\log(\text{size})_{i,t} + b_2\log(\text{b/m})_{i,t} + b_3 \text{ loss dummy}_{i,t} + b_4 \text{ year 1991 dummy}_{i,t} + \ldots + b_{14} \text{ year 2001 dummy}_{i,t} + e_{i,t}$$

Dispersion and error are the dependent variables, while size, book-to-market ratio, and a loss dummy variable are the independent variables. The annual model appears below:

$$\text{Forecast property}_{i,q} = a + b_1 \log(\text{size})_i + b_2\log(\text{b/m})_i + b_3 \text{ loss dummy}_{i,q} + e_{i,q} \quad (2)$$

The results of these regressions appear on Table 4. Once again, the portfolio results are confirmed. For example, using dispersion as the dependent variable, the coefficient on the loss dummy variable decreases sharply over the sample period, dropping from 0.83 and 0.86 in 1990 and 1991, respectively, to 0.20 in 2001.

Table 5 shows the percentage of analysts forecasting the wrong sign. In the early sample period using the annual earnings, analysts forecast profits for firms with actual losses 33.95% of the time. This number is far greater than the reverse. In the early sample period, analysts forecast losses for firms with actual profits just a little over 1% of the time. Although over the sample period, there is no improvement in predicting profits for actual profit firms (profit prediction actually gets worse), the improvement for loss firms is rather extraordinary. At the end of the sample period, profits are forecasted for loss firms only 14.24% of the time in 2000 and 12.20% of the time in 2001, consistent with the increasing tendency of firms to warn of losses.

Table 4
Annual regression results using loss dummy variables

Year	Dispersion									
	Coefficient				t Value				F value	R^2 (adjusted)
	Intercept	Size	B/M	Loss dummy	Intercept	Size	B/M	Loss dummy		
1990	−0.14	0.03	0.12	0.83	−0.76	2.22	3.41	12.94	65.43	0.21
1991	0.14	0.01	0.12	0.86	0.88	1.11	4.97	17.19	115.18	0.18
1992	0.10	0.01	0.11	0.73	1.80	0.96	6.86	22.20	189.14	0.14
1993	0.20	0.00	0.06	0.73	2.61	0.10	4.29	27.04	258.12	0.14
1994	0.20	0.00	0.07	0.63	2.93	0.31	6.51	27.26	268.99	0.12
1995	0.15	0.00	0.04	0.66	2.39	0.65	4.10	31.80	354.31	0.13
1996	0.37	−0.01	0.04	0.62	6.81	−3.34	5.02	35.40	455.72	0.14
1997	0.25	−0.01	0.04	0.38	5.85	−2.05	5.95	29.54	324.43	0.09
1998	0.13	0.00	0.05	0.34	3.08	1.08	6.67	28.82	299.31	0.08
1999	0.08	0.01	0.06	0.29	1.73	2.43	10.13	23.20	218.10	0.07
2000	0.16	−0.00	0.04	0.22	3.66	−0.09	7.17	18.48	126.99	0.05
2001	−0.08	0.02	0.04	0.20	−1.77	5.29	6.51	16.95	103.18	0.05

Year	Error									
	Coefficient				t Value				F value	R^2 (adjusted)
	Intercept	Size	B/M	Loss dummy	Intercept	Size	B/M	Loss dummy		
1990	0.77	−0.02	0.09	0.51	3.09	−0.88	1.93	5.80	14.98	0.04
1991	1.16	−0.05	0.09	0.50	6.97	−3.71	3.12	8.96	45.28	0.05
1992	0.81	−0.03	0.07	0.60	7.77	−3.71	4.01	17.03	118.41	0.06
1993	1.02	−0.05	0.09	0.54	10.88	−6.21	5.40	17.58	146.80	0.06
1994	1.18	−0.06	0.07	0.58	13.82	−8.91	4.86	21.00	213.69	0.07
1995	1.06	−0.05	0.04	0.68	12.83	−8.18	2.41	25.27	285.53	0.08
1996	1.13	−0.06	0.04	0.54	16.23	−10.77	3.72	24.18	287.19	0.07
1997	0.95	−0.05	0.03	0.41	14.56	−9.22	3.10	21.17	228.30	0.05
1998	0.86	−0.04	0.08	0.35	13.78	−7.35	7.46	19.78	214.93	0.05
1999	0.78	−0.03	0.07	0.37	11.79	−5.87	6.69	19.09	192.21	0.05
2000	0.76	−0.03	0.06	0.35	11.29	−5.70	7.11	18.84	168.52	0.04
2001	0.70	−0.02	0.06	0.19	8.91	−3.94	4.90	9.36	58.84	0.02

This table reports the results of an annual regression model, run every sample year from 1990 through 2001. Either forecast dispersion or error is the dependent variable. The independent variables are the logarithm of size (price times shares) in thousands, the logarithm of book-to-market value (equity/size), and a loss dummy equal to one if the actual quarterly First Call earnings are negative and zero otherwise. The regression model is below:

$$\text{Forecast property}_i = a + b_1\log(\text{size})_i + b_2\log(\text{b/m})_i + b_3 \text{ loss dummy}_i + e_i$$

To directly examine forecast performance when actual profitability differs from forecasted profitability, firms are separated into four portfolios based on actual versus expected profits or losses. For example, one portfolio includes firms with expected profits that report actual losses, while another includes firms with expected losses reporting actual losses. Mean dispersion and error are computed for each of the four portfolios. The results are presented in Table 6.

In an unsurprising result, firms with expected and actual profits have the lowest dispersion and error. Interestingly, however, firms with expected and actual losses have the

Table 5
Percentage of firms with wrong sign mean forecasts

	Quarterly forecasts	
	Forecasted loss, actual profit (%)	Forecasted profit, actual loss (%)
All years	1.79	23.31
1990–1995	1.22	33.95
1996–2001	2.11	19.80
Difference	− 0.89*	14.15*
1990	0.89	44.79
1991	1.58	35.11
1992	1.38	30.79
1993	1.04	31.85
1994	1.18	32.15
1995	1.27	37.08
1996	1.72	29.57
1997	1.73	24.28
1998	1.86	21.42
1999	2.52	19.59
2000	2.49	14.24
2001	2.89	12.20

This table reports the percentage of analysts forecasting the wrong sign (e.g., forecasting a profit when an actual loss is eventually reported) over the sample period 1990 through 2001. All numbers are in percent.

* Difference is significantly different from zero with 99% confidence.

second lowest dispersion and error, while the two portfolios containing firms with actual profitability different from expected profitability have the highest dispersion and error. In addition, although error does decrease in the portfolio of expected loss, actual loss firms throughout the sample period, the trend is not nearly as clear and the differences not nearly as large compared with the Table 2 results. These results, combined with the results from Table 5, suggest that a large portion of the decrease in loss firm error comes from two sources: (1) improvement in the error of expected profit, actual loss firms and (2) the higher percentage of losses being predicted (i.e., less expected profit, actual loss firms).

The final testing in this section examines the error and optimism of the mean analyst forecast versus the error and optimism of a "naïve" forecast, the actual First Call earnings in the prior fiscal period.[8] This test addresses several important issues. It provides a measure of the amount of value that analysts provide over and above a forecasting method simple enough to be employed by even the most unsophisticated of individual investors. The test also provides a standard by which to measure earnings predictive difficulty. Firms with accurate naïve forecasts can be thought of as having earnings that are relatively easy to predict. Related to prediction difficulty, the test also somewhat controls for earnings

[8] For the tabulated quarterly results, the naïve model compares the current quarter earnings with the prior quarter earnings (e.g., third quarter 1992 compared with second quarter 1992). To control for earnings seasonality, the prior year quarterly earnings are also used to compute naïve forecasts (e.g., second quarter 1993 compared with second quarter 1992). However, because these naïve forecasts are less accurate than the naïve forecasts using the prior quarter earnings, the results are presented using the more accurate prior quarter naïve forecasts. (Using all sample firms, the average naïve error is 0.82 using prior year quarterly earnings and 0.72 using prior quarter earnings.) The results using the prior year naïve forecasts are similar although analyst superiority is greater.

Table 6
Dispersion and error by expected and actual profitabilily

Expected	Quarterly forecasts							
	Dispersion				Error			
	Profit	Profit	Loss	Loss	Profit	Profit	Loss	Loss
Actual	Profit	Loss	Profit	Loss	Profit	Loss	Profit	Loss
All years	0.13	0.93	1.07	0.42	0.31	1.97	2.38	0.42
1990–1995	0.16	1.17	1.37	0.74	0.35	2.06	2.59	0.50
1996–2001	0.12	0.82	0.98	0.35	0.29	1.91	2.31	0.40
Difference	0.04*	0.35*	0.39*	0.39*	0.06*	0.15*	0.28*	0.10*
1990	0.19	1.31	0.67	0.98	0.47	2.01	2.09	0.49
1991	0.23	1.30	0.99	1.01	0.44	1.97	2.90	0.62
1992	0.19	1.38	2.00	0.76	0.34	2.06	2.76	0.46
1993	0.16	1.24	1.33	0.76	0.35	2.03	2.44	0.46
1994	0.15	1.08	1.30	0.68	0.33	2.07	2.57	0.49
1995	0.14	1.04	1.26	0.69	0.32	2.12	2.55	0.51
1996	0.13	1.04	1.22	0.57	0.30	1.89	2.25	0.43
1997	0.11	0.84	1.00	0.40	0.28	1.94	2.42	0.41
1998	0.11	0.75	1.08	0.40	0.28	1.88	2.11	0.39
1999	0.12	0.73	0.94	0.32	0.28	1.90	2.38	0.41
2000	0.11	0.68	0.84	0.24	0.28	1.98	2.18	0.41
2001	0.13	0.77	0.77	0.27	0.29	1.93	2.54	0.37

This table reports mean analyst quarterly forecast properties sorted by expected and actual profitability over the sample period 1990 through 2001. An actual profit occurs when actual quarterly earnings are greater than or equal to zero, while an actual loss occurs otherwise. A forecasted profit occurs when mean forecasted earnings are greater than or equal to zero, while a forecasted loss occurs otherwise. See Table 1 for variable definitions.

*Difference is significantly different from zero with 99% confidence.

volatility or earnings management (see also next section). Firms with managed or less volatile earnings would probably have more accurate naïve forecasts.

Error, raw error, and optimism are computed using both the analyst forecasts and the naïve forecasts for all sample firms having the required prior period actual earnings information. The sample size is 103,778 firm-quarter observations: 82,203 with profits and 21,575 (20.8%) with losses.

Table 7 reports the results for two forecast properties: error and raw error. For each sample firm, the analyst forecast error is subtracted from the naïve forecast error. For example, if the naïve forecast error is 0.90 and the analyst forecast error is 0.40, then the difference is 0.50. The mean of these differences is computed and reported in the table. Note that in the table, positive numbers indicate analyst superiority, and the larger the difference, the more accurate analyst forecasts are versus naïve forecasts.

Several findings are important. Analyst forecasts are considerably more accurate in every sample year indicating that analysts provide a great deal of value in forecasting earnings versus a simple naïve model. However, they provide more value when forecasting the earnings of loss firms. For example, for all years, the difference between the naïve and analyst error is on average 0.26 for profit firms and 0.45 for loss firms.

Analysts have also slightly increased the value of their forecasting during the sample period, particularly for loss firms. For example, in the early sample period, the analysts are

Table 7
Differences between naïve and analyst forecasts: error and raw error

	Quarterly forecasts					
	Error differences (naïve error − analyst error)			Raw error (RE) differences (naïve RE − analyst RE)		
	All	Profit	Loss	All	Profit	Loss
All years	0.30	0.26	0.45	0.08	0.07	0.08
1990–1995	0.26	0.24	0.39	0.07	0.07	0.07
1996–2001	0.32	0.27	0.47	0.08	0.08	0.08
Difference	−0.06*	−0.03*	−0.08*	−0.01*	−0.01*	−0.01
1990	0.27	0.23	0.48	0.07	0.05	0.18
1991	0.19	0.17	0.32	0.08	0.08	0.11
1992	0.29	0.26	0.45	0.08	0.08	0.06
1993	0.26	0.24	0.38	0.05	0.05	0.06
1994	0.27	0.25	0.35	0.07	0.07	0.06
1995	0.26	0.24	0.40	0.08	0.08	0.08
1996	0.32	0.28	0.55	0.08	0.08	0.07
1997	0.30	0.27	0.46	0.08	0.08	0.07
1998	0.36	0.29	0.59	0.09	0.09	0.10
1999	0.33	0.30	0.44	0.09	0.09	0.08
2000	0.31	0.29	0.39	0.08	0.09	0.07
2001	0.25	0.17	0.38	0.08	0.08	0.08

This table reports the difference between naïve forecast errors and analyst forecast errors over the sample period 1990 through 2001. Analyst forecast error and raw error are defined as in Table 1. Naïve forecast raw error is defined as the absolute value of actual quarterly earnings less the previous quarter's actual earnings. Naïve forecast error deflates this number by the absolute actual quarterly earnings. The reported differences are computed as the naïve error less the analyst error. Thus, positive differences indicate analyst superiority (i.e., lower errors): the higher the difference, the greater the analyst superiority.

*Difference is significantly different from zero with 99% confidence.

superior by 0.39 in predicting error. In the later sample period, this superiority increases to 0.47.

Although not tabulated, naïve forecasts for loss firms are markedly less accurate versus naïve forecasts for profit firms. The mean quarterly naïve forecast error is 0.60 for profit firms and 1.22 for loss firms. The differences remain fairly stable across the sample period. This suggests that loss firm earnings are much more difficult to predict. Thus, considering both the inherent difficulties and the trends of reduced error, analysts seem to be doing an adequate job when forecasting loss firm earnings.

Table 8 presents the results for differences in optimism. With respect to the percentage of optimism, it is assumed that the goal when forecasting is to achieve a systematically unbiased 50%. Therefore, the comparison of analyst forecast optimism versus naïve forecast optimism is computed using 50% as a reference. For example, if analysts are optimistic 45% of the time and naïve forecasts are optimistic 65% of the time, then analyst forecasts are superior by 10% with respect to the 50% goal [(65% − 50%) − (50% − 45%) = 10%]. A positive sign indicates better analyst performance; a negative sign indicates better naïve performance.

The results are fascinating. Naïve forecasts for loss firms are primarily optimistic (63.75%) while naïve forecasts for profit firms are primarily pessimistic (35.58%). Thus,

Table 8
Differences between naïve and analyst forecasts: optimism

	Quarterly forecasts					
	Profit			Loss		
	Percent optimistic, analysts	Percent optimistic, naïve	Analyst superiority versus unbiased 50%	Percent optimistic, analysts	Percent optimistic, naïve	Analyst superiority versus unbiased 50%
All years	33.42	35.58	− 2.16	64.43	63.75	− 0.68
1990–1995	40.29	35.63	4.66	76.70	68.10	− 8.60
1996–2001	29.78	35.56	− 5.78	60.69	62.43	1.74
Difference	10.51*	0.07	− 10.44	16.01*	5.67*	10.34
1990	53.13	35.78	11.09	84.07	69.91	− 14.16
1991	51.88	37.62	10.50	78.77	68.49	− 10.28
1992	41.32	35.84	5.48	77.97	65.85	− 12.12
1993	41.90	36.01	5.89	75.00	66.67	− 8.33
1994	37.95	35.23	2.72	74.69	68.19	− 6.50
1995	37.75	35.29	2.46	77.92	70.13	− 7.79
1996	32.50	33.78	− 1.28	72.67	69.16	− 3.51
1997	31.95	33.86	− 1.91	67.54	64.96	− 2.58
1998	30.53	37.15	− 6.62	64.97	65.22	0.25
1999	26.86	35.30	− 8.44	58.83	60.38	1.55
2000	26.18	34.90	− 8.72	52.21	60.58	8.37
2001	29.11	40.99	− 11.88	51.36	55.75	4.39

This table reports the difference between naïve forecast optimism and analyst forecast optimism over the sample period 1990 through 2001. Optimism is present if the mean forecast is greater than the corresponding actual earnings. As 50% is considered the unbiased target, analyst superiority is determined using 50% as the benchmark. Positive numbers in the "analyst superiority versus unbiased 50%" column indicate analyst superiority, while negative numbers indicate naïve forecast superiority. The analyst superiority column is computed as follows:

$$\text{Analyst superiority} = (|\%\ \text{optimistic naïve} - 50\%|) - (|\%\ \text{optimistic analysts} - 50\%|)$$

*Difference is significantly different from zero with 99% confidence.

the optimism analysts show toward loss firms and the pessimism analysts show toward profit firms is perhaps a natural reflection of an easy starting point. For profit firms, in the early sample period, analysts are nearly unbiased. However, as analyst pessimism increases during the sample period for profit firms, analyst superiority with regard to systematic biases steadily changes to inferiority. As an example, analysts are superior relative to the 50% reference for profit firms by 11.09% in 1990 and 10.50% in 1991. However, these numbers decrease to − 8.72% in 2000 and − 11.88% in 2001, indicating a decline in analyst performance. In contrast, for loss firms, analysts move steadily from inferior performance to superior performance. Fig. 2 shows the trends graphically. Like the corresponding table, positive numbers in the figure indicate superior analyst performance.

5. Earnings management, smoothing, and guidance issues

The increase in forecast pessimism (positive surprises) and decrease in forecast error seen in this and other studies is consistent with earnings management, guidance, and

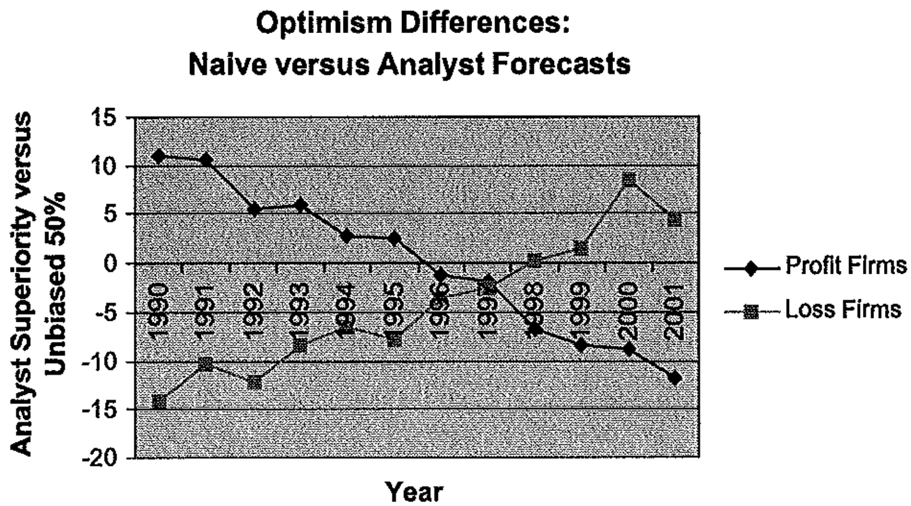


Fig. 2. Analyst versus naïve forecast differences in optimism by year. Note: positive numbers indicate analyst superiority; negative numbers indicate naïve superiority.

smoothing. Various tests are performed to see whether the trends are related to these issues and to differentiate among the potential explanations.

The first procedure examines the subset of firms that failed to meet all three incentives mentioned by Degeorge et al. (1999) when managing earnings: incentives of avoiding losses, avoiding earnings declines, and meeting analyst expectations. Thus, these firms are considered unlikely to be managing earnings as none of the incentives is reached.

Table 9 reports the results. Although the average dispersion, error, and raw error are all higher for this sample of firms versus the full loss firm subsample, similar degrees of improvement in each property are seen. As an example, the average error of these firms drops from 1.23 in the early sample period to 0.93 in the later sample period. This compares with the results for loss firms with either type of surprise from Table 2: 1.02 in the early sample period, decreasing to 0.70 in the later sample period.

To investigate smoothing, trends in earnings volatility are examined. If the decrease in forecasting performance is attributable to increased smoothing, earnings volatility should decrease as well. Earnings volatility is computed as the standard deviation of earnings from the eight most recent quarters. The sample of firms with eight quarters of earnings begins in 1992 and consists of 51,965 firms: 42,543 with profits and 9422 (18.1%) with losses. The trends in earnings volatility are reported in Table 10. Although loss firm earnings volatility decreases, profit firm volatility remains fairly stable across the sample period. Thus, earnings smoothing does not explain trends in profit firm forecasts. For loss firms, the magnitude of the decrease in earnings volatility is far less than the magnitude of the decrease in error and dispersion. Therefore, earnings volatility probably does not explain a large proportion of the trends in loss firm forecasts.

Related testing looks at forecasting trends in a set of firms considered unlikely candidates to smooth earnings, those firms with high earnings volatility. Thus, in each sample year, firms with high earnings volatility are separately analyzed. Both absolute and relative measures of high volatility are used. Absolute measures specify an arbitrary

Table 9
Forecast dispersion, error, and raw error: firms with optimistic forecasts (negative surprises), earnings declines, and losses

	Quarterly forecasts		
	Dispersion	Error	Raw error
All years	0.71	1.01	0.36
1990–1995	1.00	1.23	0.46
1996–2001	0.61	0.93	0.33
Difference	0.39*	0.30*	0.13*
1990	0.87	1.28	0.52
1991	1.20	1.27	0.65
1992	1.12	1.19	0.46
1993	1.03	1.14	0.52
1994	0.94	1.21	0.44
1995	0.93	1.31	0.39
1996	0.87	1.08	0.38
1997	0.66	0.99	0.34
1998	0.63	0.95	0.29
1999	0.54	0.94	0.33
2000	0.47	0.85	0.35
2001	0.50	0.74	0.25

This table reports mean analyst quarterly forecast properties for firms with optimistic forecasts, earnings declines, and losses over the sample period 1990 through 2001. An earnings decline is when actual quarterly earnings are less than the previous quarter's actual earnings. See Table 1 for the other variable definitions.

* Difference is significantly different from zero with 99% confidence.

earnings volatility number to which each firm's earnings volatility is compared, thus controlling for any changes in average volatility during the sample period. Quarterly earnings volatility is considered high if the standard deviation of the actual Street earnings is greater than US$0.50 per share over the prior eight quarters.[9] Under the relative measures of volatility, a firm is considered to have high earnings volatility if its volatility is in the top 10% during the year. Although the results are not tabulated, the same trends of decreasing dispersion, error, and optimism throughout the sample period still exist for the high earnings volatility sample of firms using either the absolute or relative volatility measures.

The next test investigates earnings guidance by isolating firms with high dispersion. These firms are often considered to have a greater disparity of opinion (e.g., Krishnaswami & Subramaniam, 1999) and are, therefore, unlikely to be guiding analysts toward a specific earnings target.

Similar to the volatility tests, absolute and relative measures are used. Under the absolute method, firms are considered to have high dispersion if their dispersion measure is greater than or equal to 0.50.[10] This sample contains 8225 firms (9.7% of the full dispersion sample), 4028 with profits and 4197 (51.0%) with losses. Under the relative measure, firms are considered to have high dispersion if their dispersion measure is in the top 10% during the relevant year.

[9] Other arbitrary cutoff points are employed with similar results.

[10] Other arbitrary cutoff points are employed with similar results.

Table 10
Earnings volatility by year

	Eight quarter earnings volatility		
	All	Profit	Loss
All years	0.17	0.14	0.28
1992–1996	0.17	0.14	0.36
1997–2001	0.16	0.14	0.25
Difference	0.01*	0.00	0.11*
1992	0.18	0.16	0.32
1993	0.18	0.15	0.35
1994	0.18	0.16	0.35
1995	0.18	0.14	0.43
1996	0.16	0.13	0.33
1997	0.16	0.14	0.29
1998	0.15	0.13	0.23
1999	0.16	0.14	0.24
2000	0.16	0.14	0.26
2001	0.18	0.15	0.26

This table reports mean quarterly earnings volatility over the sample period 1992 through 2001. Quarterly earnings volatility is defined as the standard deviation of actual earnings from the eight previous quarters. As 2 years of earnings are needed before the volatility can be computed, the sample period does not include 1990 and 1991.

* Difference is significantly different from zero with 99% confidence.

Table 11
Forecast error, raw error, and optimism by profitability: firms with dispersion greater than 0.50

	Quarterly forecasts								
	Error			Raw error			Percent optimistic		
	All	Profit	Loss	All	Profit	Loss	All	Profit	Loss
All years	1.09	1.14	1.04	0.23	0.13	0.33	64.61	39.95	88.28
1990–1995	1.21	1.24	1.17	0.30	0.19	0.42	69.24	49.36	90.93
1996–2001	1.01	1.07	0.96	0.19	0.08	0.28	61.76	33.51	86.81
Difference	0.20*	0.17*	0.21*	0.11*	0.11*	0.14*	7.48*	15.85*	4.12*
1990	1.35	1.60	1.09	0.55	0.37	0.74	73.85	58.82	90.32
1991	1.15	1.18	1.13	0.38	0.17	0.60	68.05	48.77	88.74
1992	1.11	1.13	1.09	0.32	0.21	0.45	66.73	47.71	90.00
1993	1.20	1.27	1.12	0.26	0.19	0.34	69.06	49.37	91.43
1994	1.23	1.21	1.25	0.30	0.21	0.40	67.97	48.56	90.12
1995	1.26	1.30	1.22	0.24	0.12	0.35	71.90	50.00	92.65
1996	1.12	1.13	1.11	0.24	0.11	0.38	66.83	41.83	91.40
1997	1.01	1.06	0.97	0.20	0.08	0.31	63.19	36.77	87.94
1998	0.97	1.03	0.93	0.17	0.07	0.26	64.15	35.50	86.82
1999	0.98	1.08	0.90	0.18	0.08	0.27	56.75	25.67	85.02
2000	1.02	1.09	0.96	0.16	0.08	0.22	56.10	29.21	80.94
2001	0.90	0.95	0.87	0.16	0.08	0.22	60.13	25.95	86.47

This table reports mean analyst quarterly forecast properties for firms with forecast dispersion greater than 0.50 over the sample period 1990 through 2001. See Table 1 for variable definitions.

* Difference is significantly different from zero with 99% confidence.

Table 11 presents the results using the absolute measure. (The results using the relative measure are similar.) There is a clear reduction in forecast error and raw error during the sample period for both profit and loss firms. Optimism also decreases dramatically for profit firms, starting around 50% in the first few sample years, but reaching below 30% for the last three sample years. Loss firms, however, are dominated by overwhelming optimism throughout the sample period (an average of 88.28%), the lack of improvement indicating a problem area that analysts should address. Thus, although analysts have reduced the size of their errors for firms with high dispersion, they still tend to overestimate the earnings of high dispersion, loss firms. This testing suggests that systematic profit firm pessimism occurs regardless of whether the forecasts are guided. However, the reduction of loss firm optimism occurs when firms warn analysts of the impending loss.

Overall, the improved forecasting ability of analysts occurs regardless of increases in earnings management, guidance, or smoothing. The trends are consistent with concerns of legal liability as most of the reduction in dispersion and error is due to loss firms. The trends are also consistent with improved analyst forecasting abilities. The increase in pessimism for profit firms may be partly attributed to an overreliance on the previous period's earnings.

6. GAAP versus Street earnings and Regulation FD

Another issue is related to the Street versus GAAP earnings debate. Abarbanell and Lehavy (2000) suggest that using forecast provider databases, such as First Call, to obtain earnings data might impact conclusions reached in earnings-related studies. First Call collects data based on the earnings that firms publicize to the market, often known as Street earnings, which may be different from GAAP earnings. Therefore, following the procedure of Brown (2001), the sample of firms in which GAAP earnings from Compustat equal Street earnings from First Call are examined separately. The earnings are considered equal if the absolute value of the difference is less than US$0.02 to control for rounding differences and materiality. The results (not shown) are similar to the previous results for the reduced sample. Moreover, the difference in Street versus GAAP earnings has not increased over the sample period (not shown).

Finally, the passage of Regulation FD in August 2000 and its subsequent implementation on October 23, 2000 might affect forecasts made during the surrounding time periods. To investigate this issue, the quarterly forecast properties from the beginning of 1999 through the end of 2001 are computed for only firms that have fiscal quarters on a March, June, September, December cycle. This provides a sample with three distinct, easily identifiable subperiods: (1) a pre-Regulation FD period, from the first quarter of 1999 through the second quarter of 2000; (2) a period during the implementation of Regulation FD, the third and fourth quarters of 2000; and (3) a post-Regulation FD period, the first quarter of 2001 through the fourth quarter of 2001. The second period, during the implementation, includes the quarter in which the regulation was passed.

Table 12
Forecast dispersion, error, raw error, and optimism surrounding implementation of regulation FD

Year: month	Profit firms				Loss firms			
	Dispersion	Error	Raw error	Percent optimistic	Dispersion	Error	Raw error	Percent optimistic
Pre								
1999: 3	0.15	0.35	0.05	27.35	0.39	0.66	0.15	56.36
1999: 6	0.13	0.33	0.05	26.49	0.40	0.67	0.16	57.89
1999: 9	0.14	0.34	0.05	27.96	0.41	0.66	0.19	56.41
1999: 12	0.15	0.34	0.06	25.42	0.37	0.74	0.28	59.95
2000: 3	0.13	0.35	0.05	23.89	0.34	0.59	0.17	50.55
2000: 6	0.13	0.32	0.05	24.49	0.28	0.64	0.19	49.63
During								
2000: 9	0.13	0.31	0.06	28.71	0.23	0.60	0.19	47.68
2000: 12	0.14	0.32	0.06	29.63	0.30	0.64	0.26	56.54
Post								
2001: 3	0.14	0.33	0.05	30.90	0.33	0.51	0.17	52.74
2001: 6	0.16	0.35	0.05	27.40	0.30	0.53	0.14	51.75
2001: 9	0.16	0.37	0.06	34.47	0.34	0.56	0.18	54.89
2001: 12	0.15	0.33	0.05	22.41	0.32	0.54	0.13	47.02

This table reports mean analyst quarterly forecast properties for the quarters surrounding the implementation of Regulation Free Disclosure (Reg FD). Reg FD was passed in August 2000 and implemented in October 2000. See Table 1 for variable definitions. Only firms with fiscal quarters ending in March, June September, and December are included in the sample.

After evaluating the results, presented in Table 12 for profit and loss subsamples, there are no identifiable differences in the forecast property trends during the three periods surrounding Regulation FD implementation regardless of whether the sample includes all firms, profit firms, or loss firms.

7. Conclusions

This study documents almost continuous reductions in analyst forecast dispersion, error, and optimism during the time period 1990 through 2001. The reductions, however, primarily come about due to staggering advances in forecasting loss firm earnings. At the end of the sample period, differences in forecasting performance between profit and loss firms are relatively small. Attempts are made to control for various issues that might affect the conclusions, such as earnings management, guidance, and smoothing, Street versus GAAP earnings, or Regulation FD. None of those issues can wholly explain the trends.

In addition, it appears that loss firm earnings are more difficult to predict. Given the prediction difficulties, the value provided to the market by analysts appears to be greater for loss firms versus profit firms.

While this study does not contradict prior studies showing increases in earnings management or guidance, it does shed additional light on the issue. Analysts are undoubtedly not as optimistic, their incentives to get investment banking clients or private information perhaps no longer as important as the notoriety they receive when they mislead investors.

Future studies can examine trends in analyst buy, sell, or hold recommendations, another area in which the media and academic research (and also the Securities and Exchange Commission) have criticized analysts. Analysts are known to frequently make buy recommendations but rarely make sell recommendations, often preferring to drop coverage of a firm rather than issue a sell recommendation (e.g., Barber, Lehavy, McNichols, & Trueman, 2001; McNichols & O'Brien, 1997; Stickel, 1995).

Acknowledgements

This paper has benefited from the comments of Afshad Irani, Ahmad Etebari, Werner DeBondt, John Becker-Blease, participants at the 2003 Financial Management Conference (especially the discussant Larry Wall), participants at a University of New Hampshire Seminar, and the referee. The author would like to thank Pamela Grant and Chuck Hill of Thomson Financial Services, who have generously provided the First Call data.

References

Abarbanell, J., & Bernard, V. (1992). Tests of analysts' overreaction/underreaction to earnings information as an explanation for anomalous stock price behavior. *Journal of Finance*, *47*, 1181–1208.

Abarbanell, J., & Lehavy, R. (2000). *Differences in commercial database reported earnings: Implications for inferences concerning analyst forecast rationality, the association between prices and earnings, and firm reporting discretion.* Working paper, University of North Carolina at Chapel Hill.

Ang, J., & Ciccone, S. (2001). *Analyst forecasts and stock returns.* Working paper. University of New Hampshire.

Asthana, S. (2003). Impact of information technology on post-earnings announcement drift. *Journal of Information Systems*, *17*, 1–17.

Atiase, R. (1985). Predisclosure information, firm capitalization, and security price behavior around earnings announcements. *Journal of Accounting Research*, *23*, 21–36.

Barber, B., Lehavy, R., McNichols, M., & Trueman, B. (2001). Can investors profit from the prophets? Security analyst recommendations and stock returns. *Journal of Finance*, *56*, 531–563.

Bartov, E. (1993). The timing of asset sales and earnings manipulation. *Accounting Review*, *68*, 840–855.

Bradshaw, M., & Sloan, R. (2002). GAAP versus the street: An empirical assessment of two alternative definitions of earnings. *Journal of Accounting Research*, *40*, 41–65.

Brown, L. (1997). Analyst forecasting errors: Additional evidence. *Financial Analysts Journal*, *53*, 81–88.

Brown, L. (2001). A temporal analysis of earnings surprises: Profits versus losses. *Journal of Accounting Research*, *39*, 221–241.

Burgstahler, D., & Dichev, I. (1997). Earnings management to avoid earnings decreases and losses. *Journal of Accounting and Economics*, *24*, 99–126.

Butler, K., & Saraoglu, H. (1999). Improving analysts' negative earnings forecasts. *Financial Analysts Journal*, *55*, 48–56.

Ciccone, S. (2001). *Analyst forecasts, financial distress, and business risk.* Working paper. University of New Hampshire.

Collingwood, H. (2001). The earnings game: Everybody plays, nobody wins. *Harvard Business Review*, *79*, 65–74.

DeBondt, W., & Thaler, R. (1990). Do security analysts overreact? *American Economic Review*, *80*, 52–57.

Degeorge, F., Patel, J., & Zeckhauser, R. (1999). Earnings management to exceed thresholds. *Journal of Business*, *72*, 1–33.

Diether, K., Malloy, C., & Scherbina, A. (2002). Differences of opinion and the cross-section of stock returns. *Journal of Finance*, *57*, 2113–2141.

Dische, A. (2002). Dispersion in analyst forecasts and the profitability of earnings momentum strategies. *European Financial Management*, *8*, 211–228.

Dowen, R. (1996). Analyst reaction to negative earnings for large well-known firms. *Journal of Portfolio Management*, *23*, 49–55.

Dreman, D., & Berry, M. (1995). Analyst forecasting errors and their implications for security analysis. *Financial Analysts Journal*, *51*, 30–41.

Easterwood, J., & Nutt, S. (1999). Inefficiency in analyst forecasts: Systematic misreaction or systematic optimism? *Journal of Finance*, *54*, 1777–1797.

Fox, J. (1997, March 31). Learn to play the earnings game (and wall street will love you). *Fortune*. 76–80.

Fried, D., & Givoly, D. (1982). Financial analysts' forecasts of earnings: A better surrogate for market expectations. *Journal of Accounting and Economics*, *4*, 85–107.

Fudenberg, D., & Tirole, J. (1995). A theory of dividend smoothing based on incumbency rents. *Journal of Political Economy*, *103*, 75–93.

Gu, Z., & Wu, J. (2003). Earnings skewness and analyst forecast bias. *Journal of Accounting and Economics*, *35*, 5–29.

Healy, P. (1985). The effect of bonus schemes on accounting decisions. *Journal of Accounting and Economics*, *7*, 85–107.

Holthausen, R., Larcker, D., & Sloan, R. (1995). Annual bonus schemes and the manipulation of earnings. *Journal of Accounting and Economics*, *19*, 29–74.

Hong, H., Kubik, J., & Solomon, A. (2000). Security analysts' career concerns and herding of earnings forecasts. *Rand Journal of Economics*, *31*, 121–144.

Imhoff, E., & Lobo, G. (1992). The effect of ex-ante earnings uncertainty on earnings response coefficients. *Accounting Review*, *67*, 427–439.

Kasznik, R., & Lev, B. (1995). To warn or not to warn: Management disclosures in the face of an earnings surprise. *Accounting Review*, *70*, 113–134.

Krishnaswami, S., & Subramaniam, V. (1999). Information asymmetry, valuation, and the corporate spin-off decision. *Journal of Financial Economics*, *53*, 73–112.

Matsumoto, D. (2002). Management's incentives to avoid negative earnings surprises. *Accounting Review*, *77*, 483–514.

McNichols, M., & O'Brien, P. (1997). Self-selection and analyst coverage. *Journal of Accounting Research*, *35*, 167–199.

O'Brien, P. (1988). Analysts' forecasts as earnings expectations. *Journal of Accounting and Economics*, *10*, 53–83.

Olsen, R. (1996). Implications of herding behavior for earnings estimation, risk assessment, and stock returns. *Financial Analysts Journal*, *52*, 37–41.

Richardson, S., Teoh, S., & Wysocki, P. (2001). *The walkdown to beatable analyst forecasts: The role of equity issuance and insider trading incentives*. Working paper. University of Michigan.

Skinner, D. (1994). Why firms voluntarily disclose bad news. *Journal of Accounting Research*, *32*, 38–60.

Skinner, D. (1997). Earnings disclosures and stockholder lawsuits. *Journal of Accounting and Economics*, *23*, 249–282.

Skinner, D., & Sloan, R. (2002). Earnings surprises, growth expectations, and stock returns or don't let an earnings torpedo sink your portfolio. *Review of Accounting Studies*, *7*, 289–312.

Stickel, S. (1990). Predicting individual analyst earnings forecasts. *Journal of Accounting Research*, *28*, 409–417.

Stickel, S. (1995). The anatomy of the performance of buy and sell recommendations. *Financial Analysts Journal*, *51*, 25–39.

Trueman, B. (1994). Analyst forecasts and herding behavior. *Review of Financial Studies*, *7*, 97–124.

Trueman, B., & Titman, S. (1988). An explanation of accounting income smoothing. *Journal of Accounting Research*, *26*, 127–139.

Vickers, M. (1999, May 29). Ho-Hum, another earnings surprise. *Business Week*. 83–84.

Welch, I. (2000). Herding among security analysts. *Journal of Financial Economics*, *58*, 369–396.

JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS VOL. 41, NO. 1, MARCH 2006

Are Analyst Recommendations Biased? Evidence from Corporate Bankruptcies

Jonathan Clarke, Stephen P. Ferris, Narayanan Jayaraman, and Jinsoo Lee*

Abstract

We test whether a bias exists in analyst recommendations for firms that file for bankruptcy during 1995–2001. We fail to find overoptimism in analyst recommendations, including those of affiliated analysts. Our multivariate analysis of the market reaction to changes in analyst recommendations indicates that prior affiliation exerts no impact on either returns or trading volume. We find that the market does not view recommendation upgrades by affiliated analysts as biased since there is no price reversal following these recommendation changes. Overall, our results suggest that recently passed legislation to reduce analysts' conflicts of interest might be an overreaction.

I. Introduction

The nature of analyst recommendations and the extent to which they might be biased by conflicts of interest has recently attracted the attention of regulatory and legislative bodies that oversee U.S. capital markets. In May 2002, the Securities and Exchange Commission (SEC) approved measures to strengthen disclosures made by analysts and brokerage firms.[1] These measures represent an attempt to address conflicts of interest that can arise when analysts are employed

*Clarke, jonathan.clarke@mgt.gatech.edu, Jayaraman, narayanan.jayaraman@mgt.gatech.edu, and Lee, jinsoo.lee@mgt.gatech.edu, Georgia Institute of Technology, College of Management, 800 W. Peachtree St. NW, Atlanta, GA 30332; Ferris, ferriss@missouri.edu, University of Missouri-Columbia, College of Business and Public Administration, 404F Cornell Hall, Columbia, MO 65211. We acknowledge the use of IBES data, a service of Thompson Financial, in the preparation of this study. The data is provided as part of a broad academic program to encourage earnings expectation research. We thank Hendrik Bessembinder (the editor), Don Chance, Bryan Church, Craig Dunbar (the referee), Jere Francis, Narasimhan Jegadeesh, Robert Lawless, Anil Makhija, Connie Mao, Frank Reilly, John Stowe, and seminar participants at the 2003 Financial Management Association annual meeting, the 2005 Financial Management Association Europe annual conference, University of Missouri, and Wayne State University for their comments on earlier versions of this paper. We especially thank Chris Carman of AG Edwards for helpful comments on the process by which analysts formulate their recommendations. All remaining errors are our responsibility.

[1]The new rules were announced by the SEC on May 10, 2002 and were phased in over the following 180 days to provide firms with a reasonable amount of time to develop procedures and policies compliant with the new requirements.

by investment banks that have relationships with issuers of recommended securities or when the analyst/bank has purchased the securities of the recommended issuer.

Houston, James, and Karceski (2006) in this issue examine this conflict of interest between investment banks and analysts in the context of IPO underpricing and subsequent firm market valuation during the tech bubble of 1999–2000. They contend that the reduced legal liability of analysts relative to investment bankers explains the inflated analyst equity valuations in the immediate post-IPO period.

A triggering event that resulted in the call for new legislation and prompted extensive criticism of analysts by the press, investors, politicians, and regulators was the meltdown of Enron in late 2001. Although Enron filed for bankruptcy in December 2001, analysts continued to be optimistic about the stock as late as October 2001.[2] Indeed, of the 17 analysts then following the company, 10 had a strong buy rating on the stock and five others had a buy rating, despite massive reported accounting losses and a 50% loss in Enron's market value during the quarter preceding bankruptcy.

In addition to stimulating new regulations, the apparent persistence of analyst optimism about a firm in financial distress resulted in the passage of new legislation affecting analysts as well as raising two important research questions.[3,4] The first question focuses on the extent to which analysts are reluctant to issue negative recommendations because of the potential loss of future investment banking deals.[5] Such behavior would produce positive biases in their recommendations (i.e., overly optimistic recommendations). The second question concerns the potential for conflicts of interest among analysts that have ongoing business dealings with a firm. Such analysts might face pressure to compromise their recommendations for these firms even as they become financially distressed. This is because subsequent underwriting and related services often provide higher levels of revenue for the brokerage firm than securities research or brokerage. Through an examination of analyst recommendations for firms that eventually file for bankruptcy, our study provides useful insights into these two questions.

The existing literature examining security analyst activity for bankrupt firms, such as Moses (1990) and Espahbodi, Dugar, and Tehranian (2001), focuses on

[2]*The Wall Street Journal*, "Most Analysts Remain Plugged in to Enron," Oct. 26, 2001, p. C1.

[3]In December 2002, responding to the legal prodding of the New York state attorney general, the SEC, the North American Securities Administrators Association, the National Association of Securities Dealers, and the New York Stock Exchange reached a settlement with the largest investment banking firms to resolve issues associated with analyst conflicts of interest. Three aspects of this settlement directly impact analysts. The first is the requirement that research analysts be insulated from investment banking pressures. Second, for a five-year period, each of the defendant brokerage firms must contract with no less than three independent research firms to provide analyst recommendations to the firm's customers. Finally, the firms must disclose their analyst recommendations in an effort to allow public evaluation of their performance.

[4]Further, the Sarbanes-Oxley Act of 2002 requires that the SEC adopt rules to address conflicts of interest that can arise when analysts working for an investment banking firm recommend equities in research reports and public appearances. Sarbanes-Oxley instructs the SEC to draft regulations limiting the access to analysts by individuals within a brokerage house whose interests reside in the firm's other investment banking activities.

[5]This study examines the possibility of bias only among sell-side analysts. Cheng, Liu, and Qian (2006) in this issue, however, develop a theoretical model that incorporates a biased sell-side analyst simulation with the presence of an unbiased buy-side analyst.

earnings forecasts rather than recommendations. Our use of analyst recommendations complements the literature and is motivated by previous research that establishes the investment value of recommendations by security analysts. Womack (1996) finds that buy recommendations generate a 3.0% announcement period abnormal return, while sell recommendations generate a −4.7% abnormal return. Subsequent research by Brown, Foster, and Noreen (1985), Stickel (1990), Dugar and Nathan (1995), Lin and McNichols (1998), Dechow, Hutton, and Sloan (1998), and Michaely and Womack (1999) shows that the information value of recommendations can be obscured by conflicts of interest among security analysts.

Green (2006) in this issue provides evidence that early access to stock recommendations provides the clients of brokerage firms with incremental investment value. After controlling for transaction costs, he shows that purchasing (selling) immediately following upgrades (downgrades) results in average two-day returns of 1.02% (1.56%). Conrad, Cornell, Landsman, and Rountree (2006) in this issue present evidence consistent with an asymmetry in analyst recommendations following either large positive or negative returns. Specifically, they find that analysts are equally likely to upgrade or downgrade following a large price increase, but are more likely to downgrade after a large stock price decline.

We believe that there are several reasons why a set of bankrupt firms provides a useful sample over which to examine possible recommendation bias by security analysts. First, studies by Altman (1968), (1970), Westerfield (1971), Aharony, Jones, and Swary (1980), and Clark and Weinstein (1983) report that financial deterioration of the firm occurs long before the actual bankruptcy filing, suggesting that alert analysts should begin revising their recommendations far in advance of the bankruptcy announcement.

Bankruptcy also causes firms to incur substantial direct and indirect costs, which impacts profitability and consequently should be reflected in analyst recommendations. Warner (1977) finds that the direct costs of bankruptcy are approximately 5.3% of the firm's value immediately prior to bankruptcy while Weiss (1990) reports that these costs average 3.1% of total firm value. Ferris and Lawless (2000) measure the median direct costs of bankruptcy as 3.5% of firm assets. Indirect costs are even more significant. Altman (1984) estimates that mean indirect bankruptcy costs approximate 17.5% of the firm's value one year prior to bankruptcy.

Previous studies also indicate that investing in bankrupt stocks is not particularly profitable, suggesting that analysts should downgrade their recommendations as a firm moves toward bankruptcy. Morse and Shaw (1988) note that while trading in a bankrupt firm's securities is common, this strategy does not yield significant positive abnormal returns. Hubbard and Stephenson (1997) likewise document the poor returns from investing in bankrupt firms. Thus, positive recommendations about the investment value of trading in bankrupt stocks are difficult to justify.

Because of the prolonged deterioration in a firm's financial condition preceding bankruptcy, the substantial direct and indirect costs associated with bankruptcy, and the losses resulting from a strategy of trading bankruptcy equities, we expect analysts to downgrade their recommendations as a firm experiences financial distress. Thus, our sample is especially useful for testing whether analysts are systematically overoptimistic. Indeed, we expect to observe considerable revision by analysts in their recommendations and movement away from positive recommendations. If, however, we observe a pattern of non-revision or "stickiness" in analyst recommendations, then media claims of analyst overoptimism might be justified.

We also test whether affiliated analysts—analysts employed by banks that have a history of previous transactions with a firm—provide significantly different recommendations than other analysts. More specifically, we test whether affiliated analysts suffer from a conflict of interest when forming their recommendations. Because of a brokerage house's potential to earn additional underwriting fees, an affiliated analyst might be encouraged to issue more positive recommendations for a firm than its financial circumstances warrant. Affiliated analysts might also be conflicted by reputational effects on their employer. For instance, if a brokerage house helps to raise external capital for a firm through a new securities issuance, but the firm subsequently enters bankruptcy, then that house would suffer a reputation cost. This cost can be avoided or at least diminished if the firm's bankruptcy can be delayed, and one potential way to delay bankruptcy would be for the affiliated analyst to issue positive recommendations.

Based on a set of 384 sample firms that file for bankruptcy during the period 1995–2001 and a corresponding set of industry and Altman z-score matched firms that do not enter into bankruptcy, we fail to find evidence of a positive bias in analyst recommendations. Over the eight quarters preceding bankruptcy as well as the quarter of the bankruptcy filing, mean recommendations monotonically decline. This trend is confirmed in our multivariate analysis of analyst recommendations. There is also a corresponding decline in the percentage of buy recommendations. When we benchmark the recommendations for the sample firms against their matches, we find that analysts more aggressively revise downward their assessments for the sample firms. An analysis of abnormal returns surrounding changes in recommendations for the sample and matched firms provides additional evidence of a lack of bias in analyst recommendations.

We find that affiliated analysts' recommendations are not influenced by previous relationships between the analyst's employer and the sample firm. We estimate analyst affiliation in a number of different ways, including measures based on the kind of transaction, the elapsed time since the last transaction, and the number of investment bankers involved in the transaction. Our results remain robust and indicate that affiliated analysts, in general, do not let potential conflicts of interest influence their recommendations.

Our multivariate analysis of the market's reaction to changes in analyst recommendations offers further confirmation that a previous affiliation has no impact. The recommendations of affiliated analysts affect neither the firm's abnormal returns nor its trading volume. Further, we fail to observe that the market views recommendation upgrades by affiliated analysts as biased since there is no pattern of price reversal following such changes.

Our conclusion that affiliated analysts are no more optimistic than unaffiliated analysts differs from the conclusions of Dechow, Hutton, and Sloan (1998) and Michaely and Womack (1999).[6] Our conclusion is consistent, however, with Kolasinski and Kothari (2003). Our findings suggest that the recently passed regulations and laws to reduce analyst conflict might be an overreaction by regulatory authorities. This conclusion is consistent with the arguments of Holmström and Kaplan (2003) regarding U.S. corporate governance and the possibility of "overreacting to extreme events."

The remainder of this paper is organized as follows. The next section provides a description of our data and sample characteristics. Section III contains empirical results concerning the presence of analyst bias in recommendations for sample firms. Section IV presents our findings from an examination of affiliated analysts and the extent to which conflicts of interests might influence their recommendations. We conclude with a brief summary and discussion in Section V.

II. Data Description, Sample Characteristics, and Recommendation Estimation

A. Data Description

We identify firms that enter the Chapter 11 bankruptcy process through Bankruptcy DataSource. This is an online database that contains reorganization plans and news related to the bankruptcy process for all publicly traded companies with assets in excess of $50 million. Our initial sample consists of 995 firms that file for bankruptcy over the period 1995–2001. We eliminate 263 firms that lack Compustat data.

We obtain analyst recommendations from IBES. The database begins in October 1993 and contains recommendations from a wide range of brokerage firms. It tracks the analyst issuing the forecast, the analyst's current employer, the recommendation report date, and the recommendation itself. Recommendations are based on a five-point scale and are coded as follows: (1) strong buy, (2) buy, (3) hold, (4) underperform, and (5) sell. We then determine the intersection between the sample of bankrupt firms and those firms included on the IBES recommendations database. Of the remaining 732 firms, we lose 348 firms because IBES does not contain recommendations for them.[7] Our final sample consists of 384 firms. The distribution of bankruptcies over the sample period is as follows: 1995 (24), 1996 (28), 1997 (25), 1998 (46), 1999 (71), 2000 (80), and 2001 (110).

We begin our analysis in 1995 for two reasons. First, because recommendation data only begins in October 1993, the study cannot be undertaken earlier. Second, our research questions focus on the time-series behavior of analyst recommendations during the period preceding bankruptcy. We select eight quarters

[6]The findings of Bradley, Clarke, and Cooney (2005) imply a possible explanation for the difference between our findings and those of earlier researchers. They find that unaffiliated analysts are less optimistic than affiliated analysts in the early 1990s, but become equally optimistic in the late 1990s. Bradley, Clarke, and Cooney contend that this increase in optimism by unaffiliated analysts is due to the growing importance of research coverage for issuing firms and the need to compete for underwriting revenues by issuing favorable research and recommendations.

[7]We test to determine whether there are any significant differences among the sample firms and those that we eliminate. Analysts, in general, do not cover small firms. Indeed, the mean (median) market capitalization of our sample firms is $599.3 million ($122.3 million) whereas the mean (median) size of the firms we eliminate is $48.2 million ($19.3 million).

as a reasonable time period over which to examine the nature of analyst recommendations. This collapses our recommendations time series back to 1993, the starting point for their inclusion on IBES. We obtain stock market returns from the Center for Research in Security Prices (CRSP) and data on firm characteristics from Compustat.

Our sample period terminates in 2001. We conclude our analysis in this year since it immediately precedes the many legislative and regulatory changes resulting from the Enron scandal. Consequently, this sample period provides a homogeneous legal and regulatory environment for our examination and allows a more controlled analysis. Further, by examining analyst behavior prior to these changes, this study can assess the usefulness of the new laws and regulations.

To define an affiliated analyst, we compile a comprehensive database of investment banking deals between 1986 and 2001 from *Thompson Financial's Securities Data* New Issues and Mergers & Acquisitions (M&A) databases. From the new issues database, we obtain the identity of the investment banker/bankers retained by the issuer for every initial public offering (IPO), seasoned equity offering (SEO), and bond offering. From the mergers and acquisitions database, we obtain the identity of the investment banker/bankers for the target and acquirer as well as the announcement and effective dates of the transaction.

There are a total of 67,995 deals in our database. The deals are distributed as follows: 8,125 initial public offerings; 9,342 seasoned equity offerings; 21,541 bond offerings; and 28,987 instances in which either the target or acquirer retains the services of an investment bank.

We define an affiliated analyst as one whose investment bank has acted as an advisor to the firm in a financial transaction (i.e., bond offering, M&A deal, SEO, or IPO) during the three years prior to the recommendation. We use the IBES Broker Code Key to match the recommendation data to the investment banking deal data.

B. Sample Characteristics

To further analyze the nature of the recommendations for the sample firms, we identify a set of firms that is covered by IBES that does not file for bankruptcy. We then match our sample firms with these firms on the basis of a two-digit SIC code and an Altman z-score.[8] The Altman z-score is estimated two years preceding the year of bankruptcy for the sample firm. We further require that the matched firms have at least one recommendation for the year following their sample firm's bankruptcy. Of the original 384 sample firms, we eliminate 94 because z-scores two years prior to bankruptcy cannot be calculated. Of the remaining 290 firms, we are able to identify matches for 289 firms. The 289 sample firms and their corresponding matches serve as the focus of this study.[9] This approach provides us with an initial characterization of our sample firms and allows us to

[8]Boni and Womack (2006) in this issue provide evidence that analysts create value in their recommendations through their ability to rank stocks within industries.

[9]For purposes of robustness, we also calculate a narrower set of matches by imposing the requirement that the matched firm's z-score resides within a 20% band of the z-score for its corresponding sample firm. This reduces the number of matches from 289 to 241. Our empirical results, however, remain qualitatively unchanged and we conclude that our set of matched firms is appropriate.

compare analyst recommendations with a set of similarly financially distressed firms that do not file for bankruptcy.

Limiting our sample to firms filing for bankruptcy could introduce a selection bias since analysts cannot know which firms will go bankrupt ex ante. Our construction of a matched sample of non-bankrupt firms with comparable levels of financial distress provides a benchmark against which to evaluate the recommendations provided by analysts for those firms that ultimately go bankrupt. This comparison of recommendations for sample firms against their matched firm counterparts allows us to control for any possible selection bias and permits useful conclusions regarding the nature of analyst recommendations for financially distressed firms.

Table 1 provides a comparison of select accounting and financial variables between the sample and matched firms of this study. The results show that the sample firms are smaller than their matches. The sample firms have a mean equity market capitalization of $628.9 million, compared to $1,525.2 million for the matched firms. We observe comparable values when we measure firm size by the book value of total assets. The sample firms are more highly leveraged and less profitable than the matched firms. Given that these sample firms are approaching bankruptcy, such differences in leverage and profitability are not surprising. Important to this analysis, however, is the finding that there is no statistically significant difference in the number of analysts covering the firms in the two different groups.

TABLE 1

Comparison between Sample and Matched Firms Two Years Prior to Bankruptcy

	Sample Firms (A)			Matched Firms (B)			Mean Difference (A−B)	Median Difference (A−B)
	No.	Mean	Median	No.	Mean	Median	t-Test (t-value)	Median Test (t-value)
Market equity capitalization ($mill.)	289	628.9	115.2	289	1,525.2	253.0	−3.11**	−3.57**
Book value of total asset ($mill.)	289	731.7	226.1	289	1,987.5	314.9	−3.43**	−2.24*
z-score	289	4.49	1.89	289	3.93	2.05	0.49	−0.91
Total liability/market value	289	0.514	0.531	289	0.463	0.477	2.31*	2.08*
EBIT/Total assets	273	−0.083	−0.031	270	−0.040	0.048	−1.99*	−5.53**
No. of analysts per company	289	1.78	1.33	289	1.80	1.50	−0.27	−1.57

The non-bankrupt matched firms are selected on the basis of a two-digit SIC code and an Altman z-score two years preceding bankruptcy. The matched firm is also required to have at least one recommendation during the following year. The variable definitions given by Fama and French (2002) are used. Statistical significance at the 1% and 5% levels are indicated by ** and *, respectively.

C. Estimation of Recommendations

The mean analyst recommendation for any quarter includes both actual and inferred recommendations for the quarter of interest. Actual recommendations are those made and issued by the analyst. They are readily obtained from the IBES databases. Limiting our analysis to only these recommendations ignores information when no recommendations are available for a specific quarter. For any quarter, a recommendation will be missing because either no new recommendation is made or the analyst decides to drop coverage.

Using the IBES stopped recommendations file, we attempt to discriminate between these two possibilities and to infer an appropriate recommendation. In the first possibility, a recommendation is missing for a specific quarter, but the analyst continues to provide recommendations for subsequent quarters. Since the analyst has not dropped coverage of this firm, we simply infer that the recommendation of the preceding quarter remains valid.

The second possibility occurs when the missing recommendation is due to the analyst dropping coverage of the firm. If the last recommendation issued by the analyst prior to dropping coverage is a strong buy (1) or a buy (2), we infer an underperform (4). Otherwise, the recommendation is inferred to be a sell (5). Because an analyst generally remains at the same brokerage company after dropping coverage, the fact that the analyst no longer issues a recommendation is likely to be associated with negative expectations regarding the firm's prospects.

III. Are Analysts Positively Biased?

A. Time Trend in Aggregate Analyst Recommendations

Table 2 presents the time series of analyst recommendations for the eight quarters preceding the quarter of the bankruptcy filing. We calculate analyst recommendations using only observed recommendations (Panel A) as well as a combination of observed and inferred recommendations (Panel B).

In Panel A, the mean analyst recommendation increases from 2.06 at eight quarters prior to bankruptcy to 3.22 in the quarter of the bankruptcy filing, indicating growing analyst pessimism about the stock. Similarly, the median recommendation deteriorates in quality as it increases from 2 to 3. The percentage of recommendations that are buys or strong buys also declines, falling from 66% to only 20%. These results suggest that analysts react to the financial circumstances of our sample firms and adjust their recommendations accordingly.

We present an expanded set of analyst recommendations in Panel B, consisting of both observed and inferred recommendations. Our results are similar to those obtained in Panel A that use only observed recommendations. Both the mean and median analyst recommendation declines from a buy to a recommendation between a hold and an underperform over the nine quarters of our analysis. The decrease in the percentage of buy recommendations is virtually identical to that observed for observed recommendations.

The combined findings of Panels A and B in Table 2 suggest that analysts are capable of discerning and responding with revisions of their recommendations to negative developments regarding a firm's financial performance in advance of an actual bankruptcy filing. Further, this conclusion is robust to the inclusion of inferred recommendations.

Using the combined set of observed and inferred recommendations, Panel C presents another analysis of the overall trend in analyst recommendations for the sample firms benchmarked against their matched firms. We observe significant differences in the mean (median) recommendations and the percentage of buys

TABLE 2

Quarterly Trend in Analysts' Observed and Inferred Recommendations

Relative Quarter	No. of Recommend.	No. of Firms	Mean Analyst Recommend.	Median Analyst Recommend.	Percentage Buys
Panel A. Observed Recommendations for Sample Firms					
0	103	53	3.22	3	0.20
−1	258	114	2.90	3	0.23
−2	289	138	2.61	3	0.38
−3	328	156	2.52	3	0.41
−4	386	172	2.33	2	0.55
−5	449	205	2.20	2	0.61
−6	409	191	2.08	2	0.66
−7	447	210	2.03	2	0.66
−8	437	203	2.06	2	0.66
Panel B. Recommendations Including Inferred Recommendations for Sample Firms					
0	1,332	341	3.36	3	0.21
−1	1,459	352	3.04	3	0.28
−2	1,514	360	2.75	3	0.38
−3	1,504	361	2.56	3	0.46
−4	1,446	355	2.37	2	0.54
−5	1,305	342	2.22	2	0.60
−6	1,061	314	2.11	2	0.63
−7	798	278	2.08	2	0.64
−8	437	203	2.06	2	0.66

Relative Quarter	Recommend.	Sample Firms	Matched Firms	Statistical Significance
Panel C. Analysts' Recommendations Including Inferred Recommendations for Sample and Matched Firms				
0	Mean	3.37	2.44	**
	Median	3.00	2.00	**
	Percentage Buy	0.21	0.53	**
−1	Mean	3.04	2.39	**
	Median	3.00	2.00	**
	Percentage Buy	0.28	0.54	**
−2	Mean	2.76	2.33	**
	Median	3.00	2.00	**
	Percentage Buy	0.38	0.55	**
−3	Mean	2.56	2.22	**
	Median	3.00	2.00	**
	Percentage Buy	0.46	0.60	**
−4	Mean	2.34	2.10	**
	Median	2.00	2.00	**
	Percentage Buy	0.55	0.66	**
−5	Mean	2.21	2.06	**
	Median	2.00	2.00	**
	Percentage Buy	0.60	0.68	**
−6	Mean	2.09	2.05	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.65	0.68	Not Significant
−7	Mean	2.05	2.02	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.66	0.70	Not Significant
−8	Mean	2.02	1.96	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.68	0.73	Not Significant

The recommendations are those made or inferred during each of the eight fiscal quarters preceding the quarter of the bankruptcy filing. Recommendations are coded as 1 (strong buy), 2 (buy), 3 (hold), 4 (underperform), and 5 (sell). We calculate analyst recommendations using only observed recommendations (Panel A) as well as a combination of observed and inferred recommendations (Panel B). We infer an underperform (4) for analysts who drop coverage if the last recommendation is either a strong buy (1) or a buy (2). Otherwise, the recommendation is inferred to be a sell (5). If no recommendation is made during a quarter without dropping coverage, we assume that the previous recommendation applies. In Panel C, we report analysts' recommendations including inferred recommendations for sample and matched firms. The matched firms are selected on the basis of a two-digit SIC industry code and Altman's z-score two years preceding the bankruptcy of the corresponding sample firm. The matched firm is required to have at least one recommendation during the next year and has the closest z-score to that of the sample firm within the same two-digit SIC industry code. The percentage of buys represents the percentage of all recommendations that are coded as either a 1 or a 2. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

between the sample and matched firms. Panel C shows that analysts are more aggressive in downgrading their recommendations for the sample firms relative to the matched firms as they approach the quarter of bankruptcy filing. The findings of Panel C further confirm that analysts are responsive to the financial deterioration of the sample firms and manage their recommendations accordingly. We obtain quantitatively similar results using only the observed recommendations.

B. Market Reaction to Changes in Analyst Recommendations

In this section, we examine the extent to which the market reacts to changes in the recommendations made by analysts for the sample firms. If analysts have superior information about a firm, then changes in their recommendations should provoke a market response. If, however, the market recognizes that analysts have a positive bias toward distressed firms, then its response to a recommendation upgrade will be insignificant. We measure the abnormal return to a recommendation change using market-adjusted returns over the three-day window from recommendation release date, day −1, to recommendation release date, day +1.

Table 3 presents our findings. We observe that the market generally ignores upgrades for the sample firms, especially when they occur within a year of the bankruptcy filing. Upgrades occurring at quarters further from the filing such as quarters −4, −6, and −7 are met with positive excess returns, suggesting that the market views these changes as credible and perhaps suggestive of future performance improvements. The market appears to ignore reiterations, with the excess returns surrounding reiterations statistically insignificant for seven of the nine quarters of our study period.[10] Our findings are most dramatic for downgrades, with excess returns significantly negative for all of the sample quarters. The average three-day abnormal return across the eight quarters preceding bankruptcy for these downgraded firms is −14.8%.

We obtain similar results for the matched firms. The trends in the returns to these firms are comparable to those of the sample firms, that is, we find a significant negative response to downgrades, no meaningful reaction to reiterations in most quarters, and a positive market response for most upgrades occurring prior to quarter −1.

We find with our comparison of market returns between the sample and matched firms that the market reacts more significantly to downgrades for our sample firms than for matched firms. There is also some evidence that the market response to reiterations is also more negative for the sample firms. The differences in upgrades between the sample and matched firms are generally insignificant. We conclude that the market reacts more negatively to downgrades of the sample firms while the responses to other changes are more similar between the two groups.

[10] We further examine the market reaction to reiterations by reviewing the IBES database to identify the actual wording used by the brokerage house to describe a recommendation. In 31 cases, recommendations are classified as reiterations even though the text of the actual recommendation indicates a change has been made. For instance, a recommendation change from "perform in line" to "neutral" is classified as a reiteration since both recommendations are coded by IBES as a "hold." Even after eliminating these cases, our results remain qualitatively similar.

TABLE 3

Market Reaction to Announcements of Observed Recommendation Changes

	Downgrades			Reiterate			Upgrades		
Quarter Relative to Bankruptcy	Abnormal Returns (sample)	Abnormal Returns (match)	Statistic for Differences	Abnormal Returns (sample)	Abnormal Returns (match)	Statistic for Differences	Abnormal Returns (sample)	Abnormal Returns (match)	Statistic for Differences
0	−0.254**	−0.050*	−4.83**	−0.072	0.000	−1.25	−0.019	0.053	−0.60
	[−0.195]**	[−0.031]**	[−4.04]**	[−0.186]	[0.002]	[−0.36]	[−0.121]	[0.020]	[−1.41]
	<71>	<43>		<11>	<25>		<10>	<38>	
−1	−0.226**	−0.077**	−5.20**	−0.085*	0.045	−3.09**	0.082	0.016	1.34
	[−0.176]**	[−0.023]**	[−5.25]**	[−0.048]	[0.023]*	[−3.02]**	[0.017]	[0.000]	[0.66]
	<171>	<78>		<32>	<39>		<31>	<61>	
−2	−0.161**	−0.115**	−1.99*	−0.037	0.000	−1.22	−0.010	0.040**	−1.09
	[−0.126]**	[−0.067]**	[−2.03]*	[−0.023]	[−0.008]	[−0.32]	[−0.019]	[0.028]**	[−2.13]*
	<168>	<148>		<44>	<45>		<23>	<75>	
−3	−0.146**	−0.078**	−2.85*	−0.071**	−0.018	−2.74**	0.044	0.057**	−0.39
	[−0.109]**	[−0.038]**	[−4.26]**	[−0.066]**	[−0.024]*	[−2.77]**	[0.023]	[0.026]	[−0.12]
	<160>	<128>		<37>	<53>		<41>	<68>	
−4	−0.131**	−0.096**	−1.76	−0.023	0.002	−0.96	0.050**	0.028**	0.83
	[−0.088]**	[−0.050]*	[−1.88]	[−0.034]	[−0.002]	[−2.24]*	[−0.002]	[0.015]	[−1.08]
	<187>	<97>		<46>	<63>		<50>	<80>	
−5	−0.101**	−0.087**	−0.60	−0.004	0.005	−0.47	−0.008	0.026*	−1.51
	[−0.064]**	[−0.037]**	[−1.47]	[−0.004]	[−0.020]	[−1.13]	[−0.004]	[0.015]	[−1.22]
	<163>	<94>		<55>	<42>		<55>	<79>	
−6	−0.080**	−0.071**	−0.48	0.004	0.014	−0.66	0.031*	0.007	1.56
	[−0.046]**	[−0.045]**	[−0.13]	[0.002]	[0.005]	[−0.62]	[0.022]	[−0.006]	[1.17]
	<141>	<107>		<53>	<41>		<69>	<73>	
−7	−0.132**	−0.085**	−2.15*	0.017	−0.011	0.58	0.067**	0.020	2.30*
	[−0.075]**	[−0.053]**	[−1.59]	[−0.014]	[0.000]	[−1.46]	[0.035]**	[0.019]	[0.84]
	<138>	<83>		<39>	<55>		<83>	<61>	
−8	−0.099**	−0.062**	−1.59	−0.014	0.002	−0.82	0.023	0.030*	−0.34
	[−0.055]*	[−0.030]*	[−1.50]	[0.001]	[0.006]	[−0.42]	[0.004]	[0.010]*	[−0.24]
	<120>	<57>		<46>	<42>		<80>	<67>	

Abnormal returns are three-day cumulative abnormal returns computed from market-adjusted returns. Downgrades (upgrades) are any recommendations for an issue that is numerically higher (lower) than that observed for the preceding quarter. Reiterated recommendations are those that are numerically equal to the previous quarter's recommendation. The matched firms are selected on the basis of a two-digit SIC code and Altman's z-score two years preceding the bankruptcy of the corresponding sample firm. The matched firm is required to have at least one recommendation for the following year and has the closest z-score to that of a sample firm among firms with the same two-digit SIC industry code as the sample firm. The medians are reported in the square brackets. The number of observations is presented in the angle brackets. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

In untabulated findings, we also examine the impact of recommendation changes on trading volume beginning eight quarters prior to the quarter of bankruptcy filing. Using the methodology of Campbell and Wasley (1996) to measure abnormal volume, we find a substantial increase in trading volume on days when a recommendation is released. For recommendation upgrades, log transformed abnormal trading volume averages 0.77% (t-statistic of 11.0) for the three-day window surrounding the recommendation release date. For recommendation downgrades, log transformed abnormal trading volume averages 1.49% (t-statistic of 24.4) over the same interval. These results suggest that both types of recommendation changes can influence trading volume, but the magnitude of the effect due to a downgrade is nearly twice as large as that of an upgrade.[11]

[11]For the Nasdaq-listed firms in our sample, we also examine closing bid-ask spreads over the same period. We estimate OLS regressions of the quoted half spread divided by price on the natural log of trading volume, the closing price, and three dummy variables that indicate whether an upgrade, downgrade, or reiteration is issued on a given trading day. We find no change in the bid-ask spread on days when an upgrade or reiteration is released and a significant increase in the bid-ask spread

C. The Impact of Reputation: All-Star Analysts

It is widely recognized that there is significant variation in the ability of analysts, and the competition to hire and retain top-rated analysts is intense. Indeed, Clarke, Dunbar, and Kahle (2002) report that investment banks acquiring an all-star analyst experience an increase in their IPO market share of 1.25%. Because of the market value associated with their reputation, it might be that highly regarded analysts are less prone to exhibit bias in their recommendations, especially for failing firms. In this section, we examine whether top-rated analysts demonstrate a pattern different from that of other analysts in their recommendations.

Consistent with Dunbar (2000), Krigman, Shaw, and Womack (2001), and Clarke, Dunbar, and Kahle (2002), we define an all-star analyst as one who is named to *Institutional Investor*'s All-America Research Team the year the recommendation is released. Leone and Wu (2002) find that *Institutional Investor* All-Americans have better earnings forecast accuracy, superior stock recommendation returns, and less bias than other analysts. Leone and Wu also report that ranked analysts are bolder than others in the sense that they deviate more often from the consensus forecast. They conclude that ranked analysts possess an innate superior ability that is not solely attributable to experience and are more likely to be promoted to larger brokerage houses.

Table 4 compares the time series of recommendations by all-star analysts with other analysts (i.e., those analysts not selected as all-stars). The mean recommendation for both sets of analysts monotonically increases over the sample period, indicating a consistent decline in the investment attractiveness of these issues. The greater pessimism reflected in the increasing value of the mean recommendation, however, is consistently higher for those selected as all-stars. About half of the differences between the quarterly recommendations of the two groups of analysts are statistically significant. The percentage of buy recommendations demonstrates a similar pattern, but two-thirds of the differences are statistically significant. The median values also show that all-star analysts provide less favorable recommendations than other analysts. Indeed, the differences are statistically significant for seven of the nine quarters that we examine. Similarly, the median recommendation increases from 2 to 3 one quarter earlier for the all-star analysts, indicating an earlier downgrade by these analysts. The differences in medians between these groups are also significant for seven of the eight quarters preceding bankruptcy. The percentage of buy recommendations demonstrates a similar pattern, but two-thirds of the quarterly differences are statistically significant. These findings suggest that all-star analysts tend to move away from a buy recommendation for firms approaching bankruptcy both earlier and more forcefully than other analysts.

The results in Table 4 indicate that there are some modest differences in the recommendations provided by all-star analysts relative to other analysts. Although both sets of analysts revise their recommendations as a firm approaches bankruptcy, it appears that the all-stars do it more extensively than others. Both sets of analysts recognize the deterioration of the firm's investment potential, but

on days when a downgrade is released. These results further suggest that the information content of recommendation downgrades is larger than for other types of recommendation changes.

TABLE 4

The Impact of Analyst All-Star Status on Recommendations

Relative Quarter	Recommendations	All-Star	Non-All-Star	Statistical Significance
0	Mean	3.43	3.34	Not Significant
	Median	3.00	3.00	Not Significant
	Percentage Buy	0.15	0.23	**
−1	Mean	3.19	3.00	**
	Median	3.00	3.00	**
	Percentage Buy	0.21	0.30	**
−2	Mean	2.86	2.73	Not Significant
	Median	3.00	3.00	*
	Percentage Buy	0.33	0.40	*
−3	Mean	2.74	2.52	**
	Median	3.00	3.00	**
	Percentage Buy	0.38	0.48	**
−4	Mean	2.52	2.33	**
	Median	3.00	2.00	**
	Percentage Buy	0.46	0.56	**
−5	Mean	2.31	2.20	Not Significant
	Median	2.00	2.00	*
	Percentage Buy	0.56	0.61	Not Significant
−6	Mean	2.24	2.08	*
	Median	2.00	2.00	**
	Percentage Buy	0.57	0.65	*
−7	Mean	2.20	2.04	*
	Median	2.00	2.00	*
	Percentage Buy	0.58	0.66	Not Significant
−8	Mean	2.17	2.03	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.61	0.67	Not Significant

The recommendations are based on forecasts made or inferred during each fiscal quarter preceding the quarter of the bankruptcy filing. Recommendations are coded as 1 (strong buy), 2 (buy), 3 (hold), 4 (underperform), and 5 (sell). We infer an underperform (4) for analysts who drop coverage if the last recommendation is either a strong buy (1) or a buy (2). Otherwise, the recommendation is inferred to be a sell (5). If no recommendation is made during a quarter without dropping coverage, we infer that the previous recommendation applies. All-star analysts are those listed on the annual *Institutional Investor* All-America Research Team. The percentage of buys represents the percentage of all recommendations that are coded as either a 1 or a 2. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

the all-star analysts issue a lower percentage of buy recommendations for these firms. We conclude that all-star analysts have a greater ability to recognize failing firms and are more aggressive in revising their recommendations than other analysts.

D. Firm and Accounting Characteristics

In this section, we examine whether certain firm and accounting characteristics result in the generation of higher mean (median) recommendations. We first determine whether analysts are able to discriminate between those firms in financial distress possessing the potential for a return to strong financial performance and those lacking it. We then investigate whether analysts respond to the signals that might be contained in a qualified auditor opinion or a change in the firm's auditor or investment banker.

1. Firm Performance

Table 5 dichotomizes our sample of firms based on whether they experience positive or negative abnormal returns over the one-year period following bankruptcy. These one-year returns proxy for the firm's potential to recover from bankruptcy and return to profitability. If analysts provide unbiased recommendations for sample firms, then we should observe more positive recommendations for those firms that earn positive abnormal returns following bankruptcy filing. Conversely, we anticipate that analysts will issue less favorable recommendations for firms reporting negative post-bankruptcy abnormal returns.

TABLE 5

The Influence of Post-Bankruptcy Performance on Analyst Recommendations

Relative Quarter	Recommendations	Positive Abnormal Returns	Negative Abnormal Returns	Statistical Significance
0	Mean	3.00	3.43	**
	Median	3.00	3.00	**
	Percentage Buy	0.28	0.19	*
−1	Mean	2.78	3.12	**
	Median	3.00	3.00	**
	Percentage Buy	0.34	0.24	**
−2	Mean	2.54	2.80	**
	Median	3.00	3.00	*
	Percentage Buy	0.42	0.35	Not Significant
−3	Mean	2.36	2.64	**
	Median	3.00	3.00	*
	Percentage Buy	0.50	0.41	*
−4	Mean	2.25	2.44	*
	Median	2.00	3.00	Not Significant
	Percentage Buy	0.56	0.50	Not Significant
−5	Mean	2.11	2.28	*
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.64	0.56	Not Significant
−6	Mean	2.03	2.15	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.66	0.59	Not Significant
−7	Mean	1.94	2.12	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.68	0.60	Not Significant
−8	Mean	2.07	2.07	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.62	0.64	Not Significant

The recommendations are based on forecasts made or inferred during each fiscal quarter preceding the quarter of the bankruptcy filing. Recommendations are coded as 1 (strong buy), 2 (buy), 3 (hold), 4 (underperform), and 5 (sell). We infer an underperform (4) for analysts who drop coverage if the last recommendation is either a strong buy (1) or a buy (2). Otherwise, the recommendation is inferred to be a sell (5). If no recommendation is made during a quarter without dropping coverage, we assume that the previous recommendation applies. Abnormal returns are computed either over a one-year period or till a stock is delisted following bankruptcy, whichever comes first. Abnormal returns are calculated from the market-adjusted returns. The percentage of buys represents the percentage of all recommendations that are coded as either a 1 or a 2. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

We find that the mean recommendation is consistently more optimistic for firms with positive abnormal returns, although the difference between these firms and those that experience negative abnormal returns is statistically significant for only the last several quarters of our sample period. We obtain similar results for the median recommendation and the percentage of buy recommendations. We conclude from our analysis that in the year before bankruptcy, analysts appear able to discriminate between firms likely to perform well following a bankruptcy filing and those that will not: This result is inconsistent with a positive recommendation bias by analysts for the sample firms.

2. Accounting Information

Analyst recommendations are based on earnings projections that, in turn, are derived from accounting data. The importance of truthful accounting data has assumed renewed importance following the Enron scandal. In this section, we examine the influence of auditor opinion and auditor choice on analyst recommendations.

An auditor's opinion is the section of an audit that establishes the credibility of the firm's financial statements. To the extent that a qualified auditor's opinion implies that the firm's financial condition is uncertain, analysts might be less willing to recommend such stocks. Hence, we compare analyst recommendations between firms with qualified and unqualified opinions. We find in untabulated results that during the eight quarters preceding the quarter of bankruptcy filing, the average recommendation for firms with qualified opinions is not generally different from those with unqualified opinions.

Chow and Rice (1982), Craswell (1988), and Citron and Taffler (1992) suggest that managers will change auditors to avoid the release of unfavorable information to investors. Consequently, analysts following firms reporting an auditor change might tend to issue less favorable recommendations than those covering firms without an auditor change. Based on an analysis of both the level of analyst following and the percentage of buy recommendations, we find that a change in auditor fails to influence analyst perceptions regarding the investment attractiveness of a firm's equity.

3. Changes in Investment Banks

As a firm's performance deteriorates, its securities become less attractive to investors and consequently more difficult to distribute. Thus, high-prestige investment banking firms might be less interested in retaining the firm as a client. We test for such a possibility by identifying any change in investment bankers among our sample firms within a three-year period prior to the quarter of the recommendation. We find that there are generally no significant differences between the average recommendation or the percentage of buys for subsamples constructed on the basis of a change in investment bankers.

E. A Logit Analysis of Analyst Recommendations

In this section, we compare the nature of analyst recommendations between the sample and matched firms in a multivariate framework that allows us to pool the recommendations for these firms while controlling for various analyst, investment bank, and firm characteristics. Specifically, in Table 6 we present the results from a logistic regression where the dependent variable assumes a value of 1 if the recommendation is either a strong buy or a buy and is 0 otherwise. We estimate two regression models. The first model does not control for analyst affiliation while the second model contains a dummy variable that represents analyst affiliation based on whether the brokerage house and the firm have done a deal within three years. Other independent variables relating to the analyst are dummy variables that capture an analyst's all-star status and employment by a high-prestige investment banking firm. We also include dummy variables to reflect the presence of a qualified auditor's opinion and changes in the choice of auditor. The firm's potential for reorganization is captured with its Altman z-score. We also include as independent variables a dummy variable incorporating the nature of the previous recommendation, the one-month cumulative abnormal return prior to the release of the recommendation, a dummy variable to capture the firm's status as either a sample or matched firm, a set of dummy variables to control for the quarter in which the forecast is issued, and a dummy variable to capture the recommendation date relative to that of the firm's earnings announcement date.[12,13]

The results for the first model, which does not control for analyst affiliation, show that analysts begin to react to firm financial deterioration as much as a year in advance of the actual bankruptcy filing. We also observe a significantly negative coefficient for the sample firm dummy variable, indicating that these firms have lower recommendations than their matched firms. These findings suggest the ability of analysts to recognize the negative developments occurring within these firms and to revise their recommendations accordingly. The results for the second model, which include a control for analyst affiliation, are discussed in Section IV.B where we examine the issue of analyst conflict of interest in detail.

IV. Are Affiliated Analysts Subject to Conflicts of Interest?

A. The Impact of Investment Banking Affiliation

The flashpoint for the controversy regarding analyst recommendations has been the perceived linkage between the favorableness of a recommendation and the potential for subsequent investment banking business. Underwriting a firm's security offerings and providing related services can generate more revenue for firms than from brokerage or securities research. Hence, recent public interest has focused on analyst impartiality concerning recommendations for securities issued by firms that maintain other business affiliations with the brokerage company.

We initially define an affiliated analyst as one who has issued a recommendation for a client for which the analyst's firm undertook a transaction within three years of the recommendation.[14] Transactions for this purpose are bond

[12]Ivkovic and Jegadeesh (2004) find that recommendation revisions released in the week after an earnings announcement are significantly less informative than those released during other periods. Using their methodology, we include in our analysis a dummy variable, EAD, that assumes a value of 1 if a recommendation is released the week after the earnings announcement date and is 0 otherwise.

[13]We also estimate two other specifications of this model to allow for robustness testing. In the first robustness specification, we interact the sample dummy variable with each of the quarter dummy variables. Our results are consistent with the results from our earlier univariate analysis that analysts are more aggressive in downgrading their recommendations for sample firms relative to matched firms. In the second robustness specification, we separately and simultaneously interact the sample, analyst affiliation, and all-star dummy variables with the quarter dummy variables. We continue to find that analysts are more aggressive in downgrading their recommendations for the sample firms, but the results for affiliation and all-star suggest no difference in analyst recommendations between the sample and matched firms.

[14]We also consider other methods for determining an affiliated analyst such as the number of deals completed and the size of deals completed with the firm. The results are qualitatively similar. We also define affiliated analysts using only deals completed during a five-year window prior to bankruptcy, with qualitatively identical results. Finally, we consider an analyst to be affiliated based on any deal

TABLE 6

Logit Model Analysis for Analyst Recommendations

Variable	Model 1	Model 2
Intercept	0.9745 (0.1632)**	0.9734 (0.1632)**
ABRET	1.0418 (0.1674)**	1.0462 (0.1675)**
AFFIF		0.1728 (0.1301)
ALLSTAR	0.0698 (0.0938)	0.0522 (0.0948)
AUDIT	−0.0108 (0.1169)	−0.0158 (0.1170)
EAD	−0.0389 (0.0908)	−0.0397 (0.0908)
IBRANK	−0.2206 (0.0817)**	−0.2307 (0.0820)**
OPIN	0.0962 (0.0855)	0.0977 (0.0855)
PREBUY	−0.0569 (0.0838)	−0.0647 (0.0841)
SAMPLE	−0.5747 (0.0786)**	−0.5812 (0.0788)**
ZSCORE	0.2486 (0.0764)**	0.2525 (0.0765)**
Q0	−0.8303 (0.2077)**	−0.8294 (0.2077)**
Q1	−0.9132 (0.1642)**	−0.9130 (0.1642)**
Q2	−0.8867 (0.1558)**	−0.8846 (0.1558)**
Q3	−0.8974 (0.1542)**	−0.8959 (0.1542)**
Q4	−0.5258 (0.1518)**	−0.5230 (0.1518)**
Q5	−0.2563 (0.1499)	−0.2590 (0.1499)
Q6	−0.2090 (0.1522)	−0.2100 (0.1523)
Q7	−0.2479 (0.1528)	−0.2467 (0.1528)
N	3,368	3,368
Likelihood Ratio	237.5**	239.3**

The dependent variable in the logit regression is a dummy variable that assumes a value of unity if the recommendation is either 1 (strong buy) or 2 (buy) and zero otherwise. ABRET denotes the one-month cumulative abnormal return prior to the release of the recommendation. Abnormal returns are calculated from the market-adjusted returns. The AFFIF dummy takes the value of unity if the analyst has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the issue of the recommendation and zero otherwise. The ALLSTAR dummy assumes a value of unity if the analyst is included on the annual Institutional Investor All-America Research Team and zero otherwise. The AUDIT dummy takes the value of unity if there was any change in the auditor for the firm during the two years prior to the quarter of bankruptcy and zero otherwise. The EAD dummy assumes a value of unity if a recommendation is issued for the period between the next and fifth trading day after the earnings announcement and zero otherwise. The IBRANK dummy takes the value of unity if the analyst's firm is a high-prestige investment bank and zero otherwise. The OPIN dummy takes the value of unity if the most recent auditor's opinion is unqualified and zero otherwise. The PREBUY dummy takes the value of unity if the previous recommendation is coded either 1 (strong buy) or 2 (buy) and zero otherwise. The SAMPLE dummy takes the value of unity for the sample firm and zero otherwise. The ZSCORE dummy takes the value of unity if the firm is above the median Altman (1968) z-score for our sample and zero otherwise. The Q0 (Q1, Q2, Q3, Q4, Q5, Q6, or Q7) dummy variable takes the value of unity if the recommendation is made in the bankruptcy quarter (1, 2, 3, 4, 5, 6, or 7 quarter(s) before the bankruptcy) and zero otherwise. The associated standard deviations are reported within parentheses. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

or equity offerings (seasoned equity offerings or an initial equity offering) or a merger/acquisition. We include cases where the brokerage house assisted either the target or the purchaser in the merger and acquisition deals.

Table 7 presents a comparison of average recommendations between affiliated and unaffiliated analysts. These two groups experience similar declines in mean recommendations throughout the pre-bankruptcy period. For both sets of analysts, the mean recommendation falls from a buy to a recommendation between a hold and underperform. The differences in means between these groups are generally statistically insignificant, suggesting a similarity in the pattern of recommendations for affiliated and unaffiliated analysts. The median recommendations behave in a similar fashion, further confirming that affiliated analysts do not generally provide biased recommendations. We also find that the percentage of recommendations classified as buys is similar between the two groups, although there is a tendency for that of the affiliated analysts to be slightly higher. The results in Table 7 suggest that analysts are responsive to changes in an issuer's financial circumstances regardless of their previous or current investment banking relationships with the firm.

To further investigate the potential of bias among affiliated analysts, we consider alternative definitions of affiliation. We first separate our sample of affiliated analysts into those affiliated due to a capital formation transaction and those with affiliations resulting from an M&A deal. Neither is there a difference in the pattern of recommendations between these two types of affiliated analysts, nor is there any significant difference in recommendations between M&A-affiliated analysts and unaffiliated analysts or between capital formation-affiliated and unaffiliated analysts.

We then decompose our sample of firms into two subsamples based on whether they use one or multiple investment bankers to complete a transaction. In the case of a single investment banker, the affiliated analyst is the only analyst participating in the transaction and faces considerable reputation risk resulting from the pressure applied by the investment banker. With multiple investment bankers, the affiliated analyst is simply one of a number of participating analysts and consequently bears less reputation risk.

Similar to our results regarding the type of investment banking transaction, there are no significant differences between our subsamples. That is, we observe no significant differences in the pattern of recommendations by affiliated analysts whether the firm uses one or a number of investment bankers. Likewise, there are no significant differences in the average recommendations between the affiliated and unaffiliated analysts within these groups.

These robustness tests confirm our initial conclusion that affiliated analysts are no more likely than unaffiliated analysts to issue positive recommendations for firms that become bankrupt. The tests might further suggest that the conflict of interest attributed to affiliated analysts is overstated.

done within either one or two years of a recommendation. Our findings again remain virtually identical.

TABLE 7

Impact of Analyst Affiliation on Recommendations

Relative Quarter	Recommendations	Affiliated	Unaffiliated	Statistical Significance
0	Mean	3.28	3.37	Not Significant
	Median	3.00	3.00	Not Significant
	Percentage Buy	0.27	0.20	Not Significant
−1	Mean	2.99	3.05	Not Significant
	Median	3.00	3.00	Not Significant
	Percentage Buy	0.34	0.27	Not Significant
−2	Mean	2.59	2.78	*
	Median	2.00	3.00	**
	Percentage Buy	0.52	0.36	**
−3	Mean	2.42	2.58	Not Significant
	Median	2.00	3.00	**
	Percentage Buy	0.55	0.45	**
−4	Mean	2.24	2.39	Not Significant
	Median	2.00	2.00	*
	Percentage Buy	0.62	0.53	*
−5	Mean	2.05	2.25	*
	Median	2.00	2.00	*
	Percentage Buy	0.70	0.59	**
−6	Mean	2.00	2.13	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.72	0.62	*
−7	Mean	2.11	2.08	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.66	0.64	Not Significant
−8	Mean	2.00	2.07	Not Significant
	Median	2.00	2.00	Not Significant
	Percentage Buy	0.68	0.66	Not Significant

The recommendations are based on forecasts made or inferred during each fiscal quarter preceding the quarter of the bankruptcy filing. Recommendations are coded as 1 (strong buy), 2 (buy), 3 (hold), 4 (underperform), and 5 (sell). We infer an underperform (4) for analysts who drop coverage if the last recommendation is either a strong buy (1) or a buy (2). Otherwise, the recommendation is inferred to be a sell (5). If no recommendation is made during a quarter without dropping coverage, we assume that the previous recommendation applies. An affiliated analyst is defined as one who has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the recommendation. The percentage of buys represents the percentage of all recommendations that are coded as either a 1 or a 2. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

B. Multivariate Analysis of Analyst Recommendations

In the second model contained in Table 6, we present a multivariate analysis of analyst recommendations that controls for analyst affiliation. Consistent with our definition of an affiliated analyst used in Section IV.A, we construct a dummy variable that assumes a value of 1 if the analyst provides a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the issue of the recommendation and is 0 otherwise. The estimate for the affiliation dummy variable is statistically insignificant, indicating that there is no difference in the recommendations between affiliated and unaffiliated analysts. The results from this multivariate examination confirm the analysis contained in Table 7 that analyst affiliation exerts no consistent significant influence on the recommendations issued for the sample firms.

C. Market Reaction to Recommendation Changes

In Table 8, we examine whether affiliated analysts suffer from a conflict of interest by comparing the market's reaction to their recommendation changes with those of unaffiliated analysts. Our results show that this difference is almost uniformly insignificant across the downgrade, upgrade, and reiteration subsamples. In untabulated findings, we further find that the percentage of downgrade recommendations does not significantly differ between affiliated and unaffiliated analysts.

TABLE 8

Comparison of Abnormal Returns between Affiliated and Unaffiliated Analysts' Recommendation Changes

	Downgrade	Reiteration	Upgrade
Relative Quarter	Mean Difference [Median Difference]	Mean Difference [Median Difference]	Mean Difference [Median Difference]
0	−0.80 [0.04]	−1.42 [−1.36]	−0.24 [1.00]
−1	−1.23 [−0.45]	−0.86 [−0.60]	—
−2	−1.17 [−0.95]	−0.37 [0.00]	−0.59 [0.69]
−3	−0.56 [−0.24]	0.76 [0.64]	−0.64 [−1.36]
−4	0.06 [−0.64]	0.76 [1.43]	0.74 [0.59]
−5	0.90 [0.51]	−0.97 [0.06]	−0.81 [−1.32]
−6	−0.58 [−0.50]	2.23* [1.07]	1.31 [0.40]
−7	−1.29 [−0.24]	−1.05 [−1.99]	−0.15 [−0.70]
−8	−1.23 [−1.07]	−0.11 [1.10]	1.13 [2.04]*

Abnormal returns are three-day cumulative abnormal returns calculated from the market-adjusted returns. Downgrades (upgrades) are any recommendations for an issue that are numerically higher (lower) than that observed for the preceding quarter. Reiterated recommendations are those that are numerically equal to the previous quarter's recommendation. An affiliated analyst is defined as one who has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the recommendation. The t-statistic for the mean difference is reported. The z-statistic for the median difference is reported in the square brackets. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

The combined results of Tables 7 and 8 suggest that a prior relationship with a client firm does not meaningfully impact the kind of recommendation an affiliated analyst will issue. This result appears robust to a number of alternative definitions of affiliation. Additionally, the market does not react differently to recommendation changes by affiliated analysts, suggesting that the market does not view the opinions of affiliated analysts as compromised.

D. A Logit Analysis of Changes in Recommendation

In this section, we extend our comparison of affiliated and unaffiliated analysts by examining the extent to which analyst affiliation influences changes in recommendations while simultaneously controlling for various analyst, investment bank, and firm characteristics.

To begin our examination of recommendation changes, we classify all recommendations as upgrades, downgrades, or reiterations by comparing the current recommendation to the most recent previous recommendation. We then estimate three separate logistic models. In model 1, the dependent variable assumes a value of 1 if a recommendation is an upgrade and is 0 otherwise. In model 2, the dependent variable is assigned a value of 1 if the recommendation is a downgrade and is 0 otherwise. In model 3, we examine all recommendations by estimating an ordered logit regression. The dependent variable in this regression assumes one of three different values: 1 for an upgrade recommendation, 0 for a reiteration, and -1 for a downgrade.

These three different models for our logit analysis allow us to focus separately on downgrade recommendations, upgrade recommendations, and the set of all recommendations. For each logit model, we use the same independent variables described in Table 6.

For each model contained in Table 9, the affiliation dummy is statistically insignificant. Thus, even after controlling for a number of other possible factors, we fail to find evidence that a previous relationship with a firm influences an analyst's change in recommendation for that stock. These results confirm those contained in Tables 6, 7, and 8 that affiliated analysts appear to suffer no conflict of interest resulting from their employer's earlier association with the firm they are recommending.

We find other interesting relations in our regression results as well. There is a strong relation between the abnormal stock price performance during the month prior to the release of the recommendation, ABRET, and the probability of a recommendation change. Stronger stock price performance increases the probability of an upgrade. When the abnormal returns change from one standard deviation below the mean (-36.5%) to one standard deviation above the mean (17.0%), the probability of an upgrade increases by 4.2%. There is a strong relation between the previous recommendation and the likelihood of a recommendation change. A previous strong buy or buy recommendation increases the likelihood of a recommendation downgrade. There is no evidence that affiliated analysts are more likely to upgrade their recommendation. We find, however, some evidence that high-reputation investment banks are less likely to issue upgrades around bankruptcy. We also find that changes in either direction are more likely to occur in the week following an earnings announcement than at other times.

The probability of an upgrade is positively related to the Altman z-score. This suggests that the likelihood of a ratings upgrade is higher for firms with higher z-scores and consequently greater potential for a successful reorganization. There is no evidence, however, that the probability of a recommendation change is related to either the quality of an auditor's opinion or a change in the auditor's identity.

E. The Influence of Analyst Affiliation on Returns and Volume

In this section, we examine whether an analyst's affiliated status influences the nature of the market's response to a change in recommendation while controlling for a variety of other factors. More specifically, we separately examine market returns and trading volume surrounding changes in analyst recommendations. In Table 10, the three-day cumulative abnormal return obtained from the

TABLE 9

Logit Model Analysis for Changes in Analyst Recommendations

Variable	Model 1	Model 2	Model 3
Intercept 1	0.6795	−1.9405	0.7333
	(0.2687)*	(0.2603)**	(0.2235)**
Intercept 0			1.8667
			(0.2297)**
ABRET	0.5827	−1.1839	0.8917
	(0.2837)*	(0.2548)**	(0.2276)**
AFFIF	−0.0483	0.0209	−0.0923
	(0.2676)	(0.2128)	(0.2021)
ALLSTAR	−0.1154	0.2708	−0.1411
	(0.2174)	(0.1826)	(0.1682)
AUDIT	−0.3775	0.1176	−0.1686
	(0.2475)	(0.1938)	(0.1820)
EAD	0.2031	−0.0072	0.0158
	(0.1934)	(0.1665)	(0.1541)
IBRANK	−0.2964	−0.1310	−0.0680
	(0.1795)	(0.1480)	(0.1376)
LIQUID	0.1779	0.0318	0.0689
	(0.1763)	(0.1512)	(0.1390)
OPIN	−0.0957	−0.0242	−0.0708
	(0.1702)	(0.1454)	(0.1332)
PREBUY	−2.2526	2.4298	−2.2847
	(0.1647)**	(0.1673)**	(0.1411)**
ZSCORE	0.6241	−0.2660	0.4066
	(0.1616)**	(0.1347)*	(0.1245)**
Q0	−1.6387	1.6814	−1.5430
	(0.6360)**	(0.5474)**	(0.4950)**
Q1	−1.6519	1.2623	−1.4497
	(0.3818)**	(0.3231)**	(0.2967)**
Q2	−2.0116	1.0127	−1.2777
	(0.4050)**	(0.2966)**	(0.2755)**
Q3	−1.0663	0.8853	−0.9651
	(0.3213)**	(0.2736)**	(0.2506)**
Q4	−1.0275	0.8649	−0.9351
	(0.2998)**	(0.2558)**	(0.2348)**
Q5	−0.6346	0.4336	−0.5342
	(0.2727)*	(0.2362)	(0.2166)*
Q6	−0.2244	0.2541	−0.2403
	(0.2646)	(0.2400)	(0.2171)
Q7	0.1320	0.1810	−0.1136
	(0.2568)	(0.2372)	(0.2141)
N	1,339	1,339	1,339
Likelihood Ratio	295.0**	352.9**	388.9**

In model 1(2), the dependent variable in the logit regression is a dummy variable that assumes a value of unity if the recommendation is an upgrade (downgrade) and zero otherwise. In model 3, the dependent variable in the ordered logit regression assumes three values: 1 for an upgrade, 0 for a reiteration, and −1 for a downgrade. Downgrades (upgrades) are any recommendations for an issue that are numerically higher (lower) than that observed for the preceding quarter. Reiterated recommendations are those that are numerically equal to the previous quarter's recommendation. ABRET denotes the one-month cumulative abnormal return prior to the release of the recommendation. Abnormal returns are calculated from the market-adjusted returns. The AFFIF dummy takes the value of unity if the analyst has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the issue of the recommendation and zero otherwise. The ALLSTAR dummy assumes a value of unity if the analyst is included on the annual *Institutional Investor* All-America Research Team and zero otherwise. The AUDIT dummy takes the value of unity if there was any change in the auditor for the firm during the two years prior to the quarter of bankruptcy and zero otherwise. The EAD dummy assumes a value of unity if a recommendation is issued for the period between the next and fifth trading day after the earnings announcement and zero otherwise. The IBRANK dummy takes the value of unity if the analyst's firm is a high-prestige investment bank and zero otherwise. The LIQUID dummy takes the value of unity if the firm is liquidated and zero otherwise. The OPIN dummy takes the value of unity if the most recent auditor's opinion is unqualified and zero otherwise. The PREBUY dummy takes the value of unity if the previous recommendation is coded either 1 (strong buy) or 2 (buy) and zero otherwise. The ZSCORE dummy takes the value of unity if the firm is above the median Altman (1968) z-score for our sample and zero otherwise. The Q0 (Q1, Q2, Q3, Q4, Q5, Q6, or Q7) dummy variable takes the value of unity if the recommendation is made in the bankruptcy quarter (1, 2, 3, 4, 5, 6, or 7 quarter(s) before the bankruptcy) and zero otherwise. The associated standard deviations are reported within parentheses. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

market-adjusted returns serves as the dependent variable. The independent variables are the same as those used in the logit analysis of Table 9.

The affiliation dummy variable is statistically insignificant for all three categories of recommendation changes. This suggests that the market ignores the affiliation status of an analyst in responding to news of a recommendation change. The market likewise ignores the all-star status of an analyst, reacting equivalently to recommendation changes by all-star and other analysts.

We also examine the influence of affiliation status on abnormal volume surrounding a recommendation change while controlling for the same set of independent variables used with the return analysis. We find in untabulated results that the analyst's affiliation coefficient is statistically insignificant for downgrades, reiterations, and upgrades.

The return results presented in Table 10 and our untabulated volume results indicate that the affiliation status of the analyst providing a recommendation change exerts no influence in shaping the market's response. If these affiliated analysts were subject to conflicts of interest that might compromise their evaluation of these firms, we would expect the market to discount their recommendations. The general failure to obtain significant coefficients for our measures of analyst affiliation for either returns or trading volume provides strong evidence that affiliated analysts do not suffer from conflicts of interest sufficient to compromise their recommendations.

There are other significant findings regarding downgrades as well in Table 10. We observe that IBRANK is significant for downgrades, indicating that downgrades by analysts associated with a prestigious investment bank generate a more negative price reaction. Similarly, the market responds more negatively to downgrades if the firm has an unqualified auditor's opinion. This suggests that the incremental information content provided by analysts through their recommendation downgrades for firms with unqualified opinions is valuable.

F. Price Reversals and Recommendation Changes by Affiliated Analysts

Finally, we test if there are long-term negative abnormal returns following an upgrade recommendation issued by an affiliated analyst. Table 11 measures abnormal returns over trading days 2 through 40 following a change in analyst recommendation.[15] The coefficient for analyst affiliation is statistically insignificant for the subsample of upgrades. This result indicates that there is not a pattern of price reversals following recommendation upgrades by affiliated analysts. This implies that the upgrade recommendations of affiliated analysts are not viewed as excessively optimistic since the market does not react negatively to such upgrades during the post-recommendation revision period.

[15] We also examine other periods such as day two through day 50, 55, and 60 and obtain similar results.

TABLE 10

Multivariate Analysis of Abnormal Returns Surrounding a Change in Analyst Recommendations

Variable	Downgrade	Reiteration	Upgrade
Intercept	−0.081	−0.056	0.001
	(−2.28)* .	(−1.36)	(0.03)
ABRET	−0.003	0.002	−0.003
	(−0.11)	(0.05)	(−0.07)
AFFIF	−0.028	−0.019	0.020
	(−1.22)	(−0.54)	(0.57)
ALLSTAR	0.009	0.010	0.047
	(0.47)	(0.36)	(1.62)
AUDIT	0.006	−0.036	−0.061
	(0.27)	(−1.08)	(−1.85)
EAD	−0.006	−0.080	−0.027
	(−0.32)	(−2.77)**	(−1.07)
IBRANK	−0.047	0.011	−0.021
	(−2.83)**	(0.48)	(−0.91)
LIQUID	0.002	−0.024	0.042
	(0.09)	(−0.89)	(1.86)
OPIN	−0.049	0.003	0.017
	(−3.06)**	(0.12)	(0.78)
PREBUY	−0.007	0.050	0.015
	(−0.25)	(2.11)*	(0.72)
ZSCORE	0.059	0.012	−0.005
	(3.89)**	(0.55)	(−0.24)
Q0	−0.080	0.091	0.042
	(−1.57)	(0.91)	(0.47)
Q1	−0.102	0.001	0.083
	(−3.00)**	(0.02)	(1.62)
Q2	−0.032	0.037	−0.029
	(−0.97)	(0.87)	(−0.50)
Q3	−0.054	−0.014	0.003
	(−1.75)	(−0.31)	(0.08)
Q4	−0.031	0.017	0.036
	(−1.03)	(0.41)	(0.93)
Q5	0.003	0.014	−0.029
	(0.09)	(0.38)	(−0.84)
Q6	0.042	0.040	0.020
	(1.37)	(1.00)	(0.62)
Q7	−0.031	0.074	0.059
	(−1.02)	(1.78)	(1.95)
N	811	239	289
Adjusted R^2	0.046	0.024	0.012
F	3.18**	1.32	1.20

The dependent variable in each regression is the three-day cumulative abnormal return calculated from market-adjusted returns. Downgrades (upgrades) are any recommendations for an issue that are numerically higher (lower) than that observed for the preceding quarter. Reiterated recommendations are those that are numerically equal to the previous quarter's recommendation. ABRET denotes the one-month cumulative abnormal return prior to the release of the recommendation. Abnormal returns are calculated from the market-adjusted returns. The AFFIF dummy takes the value of unity if the analyst has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three-year period prior to the issue of the recommendation and zero otherwise. The ALLSTAR dummy takes the value of unity if the analyst is included on the annual *Institutional Investor* All-America Research Team and zero otherwise. The AUDIT dummy takes the value of unity if there was any change in the firm's auditor from two years prior to bankruptcy and zero otherwise. The EAD dummy takes the value of unity if a recommendation was issued for the period between the next and fifth trading day after the earnings announcement and zero otherwise. The IBRANK dummy takes the value of unity if the analyst's firm is a high-prestige investment bank and zero otherwise. The LIQUID dummy takes the value of unity if the firm is liquidated and zero otherwise. The OPIN dummy takes the value of unity if the most recent auditor's opinion is unqualified and zero otherwise. The PREBUY dummy takes the value of unity if the previous recommendation is coded either 1 (strong buy) or 2 (buy) and zero otherwise. The ZSCORE dummy takes the value of unity if the firm is above the median Altman (1968) z-score for our sample and zero otherwise. The Q0 (Q1, Q2, Q3, Q4, Q5, Q6, or Q7) dummy variable takes the value of unity if the recommendation is made in the bankruptcy quarter (1, 2, 3, 4, 5, 6, or 7 quarter(s) before the bankruptcy) and zero otherwise. The associated t-values are reported within parentheses. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

TABLE 11

Multivariate Analysis of Returns Reversal after a Change in Analyst Recommendations

Variable	Downgrade	Reiteration	Upgrade
Intercept	−0.068	−0.073	−0.105
	(−1.15)	(−0.77)	(−1.39)
ABRET	−0.077	−0.013	−0.135
	(−1.56)	(−0.16)	(−1.54)
ABRET_3DAY	−0.044	−0.073	0.296
	(−0.73)	(−0.51)	(2.26)*
AFFIF	0.025	0.041	−0.118
	(0.64)	(0.55)	(−1.58)
ALLSTAR	−0.017	−0.033	−0.053
	(−0.50)	(−0.54)	(−0.80)
AUDIT	0.029	−0.013	−0.035
	(0.83)	(−0.17)	(−0.48)
EAD	0.023	0.061	0.061
	(0.70)	(0.91)	(1.16)
IBRANK	0.022	0.030	0.024
	(0.79)	(0.56)	(0.47)
LIQUID	−0.03	−0.013	0.000
	(−1.06)	(−0.22)	(0.01)
OPIN	−0.012	−0.024	−0.049
	(−0.43)	(−0.44)	(−1.02)
PRE_BUY	−0.023	−0.019	−0.048
	(−0.52)	(−0.34)	(−1.10)
ZSCORE	0.029	0.016	0.010
	(1.11)	(0.33)	(2.27)*
Q0	0.019	−0.659	−0.586
	(0.24)	(−1.90)	(−2.96)**
Q1	−0.349	−0.097	−0.029
	(−6.00)**	(−0.84)	(−0.26)
Q2	−0.191	−0.206	−0.022
	(−3.44)**	(−2.15)*	(−0.16)
Q3	−0.121	−0.220	−0.214
	(−2.34)*	(−2.17)*	(−2.21)*
Q4	−0.118	0.012	−0.086
	(−2.34)*	(0.13)	(−1.02)
Q5	−0.065	0.014	−0.012
	(−1.31)	(0.17)	(−0.16)
Q6	−0.040	0.025	−0.041
	(−0.77)	(0.28)	(−0.60)
Q7	−0.001	−0.020	0.168
	(−0.03)	(−0.22)	(2.58)*
N	732	204	237
Adjusted R^2	0.057	0.006	0.11
F	3.33**	1.06	2.54**

The dependent variable in each regression is calculated from the market-adjusted returns from two through 40 trading days following a change in recommendation. Downgrades (upgrades) are any recommendations for an issue that are numerically higher (lower) than that observed for the preceding quarter. Reiterated recommendations are those that are numerically equal to the previous quarter's recommendation. ABRET denotes the one-month cumulative abnormal return prior to the release of the recommendation. Abnormal returns are calculated from the market-adjusted returns. ABRET_3DAY is the three-day cumulative abnormal return around the change date in analyst recommendations. The AFFIF dummy takes the value of unity if the analyst has provided a recommendation on an issuer for which the analyst's firm undertook a transaction during the three years prior to the issue of the recommendation and zero otherwise. The ALLSTAR dummy takes the value of unity if the analyst is included on the annual *Institutional Investor* All-America Research Team and zero otherwise. The AUDIT dummy takes the value of unity if there was any change in the firm's auditor from two years prior to bankruptcy and zero otherwise. The EAD dummy takes the value of unity if a recommendation was issued for the period between the next and fifth trading day after the earnings announcement and zero otherwise. The IBRANK dummy takes the value of unity if the analyst's firm is a high-prestige investment bank and zero otherwise. The LIQUID dummy takes the value of unity if the firm is liquidated and zero otherwise. The OPIN dummy takes the value of unity if the most recent auditor's opinion is unqualified and zero otherwise. The PREBUY dummy takes the value of unity if the previous recommendation is coded either 1 (strong buy) or 2 (buy) and zero otherwise. The ZSCORE dummy takes the value of unity if the firm is above the median Altman (1968) z-score for our sample and zero otherwise. The regressions include Q0 (Q1, Q2, Q3, Q4, Q5, Q6, or Q7) dummy that takes the value of unity if the recommendation was made in the bankruptcy quarter (1, 2, 3, 4, 5, 6, or 7 quarter(s) before the bankruptcy) and zero otherwise. The associated *t*-values are reported within parentheses. Statistical significance at the 1% and 5% levels is indicated by ** and *, respectively.

We also observe in Table 11 that both the z-score and the announcement period abnormal return are positively related to the market-adjusted returns from two through 40 trading days after a recommendation upgrade. These results indicate that firms with a lower level of financial distress (i.e., higher z-score) or firms for which the market responded favorably to the initial recommendation change generate higher long-run returns.

V. Conclusions

Recent public controversy about overoptimistic stock recommendations by analysts suffering from various conflicts of interest has resulted in a wave of regulatory and legislative changes. These regulations and laws will impact the way that analysts perform their duties and how investment banks relate to their research departments. In this study, we examine whether such optimism actually exists in analyst recommendations or is the product of media hype. We undertake this analysis on a sample of firms that file for bankruptcy between 1995 and 2001.

We first examine whether there is a bias in the recommendations issued by analysts covering the sample firms. We compare their recommendations against those provided for a set of matched firms. The mean recommendation for our sample firms is remarkably responsive to the distressed circumstances of these firms; it declines from a buy approximately two years prior to bankruptcy to midway between a hold and an underperform during the actual quarter of bankruptcy. The recommendations for our sample firms monotonically decline while those of the matched firms remain fairly constant. Our multivariate analysis offers further confirmation of this trend. We additionally find that the market does not generally differentiate in its response to recommendation changes for either the sample or matched firms, suggesting that in the aggregate, analysts correctly revise their recommendations. We do find, however, that all-star analysts are more pessimistic than other analysts in their recommendations for our sample firms.

The second issue examined in this study is the extent to which affiliated analysts might suffer from a conflict of interest that would result in overoptimistic recommendations. After considering several measures of affiliation, we fail to find consistent and convincing evidence that such analysts are compromised. Indeed, the preponderance of our findings suggests the opposite. Neither is there a difference in the average recommendation between affiliated and non-affiliated analysts in the eight quarters preceding the quarter of bankruptcy filing, nor is there any difference in the market response to changes in their recommendations. Our multivariate analysis of the market reaction to changes in analyst recommendations generally indicates that prior affiliation has no impact. Also we do not find that the market views recommendation upgrades by affiliated analysts as biased since there is no pattern of price reversal following such recommendation changes.

In summary, our findings indicate that analysts actively revise their recommendations downward as bankruptcy approaches. We do not find evidence that analysts are biased in their recommendations for our sample firms. There is no evidence that affiliated analysts suffer from a conflict of interest that affects the objectiveness of their recommendations. Our findings suggest that the recently passed regulations and laws to reduce analyst conflict might be an overreaction by regulatory authorities.

References

Aharony, J.; C. Jones; and I. Swary. "An Analysis of Risk and Return Characteristics of Corporate Bankruptcy Using Capital Market Data." *Journal of Finance*, 35 (1980), 1001–1016.

Altman, E. I. "Financial Ratios, Discriminant Analysis and the Prediction of Corporate Bankruptcy." *Journal of Finance*, 23 (1968), 589–609.

Altman, E. I. "Ratio Analysis and the Prediction of Firm Failure: A Reply." *Journal of Finance*, 25 (1970), 1169–1172.

Altman, E. I. "A Further Empirical Investigation of the Bankruptcy Cost Question." *Journal of Finance*, 39 (1984), 1067–1089.

Boni, L., and K. L. Womack. "Analysts, Industries, and Price Momentum." *Journal of Financial and Quantitative Analysis,* 41 (2006), 85–109.

Bradley, D.; J. Clarke; and J. Cooney. "The Changing Incentives of Financial Analysts from the Early 90s to the Bubble." Working Paper, Clemson Univ. (2005).

Brown, P.; G. Foster; and E. Noreen. "Security Analyst Multi-Year Earnings Forecasts and the Capital Market." In *Studies in Accounting Research*, American Accounting Association (1985).

Campbell, C., and C. Wasley. "Measuring Abnormal Daily Trading Volume for Samples of NYSE/ASE and NASDAQ Securities Using Parametric and Nonparametric Test Statistics." *Review of Quantitative Finance and Accounting*, 6 (1996), 309–326.

Cheng, Y.; M. H. Liu; and J. Qian. "Buy-Side Analysts, Sell-Side Analysts, and Investment Decisions of Money Managers." *Journal of Financial and Quantitative Analysis,* 41 (2006), 51–83.

Chow, C., and S. Rice. "Qualified Audit Opinions and Auditor Switching." *Accounting Review*, 57 (1982), 326–335.

Citron, D., and R. Taffler. "The Auditor Report Undergoing Concern Uncertainty: An Empirical Analysis." *Accounting and Business Research* (1992), 337–345.

Clark, T., and M. Weinstein. "The Behavior of the Common Stock of Bankrupt Firms." *Journal of Finance*, 38 (1983), 489–504.

Clarke, J.; C. Dunbar; and K. Kahle. "All Star Analyst Turnover, Investment Bank Market Share, and the Performance of Initial Public Offerings." Working Paper, Univ. of Western Ontario (2002).

Conrad, J.; B. Cornell; W. R. Landsman; and B. R. Rountree. "How Do Analyst Recommendations Respond to Major News?" *Journal of Financial and Quantitative Analysis,* 41 (2006), 25–49.

Craswell, A. "The Association between Qualified Opinions and Auditor Switches." *Accounting and Business Research*, 19 (1988), 23–31.

Dechow, P.; A. Hutton; and R. Sloan. "The Relation between Analysts' Forecasts of Long-Term Earnings Growth and Stock Price Performance Following Equity Offerings." Working Paper, Univ. of Michigan (1998).

Dugar, A., and S. Nathan. "The Effect of Investment Banking Relationships on Financial Analysts Earnings Forecasts and Investment Recommendations." *Contemporary Accounting Research*, 12 (1995), 131–160.

Dunbar, C. "Factors Affecting Investment Bank Initial Public Offering Market Share." *Journal of Financial Economics*, 55 (2000), 3–41.

Espahbodi, R.; Dugar, A.; and H. Tehranian. "Further Evidence on Optimism and Underreaction in Analysts' Forecasts." *Review of Financial Economics*, 10 (2001), 1–21.

Fama, E., and K. French. "Testing Trade-Off and Pecking Order Predictions about Dividends and Debt." *Review of Financial Studies*, 15 (2002), 1–33.

Ferris, S., and R. Lawless. "The Expenses of Financial Distress: The Direct Costs of Chapter 11." *University of Pittsburgh Law Review*, 61 (200), 629–669.

Green, T. C. "The Value of Client Access to Analyst Recommendations." *Journal of Financial and Quantitative Analysis,* 41 (2006), 1–24.

Holmström, B., and S. Kaplan. "The State of U.S. Corporate Governance: What's Right and What's Wrong?" Working Paper, European Corporate Governance Institute (2003).

Houston, J.; C. James; and J. Karceski. "What a Difference a Month Makes: Stock Analyst Valuations Following Initial Public Offerings." *Journal of Financial and Quantitative Analysis,* 41 (2006), 111–137.

Hubbard, J., and K. Stephenson. "A Fool and His Money: Buying Bankrupt Stocks." *Journal of Investing*, 6 (1997), 56–60.

Ivkovic, Z., and N. Jegadeesh. "The Timing and Value of Forecast and Recommendation Revisions." *Journal of Financial Economics*, 93 (2004), 433–463.

Kolasinski, A., and S. Kothari. "Investment Banking and Analyst Objectivity: Evidence from Forecasts and Recommendations of Analysts Affiliated with M&A Advisors." Working Paper, MIT (2003).

Krigman, L.; W. Shaw; and K. Womack. "Why Do Firms Switch Underwriters?" *Journal of Financial Economics*, 60 (2001), 245–284.

Leone, A., and J. Wu. "What Does it Take to Become a Superstar? Evidence from Institutional Investor Rankings of Financial Analysts." Working Paper, Univ. of Rochester (2002).

Lin, H., and M. F. McNichols. "Underwriting Relationships, Analysts' Earnings Forecasts and Investment Recommendations." *Journal of Accounting and Economics*, 25 (1998), 101–127.

Michaely, R., and K. Womack. "Conflict of Interest and the Credibility of Underwriter Analyst Recommendations." *Review of Financial Studies*, 12 (1999), 653–686.

Morse, D., and W. Shaw. "Investing in Bankrupt Firms." *Journal of Finance*, 43 (1988), 1193–1206.

Moses, O. "On Bankruptcy Indicators from Analysts' Earnings Forecasts." *Journal of Accounting, Auditing, and Finance*, 15 (1990), 379–409.

Stickel, S. "Predicting Individual Analyst Earnings Forecasts." *Journal of Accounting Research*, 28 (1990), 409–417.

Warner, J. "Bankruptcy Costs: Some Evidence." *Journal of Finance*, 32 (1977), 337–347.

Weiss, L. "Bankruptcy Resolution: Direct Costs and Violation of Priority of Claims." *Journal of Financial Economics*, 27 (1990), 285–314.

Westerfield, R. "Assessment of Bankruptcy Risk." Working Paper, R. L. White Center for Financial Research (1971).

Womack, K. "Do Brokerage Analysts' Recommendations Have Investment Value?" *Journal of Finance*, 51 (1996), 137–167.

An Evaluation of Security Analysts' Forecasts
Author(s): Timothy Crichfield, Thomas Dyckman, Josef Lakonishok
Source: *The Accounting Review*, Vol. 53, No. 3 (Jul., 1978), pp. 651-668
Published by: American Accounting Association
Stable URL: http://www.jstor.org/stable/245673
Accessed: 05/08/2010 09:51

THE ACCOUNTING REVIEW
Vol. LIII, No. 3
July 1978

An Evaluation of Security Analysts' Forecasts

Timothy Crichfield, Thomas Dyckman, and Josef Lakonishok

ABSTRACT: Recent literature in accounting, finance, and economics often assumes that information can be processed efficiently. Among the outputs of the processing activity are the presumably appropriate assessments of the underlying probability distributions for all important variables, and a good deal of the recent research assumes that observable realizations of the variables are drawn from these distributions. This paper provides evidence concerning the ability of selected individuals, namely security analysts, to provide estimates of earnings per share after presumably processing the available information. Several aspects of the quality of analyst forecasts are examined. The study indicated, as expected, that analysts' forecasts become more accurate as the reporting date is approached. Furthermore, the predictions of changes in earnings per share data contain no significant systematic bias. However, the authors do not find sufficient support for the expected decline in forecast variability among analysts as the reporting date is approached.

THE subject of forecasting financial variables for firms has received wide attention recently, particularly since the Securities and Exchange Commission (SEC) announced in February, 1973, its intention to require that certain disclosures of forecasts be made public (see Gonedes, Dopuch, and Penman, [1976]). One aspect of these proposals was to require that if company officials report forecasts to outsiders, then these forecasts would have to be made public through filings with the SEC. Although the SEC has since altered its basic position, the widespread interest in forecast disclosure remains. As Gonedes, Dopuch, and Penman (GDP) point out, the basic arguments in the debate concerning public disclosure of managements' forecasts revolve around two issues: (1) the extent to which required forecasts embody information useful for establishing equilibrium values for firms, and (2) the extent to which the proposed requirements are consistent with an optimal allocation of resources for society. GDP provide an empirical analysis of the first issue and some theoretical arguments pertaining to the second issue.

One factor which may influence the information content as well as the desirability—from a resource allocation perspective—of managements' forecasts is that security analysts also provide forecasts of company variables. If security analysts provide this service more efficiently, one could question the desirability of requiring company officials to provide forecasts. Of course, comparing

The authors wish to acknowledge the helpful comments of Kenneth J. Boudreaux and Larry L. Lookabill.

Timothy Crichfield is Assistant Professor, University of California, Berkeley; Thomas Dyckman is Professor of Accounting, Cornell University; and Josef Lakonishok is Assistant Professor, Tel-Aviv University.

Manuscript received June, 1977.
Revision received October, 1977.
Accepted November, 1977.

the efficiency of managements' forecasts to those of security analysts is a difficult task. Moreover, any such comparison would have to consider not only the relative costs of forecasting but also the effects upon users' decision processes as different forecasting sources are considered.

While it is difficult to assess the significance of competing information alternatives upon the decisions of market agents, it is possible to judge how well any of several information sources fulfill their stated or implied purposes. For example, an implied purpose of earnings per share forecasts provided by security analysts is to yield unbiased estimates of future earnings per share which would be useful for investors in assessing firms' equilibrium values. If such forecasts are found to contain systematic biases, then a minimum criticism of the forecasts is that users make adjustments to the forecasts that would be unnecessary in the absence of the bias.

Our study is an attempt to assess the significance of any bias in the forecasts of earnings per share by security analysts. We are concerned with the performance of security analysts over a relatively long period of time. This differs from most published studies of forecast accuracy (for example, Barefield and Comiskey, [1975]) which deal with relatively few points in time. However, by requiring extensive time series observations, we encounter data-gathering problems that did not plague other researchers. These data problems are discussed subsequently.

Forecasts of Earnings Per Share

Forecasting is one useful means for estimating the values of important variables under uncertainty. A forecast, or prediction, is simply a statement about an unknown event or events. Typically, as is true in our case, they are future events. The forecast is useful if it influences the decision makers' estimates of the parameters of the relevant probability distribution.[1]

In the present study, we are concerned with security analyst (SA) predictions of earnings per share (EPS) figures for major corporations. The SAs have no direct control over the eventual realization of the prediction and, hence, following Theil [1966], we might call these predictions anticipations.[2] The predictions made are single-valued point estimates of each firm's EPS for the current fiscal year. These estimates are based on primary accounting earnings before extraordinary items and, where necessary, these EPS figures have been adjusted for stock splits and dividends. The assumption is that SAs attempt to predict a normalized figure free from the impact of non-recurring factors and unaffected by company distributions. Cragg and Malkiel [1968, p. 68] offer supportive evidence for this assumption. We will evaluate the accuracy of these forecasts as compared with predictions from alternative statistical models.

We will consider also whether point-estimate forecasts of EPS by SAs lead to efficient parameter estimates for the underlying probability distribution when considered together with the existing set of information available to the market.

[1] The notion of usefulness here ignores the cost of the forecast. While it is simple enough to state that the forecast's cost should be less than the benefit obtained, this is not easily done. The difficulties arise not only because of measurement problems, but also because it is not easy to establish who bears the costs. Further, the costs and benefits may fall selectively across individuals creating the problem of measuring the impact of wealth transfers.

[2] The anticipations of SAs may reflect the predictions by a firm's managers. Furthermore, there may be an attempt by managers to make their own predictions come true. This could reflect on an evaluation of SAs' forecasts. Nevertheless, the lack of a direct effect still remains.

This is the second objective of a useful forecast as discussed above. We now turn to a discussion of the means by which such an evaluation can be made.

Forecasts are based on *ex ante* assessments. Recognizing the uncertainty inherent in the process, the eventual realization can be treated as an observation on a random variable. Forecasters, and in particular SAs, should not, then, be expected to predict the realizations precisely. Rather, they can be expected to predict the parameters, such as the mean, of the probability distribution governing the random variable. We would, then, expect the actual realization to differ from this mean predicted value.

This discussion implies that a relatively long time span is required to test the ability of SAs to estimate the mean of the EPS distribution. If true, studies based on a comparison of realizations with forecasts over a short time horizon are likely to be deficient. We should not expect to predict the actual observations with perfect accuracy.[3]

The discussion further implies that if we can assume the mean of the probability distribution to be stable over time, the predictions should, on average, be very close to the mean of the true probability distribution. This suggests in turn that there should not be a systematic bias in the predictions.[4] Moreover, if essentially costless information is available to the forecaster, it should already be impounded in the forecast. It should not be possible to improve on the predictions by incorporating such data as, for example, predictions based on statistical models incorporating past realization data. Our tests will reflect these ideas.

Data Base

The basic source of data for this study was selected copies of the *Earnings Forecaster* (*EF*), published by Standard and Poor's. Our data cover forecasts for the period from 1967, when the *EF* was first published, to 1976. The same publication also provides actual EPS data.[5]

The *EF* is published bi-weekly and contains annual EPS forecasts for several hundred companies. Over 50 different investment firms are responsible for these forecasts. There may be from one to ten or more forecasts for a single firm in each issue.

Due to the nature of the available data, the firms used in this study could not be selected in a truly random fashion. Instead, we were constrained to select several consecutive pages at two different starting points in the last issue of the *EF* for each month from January, 1967, through May, 1976. Thus, we obtained data for 113 consecutive months. Firms for which forecasts did not appear in every year of the *EF* were deleted from the sample. But a firm was not deleted if data were missing only for some months in a given year; hence, missing data points were a problem for some firms. We will discuss this problem in more detail subsequently.

The final sample consisted of 46 firms. Where more than one forecast was pre-

[3] See Basi, Carey, and Twark [1976] for an example. Furthermore, at any point in time, forecasts for all companies may be cross-sectionally correlated due to aggregate market events. Thus, there may be a tendency for all forecasts to be either optimistic or pessimistic.

[4] Theil [1966, p. 14], based on certain macro economic data, states that "generally speaking, forecasters tend to be between the limits of naive no-change extrapolators and perfect predictors in the sense that they underestimate changes more frequently than they overestimate them." Studies involving earnings forecasts have not been consistent with this statement by Theil. McDonald [1973, p. 509] and Barefield and Comiskey [1975, p. 244] both observed "a persistent optimistic bias." (Since, during the periods covered in these two studies, earnings and EPS tended to increase, the result is an overestimation of the change.)

[5] Actual EPS data for some firms in 1976 were obtained from *The Wall Street Journal* and Annual Reports since they were not included in copies of the *Earnings Forecaster* available to us at the time of the analysis.

sented in a single month for a given firm, the mean forecast was used. This was necessary due to the complexity of attempting to track particular analysts over long time periods. Thus, we are examining the forecasts of analysts as a group. We also calculated the standard deviation of the forecasts among analysts in each month.

The analysis for each firm in each year used 13 months of predictions rather than 12. This was done because forecasts are made in the month following the end of the firm's fiscal year but before the actual EPS figure is released. For example, a firm with a fiscal year ending June 30, 1971, would have forecast data for that same year from July, 1970, through July, 1971, inclusive.[6] In total, but subject to missing observations, we have 13 monthly predictions on each firm for each of 10 years; a total of 130 predictions for each firm.[7]

Because the firm selection process was not random, it is possible that some selection bias exists for at least two reasons. First, there may be an industry bias created by industry clustering in the alphabetical listing used by the *EF*. Table 1 provides a distribution of the 46 sample firms by industry. We also know that most firms have December 31 fiscal years. Sixty-eight percent of our sample firms also have December 31 fiscal years. Although we performed out analyses separately for calendar year firms and non-calendar-year firms, there were no pronounced differences in the separate analyses, and only the analyses for all firms regardless of fiscal year are provided here.

Second, there is likely to be some sample bias due to the limited coverage of firms by companies providing forecast data. This bias is toward a greater coverage of large and somewhat older firms that have had forecast data reported for the ten years used in this study. For this reason, any conclusions obtained from this research apply, strictly, only to those firms covered by the *EF*. Extrapolation to larger populations should be made with care.

TABLE 1
INDUSTRY CLASSIFICATION
(2 Digit SIC Code)

Industry	*Number of Companies*
Mining	
Metal Mining	1
Oil and Gas Extraction	1
Manufacturing	
Food and Kindred Products	1
Textile Mill Products	1
Apparel and Other Fabrics	1
Furniture and Fixtures	1
Paper and Allied Products	1
Chemicals and Allied Products	6
Stone, Clay, Glass, and Concrete Products	4
Primary Metal	6
Machinery, Except Electrical	5
Instruments: Measuring, Photographic, Optical Medical, Watches and Clocks	1
Transportation, Communication, and Other Public Utilities	
Transportation	1
Electric, Gas, and Sanitation	7
Retail Trade	
General Merchandise Stores	3
Food Stores	2
Apparel and Accessories	1
Finance, Insurance, and Real Estate	
Holding and Other Investment Companies	2
Services	
Business Services	1
Total	46

[6] Occasionally, the forecast data occur before July, 1970, and after July, 1971.

[7] If a firm changed fiscal years, all observations before the change were treated as missing observations.

THE ANALYSIS

Following Theil's approach, we use the mean-square prediction error to evaluate the goodness of any forecast.[8] Summing over all sample firms for a given point in time yields:

$$\frac{1}{n}\sum_{j=1}^{n}(P_j^* - A_j^*)^2 \tag{1}$$

where P_j^* is the predicted level of EPS for firm j; and A_j^* is the actual level of EPS for firm j.

If these prediction errors (*i.e.*, $P_j^* - A_j^*$) can be considered random variables, then the results from (1) can be used to formulate probability statements concerning predictions. Standard statistical tools invariably require that successive elements in any summation be independent. This assumption, however, is unrealistic if the forecast errors are measured in terms of levels of EPS. As the level of EPS increases in absolute magnitude, we should expect analysts' forecast errors likewise to increase in absolute magnitude. In a cross-sectional sense, performance measures which evaluate differences between the levels of forecasted EPS and the levels of actual EPS would be biased against firms with high absolute levels of EPS and biased in favor of firms with low absolute levels of EPS. This would make empirical results based upon such measures difficult to interpret.

For these reasons, we chose to work in terms of percentage changes in EPS. In order to avoid asymmetry problems, percentage changes are measured as log relatives of EPS (*e.g.*, using log relatives, a change in EPS from $2.10 to $2.00 is the negative of the change in EPS from $2.00 to $2.10).

Specifically, we define:

$$A_t \equiv ln\,(A_t^* \div A_{t-1}^*) \tag{2a}$$

$$P_{it} \equiv ln\,(P_{it}^* \div A_{t-1}^*) \tag{2b}$$

$$P_{itk} \equiv ln\,(P_{itk}^* \div A_{t-1}^*) \tag{2c}$$

where:

- A_t is the actual log relative EPS from year $t-1$ to year t;
- P_{it} is the analysts' prediction of the log relative EPS from year $t-1$ to year t for the prediction made in month i, $i = 1, 2, \cdots, 13$;
- P_{itk} is P_{it} for the kth statistical forecast model (to be specified in the next subsection);
- A_t^* is the actual EPS in year t;
- P_{it}^* is the mean of the analyst predictions of EPS for year t for the predictions made in month i; and
- P_{itk}^* is the prediction of EPS for year t using model k where the prediction is made in month i.

The quality of the analysts' forecasts can be evaluated using Theil's [1966] U^2 statistic given in the following form:

$$U_{itk}^2 = \sum_{j=1}^{n}(P_{jit} - A_{jt})^2 \div \sum_{j=1}^{n}(P_{jitk} - A_{jt})^2 \tag{3}$$

where:

- U_{itk}^2 is computed using cross-sectional data for $j = 1, \cdots, n$ firms for every month i in year t (for which forecasts were made) with model k as a standard. If the average of the analysts' predictions for each firm in month i were to be exactly realized, then $(P_{jit} - A_{jt})$ will be zero for all firms and so will U_{itk}^2. Increasing values of U_{itk}^2 indicate increasingly poor forecasting ability.

[8] Use of the mean square error implies that the loss from an inaccurate forecast is symmetrical and that the effect is captured by the square of the error.

Comparison Models

Analysts' forecasts ought to be compared with a standard, namely with how well forecasts could be made using simple statistical models not based on the expertise of the forecaster. We have selected the following five simple statistical models for this comparison:

1. $k=1$: The naive forecast model: Last year's EPS for firm j will be repeated. $P^*_{it1}=A^*_{t-1}$ for all i. (We note that for model $k=1$, $P_{it1}=0$, for all i and t.)
2. $k=2$: A 3-year moving average: This year's EPS for firm j will equal the average EPS over the last 3 years

$$P^*_{it2}=\frac{1}{3}\left[\sum_{m=1}^{3}A^*_{t-m}\right]$$

for all i.

3. $k=3$: A quarterly model: Each quarterly reported EPS serves as an independent prediction of annual EPS.

$$P^*_{it3}=\begin{cases} A^*_{t-1} & i=1,2,3 \\ 4Q_{1t} & i=4,5,6 \\ 4Q_{2t} & i=7,8,9 \\ 4Q_{3t} & i=10,11,12,13 \end{cases}$$

where Q_{jt} is the EPS for the jth quarter of year t.

4. $k=4$: A quarterly model: Each quarterly reported EPS is averaged with previous quarters' EPS.

$$P^*_{it4}=\begin{cases} A^*_{t-1} & i=1,2,3 \\ 4Q_{1t} & i=4,5,6 \\ 4\left[\dfrac{Q_{1t}+Q_{2t}}{2}\right] & i=7,8,9 \\ 4\left[\dfrac{Q_{1t}+Q_{2t}+Q_{3t}}{3}\right] & i=10,11,12,13 \end{cases}$$

5. $k=5$: A quarterly model: Each quarterly reported EPS serves as a prediction of annual EPS after adjusting for the error in the previous year.

$$P^*_{it5}=\begin{cases} A^*_{t-1} & i=1,2,3 \\ 4Q_{1t}+(A^*_{t-1}-4Q_{1t-1}) & i=4,5,6 \\ 4Q_{2t}+(A^*_{t-1}-4Q_{2t-1}) & i=7,8,9 \\ 4Q_{3t}+(A^*_{t-1}-4Q_{3t-1}) & i=10,11,12,13 \end{cases}$$

The above models were chosen as standards due to their simplicity and acceptance in similar forms in the literature. For example, model $k=3$ was used by Green and Segall [1967].

The numerator, $\sum(P_{jit}-A_{jt})^2$, of Theil's U^2 is the critical component. The denominator is merely a means of facilitating interpretation of the measure. Values of U^2_{itk} greater than one indicate that, on the average, forecasts using model k are more accurate than those made by the analysts. By decomposing this numerator several useful insights are obtained. The following specific decomposition will prove most useful to our purpose.[9]

$$\sum_{j=1}^{n}(P_{jit}-A_{jt})^2=n(\bar{P}_{it}-\bar{A}_t)^2+n(s_p-rs_A)^2+n(1-r^2)s_A^2 \quad (4)$$

where:

$\bar{P}_{it}$ and $\bar{A}_t$ are the mean values of P_{it} and A_t

s_p and s_A are the standard deviations of P_{it} and A_t and

r is the correlation coefficient between the predicted and realized changes.

[9] See Theil [1958, pp. 33–35] and Granger and Newbold [1973, p. 46]. Granger and Newbold argue that Equation (4) is the more appropriate decomposition.

Note that:

$$r = \frac{\sum_{j=1}^{n} (P_{jit} - \bar{P}_{it})(A_{jt} - \bar{A}_t)}{n s_p S_A}.$$

The interpretation of the terms in (4) is based on a model of the forecaster's decision process. Suppose the forecaster regards any forecast as consisting of (1) a systematic and (2) a nonsystematic part of the realization. It would be reasonable for the forecaster to concentrate attention on the systematic portion. If the forecaster is able to predict the systematic portion exactly, then the realization, A_t, can be viewed as consisting of the systematic portion P_{it} and a random component which has mean zero and which is independent of P_{it}. In this situation a regression of the form:

$$A_t = \alpha + \beta P_{it} + e_{it} \qquad (5)$$

would show $\alpha = 0$ and $\beta = 1$. In other words, a regression of the actual change in EPS on the predicted change would detect no systematic bias.[10]

Now, since the residuals in (5) have zero mean, the mean values of A_t and P_{it} are identical and the first term on the right of the equal sign in (4) should tend to disappear as predictors do a better job of evaluating the systematic proportion.

Next it can be shown that:

$$\beta = \frac{r s_A}{s_p} \qquad (6)$$

and if, in addition, $\beta = 1$ then

$$r = \frac{s_p}{s_A} \quad \text{and} \quad r s_A = s_p.$$

Under these conditions the second term on the right-hand side of (4) also tends to vanish as predictors improve. If analysts predict EPS without systematic linear bias, then we should observe α near zero and β near one.

Even if analysts' predictions contain bias, the worth of the forecast is not necessarily destroyed. If the user can detect the bias and adjust for it, then the corrected forecasts will be just as useful as forecasts that contain no bias; however, the corrected forecasts may (though not necessarily) be obtained at higher cost than unbiased forecasts from analysts. If we assume that analysts' forecast bias is of a linear nature and constant over time, then users may use Equation (5) to obtain estimates of α and β. If the corrected forecasts $\hat{\alpha} + \hat{\beta} P_{it}$ are used as the predictions in Equation (4), then the right hand side would again reduce to $n(1 + r^2)s_A^2$.

For reporting the empirical results of our work, we divide each term on the right-hand side of (4) by the total to obtain:

$$\frac{n(\bar{P}_{it} - \bar{A}_t)^2}{\sum_{j=1}^{n} (P_{jit} - A_{jt})^2} = U^M \qquad (7a)$$

$$\frac{n(s_p - r s_A)^2}{\sum_{j=1}^{n} (P_{jit} - A_{jt})^2} = U^R \qquad (7b)$$

$$\frac{n(1 - r^2)s_A^2}{\sum_{j=1}^{n} (P_{jit} - A_{jt})^2} = U^D. \qquad (7c)$$

Hence $U^M + U^R + U^D = 1$.

It is our contention that Theil's development of a forecast evaluation technique provides superior measures to those typically found in the accounting literature.

Hypotheses

1. Analysts' forecasts of EPS in any

[10] It should be noted that our tests result from cross-sectional regressions. This was necessary in order to have enough observations for efficient parameter estimates. The interpretation of the parameters is very similar to that which would result from time series regressions.

year are more accurate as the end of that year is approached.
2. Analysts predict changes in EPS without systematic bias. In terms of equation (5), α should be close to zero and β should be close to one; furthermore U^D should be large relative to U^M and U^R.
3. The standard deviations of the forecasts among analysts for any year's EPS will decline as the end of the year is approached.

Results

Tables 2–6 give Theil's U^2 statistic for the five comparison models. In each table, the values given are $1-U^2$. Thus, unity represents a perfect forecast in these tables. The values of $1-U^2$ are given for each year from 12 months prior to one month following the end of the fiscal year. The bottom row provides an average across the ten years used in the study.

Applying the Cox-Stuart [1955] Trend Test yields a significant upward trend at the 0.016 probability level for the years 1967, 1968, 1970–1973, and 1975 in both Tables 2 and 3. The level of significance is greater for the other years. The pooled observations in the last rows of the tables are significant at the 0.001 probability level. These results are consistent with improved analyst forecast accuracy over the year.

When the statistical models incorporate quarterly EPS, however, the upward trend is less pronounced. This can be observed in Tables 4–6, particularly Table 5. In Tables 4 and 6, the upward trend in forecast accuracy is fairly significant, though the significance does not appear to be as strong as in Tables 2 and 3. These results imply that, as the end of the year approaches, the analysts' predictions become increasingly better than the predictions given by models $k=3$ and $k=5$ but do not become increasingly better than the predictions given by quarterly model $k=4$. By noting that Table 6 contains more negative values than any other table, we conclude that model $k=5$ was the most difficult of the five standards for the analysts to match. The large number of positive values in Table 2–6 provides evidence that the analysts performed well in terms of forecast accuracy when compared to the performance of the five statistical models.

One explanation for the low values of $1-U^2$ (and consequent upward trend for the year) in the early months in Tables 2 through 6 is that the statistical models used as standards assume that analysts have knowledge of the previous year's EPS in the first month of the current year. An examination of announcement dates for EPS in *The Wall Street Journal Index* revealed that less than 50 percent of our firms had announced the year's EPS by the end of the month immediately following the close of the fiscal year.[11] Nearly all firms had announced annual EPS by the second month of the subsequent year. In contrast, nearly all firms reported quarterly EPS within one month of the statement date. Thus the statistical models used for measuring analysts' forecast accuracy are somewhat biased against the analysts. In other words, that analysts do somewhat better than our tests suggest. On the other hand, we have not examined all possible alternative models. There may well be simple statistical models that do better than the ones we selected for comparison. Further, the appropriate statistical model may change over time and from firm to firm. Such ideas await further study.

[11] It is, of course, possible that for some firms in some years, the EPS data may reach the market sooner than indicated by *The Wall Street Journal Index*.

TABLE 2

THEIL'S U^2: NAIVE ANNUAL MODEL:* ($k=1$)

Year	Month													Significance Level of Trend Cox-Stuart Test
	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
1967	−0.017	0.197	0.054	0.067	0.088	0.097	0.175	0.234	0.345	0.415	0.459	0.605	0.659	0.016
1968	0.462	0.494	0.481	0.534	0.539	0.630	0.590	0.608	0.668	0.604	0.749	0.751	0.681	0.016
1969	0.575	0.581	0.583	0.473	0.483	0.504	0.512	0.558	0.663	0.659	0.666	0.719	0.778	0.109
1970	−0.292	−0.040	0.078	0.014	0.100	0.190	0.097	0.220	0.014	0.347	0.447	0.552	0.714	0.016
1971	0.219	0.055	0.113	0.119	0.243	0.320	0.451	0.698	0.795	0.799	0.827	0.871	0.902	0.016
1972	0.719	0.749	0.390	0.373	0.434	0.453	0.458	0.871	0.870	0.845	0.832	0.941	0.961	0.016
1973	0.236	0.237	0.271	0.266	0.281	0.334	0.348	0.402	0.497	0.510	0.607	0.702	0.709	0.016
1974	0.121	0.152	0.195	−0.622	−0.362	−0.298	−0.406	−0.024	0.293	0.326	0.539	0.589	0.586	0.109
1975	0.106	0.295	0.302	0.324	0.266	0.387	0.320	0.614	0.791	0.788	0.772	0.813	0.859	0.016
1976	0.759	0.756	0.212	0.138	0.176	0.174	0.284	0.319	0.397	0.592	0.795	0.806	0.890	0.344
10-Year Average	0.2888	0.3472	0.2679	0.1686	0.2248	0.2791	0.2829	0.4500	0.5333	0.5885	0.6693	0.7349	0.7739	<0.001

* Unity represents a perfect forecast. $1-U^2$ is tabulated.

TABLE 3

THEIL'S U^2: MOVING AVERAGE ANNUAL MODEL* ($k=2$)

Year	Month													Significance Level of Trend Cox-Stuart Test
	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
1967	0.290	0.045	0.211	0.173	0.207	0.222	0.266	0.352	0.428	0.504	0.550	0.729	0.762	0.016
1968	0.075	0.161	0.171	0.283	0.265	0.464	0.378	0.421	0.511	0.477	0.600	0.602	0.589	0.016
1969	0.732	0.372	0.498	0.392	0.384	0.371	0.417	0.459	0.564	0.582	0.624	0.689	0.727	0.109
1970	−0.274	0.031	0.171	0.109	0.182	0.283	0.223	0.323	0.111	0.422	0.504	0.599	0.748	0.016
1971	0.351	0.331	0.343	0.355	0.437	0.496	0.601	0.781	0.850	0.850	0.872	0.900	0.923	0.016
1972	0.559	0.610	0.340	0.326	0.337	0.356	0.347	0.770	0.843	0.813	0.803	0.895	0.934	0.016
1973	0.356	0.309	0.360	0.349	0.379	0.416	0.426	0.470	0.560	0.578	0.669	0.741	0.751	0.016
1974	0.423	0.440	0.479	−0.012	0.163	0.195	0.130	0.341	0.549	0.573	0.706	0.736	0.738	0.109
1975	0.196	0.293	0.327	0.349	0.293	0.390	0.323	0.616	0.684	0.679	0.656	0.719	0.791	0.016
1976	0.648	0.644	0.187	0.111	0.150	0.148	0.262	0.257	0.342	0.555	0.777	0.774	0.880	0.344
10-Year Average	0.3356	0.3236	0.3087	0.2435	0.2797	0.3341	0.3373	0.4790	0.5442	0.6033	0.6761	0.7384	0.7843	<0.001

* Unity represents a perfect forecast. $1-U^2$ is tabulated.

TABLE 4

THEIL'S U^2: QUARTERLY MODEL* ($k=3$)

Year	Month**										Significance Level of Trend Cox-Stuart Test
	4	5	6	7	8	9	10	11	12	13	
1967	0.533	0.611	0.580	0.354	0.395	0.481	0.783	0.815	0.861	0.880	0.016
1968	0.733	0.711	0.779	0.771	0.779	0.813	0.697	0.784	0.803	0.760	0.109
1969	0.928	0.932	0.934	0.812	0.828	0.795	0.811	0.789	0.848	0.875	0.344
1970	0.271	0.335	0.393	−0.069	−0.003	−0.399	0.546	0.583	0.660	0.732	0.109
1971	0.665	0.712	0.759	0.489	0.764	0.793	0.789	0.820	0.856	0.921	0.016
1972	0.955	0.954	0.956	0.909	0.938	0.940	0.927	0.916	0.962	0.975	0.109
1973	0.340	0.293	0.348	−0.107	0.377	0.478	0.232	0.378	0.438	0.442	0.109
1974	−0.120	0.099	0.124	0.622	0.758	0.768	0.044	0.307	0.387	0.412	0.109
1975	0.464	0.539	0.562	0.396	0.582	0.670	0.833	0.861	0.836	0.840	0.016
1976	0.489	0.511	0.510	0.738	0.728	0.759	0.217	0.607	0.548	0.788	0.109
10-Year Average	0.5258	0.5697	0.5945	0.4915	0.6146	0.6098	0.5879	0.6860	0.7199	0.7625	<0.001

* Unity represents a perfect forecast. $1-U^2$ is tabulated.
** Months 1–3 are identical to the numbers in Table 2.

TABLE 5
THEIL'S U^2: QUARTERLY MODEL* ($k=4$)

Year	Month** 7	8	9	10	11	12	13	Significance Level of Trend Cox-Stuart Test
1967	0.314	0.353	0.510	−0.499	−0.272	0.577	0.640	>0.500
1968	0.715	0.723	0.766	0.482	0.570	0.575	0.595	>0.500
1969	0.804	0.815	0.563	0.695	−0.073	0.755	0.798	>0.500
1970	−0.422	−0.321	−0.970	−2.413	−1.886	−1.347	−0.477	>0.500
1971	0.445	0.635	0.680	−0.268	−0.0045	0.151	0.351	>0.500
1972	0.930	0.949	0.951	0.831	0.801	0.912	0.941	>0.500
1973	−0.608	0.019	0.167	−0.454	−0.189	0.119	0.124	>0.500
1974	−0.606	−0.269	0.129	−0.234	0.042	0.132	0.240	>0.500
1975	0.414	0.594	0.679	0.082	0.187	0.372	0.406	>0.500
1976	0.520	0.562	0.612	0.555	0.777	0.771	0.880	0.344
10-Year Average	0.2506	0.4060	0.4087	−0.1223	−0.0088	0.3017	0.4498	>0.500

* Unity represents a perfect forecast. $1-U^2$ is tabulated.
** Months 1–3 are identical to the numbers in Table 2, and Months 4–6 are identical to the numbers in Table 4.

Table 7 provides several additional measures of the ability of analysts to forecast EPS. Column 1 gives the mean absolute deviation (MAD) of the forecasts computed as:

$$\mathrm{MAD}_{it} = \frac{1}{n}\sum_{j=1}^{n}\left|\frac{P_{jit}-A_{jt}}{A_{jt}}\right| \qquad (8)$$

where the symbols are as defined follow-lowing Equation (2). Decreasing values of MAD indicate increasing forecast accuracy. Commencing with month 6, the values of MAD decline monotonically, providing further evidence that analysts show increasing forecast accuracy with time.

Still further information on forecasters' ability is provided in columns 5 through 9 of Table 7. Cross-sectional data for each month $i=1$ to 13 are used to fit equation (5) to the predicted values. Unbiased forecasts would be reflected by α's insignificantly different from zero and β's close to one. Columns 7, 8 and 9 provide the t statistics for the null hypotheses that $\alpha=0$, $\beta=1$ and $\beta=0$ respectively.[12]

Due simply to the number of t statistics computed some are bound to be significant. However, on the average, α is not significantly different from zero ($t=\pm 1.68$ at the 0.10 probability level for a two-tail test given d.f.$=40$), although there is a tendency for α to be negative on the average. We are also not able to reject the null hypothesis that $\beta=1$. The fact that the null hypothesis

[12] Column 10 gives the degrees of freedom for the t statistics. The low value is due to the single year 1976 when observations were available only up to May.

TABLE 6

THEIL'S U^2: QUARTERLY MODEL* ($k=5$)

Year	*Month***										*Significance Level of Trend Cox-Stuart Test*
	4	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	
1967	0.409	0.505	0.494	0.069	0.122	0.286	0.780	0.813	0.858	0.877	0.016
1968	−0.192	−0.093	0.083	−0.170	−0.115	0.058	0.505	0.560	0.563	0.579	0.344
1969	0.474	0.521	0.536	0.559	0.605	0.694	0.615	0.624	0.689	0.746	0.016
1970	−0.425	−0.505	−0.366	−0.101	−0.218	−0.887	−0.184	−0.006	0.200	0.488	0.344
1971	−0.291	−0.342	−0.147	0.625	0.827	0.849	0.444	0.525	0.624	0.707	0.016
1972	0.572	0.586	0.610	0.766	0.745	0.759	0.595	0.516	0.798	0.865	0.016
1973	−0.499	−0.454	−0.350	−0.001	0.058	0.207	0.278	0.449	0.794	0.817	0.344
1974	−0.310	−0.116	−0.085	−3.095	−1.951	−0.954	0.407	0.595	0.640	0.636	0.344
1975	0.455	0.440	0.509	0.693	0.757	0.793	0.803	0.836	0.806	0.784	0.016
1976	−3.973	−3.758	−3.764	−0.873	−1.022	−0.791	−2.001	−0.506	−0.447	0.189	>0.500
10-Year Average	−0.3780	−0.3216	−0.2480	−0.1528	−0.1920	0.1014	0.2242	0.4406	0.5525	0.6688	<0.010

* Unity represents a perfect forecast. $1-U^2$ is tabulated.

** Months 1–3 are identical to the numbers in Table 2.

TABLE 7

MAD; DECOMPOSITION OF U^2; AND REGRESSION STATISTICS

Month		1 *MAD*	2 U^M	3 U^R	4 U^D	5 α	6 β	7 $t_{\alpha=0}$	8 $t_{\beta=0}$	9 $t_{\beta=0}$	10 *d.f.*
1	10-Year Ave.	1.2313	0.1307	0.0462	0.8230	−0.0739	1.0837	−0.8676	−0.471	2.9572	21.2
	Range	0.874 · 1.684	0.001 · 0.619	0.001 · 0.280	0.375 · 0.997	−0.360 · 0.076	0.584 · 1.446	−3.709 · 1.089	−2.289 · 0.832	1.617 · 6.597	2·29
2	10-Year Ave.	1.4118	0.1181	0.0536	0.8281	−0.1098	1.3560	−1.7086	0.7069	4.2710	26.6
	Range	0.795 · 3.764	0.000 · 0.414	0.000 · 0.200	0.527 · 0.992	−0.305 · 0.022	0.805 · 2.053	−4.612 · 0.236	−0.686 · 2.532	1.826 · 8.736	3·38
3	10-Year Ave.	1.3716	0.1360	0.0494	0.8145	−0.0787	1.2305	−1.6263	0.6589	3.9001	29.8
	Range	0.693 · 2.588	0.002 · 0.459	0.000 · 0.132	0.501 · 0.993	−0.318 · 0.239	−0.127 · 2.259	−4.506 · 1.331	−0.514 · 3.111	−0.098 · 7.622	4·38
4	10-Year Ave.	1.5344	0.1326	0.1075	0.7600	−0.0693	1.0395	−1.5977	0.2147	3.4035	32.8
	Range	0.707 · 2.603	0.000 · 0.427	0.000 · 0.411	0.539 · 0.966	−0.252 · 0.289	−0.762 · 2.044	−5.038 · 1.640	−5.166 · 2.867	−2.235 · 6.527	4·41
5	10-Year Ave.	1.5344	0.1390	0.0537	0.8074	−0.0779	1.0810	−1.5412	0.2116	3.5511	31.6
	Range	0.612 · 4.010	0.000 · 0.412	0.000 · 0.246	0.565 · 0.983	−0.209 · 0.201	−0.277 · 2.166	−4.508 · 0.317	−3.596 · 1.972	−0.779 · 6.607	4·40
6	10-Year Ave.	1.5394	0.1403	0.0554	0.8044	−0.0710	1.1272	−1.6255	0.5201	4.2333	33.2
	Range	0.554 · 3.203	0.000 · 0.412	0.001 · 0.196	0.578 · 0.985	−0.180 · 0.197	−0.012 · 1.753	−4.447 · 1.453	−3.188 · 2.438	−0.037 · 6.985	4·41
7	10-Year Ave.	1.3270	0.1218	0.0734	0.8046	−0.0697	1.0637	−1.5538	0.1660	4.2193	32.6
	Range	0.526 · 3.189	0.000 · 0.379	0.002 · 0.248	0.619 · 0.982	−0.178 · 0.168	−0.041 · 1.757	−3.652 · 1.326	−3.783 · 3.069	−0.150 · 7.800	4·42
8	10-Year Ave.	1.1542	0.1325	0.0919	0.7756	−0.0657	1.1805	−1.6836	1.0605	6.7416	33.6
	Range	0.581 · 2.452	0.000 · 0.449	0.001 · 0.251	0.550 · 0.906	−0.123 · 0.180	0.555 · 1.799	−3.424 · 1.764	−1.672 · 3.528	1.228 · 13.510	5·54
9	10-Year Ave.	1.0856	0.1901	0.1229	0.7682	−0.0597	1.1455	−1.6055	1.1823	8.4434	34.4
	Range	0.553 · 2.215	0.000 · 0.485	0.002 · 0.285	0.505 · 0.928	−0.138 · 0.166	0.457 · 1.699	−3.170 · 1.822	−3.196 · 3.846	1.731 · 16.878	5·42
10	10-Year Ave.	1.0199	0.1253	0.1102	0.7644	−0.0600	1.2197	−1.5370	1.4183	8.5668	33.5
	Range	0.481 · 2.162	0.006 · 0.640	0.000 · 0.364	0.284 · 0.936	−0.122 · 0.142	0.833 · 1.942	−3.071 · 2.542	−0.932 · 4.242	3.998 · 16.038	5·43
11	10-Year Ave.	0.8862	0.1057	0.0855	0.8090	−0.0575	1.1502	−1.4976	1.3079	9.2539	34.2
	Range	0.466 · 1.519	0.004 · 0.446	0.002 · 0.236	0.551 · 0.896	−0.127 · 0.094	0.853 · 1.444	−2.709 · 1.544	−0.889 · 3.431	4.074 · 15.081	5·42
12	10-Year Ave.	0.8203	0.0859	0.0601	0.8538	−0.0323	1.0980	−0.9958	0.9682	10.9169	34.2
	Range	0.540 · 1.397	0.000 · 0.427	0.000 · 0.195	0.569 · 0.961	−0.100 · 0.095	0.876 · 1.405	−2.198 · 1.308	−0.941 · 2.912	3.700 · 22.591	4·41
13	10-Year Ave.	0.7026	0.0801	0.1180	0.8019	−0.0391	1.1650	−1.1174	1.5649	13.4861	33.8
	Range	0.405 · 1,382	0.003 · 0.372	0.000 · 0.497	0.131 · 0.957	−0.078 · 0.003	0.940 · 1.599	−2.048 · 0.234	−0.499 · 4.359	7.866 · 31.398	5·43

that $\beta=0$ can be rejected ($t=\pm 2.08$ at the 0.05 probability level for a two-tail test given d.f.$=21$) indicates that, on average, analysts can predict the direction of earnings changes. These tests provide information which supports the hypothesis that analysts predict EPS changes without significant systematic bias.[13] This evidence supports the second hypothesis.

Columns 2 through 4 of Table 7 provide the decomposition of U^2 as given by equations (7a), (7b), (7c). As expected, and hypothesized, U^D constitutes a large fraction (between 76 to 85 percent) of U^2 in every year. Hence, we conclude that most of the error in the forecasters' predictions is due to factors that could not be eliminated simply by applying a linear correction to the forecasts. This is again consistent with the second hypothesis.

The third hypothesis concerns forecast variability. Specifically, we hypothesized that the variability among analysts' forecasts declines as the end of the year is approached.

In Table 8 we provide specific information on the variability of earnings forecasts among analysts in any given month. The mean standard deviation is given for each year and each month. The data are inconclusive. While there is a tendency for the variation to decline, the decline is uneven and often shows some increase in the middle months. The years 1969, 1971, 1973, and 1974 (4 of 10 years in the study) either do not show the anticipated decline or it is not significant.

The Cox-Stuart Trend Test support the hypothesized downward trend at the 0.02 probability level for 1967, 1972, 1975 and 1976; and at the 0.11 probability level for 1968 and 1970. The information is not, in our opinion, sufficient to support the third hypothesis, and we can find no convincing explanation for the result.

Table 8 also suggests that the standard deviation of the forecasts has tended to be higher over the last three years of the study, a result whose cause is unclear. Further observations and further analysis of these issues constitute part of our continuing research interest in analysts' forecasts.

LIMITATIONS

Data-gathering difficulties are probably the most serious obstacle to undertaking studies which evaluate analysts' predictions over long periods of time. Although we were successful in gathering ten years of data, as can be seen in Table 9, we were faced with missing forecasts for some firms in several months. While most of the cell values in Table 9 are of comparable size, this is not the case for 1976. However, the analysis in 1976 is confined to non-December firms. Although our separate analysis of December and non-December firms did not yield pronounced differences, there was a slight tendency for non-December firms to pose more difficulty for analysts (at least in our limited sample of non-December firms). Therefore, the 1976 data should bias our results against the analysts. Since our overall conclusions support the quality of analysts' predictions, we can conclude that missing data problems probably did not seriously affect our results.

It would also be useful to investigate forecast-accuracy by industry. It may be the case that different industries pose different forecasting problems for ana-

[13] The tendency for α to be negative and for β to exceed one are not statistically significant. The results are inconsistent with the conclusion reached by Barefield and Comiskey [1976, p. 244] and McDonald [1973, p. 509]. Both of these studies report a persistent optimistic bias in the analysts' forecasts observed. We note that their methodology of examining the percent of forecasts made which exceeded actual is quite different from ours.

TABLE 8

AVERAGE STANDARD DEVIATION AMONG ANALYSTS' FORECASTS

Year		Month 1	2	3	4	5	6	7	8	9	10	11	12	13
1967	Average Standard Deviation	0.1458	0.1802	0.1480	0.1723	0.2023	0.1542	0.1249	0.1399	0.1639	0.1380	0.1288	0.1316	0.1181
	Number of Observations	11	18	18	27	22	24	27	28	30	31	27	33	30
1968	Average Standard Deviation	0.2307	0.2603	0.2265	0.2080	0.1801	0.1766	0.1775	0.1806	0.1624	0.1866	0.1716	0.1748	0.2411
	Number of Observations	20	26	30	33	31	35	35	37	36	31	31	33	29
1969	Average Standard Deviation	0.2410	0.2417	0.1454	0.1449	0.1436	0.1421	0.1668	0.1913	0.1530	0.1537	0.1543	0.1640	0.1642
	Number of Observations	9	14	29	29	26	28	30	30	28	31	31	32	32
1970	Average Standard Deviation	0.1858	0.1789	0.2104	0.1712	0.1800	0.1669	0.1462	0.1284	0.0988	0.1189	0.1740	0.1523	0.1513
	Number of Observations	11	19	26	29	22	28	28	28	31	30	25	21	20
1971	Average Standard Deviation	0.1932	0.2141	0.1540	0.1627	0.1947	0.1744	0.1684	0.1898	0.1510	0.1795	0.1783	0.0975	0.1085
	Number of Observations	12	20	21	24	24	24	26	26	26	21	20	25	20
1972	Average Standard Deviation	0.1786	0.1765	0.1999	0.1587	0.2047	0.2285	0.2040	0.1771	0.1604	0.1175	0.1342	0.1563	0.1259
	Number of Observations	7	12	16	21	25	22	17	20	22	25	24	20	19
1973	Average Standard Deviation	0.1708	0.1475	0.1650	0.1588	0.1594	0.2284	0.2134	0.2032	0.2018	0.1940	0.1874	0.1513	0.1642
	Number of Observations	13	15	18	17	20	26	26	26	27	23	30	24	28
1974	Average Standard Deviation	0.2663	0.2045	0.2233	0.2138	0.3309	0.3109	0.3158	0.4457	0.4492	0.4710	0.4986	0.4355	0.3930
	Number of Observations	14	11	18	22	26	26	25	23	29	29	27	22	26
1975	Average Standard Deviation	0.7552	0.4435	0.5939	0.5040	0.4217	0.4239	0.3917	0.3878	0.4183	0.3683	0.3299	0.3247	0.2770
	Number of Observations	15	21	23	26	28	28	28	27	27	28	28	27	24
1976	Average Standard Deviation	0.4262	0.4735	0.3881	0.3730	0.3368	0.2789	0.3494	0.3654	0.4317	0.2202	0.2393	0.1580	0.1830
	Number of Observations	14	17	22	26	30	7	8	8	5	4	2	2	4

TABLE 9
NUMBER OF AVAILABLE OBSERVATIONS

	Month												
Year	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*
1967	23	23	23	33	29	30	34	35	37	38	32	38	37
1968	30	31	35	38	36	37	36	38	38	33	34	35	32
1969	15	25	35	38	34	32	33	36	33	36	41	41	38
1970	18	37	40	40	35	42	38	43	42	41	38	43	37
1971	31	40	40	43	38	43	44	45	44	39	39	43	39
1972	26	36	36	40	42	39	35	35	36	36	40	36	39
1973	25	27	31	33	36	40	36	35	41	38	44	37	43
1974	30	27	33	37	40	42	43	41	44	45	44	40	45
1975	30	35	39	40	40	41	41	41	42	42	43	43	41
1976	4	5	6	6	6	6	6	7	7	7	7	6	7

lysts. Unfortunately, our data base was insufficient to perform a meaningful analysis by industry. Such an analysis was conducted by Richards [1976] who concluded "that there are significant differences in forecast errors for different industries and even for different firms within industries; however, the differences among analysts are not significant."

CONCLUSIONS

If security analysts' forecasts are to be useful, they should influence users' estimates of parameters of appropriate probability distributions. While we cannot provide direct evidence for this usefulness criterion, we are able to provide evidence that analysts' predictions are accurate in the sense that we have described. This provides indirect evidence concerning the usefulness of analysts' forecasts.

Some specific results include the fact that analysts' forecasts become more accurate as the end of the forecast year approaches. Moreover, these forecasts do not exhibit any significant systematic bias. We also find, using an approach developed by Theil, that the accuracy in the analysts' forecasts cannot be substantially reduced by linear correction models. Without addressing cost issues, however, we can make no statements concerning the efficiency of this activity.

On the other hand, the expected decline in the variability of analysts' forecasts as the end of the forecast year approaches is not supported by our data. In fact, there is some suggestion that the variability near the end of the year has increased in recent years.

Finally, our results are consistent with a large body of empirical research which finds that the market reflects an efficient processing of publicly available information.[14]

[14] It should, perhaps, be mentioned that our work does not speak to the question of the relative accuracy of management versus analyst forecasts. We do not present any management forecast data in this study.

REFERENCES

Ahlers, D., J. Lakonishok, and J. McCann, "How Good Are Economic Forecasts?," Cornell Working Paper, 1976.

Barefield, R., and E. Comiskey, "The Accuracy of Analysts Forecasts of Earnings Per Share," *Journal of Business Research* (July 1975), pp. 241–252.

Basi, B., K. Carey, and R. Twark, "A Comparison of the Accuracy of Corporate and Security Analysts' Forecasts of Earnings," THE ACCOUNTING REVIEW (April 1976), pp. 244–254.

Buckman, J., "The Reliability of Earnings Forecasts," Trust Investment Committee of the Continental Illinois Bank, (September 1963).

Copeland, R., and R. Marioni, "Executives' Forecasts of Earnings Per Share versus Forecasts of Naive Models," *Journal of Business* (October 1972), pp. 497–512.

Cox, D. R., and A. Stuart, "Some Quick Tests for Trend in Location and Dispersion," *Biometrika* (June 1955), pp. 80–95.

Cragg, J., and B. Malkiel, "The Concensus and Accuracy of Some Predictions of the Growth of Corporate Earnings," *The Journal of Finance* (March 1968), pp. 67–84.

Gonedes, N. J., N. Dopuch, and S. H. Penman, "Disclosure Rules, Information-Production, and Capital Market Equilibrium: The Case of Forecast Disclosure Rules," *Journal of Accounting Research* (Spring 1976), pp. 89–137.

Granger, C., and P. Newbold, "Some Comments on the Evaluation of Economic Forecasts," *Applied Economics* (1973), pp. 35–47.

Green, D., and J. Segall, "The Predictive Power of First-Quarter Earnings Reports," *Journal of Business* (January 1967), pp. 44–55.

Johnston, J., *Econometric Methods*, 2 ed. (McGraw-Hill Book Co., 1972).

McDonald, C., "An Empirical Examination of the Reliability of Published Predictions of Future Earnings," THE ACCOUNTING REVIEW (July 1973), pp. 502–510.

Niederhoffer, V., and P. Regan, "Earnings Changes and Stock Prices," *Financial Analysts Journal* (May–June 1972), pp. 65–71.

Richards, R., "Analysts' Performance and the Accuracy of Corporate Earnings Forecasts," *The Journal of Business* (July 1976), pp. 350–357.

Theil, H., *Applied Economic Forecasting* (North-Holland Publishing Company, 1966).

———, *Economic Forecasts and Policy* (North-Holland Publishing Company, 1958).

JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS VOL. 19, NO. 4, DECEMBER 1984

Professional Expectations: Accuracy and Diagnosis of Errors

Edwin J. Elton, Martin J. Gruber, and Mustafa N. Gultekin*

Abstract

The purpose of this paper is to analyze the errors made by professional forecasters (analysts) in estimating earnings per share for a large number of firms over a number of years. We have demonstrated in a previous paper that consensus (average) estimates of earnings per share play a key role in share price determination. In this paper, we examine consensus estimates with respect to the following questions: (1) What is the size and pattern of analysts' errors? (2) What is the source of errors? (3) Are some firms more difficult to predict than others? (4) Is there an association between errors in forecasts and divergence of analysts' estimates?

I. Introduction

Expectations play an important role in the theoretical literature of financial economics as well as in the day-to-day world of the investment community. Expectations as to the future dividend-paying capacity of the firm are often held to be a key variable in the determination of share price. Almost every model of share valuation that has been proposed, whether part of a theoretical system or invented by a practicing analyst, requires estimates of earnings or cash flow. The perceived importance of forecasts of next year's earnings to the valuation process can be seen from the fact that almost without exception, analysts at major brokerage firms and financial institutions produce estimates of next year's earnings. Firms often (and, in fact, should) forecast earnings into the future as well as a myriad of other variables. The potpourri of other forecasted variables differs from firm to firm, but forecasts of the next fiscal year's earnings per share are almost always produced.

The purpose of this paper is to analyze the errors made by professional forecasters (analysts) in estimating earnings per share for a large number of firms over a number of years.[1] We have demonstrated in a previous paper that con-

* All three authors, New York University, New York, NY 10006. This paper won a prize from the Institute of Quantitative Research in Finance competitive paper competition for the year 1982.

[1] See [2], [3], [5], and [8]. Crichfield, Dyckman, and Lakonishok [4] use data on a larger number of forecasts over a long period of time for a relatively small (46) sample of firms. This last article comes closest to the analysis in this paper. See [1] for additional discussion of related work.

sensus (average) estimates of earnings per share play a key role in share price determination. In this paper, we examine consensus estimates with respect to the following questions: 1. What is the size and pattern of analysts' errors? 2. What is the source of errors? 3. Are some firms more difficult to predict than others? 4. Is there an association between errors in forecasts and divergence of analysts' estimates?

The first of these topics involves an examination of the average size and the time pattern of analysts' errors. The second topic involves an examination of the type of errors that analysts make. For example, what percent of the error in forecasting is due to an inability to forecast correctly the average growth rate in earnings in the economy; what percent is due to the inability to forecast how well individual industries will perform; and what percent is due to an inability to forecast how well individual companies will do? The second topic also examines other forecast characteristics. The third topic involves an examination of the persistence of errors over time. Are there particular industries or companies for which it is particularly hard or easy to forecast earnings?[2] The final topic involves an examination of disagreement among analysts concerning forecasts and the relationship of this disagreement to the error in the consensus forecast.

II. Sample

Our data source was the I/B/E/S database put together by Lynch, Jones and Ryan, a New York brokerage firm. Lynch, Jones and Ryan collect, on a monthly basis, earnings estimates from all major brokerage firms on over 2,000 corporations. The earnings estimates are for each of the next two years. Lynch, Jones and Ryan publish a number of characteristics of these earnings estimates for each corporation followed. These include among others the arithmetic mean, median, range, and standard deviation of the estimates of earnings per share for each corporation.

For part of this study, we wanted to have earnings estimates prepared a given number of months before the end of the fiscal year to be at a common calendar time. This restriction means that all analysts would have access to the same macroeconomic information at the time these forecasts were prepared (*N* months before the end of the fiscal year). Because the majority of firms have fiscal years ending in December, only these firms were selected.

Our second restriction was to include only firms followed by three or more analysts. We studied properties of consensus estimates of earnings. Requiring three analysts was a trade-off between a desire for a large sample and a desire to have the forecasts reflective of a consensus rather than of the idiosyncrasies of

[2] Crichfield, Dyckman, and Lakonishok [4] examine the size and convergent rate of errors as well as present one partitioning of sources of errors. Our study differs from theirs in several ways. Our sample of firms is much larger (over 400 versus 46). We present more analysis of pattern of errors within years and the partitioning of errors. We analyze predictability of errors for individual firms and the relationship of difficulty of prediction to error size. Their sample of years was larger than ours and they placed more emphasis on pattern of errors between succeeding years.

one or two analysts. Our final sample consisted of 414 firms for each of the years 1976, 1977, and 1978.[3]

III. Size of Analysts' Errors and Their Time Series Properties

Our first set of tests involved looking at the accuracy of analysts' estimates of earnings (and growth in earnings) and the change in the error with successive forecasts over the fiscal year. We used several different measures of analysts' errors. The first measure was the dollar error, defined as the absolute value of the difference between actual earnings and forecasted earnings. If F_t is the earnings forecast made t months before the end of the fiscal year and A is the actual earnings, then dollar error is

$$(1) \qquad |A - F_t| \, .$$

The second measure of analysts' accuracy was the error in estimated growth. This is the metric that will be emphasized in the latter section of this paper. There is ambiguity in this metric if actual earnings were negative or zero. In addition, if firms with extremely small earnings were included in the sample, the average results would be dominated by these few observations. To avoid these problems, we excluded firms with earnings less than 20¢.[4] Eliminating firms with negative earnings resulted in deletion of 21 observations and eliminating firms with very small earnings resulted in deletion of an additional nine observations out of a total of 1,242 observations. With last year's actual earnings denoted by A_L, the second error measure can be expressed as the difference between the actual growth and forecasted growth, or

$$(2) \qquad \left|\left(A/A_L\right) - \left(F_t/A_L\right)\right| \quad \text{for} \quad A, A_L > 0 \, .$$

Our final measure was Theil's [10] inequality coefficient. Define the subscript i as referring to firm i and define[5]

	For Change in Earnings	For Growth in Earnings
Realized change	$R_i = A_i - A_{iL}$	$R_i = (A_i - A_{iL})/A_{iL}$
Predicted change	$P_i = F_{it} - A_{iL}$	$P_i = (F_{it} - A_{iL})/A_{iL}$

[3] A large amount of data checking was performed. We ran all the normal screens. We cross-checked all stock splits and stock dividends with CRSP and COMPUSTAT. As a further check on splits and dividends we used Moody's. In almost all cases, we were able to resolve inconsistencies. Lynch, Jones and Ryan were very helpful in this process and we thank them. In total, we deleted 11 firms in which an inconsistency existed, but we were unable to check its accuracy. An example would be the appearance of a $16 forecast when all other analysts were forecasting about 16¢. We eliminated only firms with this type of extreme divergence in estimates. In practice, we either found this type of extreme estimate or an estimate such as 36¢ that could be legitimate and, hence, was retained.

[4] At several points in the analysis, the impact of including firms with earnings of less than 20¢ is discussed. The large impact of deleting firms with earnings of less than 20¢ can be seen by the fact that while only 30 out of 1242 observations were deleted, the mean square error in the analysts' estimates of growth was cut by more than one-half when these few observations were excluded.

[5] See [9] and [10]. Once again, firms with earnings less than 20¢ were deleted when growth was examined.

Theil's inequality coefficient is

(3)
$$U = \sum_{i=1}^{N}\left(R_i - P_i\right)^2 / \sum_{i=1}^{N} R_i^2 .$$

One advantage of this measure is that it is scaled. A value of zero is associated with a perfect forecast. A value of one is associated with a forecast that on average has the same error as a "naive" no change forecast.

All the analysis in this article was done for alternative measures of error. Alternative formulations were employed because without knowledge of a potential user's loss function, one measure could not be singled out as best. Because the results of the analysis were sufficiently similar under alternative measures, in most cases the analysis is reported in terms of error in growth, and differences that arise from other measures are briefly noted.

To analyze the time-series properties of errors in forecasts, we regressed each of our measures on time. The results are presented in Table 1. Month 1 is the month in which analysts prepared their last forecast of earnings per share for a fiscal year and month 12 is 12 months earlier. Thus, the positive regression slope indicates a decrease in errors in forecasts over time. The most striking feature of Table 1 is the regularity of the decline in errors over successive forecasts. The reader might well anticipate a decline in error size over time, given that additional information is made available throughout the year. The high degree of association between error and time (over 99 percent in some cases) shows that the decline in error is about the same size from month to month over the year.

TABLE 1

Regressions of Mean Consensus Error on Time

$P = a + bT + \varepsilon$

	Dollar Error			Error in Growth			Theil's *U* in Change			Theil's *U* in Growth		
	a	*b*	R^2	*a*	*b*	R^2	*a*	*b*	R^2	*a*	*b*	R^2
Overall	.146	.036	.997	.043	.013	.998	.083	.054	.990	−.061	.061	.947
1976	.144	.035	.996	.048	.015	.998	.038	.045	.988	−.049	.048	.944
1977	.159	.036	.991	.045	.013	.991	.164	.079	.985	−.077	.081	.891
1978	.136	.037	.994	.036	.013	.993	.062	.042	.949	−.068	.064	.980

The second striking feature of Table 1 is the similarity between years for most of our error measures. For example, the change in the error for different years between months was 3.5 cents, 3.6 cents, and 3.7 cents for dollar error. Using the Chow test, we cannot reject the hypothesis that the equations are the same at the 5 percent level of significance. Thus, one cannot reject the appropriateness of pooling the observations across years.

For error in growth, the decline per month was .015, .013, and .013 in the three years. Once again, one could not reject the hypothesis that the regressions were the same in each year.[6] Similar results held for other measures.

Before leaving this section, some comments on the Theil inequality coefficient are in order. Theil's measure for growth ranged from .801 in month 12 down to .055 in month 1. This pattern implied that analysts forecasted better than the naive model of no change and that their forecasts became more accurate as the fiscal year progressed.

IV. Error Diagnosis

While the size and time pattern of analysts' error is interesting in itself, more can be learned about analysts' performance by diagnosing the source of analysts' errors. In this section, we examine two sets of error partitions:

1. Level of aggregation—how significant are errors that are unique to each company in comparison with a more general level of aggregation?
2. Forecast characteristics—are there recognizable patterns in errors?

The partition results are for the mean squared error of analysts' estimates of the growth in earnings per share. The analysis also was performed in terms of the dollar change in earnings; when differences or similarities in the alternative metrics are sufficiently interesting, we comment upon them.

The formula for the average mean squared forecast error in growth is

$$\text{MSFE} = 1/N\sum_{i=1}^{N}\left(P_i - R_i\right)^2 \tag{4}$$

where

P_i is the consensus prediction of growth for firm i

R_i is the actual of growth for firm i

N is the number of observations.

Note that MSFE can be calculated for each month in which forecasts are prepared. Thus, we have twelve values of MSFE for each year. We now examine the partitioning of the MSFE.

A. Partitioning by Level of Aggregation

Institutions differ in the way their analysts prepare forecasts for individual firms. Some institutions start with forecasts for the economy as a whole, then prepare industry studies, and finally prepare forecasts for individual firms (top-down approach). Other institutions start with the forecasts for individual firms

[6] Before eliminating firms with earnings less than 20¢, we did not observe this consistency from year to year in measures using growth, although the error declined from month to month. This inconsistency was caused primarily by a firm with earnings of 1¢ in one year causing an error in the thousands. For such a skewed sample, it is worthwhile examining the median as a measure of central tendency. We did so, and the results similar to those shown in Table 1 were obtained.

and only after such forecasts are prepared, check with the economists' forecasts for macroeconomic consistency (bottom-up approach). Thus, it is useful to examine the level of aggregation at which serious errors are being made: are they made at the economy level, the industry level, or the individual firm level?

The mean squared error of the forecasts can be partitioned as follows

$$\text{MSFE} = 1/N\sum_{i=1}^{N}\left(P_i - R_i\right)^2 = \left(\bar{P} - \bar{R}\right)^2 + 1/N\sum_{j=1}^{J}N_j\left[\left(\bar{P}_j - \bar{P}\right) - \left(\bar{R}_j - \bar{R}\right)\right]^2 \tag{5}$$
$$+ 1/N\sum_{j=1}^{J}\sum_{i=1}^{Nj}\left[\left(P_i - \bar{P}_j\right) - \left(R_i - \bar{R}_j\right)\right]^2$$

where

- $\bar{P}$ is the mean value for P across all companies
- $\bar{R}$ is the mean value for R across all companies
- $\bar{P}_j$ is the mean value for P across all companies in industry j
- $\bar{R}_j$ is the mean value for R across all companies in industry j
- J is the number of industries in our sample
- N_j is the number of firms in industry j.

The first term measures how much of the forecast error is due to the inability of analysts to predict what earnings per share will be for the economy (actually for the total of firms in our sample). The second term is a measure of how much of the total error is due to the analysts' misestimating the differential performance of individual industries. The final term measures how much of the error is due to the inability to predict how each firm will differ from its industry average.

By dividing both sides of equation (5) by MSFE and multiplying by 100, we express each source of error as a percentage of the total mean squared forecasting error. To perform this analysis, modification of our sample was necessary. In our earlier analysis, several industries were represented by very few firms. Because we are interested in errors in forecasting for industries as well as firms, for this part of our study we limited the sample to all industries containing seven or more firms. This restriction reduced our sample size to 225 firms.

B. Partitioning by Forecast Characteristics

The decomposition discussed above was designed to aid management in finding the level of aggregation at which mistakes were made. This section presents a partitioning that looks for systematic errors in analysts' forecasts to improve (either mechanically or through discussions with analysts) their forecasts. Error is partitioned into bias, inefficiency, and a random component. The partition is given by[7]

$$\text{MSFE} = \left(\bar{P} - \bar{R}\right)^2 + (1 - \beta)^2 S_P^2 + \left(1 - \rho^2\right)S_R^2 \tag{6}$$

[7] This method of partitioning was derived by Mincer and Zarnovitz [7]. It is the same method of

where

β is the slope coefficient of the regression of R on P.

ρ is the correlation of P and R.

S_p is the standard deviation of P.

S_R is the standard deviation of R.

The first term represents bias, the tendency of the average forecast to overestimate or underestimate the true average. The second term represents inefficiency or the tendency for forecasts to be underestimated at high values of P and overestimated at low values, or vice versa. If the beta of actual growth regressed on forecasted growth is greater than one, forecasts are underestimates at high values and overestimates at low values. If beta is less than one, the forecasts are overestimates at high values and underestimates at low values. The final component is the random disturbance term, a measure of error not related to the value of the prediction P or the realization R.

C. Results

The results of both decompositions are presented in Table 2.

1. Partition by Level of Aggregation

Table 2 presents the partition of MSFE, in percentage terms, by level of aggregation. Note that the error in forecasting the average level of growth in earnings per share for the economy is quite small and is below 3 percent of the total error. Analysts on average make very little error in estimating the average growth rate in earnings per share for the economy.

TABLE 2

Partitioning of Percentage Error in Growth

	Economy	Industry	Company	Bias	Inefficiency	Random Error
January	2.0	37.3	60.7	1.0	27.4	71.6
February	2.2	36.8	61.0	1.1	26.3	72.6
March	2.4	36.2	61.5	1.7	14.2	84.1
April	2.1	33.1	64.8	1.8	8.6	89.6
May	2.5	32.6	64.9	2.2	7.8	90.0
June	2.7	29.4	67.9	2.5	9.5	88.0
July	2.8	30.2	67.0	2.6	6.7	90.7
August	2.7	30.6	66.8	2.4	7.7	89.9
September	2.7	26.5	70.8	2.4	8.5	89.1
October	2.3	26.3	71.5	2.2	6.4	91.4
November	1.3	23.0	75.7	1.6	3.4	95.0
December	0.8	15.5	83.7	0.9	3.0	96.1

partitioning used by Crichfield, Dyckman, and Lakonishok [4]. Our results differ from theirs in that they examine the log of growth and used a much smaller sample size.

The vast majority of error in forecasting arises from misestimates of industry performance and company performance. The percentage of error due to industry misestimates starts as 37.3 percent in January and declines over time to 15.5 percent. Similarly, the percentage of error due to misestimating individual companies starts at 60.7 percent in January and increases to 83.7 percent by December.[8] We already know (from Section III) that analysts become more accurate as the fiscal year progresses. Now we see that while analysts become more accurate in forecasting both industry performance and company performance, their ability to forecast industry performance grows relative to their ability to forecast company performance over the year.

2. Partitioning by Forecast Characteristics

Table 2 also presents the results of partitioning analysts' mean square error by forecast characteristics. It is apparent that bias is an extremely small source of error and in all months is below 3 percent.[9] Note that inefficiency starts as a fairly important component of the error but its importance diminishes as successive forecasts are made. The percentage of error accounted for by inefficiency begins at about 27 percent for early forecasts and shrinks to 3 percent as successive forecasts are made during the year. The percent of error due to random error grows from 71.6 percent to 96.1 percent over the year. This initial importance of inefficiency is due primarily to the tendency of analysts to systematically overestimate the growth for high growth companies and to overestimate shrinkage in earnings for very low growth companies. This can be seen from the fact that the beta from equation (6) was below one for all three years examined.[10] This indicates that a linear correction applied to analysts' forecasts of growth could improve these forecasts.

V. Relationship of Errors in Adjacent Periods

Are the firms for which analysts make large errors in forecasting in one year the same as those for which they make large errors in the adjacent year? The answer to this question is clearly *yes*. For both errors in change and errors in growth, we divided firms into five equal groups by size of error in each month for each year. We then examined whether a firm that fell into one quintile in a par-

[8] This analysis was repeated for the entire industry sample, including firms with earnings less than 20¢. This increased the sample size from 216 to 225 in 1976 but resulted in an entirely different breakdown of error in growth. These firms had gigantic analysts' errors in terms of growth rate and because they were not concentrated in one industry, the importance of industry error dropped markedly. The analysis also was repeated in terms of error in earnings change per share. The partitioning is indistinguishable from that presented in Table 2.

[9] Note that the measure of bias used here is the same as the first term in the partitioning by level of aggregation. The numerical value is different because the sample is different. The analysis by level of aggregation used a subsample with heavy representation from a few industries. In this section, we use the full sample. However, note that with either sample the misestimate of average earnings is very small.

[10] When the error in forecasting earnings change was examined, beta was much closer to one and the percentage error due to inefficiency was much smaller.

ticular month in one year ended up in the same or adjacent quintiles that month in the next year.

The tendency for firms to remain in the same quintile is statistically significant in all cases (by a chi-squared test) at the 1 percent level. This is true whether the analysis is performed in terms of change in earnings or growth rates in earnings. These results support the proposition that firms for which analysts prepare poor forecasts in any year tend to be the same firms for which they prepare poor forecasts in the subsequent year.

VI. Dispersion of Analysts' Estimates

Up to this point, we have examined properties of estimates by consensus. The forecasts by consensus are an average of the forecasts produced by all analysts following that company. In this section, we examine some characteristics of the differences of opinion among analysts about a company's growth rate in earnings per share. We use the standard deviations computed across different analysts' estimates of the same company's growth rate at a point in time as our measure of difference of opinion. We examine three topics in this section. First, does the standard deviation of analysts' estimates decrease over time? Second, do the analysts consistently make more diverse forecasts for companies in some industries than they do for others? Finally, is the divergence of opinion between analysts associated with the size of forecast error in the average (consensus) forecast? When analysts disagree about the level of future earnings for any firm, a plausible reason is that earnings for that firm are difficult to forecast. If this is true, then a high standard deviation of forecasts by different analysts should be associated with a high error in the forecast by consensus.

TABLE 3

Average Standard Deviation of Analysts' Estimates of Growth

Number of Months before December	Overall	1976	1977	1978
11	.104	.134	.096	.081
10	.102	.126	.099	.080
9	.093	.105	.098	.077
8	.086	.100	.083	.074
7	.080	.092	.081	.067
6	.080	.096	.077	.066
5	.079	.094	.079	.065
4	.080	.094	.079	.068
3	.076	.087	.074	.068
2	.073	.082	.071	.066
1	.074	.086	.072	.065
0	.067	.073	.065	.062

We now examine the first of these issues, the time pattern of the divergence of analysts' estimates. Table 3 presents the average standard deviation of analysts' estimates of growth for each month from January to December. Note that, although there is some decline in the average dispersion as the estimates get closer to the end of the year, the dispersion is not uniform. Most of the decrease in dispersion across analysts occurs in the first four months of the year. From May on, there is only a slight decline and this decline does not occur in every month in either the combined three year analysis or in any individual year.[11] The only other month of major decline occurs from November to December. Note that, while the standard deviation of the analysts' estimates is fairly stable over the last eight months of the year, the accuracy of the analysts' estimate by consensus is markedly improving. Analysts are producing more accurate forecasts, but the disagreement between analysts is not shrinking.

TABLE 4

SIC	Industry Name
451	Air Transportation
331	Steel
401	Railroads
260	Paper and Paper Containers
280	Chemical
371	Automobile, Automobile Parts and Trucks
291	Integrated Oil
208	Beverages
353	Machinery Construction and Oil Well
602	Banks
492	Pipelines and Natural Gas Distribution
491	Electric Companies
271	Newspaper and Magazines
284	Soaps and Cosmetics
631	Life Insurance
357	Office and Business Equipment
283	Drug

Three digit industries ranked from (top) those industries for which analysts had most disagreement about future earnings to those for which they had least (bottom).

The second question we examined was whether the disagreement among analysts differed across industries. To test this effect, we first calculated the average standard deviation in analysts' estimates of growth for firms in each industry. This result gave us a measure of divergence of opinion of analysts' forecasts for each industry. We then calculated the Spearman rank correlation between the dispersion (standard deviation) of analysts' estimates for each industry in one year with the same measures in other years. When we compared the standard deviations for June estimates across the 17 industries for 1976 and 1977, the rank correlation was .63 and for 1977 and 1978 it was .79. The rank correlation between forecasts' dispersions for other months was similar. In all cases, the results were statistically significant at the 1 percent level. The industries we examined

[11] Crichfield, Dyckman, and Lakonishok [4] found no significant pattern when they examined the same question. They found some tendency for a decrease but not in all years. The number of analysts following the firm is fairly constant over the year.

are listed in Table 4 in order (from top to bottom) of those with the greatest disagreement on average over the three years to those with the least.

The final question we examined was whether the error in the forecast by consensus of earnings growth was related to analysts' uncertainty about earnings growth. To study this, we used the absolute error in the forecast of growth for each company as our measure of error. We used the standard deviation of analysts' estimates in growth rates as our measure of analysts' uncertainty. For each month, we regressed the absolute error in the forecasts of growth against our measure of uncertainty of analysts' forecasts. This gave us a total of 36 regressions.[12]

The results of those regressions for every other month in each year are displayed in Table 5. From the full results, we see that the t value associated with the regression coefficient was statistically significant in each of the 36 regressions. There is a strong and significant relationship between error and uncertainty. The median R-square was .40 with a range from .13 to .77. Although there was no clear time pattern to the parameters of the regression relationship, the coefficient on analysts' uncertainty appeared to be smaller in the last two months of the year.

VII. Summary

In this paper, we have explored the characteristics of analysts' estimates of the growth rate in earnings per share. We have shown that, on average, over a wide variety of error measures, analysts' errors decline monotonically as the end of the fiscal year approaches. When we partitioned analysts' error we found that analysts were accurate in estimating the average level of growth in earnings for all stocks in our sample. The error in estimating company growth (with industry error removed) was larger (and in some months much larger) than the size of the error due to misestimating the level of industry earnings. When partitioning by source of error we saw that early in the forecasted year, analysts had a marked tendency to overestimate the growth rates of securities they believed would perform well and to underestimate the growth rate of companies they believed would perform poorly. We next showed that there is persistent difficulty in forecasting growth rates for some companies. If analysts on average have large errors when forecasting the growth of a company in one year, they are likely to have difficulty in the next year.

Finally, we examined some characteristics of the divergence across analysts in their estimates of growth rates in earnings per share. Analysts tend to have greater divergence of opinion for the first four months of a year. However, there is no systematic decrease in divergence of opinion over the rest of the year. Analysts have greater disagreement about the growth of certain industries. They tend

[12] Regressions were also run between the absolute dollar error in forecast and the standard deviation of analysts' dollar forecasts. In addition, squared errors were examined. The results were consistent with the results described in the text and reported in Table 5. The relationships were not quite so strong though still statistically significant and were more unstable. For example, when the relationship was formulated in dollar values rather than growth, the median R-square was .29 instead of .40.

TABLE 5

Absolute Error in Growth = $a + b$(Divergence of Analysts' Opinion) + ε

	January			March			May			July			September			November		
	76	77	78	76	77	78	76	77	78	76	77	78	76	77	78	76	77	78
a	.134	.015	.068	.051	.007	.097	.060	.023	.068	.061	.047	.053	.051	.015	.048	.014	.021	.046
b	.769	2.030	1.585	1.565	1.891	1.107	1.401	1.701	1.296	.972	1.195	1.233	1.295	1.334	.772	1.051	.853	.402
R^2	.42	.72	.28	.34	.69	.19	.25	.65	.25	.24	.50	.29	.44	.42	.30	.50	.46	.16

Median R-Square .40
Range of R-Square .13-.77

to disagree more about the earnings of the same industries in different years. Finally, disagreement is related to analysts' errors.

References

[1] Armstrong, J. "Relative Accuracy of Methods Used to Forecast Annual Earnings." Working Paper, Wharton School, University of Pennsylvania (1982).

[2] Brown, L., and M. Rozeff. "The Superiority of Analysts Forecasts as a Measure of Expectations: Evidence From Earnings." *The Journal of Finance*, Vol. 33 (March 1978), pp. 1-16.

[3] Cragg, J., and B. Malkiel. "The Consensus and Accuracy of Some Predictions of the Growth of Corporate Earnings." *The Journal of Finance* Vol. 23 (March 1968), pp. 67-84.

[4] Crichfield, T.; T. Dyckman; and J. Lakonishok. "An Evaluation of Security Analysts' Forecasts." *The Accounting Review*, Vol. 53 (July 1978), pp. 651-668.

[5] Elton, Edwin J., and Martin J. Gruber. "Earnings Estimate and The Accuracy of Expectational Data." *Management Science*, Vol. 19 (April 1972), pp. 409-424.

[6] Elton, Edwin J.; Martin J. Gruber; and Mustafa Gultekin. "Earnings Expectations and Share Prices." *Management Science*, Vol. 27 (September 1981), pp. 975-987.

[7] Elton, Edwin J.; Martin J. Gruber; and Suk Mo Koo. "Expectational Data: The Effect of Quarterly Reports." Working Paper, New York University.

[8] Mincer, J., and V. Zarnovitz. "The Valuation of Economic Forecasts." In *Economic Forecasts and Expectations*, Jacob Mincer, ed., National Bureau of Economic Research (1969).

[9] Richard, R. "Analysts' Performance and the Accuracy of Corporate Earnings Forecasts." *The Journal of Business*, Vol. 49 (July 1976), pp. 350-357.

[10] Theil, H. *Applied Economic Forecasting*. North-Holland Publishing Company (1966).

[11] ——————. *Economic Forecasts and Policy*. North-Holland Publishing Company (1958).

(6)

PROPERTIES OF ANALYSTS' FORECASTS OF EARNINGS: A REVIEW AND ANALYSIS OF THE RESEARCH*

Dan Givoly
Associate Professor
Tel-Aviv University

and

Josef Lakonishok
Associate Professor
Tel-Aviv University

ABSTRACT

The paper provides an overview of the evidence that has accumulated on the properties of financial analysts' forecasts of earnings. Among the properties examined are accuracy, rationality, and usefulness for investors. The paper evaluates the evidence and its implications for investors and researchers and suggests avenues for further research in the area.

1. INTRODUCTION

No better proof exists for the important role that earnings play in financial markets than the handsome livelihood derived by many professionals from the production, analysis, and forecasting of earnings numbers. Investors have a keen interest in predicting future earnings: Stock valuation models commonly employ some measure of earnings as their major parameter. Earnings-per-share emerges from various studies as the single most important accounting variable in the eyes of investors. Gonedes [1974] provides evidence showing that the earnings-per-share number (EPS) has the greatest information content of an array of accounting variables. He concludes (p. 49) that "our results seem to ascribe special importance to the information reflected in the EPS variable, relative to other variables examined." In an extensive survey of hundreds of individual investors, institutional investors, and financial analysts [Chang and Most, 1980], earnings forecasts were considered by respondents in the United States to be the most important expectational data, more important than dividends and sales forecasts. Similar results are reported in that survey for the United Kingdom and New Zealand.

The information content of earnings to investors was directly tested by numerous studies originating with the seminal work of Ball and Brown [1968]. These studies found that the message contained in the earnings report is correlated with factors that determine stock prices. Since then, many other studies have confirmed the key role that earnings play in investment decisions.

* We would like to thank Robert Kaplan and two anonymous referees for their helpful comments.

The accounting information system and particularly the earnings signal are also used for purposes other than investment evaluation: the determination of "fair" rates of return in regulated industries and the formulation of contractual agreements involving management compensation which are often tied to the level, change, or trend of some measure of earnings.

Given the prominence of earnings information in investment decisions and performance evaluation, it is clear why prediction of corporate earnings has become an essential product of the financial analysts' industry and pivotal for the evaluation of the firm's financial position. Indeed, empirical evidence shows that earnings forecasts convey important information to investors [see for example Cragg and Malkiel, 1968; Gonedes, Dopuch, and Penman, 1976; and Givoly and Lakonishok, 1979, and 1980].

The objectives of this paper are to provide an overview of the evidence that has accumulated on the properties of financial analysts' forecasts of earnings (hereafter FAF), to evaluate this evidence and its implications for investors and researchers, and to suggest avenues for further research in the area.

2. PROPERTIES OF FAF AND THE ORGANIZATION OF THE PAPER

The evolution of the research on FAF parallels in many respects the investigation of the predictions of important economic variables, particularly inflation. Similar to the research on forecasts of economic variables, the focus of the early research concerning FAF has been on their relative *accuracy*. Most of these studies compared the performance of FAF with that of an array of "naive" or mechanical models.[1] Numerous articles dealing with the accuracy of FAF appeared in major accounting and finance journals in the last two decades [a short list, by no means exhaustive, includes Cragg and Malkiel, 1968; Elton and Gruber, 1972; Barefield and Comiskey, 1975; Richards, 1976; Brown and Rozeff, 1978; Crichfield, Dyckman, and Lakonishok, 1978; and Collins and Hopwood, 1980].

The preoccupation with accuracy as opposed to other properties of the earnings forecasts is understandable: next to stock recommendations, earnings forecasts are perhaps the most prominent output of the financial analysts' industry. If FAF, which are costly both socially and privately, do not outperform the much less expensive naive predictions, then their very existence becomes questionable; and because earnings predictions are used for stock valuation and selection, inaccurate predictions may lead to wrong investment decisions. The less accurate the forecast, the greater the loss from reliance thereon. The specific functional relationship between the prediction error and the loss incurred by the forecast's user is unknown; indeed, different studies have employed different error measures in their assessment of FAF performance.

[1] The term "naive" used throughout this paper does not connote inferiority but rather the exclusive reliance on past time series of the forecast variable. In fact, some of the naive models are quite sophisticated and technically involved (in particular, the Box-Jenkins models).

An additional impetus to the research on the accuracy of FAF has been the increased interest lately in the idea of a mandatory disclosure of management forecasts. Given the perceived cost and legal complications of these forecasts, their value to investors has appropriately become the subject of research. Since data on management forecasts of earnings are scarce and potentially biased (only voluntary forecasts are available), analysts' forecasts have been viewed as a "test ground" for the evaluation of management forecasts. The notion of using FAF as a surrogate for management forecasts is echoed in several studies on FAF [see for example, Collins and Hopwood, 1980; and Gonedes, Dopuch, and Penman, 1976].

The findings of the various studies, although not in complete agreement, tend to support the notion that analysts produce earnings predictions that are somewhat more açcurate than those generated by naive models. A review of these studies and a discussion of their findings are provided in Section 4.

Another property of FAF examined by previous research is the extent and nature of the *systematic* error, i.e., the error that, under certain conditions, can be removed by adjusting the forecast to account for past systematic errors. The systematic error might be considered as an element of forecast accuracy. In fact, when accuracy is measured by the squared error, the mean error can be decomposed into three distinct components: the level bias, the regression bias, and the residual error [for a detailed discussion of the decomposition, see Theil, 1966]. Whether FAF are biased and in which direction are important questions for forecast users.

A property that apparently encompasses both accuracy and systematic error and has a wider range of implications is the rationality of FAF. Muth's [1961] criterion for rationality states that rational expectations should be generated by the same stochastic process that generates the variables to be forecasted. Most tests of the Muthian hypothesis as applied to expectations of economic variables have, however, employed the weaker condition that expectations fully reflect the information in the past history of the forecast variable. This condition means that for a forecast to be rational it must not contain a systematic error; furthermore, such a forecast cannot be improved by studying past forecasts and realizations. Only a few studies, mostly very recent, have addressed the issue of rationality of earnings expectations. These studies conclude that, by and large, analysts' forecasts are formed in a rational manner: They do not contain systematic errors and, furthermore, appear to properly utilize the extrapolative nature of the earnings series as well as other nonearnings information. Summary of these results and their implications may be found in Section 5.

Another feature of FAF is their time series properties. This feature is particularly interesting because its investigation sheds some light on the perception of analysts with respect to the earnings generating process. One manner by which earnings expectations could be formed is adaptive, that is, expectations are reviewed so as to incorporate that portion of the most recent forecast error that is considered permanent. The evidence on the formation of earnings expectations, which is generally consistent with adaptive expectations, is presented and analyzed in Section 6.

The investigation of the properties of FAF is of special interest if FAF adequately represent market expectations of earnings; in such a case, the examination of the process by which analysts form their earnings expectations adds to our understanding of investor behavior, the operation of capital markets, and the relationship between accounting information and stock prices.

Several recent studies explore the relationship between earnings forecasts made by financial analysts and stock price behavior. The results show that revisions in FAF and price changes are correlated and that, moreover, investors behave as if their earnings expectations coincide with those of financial analysts. Detail and evaluation of these findings are provided in Section 7.

Section 8 discusses yet another, perhaps the least studied, property of FAF: their cross-sectional dispersion. Almost all research on FAF uses the mean, or "consensus," forecast, without giving any recognition to the dispersion around that mean. The divergence of beliefs about future earnings may convey important information about the uncertainty surrounding future earnings and, thus, the perceived importance of the respective mean forecast. The cross-sectional dispersion of analysts' forecasts may represent a surrogate for the risk associated with the firm. Such a surrogate is of unique value to empirical researchers because, unlike most other risk surrogate estimated from past-series (e.g., the standard deviation of the return or the security beta), this one presents an *ex-ante* measure of risk. The measure and its theoretical support, as well as some preliminary results, are discussed in Section 8. The last section contains concluding remarks and suggestions for further research.

Before turning to the main issues, the data sources on earnings expectations used by previous research, their limitations, and their problems are described in Section 3.

3. EXPECTATIONAL DATA: AVAILABLE SOURCES AND SOME MEASUREMENT ISSUES

3.1 DATA SOURCES

The use of expectational data in accounting is fairly new, and, as a result, many researchers may not be familiar with the main sources of these data.

There are three publicly available (although not free) sources of earnings forecasts that have been used by researchers: the *Earnings Forecaster* of Standard and Poor's (S&P), the *Value Line's Investment Survey*, and Lynch, Jones, and Ryan's *IBES Service*. The *Value Line's Survey* is apparently the most widely circulated among the three. Other sources, mostly private (forecasts made by individual brokerage houses, pension funds, etc.), have occasionally been used by researchers.

The *Earnings Forecaster* is a weekly publication by S&P that first appeared in 1967. The publication lists forecasts of annual EPS of the current year and (if available) of the following year for about 1,500 companies. The forecasts are those made by S&P itself and by about 70 other security analysts and brokerage houses who agreed to submit their forecasts, upon release, for publication. The number of contemporaneous forecasts available for each company depends on the prominence of the company and the time of the year (more forecasts become available as the year progresses); typically, however, two to four forecasts of the current year's earnings are available around April, for most companies. The *Earnings Forecaster* has been used by Barefield and Comiskey [1975], Basi, Carey and Twark [1976], Gonedes, Dopuch, and Penman [1976], Crichfield, Dyckman, and Lakonishok [1978], Ruland [1978], Givoly and Lakonishok [1979, 1980, 1982], Fried and Givoly [1982], and Givoly [1982] among others.

The *Value Line's Survey* lists one- to five-quarter-ahead forecasts for about 1,600 firms. The survey has been published weekly since 1971 and provides quarterly earnings predictions by Value Line's analysts four times a year for each firm included. The Value Line forecasts have been employed by Brown and Rozeff [1978], Collins and Hopwood [1980], and Jaggi [1980], among others.

Lynch, Jones, and Ryan, a New York based brokerage firm, has available in both manual and computer-readable form, consensus (average) earnings estimates for the current and the next fiscal year for about 1,500 firms. This service is designated by Lynch, Jones, and Ryan as IBES (Institutional Brokers Estimate System). In its monthly issues, the service includes, besides average forecasts (which are typically based on 10 to 20 different forecasts), the lowest and the highest forecast as well as the standard deviation of the estimate across forecasters, and other statistics. *IBES Service* is a relatively new research source. It was used by Elton, Gruber, and Gultekin [1981] and is currently being used in several research projects.

Another source of FAF, which has only recently become available to researchers, is the *Icarus Services* by Zacks Investment Research, Inc. This data base contains EPS estimates for some 1,500 companies, with an average of 12 forecasters per company. The estimates, made by over 50 brokerage firms, are available for the current fiscal year, the next fiscal year, and the next five years.

3.2 SELECTING A REPRESENTATIVE FORECAST

Almost all studies relying on data that consist of more than one forecaster used mean-forecast rather than individual forecasts. The use of the mean forecast is, of course, necessary when individual forecasts are not provided (as in the IBES case). However, there are certain advantages and drawbacks of the use of the mean forecast that should be considered in interpreting the results.

Averaging individual forecasts has the effect of reducing the measurement error that is inherent in each individual forecast. This effect is achieved whenever the measurement errors across forecasters are less than perfectly correlated. In addition, the use of individual forecasts may not be very meaningful for the examination of time-series properties when the identity of the individual forecaster changes over time (as is the case of forecasts made by brokerage firms).

Some aggregate measure of FAF is likely to be superior to most individual forecasters, particularly if the weight of each forecast(er) is based on past performance and its correlation with errors of other forecasts (for a discussion of this weighting scheme and an application of the technique, see Granger and

Newbold, 1977, and Figlewski, 1980). Even a simple average may outperform each of the individual forecasts when the forecast errors are not highly correlated cross-sectionally. In fact, much of the concept of efficient markets composed of unsophisticated and less than perfectly knowledgeable investors relies on the notion of the "aggregate wisdom" of the market — that is, the superiority of the consensus over individual assessments. The fact that a consensus can reflect "greater than average" knowledge is illustrated by Beaver [1981] in a seemingly unrelated context—the prediction of outcomes of football games. Beaver provides results that suggest that the consensus of game-score predictions made by staff members of a daily newspaper (the *Chicago Daily News*) consistently outperformed predictions made by each of the individual staff members. This conclusion is shared by Zarnowitz [1979], who, after investigating forecasts of economic indicators, commented, "while published forecasts by ranking practitioners are often developed with particular skill and care, group average forecasts benefit greatly from cancellations of individual errors of opposite sign" [p. 8].

Some pitfalls in using the mean forecasts should also be recognized. First, when aggregating forecasts cross-sectionally, the assumption is made that each represents an updated, contemporaneous prediction; yet, due to problems of data collection and preparation, some of the forecasts are less updated than others, thus rendering the average forecast less meaningful. A second problem arises from the change over time in the composition of the group of forecasters who participate in forecasting the earnings of a given firm. This change makes it difficult to conduct a time-series analysis of earnings forecasts.

Finally, even if all these measurement problems did not exist, the reliance on the mean forecast might obscure patterns that are present among individual forecasters. For instance, adaptive behavior by individual forecasters may not be revealed by examining the series of the mean forecast. Bierwag and Grove [1966] showed that the mean expectation does not follow necessarily an adaptive process even when individuals form their expectations adaptively. Similar difficulties lie in identifying other time-series patterns from data on the means.

4. ACCURACY OF FAF

4.1 ERROR MEASURES AND EVALUATION BENCHMARKS

The two error measures that are most widely used in assessing the accuracy of FAF are the relative (absolute) error of the form $|P\text{-}A|/A$, and the relative square error, $(P\text{-}A)^2/A$, where P and A are the predicted and realized earnings variables, respectively. The second measure is more appealing because of its mathematical and statistical tractability. Furthermore, this measure gives more than proportional weight to large errors, a property consistent with a quadratic loss (and utility) function.

Which of the error measures is selected may not be important because of the very high correlation between the measures. However, in light of the evidence that FAF produce fewer "outliers," or extreme error cases, than (at least some of) the naive models [see Collins and Hopwood, 1980], one may suspect that the use of the square error as an accuracy measure favors FAF over naive models.

In evaluating analysts' forecasts, different benchmarks have been used; one, common to many studies, is the "no-change" naive model which is usually employed in conjunction with Theil's U statistic. This measure, proposed by Theil [1966] for the evaluation of economic forecasts, is defined as

$$U = \frac{\sum_i (P_i - A_i)^2}{\sum_i A_i^2}$$

where P_i and A_i are, respectively, predicted and actual growth in earnings of firm i. When predictions are perfect, $U = 0$; when predictions are "no-change," U becomes 1. The value 1, thus, serves as a benchmark for the performance of FAF. A smaller-than-1 U-value for FAF means that FAF outperform a naive no-change prediction model. Some studies relied exclusively on Theil's U for evaluating FAF; others used more sophisticated models that generally belong to four groups:

(1) Submartingale (random walk plus drift);
(2) Box-Jenkins models (models that exploit the serial correlation of the time-series);
(3) Index Model (a model that relates the earnings of the individual company to a market-wide index of earnings); and
(4) Management forecasts.

The first two models were found by recent studies to represent quite adequately the time-series behavior of annual earnings [see Albrecht, Lookabill, and McKeown, 1977; and Watts and Leftwich, 1977].[2] Quarterly earnings, however, appear to follow an autoregressive process with seasonal and quarter-to-quarter components; this process can be formulated as a Box-Jenkins model [see Brown and Rozeff, 1977; Foster, 1977; and Griffin, 1977].

The use of the Index Model is supported by the relationship that was found between the first differences in individual company earnings and the average of the first differences in earnings across all firms [see Ball and Brown, 1968; and Gonedes, 1973].

The studies that examined the accuracy of analysts vis-à-vis management forecasts were interested primarily in the incremental value of the latter to investors. These studies provided, however, additional evidence on the performance of analysts. Our concern in this context is whether the forecasting power of analysts can compensate for the better knowledge that management is presumed to possess about its own company.

---

[2] In fact, as a general representative firm-model, the submartingale was found to perform as well as the firm specific Box-Jenkins models in describing the time-series characteristics of annual earnings [see Albrecht, Lookabill, and McKeown, 1976].

4.2 EMPIRICAL RESULTS

Research on the accuracy of FAF has been surprisingly inconclusive. While several studies conclude, perhaps counterintuitively, that analysts' performance is only as good as naive models, others claim that analysts' predictions are significantly more accurate than naive models. Of course, the diversity of the naive models might be the cause of this discord; yet a closer look reveals that agreement or disagreement between the conclusions of individual studies do not appear to be correlated with the particular models tested. Moreover, as pointed out earlier, it is unlikely that the conflicting conclusions are due to the use of different error measures by different studies. Before commenting further on possible causes for this inconclusiveness, a short review of the results is presented below. Some of the studies cited contain work that relates to other properties of FAF. However, only the findings concerning accuracy are discussed in this section.

The first comprehensive study on the accuracy of FAF is that by Cragg and Malkiel [1968]. Forecasts of five-year growth rate in earnings, made by five investment houses for 185 companies in the two years 1962-63, were confronted with two sets of naive models, one predicting no change and the other a change equal to past change. The tests led to the conclusion that "forecasts based on perceived past growth rates . . . do not perform much differently from the [FAF] predictions" [p. 77]. This conclusion does not square well with the notion of rational investors, since it suggests that the costly analysts' product is not superior to a practically costless product. Indeed, Cragg and Malkiel were not apparently at ease with their own findings, so they recommended that caution should be exercised in interpreting the results because the period might be "atypical" and "only a few firms were able to participate in the study" [p. 83].

Cragg and Malkiel's conclusion was reaffirmed, nonetheless, a few years later by Elton and Gruber [1972], who evaluated annual earnings forecasts made by analysts in a large pension fund, in an investment advisory service, and in a large brokerage house. In the three years examined (1962-64), they found no significant difference in accuracy between the best naive model (an exponential smoothing model) and each of the three groups of analysts.

Later studies reported somewhat different results. Barefield and Comiskey [1975] examined mean forecasts for 100 companies in the years 1967-72 and showed (using Theil's U) that FAF outperformed the no-change model. Furthermore, FAF's superiority was more pronounced in years characterized by a turning point in the earnings trend. Using a more elaborate research design, Brown and Rozeff [1978] tested the performance of Value Line forecasts for one to five quarters ahead for 50 randomly selected firms during the period 1972-75. These forecasts showed a lower relative absolute error than a company-specific Box-Jenkins model and seasonal martingale and submartingale models (Brown and Rozeff used nonparametric tests in their design). The superiority of FAF, however, declined as the forecast horizon was shortened.

Collins and Hopwood [1980] designed a multivariate analysis of variance which corrected for the apparent dependence in repeated samples of the same companies over time and for the possibility of a random rejection of the null hypothesis in separate individual samples. The authors evaluated the performance of Value Line earnings forecasts, one, two, three, and four quarters ahead, made for 50 companies at the beginning of each of the 20 quarters in 1970-74. They compared the accuracy of FAF with that of several Box-Jenkins models.[3]

Value Line predictions were more accurate than the competing models. The mean relative absolute error of Value Line one-quarter-ahead forecasts was 10 percent, while the error produced by the best mechanical model was 15 percent. The longer the forecast horizon, the more marked was the difference in accuracy in favor of the analysts. Collins and Hopwood also found that Value Line predictions produced fewer and smaller extreme errors, pointing to the ability of analysts to incorporate evidence on changing economic situations.

In a more recent paper, Fried an Givoly [1982] reported on the accuracy of annual EPS estimates of analysts relative to that of two naive models: a modified version of the submartingale process and the index model for first differences in earnings.[4] The results, which were based on about 100 mean forecasts in each of the 11 years 1969-79, showed FAF to be, on average, more accurate than the two competing models: The mean relative absolute error over the tested period was 16.4 percent for FAF, significantly lower than the mean error for the modified submartingale and the index model (19.3 percent and 20.3 percent, respectively).

These results, like those of other recent studies, are in conflict with the findings of the earlier studies by Cragg and Malkiel [1968] and Elton and Gruber [1972]. Several explanations for the conflicting findings might be suggested. First, Cragg and Malkiel's study used predictions of five-year growth rates rather than the more common forecasts published by analysts which are made for one year. It is possible that analysts are more trained and capable in predicting short-term changes in earnings. Factors such as new contracts, acquisitions, labor disputes, and personnel shuffles, to which naive models are "blind," are properly incorporated in FAF while long-term trends are quite adequately captured by past patterns.

Second, Cragg and Malkiel's results are subject to serious measurement errors. The definition of the earnings variable was not uniform across forecasters sampled by their study: some used reported earnings; others used their own estimate for "normalized" earnings. As a result, it is difficult to interpret and analyze the forecast errors.

Like most of the studies on FAF, Cragg and Malkiel [1968] and Elton and Gruber [1972] used forecasts relating to a few years only. Cragg and Malkiel

---

[3] The Box-Jenkins models considered were (1) a consecutively and seasonally differenced first-order moving average and seasonal moving average and (2) a seasonally differenced first-order autoregressive and seasonal moving average model. The selection of these models was guided by the findings of the research on the time-series behavior of quarterly earnings. In particular, the first model was found to be well specified by Griffin [1977], while the second and the third models were advocated by Foster [1977], and Brown and Rozeff [1978], respectively.

[4] The first model was the submartingale for most years. however, in years following large fluctuations in earnings, an exponential smoothing process was employed as the predictor; this was done in light of the findings by Brooks and Buckmaster [1976] of a mean-reverting behavior of earnings in the period immediately following large deviations of the earnings from their "norm."

examined forecasts made in 1962 and 1963, while Elton and Gruber used forecasts made in the three years 1964-66. It is conceivable that both the relative and absolute accuracy of FAF vary over time. Conclusions drawn from only two or three years of forecasts are subject to a considerable amount of noise. There are some indications that the performance of FAF relative to naive models is indeed time-dependent.[6]

A relatively long time series of FAF, 11 years, was used by Fried and Givoly [1982]. Although the accuracy of FAF was found in that study to be, on average, greater than that of two widely used naive models, FAF were outperformed (although not significantly) by the naive models, in two of the 11 years, and in three other years their superiority was not statistically significant. This pattern suggests that the reliance on short time series may lead to unwarranted conclusions. Considering the fact that all recent and methodologically more careful studies reached basically the same result, it is safe to conclude that, at least during the 1970s, analysts appear to outperform naive models that are based only on past history of the earnings series.

Most of the research on FAF accuracy suffers from several methodological flaws, which might explain, in part, the inconclusive nature of the early research on the topic. First, when an array of naive models is pitted against FAF, there is always a possibility that, even if the naive models are inferior, one of them would outperform FAF by a mere chance, particularly when the time period examined is short. Second, the null hypothesis in all studies was that FAF performed no better than naive models. Had the null been that FAF performed better than naive models, most tests would likely have been unable to reject that null hypothesis. In addition, the data base used by these studies, particularly the later ones, was susceptible to measurement errors, such as inconsistent definitions of the earnings variable in the expectational data and the actual earnings data (fully diluted vs. primary earnings-per-share, inclusion vs. exclusion of extraordinary items, etc.).

With respect to the comparison of FAF with management forecasts, all studies point to a slight and mostly insignificantly edge to management forecasts. Basi, Carey, and Twark [1976] reported that the mean absolute percentage forecast error during the years 1970 and 1971 was 10.1 percent for management forecasts compared to 13.8 percent for FAF (the data source for analysts' estimates was the Earnings Forecaster). In a follow-up study based on the years 1970-73, Ruland [1978] reached the same conclusion concerning the parity between the two types of forecasts. Similar results were also derived by Jaggi, Imhoff and Paré [1980] who examined the accuracy of management forecasts vs. FAF for the periods of 1971-74 and 1971-77, respectively.

The finding of a parity between the forecasting performance of analysts and managers is not surprising considering the similar information set and the contin-

---

[6] Brown and Rozeff [1978], for instance, concluded that Value Line predictions are better than Box-Jenkins forecasts. Yet, as was pointed out by Abdel-khalik and Thompson [1977], the pattern of Value Line superiority over Box-Jenkins is strongly temporal with only two out of the four years examined by Brown and Rozeff exhibiting significant results.

uous dissemination of "inside" information from managers to analysts (the forces behind this transfer of information were documented and analyzed by Lees, 1983).

The generalizability of the studies on the performance of management forecasts is questionable since all management forecasts used by these studies were voluntary. Presumably, management is not likely to reveal publicly its own earnings estimates unless it assigns them a high degree of certainty. As a result, the comparison between FAF and voluntary management forecasts is likely to be biased in favor of the latter.

Another problem that has not been solved satisfactorily by any of these studies is the timing of analysts' forecasts. While the exact date of the disclosure of management forecasts is a matter of public record (the forecasts are usually made as part of a press release), the determination of the timing of FAF is less precise. At least three pertinent forecast dates may exist: the date on which the forecast was finalized and released to preferred clients; the date on which the forecast was released to all clients; and the date on which the forecast is first published in the S&P or Value-Line publications. The times between these three dates are not trivial and in fact might be exploited by privileged clients [see, for example, Abdel-khalik and Ajinkya, 1982]. While the first date is the most relevant for evaluating the performance of FAF vis-à-vis competing prediction models, only the latter was available to, and therefore used by, the above studies. The performance of FAF was, therefore, underestimated by these studies, since in many instances, there existed other, more updated, yet still unpublished forecasts which were likely to be better than those available to the studies.

If a proper allowance were made for the gap in timing between management and analyst forecasts, the slight edge found for management forecasts might have been completely erased.

5. RATIONALITY OF FAF

Muth's [1961] criterion for rationality states that expectations should be generated by the same stochastic process that generates the variables to be forecasted. Most tests for the Muthian hypothesis, however, have employed a somewhat weaker condition, namely, that expectations fully reflect all the information in the past history of the forecast variable. This implies that the rational forecast cannot be improved by studying past forecasts and realizations.

The issue of rationality of earnings expectations is important since it is directly related to the efficiency of the stock market. Evidence of rational earnings forecasts would be consistent with both the finding of stock market efficiency and the important role of earnings in stock valuation. Findings of irrational forecasting by analysts would be inconsistent with stock market efficiency unless either FAF do not represent the true market expectations or earnings expectations do not play the role envisioned for them by the various valuation models.

Several testable implications of the rationality assumption exist: rational expectations should be *unbiased* and the *most accurate*, and the time-series of forecast errors should be *serially uncorrelated*. In general, all possible extrapolations of the time-series of the variable, and utilization of the cross-sectional relationship between realized earnings across companies, should be embedded in the forecast. All these implications mean, in essence, that no systematic improvement of the forecasts can be made by studying the past series of forecasts and realizations.

The concept of rational expectations has recently become the underpinning of many economic models. It is therefore not surprising to find major research efforts in the empirical evaluation of the degree of rationality in the expectations of economic variables. In particular, the manner by which inflationary expectations are formed has been examined by various studies through the use of Livingston survey data [see for example, Gibson, 1972; Pyle, 1972; Cargill, 1976; Lahiri, 1976; Figlewski and Wachtel, 1981; and Ahlers and Lakonishok, 1983]. The main conclusion that emerges from this research is that economists' expectations are not formed in a fully rational manner.

The increased availability of earnings expectation data has stimulated research on the rationality of earnings expectations. This research is discussed below.

5.1 SYSTEMATIC ERROR OF FAF

Various tests have been employed for assessing the degree of systematic error (bias) of earnings forecasts. A common procedure involves estimating a regression[6] of the form

$$A = \alpha + \beta P + u \tag{1}$$

where A is the realized earnings (or earnings growth), P is the predicted earnings (or earnings growth), and u is a random error with a zero expectation. Then, the null hypothesis $\alpha = 0$ and $\beta = 1$ is tested. Failure to reject the null hypothesis is consistent with an unbiased predictor. This test has been employed for assessing the rationality of inflationary expectations [see, for example, Fama, 1975; Frenkel, 1975; Friedman, 1979; Figlewski and Wachtel, 1981; and Ahlers and Lakonishok, 1983] exchange rate expectations [see Fama, 1976; and Agmon and Amihud, 1981], and stock market expectations [see Lakonishok, 1980]. Another approach for assessing bias and inaccuracy is the decomposition procedure, developed by Theil [1966], and Mincer and Zarnowitz [1969], whereby the accuracy of the forecasts, measured by the mean square error, is decomposed into the following structure:

$$\frac{1}{n}\sum_{i} (P_i - A_i)^2 = (\overline{P} - \overline{A})^2 + (s_P - rs_A)^2 + (1 - r^2)s_A^2 \tag{2}$$

where i denotes the observation index, $\overline{P}$ and $\overline{A}$ are the means of P and A, s denotes standard deviation, and r the correlation coefficient between A and P.

In expression (2) the error is decomposed into three components so that the relative magnitude of the systematic error, the first two terms in the righthand

[6] The regression can be estimated from a time series of company earnings or from contemporaneous cross-sectional data.

side of the expression, can be assessed. When (in equation (1) above) $\alpha = 0$ and $\beta = 1$, these two terms disappear.

The bias element has been evaluated in the literature also through other related measures such as the average error, i.e., $\overline{P} - \overline{A}$, or the relative frequency of cases of underestimation or overestimation.

The studies by Crichfield, Dyckman, and Lakonishok [1978], Givoly [1982], and Malkiel and Cragg [1980] used the regression in (1) to assess the bias of FAF. Using mean forecasts (of earnings growth) from the *Earnings Forecaster*, Crichfield, Dyckman, and Lakonishok estimated the coefficients over a cross section of FAF made for 46 companies for each of 10 years 1967-76. The coefficients averaged over the years were, in general insignificantly different from their hypothesized values (H_0: $\alpha = 0$, $\beta = 1$). However, the values of α were mostly negative and the values of β mostly above 1. These values suggest that FAF are "smoother" than actual trends: they exhibit an upward bias in predicting rate of growth in earnings in years with below-average growth rate and downward bias in predicting years with above-average growth rate, but overall the average forecast was not significantly different from the average realization. A similar finding is also reported by Malkiel and Cragg [1980] for five-year earnings growth predictions made by several investment firms in the years 1961-69.[7]

Testing the unbiasedness hypothesis through a cross-sectional test raises two problems. First, conceptually, earnings expectations are formed for each individual company. An unbiasedness in a cross section of companies does not necessarily suggest rational (unbiased) expectations with respect to all or even most companies: It is conceivable that earnings expectations of individual companies are biased in different directions so as to produce an unbiased *average*. Second, statistically, in a cross-sectional test the forecasts made for different companies are viewed as a random sample of forecasts. However, realizations of earnings growth are known to be correlated with marketwide factors so as to induce a cross-sectional dependence of the contemporaneous forecast errors. One way to circumvent the statistical problem of a cross-sectional dependence of the errors is to derive the coefficients' estimate as an average of the estimates produced by the yearly cross-sectional regressions.

A study by Givoly [1982] estimated the coefficients α and β from a time series of mean earnings forecasts made for individual companies (the mean of different contemporaneous forecasts was used as the basic observation) and from individual forecasts for the same company made by each individual forecaster over time. Although the typical time series was short (8-11 years over the period 1969-79), the results for the (about) 50 companies examined showed that FAF were unbiased. The joint hypothesis $\alpha = 0$, $\beta = 1$ could not be rejected for the vast majority of companies and for all the forecasters that were examined.

Crichfield, Dyckman, and Lakonishok [1978] assessed the bias through Theil's decomposition. They found that, on average, only 18 percent of the mean squared error in the prediction of earnings growth could be attributed to the

---

7 The number of participating firms was not disclosed, but they represent a subsample from a sample of 178 companies.

systematic error. Out of this proportion, 13 percent stems from level bias and 5 percent from regression bias.

Despite its statistical insignificance and the fact that its direction may change over time, there is an accumulation of evidence that some upward bias is present in FAF. Barefield and Comiskey [1975] reported the results for analyst forecasts made in the years 1967-72. Out of the 600 forecasts examined, 382 exceeded actual, 207 were below actual, and 11 were equal to the actual earnings. A similar tendency to overestimate earnings was also found, not surprisingly, among managers by McDonald [1975]. Fried and Givoly [1982] reported the average relative error (considering sign) of about 1,200 mean forecasts made in the years 1969-79. The average error (realized value less prediction) over time was significantly negative (indicating an upward bias), although in five of the eleven years the error was positive.

It is interesting to compare these findings with the performance of forecasts of other economic variables. Mincer and Zarnowitz [1969] presented accuracy statistics for several sets of business forecasts of levels of GNP, consumption, plant and equipment outlays, and industrial production. In most cases, the statistical tests led to the rejection of the joint hypothesis $\alpha = 0, \beta = 1$. This result was accounted for largely by level bias, and the preponderant bias was an underestimation of consumption and of GNP. Theil's decomposition revealed that the residual variance component accounted for most of the error.

Ahlers and Lakonishok [1983] investigated the performance of economists' forecasts of ten important macroeconomic variables over the 32 years 1947-78. Two forecasting horizons were examined, six months and twelve months. The joint hypothesis $\alpha = 0, \beta = 1$ for change predictions was rejected in 17 of the 20 (10 x 2) cases. Ahlers and Lakonishok's results concerning inflation forecasts are in accord with several earlier studies [see Turnovsky, 1970; Pesando, 1975; Gibson, 1977; and Figlewski and Wachtel, 1981].

It is instructive to note that while there is a downward bias in forecasting general economic variables, no significant bias could be detected among FAF. This might be a result of the degree of specialization of analysts in the history of the companies whose earnings they predict, in contrast to the wider scope of the economists' task. To be sure, this is merely a conjecture.

The importance of the unbiasedness property to the overall quality of FAF should be put in a proper perspective. Given the research on the time-series behavior of earnings, even a very naive model, whereby the expected change in earnings is equal to some deterministic growth element based on past growth, may produce unbiased predictions. However, there are good reasons to believe that FAF are based on more than mere extrapolation of past realizations: as mentioned in Section 4, FAF were found to be more accurate than naive models at turning points, suggesting the employment of exogeneous information. Indeed, Fried and Givoly [1982] showed that FAF contain autonomous information not captured by both the time-series submartingale model and the cross-sectional index model of earnings. In another study, Abdel-khalik and Ajinkya [1982] provided evidence suggesting that analysts possess inside information. The finding of unbiasedness of FAF thus indicates the proper processing and analysis of information beyond that contained in the past time series.

5.2 INCORPORATION OF AVAILABLE INFORMATION

A simple way to test whether forecasts fully incorporate available information is to regress the forecast errors on specific data that were available to the forecasters. One easily available piece of information that a rational forecaster should consider is his previous forecast error. To test whether FAF fully exploit information on past errors, current errors could be regressed on past errors. Givoly [1982] estimated a regression of the form

$$P_t - A_t = a + b(P_{t-1} - A_{t-1}) + e_t$$

using both time series (of individual companies and individual forecasters) and cross-sectional versions, for a sample of about 6,000 annual earnings forecasts made over 11 years (1969-79). The hypothesis a = 0 and b = 0 could not be rejected: In most regressions the coefficients were very small and insignificant. This result suggests that the information contained in past forecast errors is fully utilized in forming predictions of future earnings.

A broader test of expectations rationality is whether the forecasters effectively incorporate *all* historical information available. Apparently, it is unfeasible to test whether a particular set of earnings expectations incorporate all the information that can be deduced from the earnings time series. However, more limited tests were conducted by Malkiel and Cragg [1980] and Fried and Givoly [1982].

Malkiel and Cragg found no consistent combination between information on historical growth rates and analysts' forecasts that could be used to make better one- or five-year-ahead earnings predictions. These results led to the conclusion that "there is no systematic relationship between historical and realized growth that is not directly incorporated into the forecasts."

Fried and Givoly conducted a test on the degree to which analysts' forecasts exploit the time-series properties and the cross-sectional relationship of earnings as captured by following two naive prediction models:

$$\text{(a)} \quad P_t = A_{t-1} + c_t$$

and

$$\text{(b)} \quad P_t = A_{t-1} + \alpha_t + \beta_t \Delta A_{mt}$$

where c_t is the arithmetic average past growth in EPS, α and β regression parameters, and ΔA_{mt} is the change in the market earnings (represented by S&P's Composite 500). The models, the submartingale[8] and the index model, were found to represent the behavior of the individual firm's earnings (see, for example, Gonedes, 1973; and Albrecht, Lookabill, and McKeown, 1977].

---

[8] The submartingale model was replaced by a mean reverting model (exponential smoothing) in years that follow a large fluctuation in earnings. According to the findings of Brooks and Buckmaster [1976], those years' earnings behave differently. The parameters of the exponential smoothing model used here were those selected by Brooks and Buckmaster.

The partial correlation between actual earnings and the naive model's prediction, given FAF, measures the extent to which FAF exploit the information contained in the past earnings series. The reported conditional correlation coefficients were very small and not significantly different from zero. This finding suggests that analysts fully exploit at least those time-series and cross-sectional properties of the earnings series that are captured by the two frequently used prediction models.

The results so far are consistent with FAF being formed in a rational manner. This finding is of interest since earnings expectations, including FAF, play an important role in stock valuation. The result would be even more relevant if it were established also that FAF serve as a good proxy for the unobservable "market" expectation of earnings; indeed, there is some supportive evidence for this effect, which will be described in Section 7.

6. THE TIME-SERIES BEHAVIOR OF FAF

Understanding how information is put together to form an estimate of future earnings is important because market processes are typically very sensitive to the way expectations are influenced by the actual course of events. Furthermore, it is often necessary to make predictions about the way expectations would change when either the amount of available information or the structure of the system is changed.

The study on the time-series behavior of FAF is related also to the time-series properties of quarterly and annual earnings: The behavior of FAF may or may not be consistent with the observed time-series pattern of earnings with implications for both the validity of the time-series studies and the degree of rationality of FAF.

The empirical evidence on the time-series behavior of FAF is scant, due apparently to the unavailability of long enough time series of earnings estimates. The model that has been almost exclusively examined in this context uses the adaptive expectations. Under the adaptive specification, expectations are revised so as to incorporate that portion of the most recent forecast error that is considered permanent. The adaptive model has been used extensively in the economic literature to describe the formation of expectations concerning future behavior of variables such as the inflation rate [see, for example, Solow, 1969; Mussa, 1975; and Nerlove, 1958] or permanent income [see Friedman, 1957]. There is empirical support for the notion that inflation expectations, for example, are formed in an adaptive way [see the evidence provided by Turnovsky, 1970; Lahiri, 1976; and Figlewski and Wachtel, 1981]. Depending on the underlying generating process of the predicted variable, adaptive expectations represent rational expectations in the Muthian sense.[9]

The adaptive model can be formulated as

$$P_t - P_{t-1} = \theta_0 + \theta_1(A_{t-1} - P_{t-1}) + u_t.$$

---

[9] Muth [1960] has shown that expectations formed adaptively are also minimum-error variance forecasts, i.e., rational, if the underlying process is a random walk with noise.

Under the null hypothesis of adaptive behavior, the constant term is zero and the slope coefficient falls between zero and one.

Brown and Rozeff [1979] tested the behavior of revisions in FAF of quarterly earnings. Their sample consisted of 50 Value Line firms and five years of quarterly forecast data [1972-76]. They examined the revision made in the EPS forecast for the remainder of the year following the release of, separately, the first, second, and third quarters earnings reports. For each quarter, the above regression was estimated for the cross section of companies. In all three cases, a significant portion of the analysts' forecast revision was explained by the most recent one-quarter-ahead forecast. Consistent with the adaptive expectation model, the estimated regression intercepts were small and largely insignificant, while the slope coefficients were significant and fell within the range zero to one.

Interestingly, the slope coefficients for the three quarters were not the same: 0.70, 0.28, and 0.57 were observed for quarters one, two and three, respectively. It is difficult to draw conclusions from this finding about the relative degree of content of the three quarters. First, as the authors pointed out, differing coefficients could occur if the quarters are not equally difficult to predict; that is, the adaptive coefficient is a function not only of the important assigned to the recent error but also of the unpredictability of the next quarter. Second, the sample covered only five years. If the adaptive behavior varies over time, a sample that covered only five years might not be representative. If the adaptive coefficient is also firm unique, the cross-sectional tests that were conducted by Brown and Rozeff are not very meaningful. These limitations may also explain the small portion of the total variance that could be explained by the adaptive model.

Abdel-khalik and Espejo [1978] examined the manner by which forecasts of annual EPS are revised in the wake of the release of each of the quarterly reports. They expressed the relationship between the revision in the estimate of EPS and the prediction error in forecasting the last quarter through the following model:

$$F_{q,y} - F_{q-1,y} = \lambda_q D_y^q + u_{q,y}$$

where q is the quarter (q = 1, ..., 4), y is the fiscal year for which the forecasts are made, $F_{q,y}$ is the forecasted annual earnings per share made at the end of quarter q for fiscal year y, D_y^q is the forecast error for quarter q of year y, λ is the adaptation coefficient, and u is a random error.

Three alternative hypotheses concerning the way the quarterly prediction error, D, is preceived by investors were examined:

(1) D_y^q is judged as temporary with no effect on the forecasts of the remaining quarters. In this case, the revision will be in the magnitude of D_y^q, and λ is hypothesized to be equal to one.

(2) The same pattern set by D_y^q is expected to continue: In this case, the revision will be larger than D_y^q, and λ_q is, therefore, hypothesized to be greater than one, reflecting an adaptive behavior.

(3) D_y^q is expected to be compensated for in other quarters so that the entire year will be "normal." In this case, there will be a revision in a direction opposite to that of D_y^q, and λ_q is hypothesized to be smaller than one.

The empirical test was based on a random sample of 100 industrial firms from those appearing in Value Line Investment Survey in the four quarters of 1976. The results showed a clear adaptive behavior of FAF: The coefficients of D_y^q were significantly above one in all three quarters. [10] This conclusion is consistent with that of Brown and Rozeff [1979] who examined the behavior of quarterly forecasts: both studies found that the error in one quarter is perceived to contain a permanent component, thus inducing analysts to revise their forecasts for the new quarter, or for the remainder of the year, in the same direction. This pattern in FAF revisions is consistent with the time-series properties of quarterly earnings, indicating the utilization by analysts of information on past behavior of quarterly earnings.

The findings by Brown and Rozeff [1979] and by Abdel-khalik and Espejo [1978] relied on cross-sectional tests. However, the time series of earnings may vary across companies, and therefore earnings forecasts of different companies are likely to (and, in the case of rational forecasts, must) be formed according to different processes.[11] Furthermore, even if the process of expectation formation for all firms is adaptive, the coefficient of adaptation may vary across firms. Givoly [1982] tested the relationship between the formation by analysts of annual earnings forecasts and the last annual prediction error, through a time series over the years 1969-79. The tests were conducted for individual companies (with the mean forecast, computed over different contemporaneous forecasts, serving as the basic observation) as well as for individual forecasters.

The results suggest that in the vast majority of the companies the adaptive expectation model adequately represents the process by which forecasts of annual earnings are formed: The R^2 values were high (an average of 0.622), and the adaptation coefficients significant, between 0 and 1 in most cases. It is instructive to note, however, that the hypothesis of equality of the adaptation coefficients

---

[10] The following multivariate model was used by Abdel-khalik and Espejo [1978] to test their hypotheses:

$$F_y - A_y = \lambda_1 D_y^1 + \lambda_2 D_y^2 + \lambda_3 D_y^3 + e_y$$

where F_y is the forecasted annual EPS at the beginning of the year, A_y the realized annual EPS, and D_i the prediction error in forecasting the EPS of quarter i. This model was derived recursively from the univariate model described in the text of this paper. Abdel-khalik and Espejo tested each of the λ's against the null hypothesis $\lambda = 0$ rather than against Ho: $\lambda = 1$; this point was correctly made by Brown, Hughs, Rozeff, and Vanderweide [1980], who also contended that for econometric reasons, the univariate rather than the multivariate model should be tested. Nonetheless, the validity of Abdel-khalik and Espejo's findings was not impaired by this critique. This point is convincingly made in Abdel-khalik's comments [1980].

[11] In a recent methodological paper, Abdel-khalik [1982] examined the econometric properties of the univariate and the multivariate model discussed in Abdel-khalik and Espejo [1978] and in Brown et al. [1980]. He showed that both formulations had model specification and estimation problems that resulted in overfitting the models. Furthermore, he demonstrated that the R^2 of both models had considerably overstated the effect of the quarterly prediction errors on the revision of annual earnings forecasts. Despite the apparent model overfitting, the correct effect of quarterly prediction error was still significant.

among different companies was rejected. Similar results were reported for the adaptive coefficients of individual forecasters.

The study of the formation of analysts' forecasts is in its infant stages. The consistency that FAF revisions show with a simple adaptive model does not mean the model is the most appropriate to describe the formation of analysts' forecasts of earnings. More elaborate models may be examined. Furthermore, in the study of the time-series properties of FAF, there is a need for a theoretical framework, similar to that developed for the formation of inflationary expectations [see, for example, Cukierman and Wachtel, 1979; and Brunner, Cukierman, and Meltzer, 1980]. Such a framework would consider elements such as the loss function of the individual analysts, the time-series behavior of earnings, and the extent and reliability of exogeneous information available to analysts.

7. FAF AND STOCK PRICE BEHAVIOR

The relevance of the research on FAF and its most interesting implications stem, to a large extent, from the assumption that earnings forecasts by analysts are actually used by market participants. There is a considerable body of "circumstantial" evidence to suggest that this is indeed the case: Earnings forecasts, annual and sometimes quarterly, are disclosed by all major brokerage houses; many clients are ready to pay for forecasting services; and at least three organizations, S&P, Lynch Jones and Ryan, and Zacks and Co., issue a periodical summary of contemporaneous forecasts made by different analysts for a large number of companies.

Whether investors utilize the information conveyed by FAF is an empirical question. Several studies have examined the association between earnings forecasts and stock price behavior. The focus of these studies has varied, yet their conclusions seem to have the same tenor: Stock price movements are correlated with earnings forecasts and their revision thereof, issued by analysts. This section presents and discusses these findings.

7.1 THE INFORMATION CONTENT OF FAF

An early study by Niederhoffer and Regan [1972] analyzed the relationship between the error of analysts in predicting the earnings for 1970 and the performance of the respective stocks. Two groups of 50 stocks each were selected, one consisting of those with the worst stock market performance (lowest return) and the other of those with the best performance during 1970. The analysts consistently underestimated (in 89 percent of the cases) the earnings of the top firms and overestimated the earnings of *all* the firms at the hottom; in other words, earnings predictions formed hy analysts seem to he a useful signal to investors. Neiderhoffer and Regan concluded by saying that "these results present both challenge and opportunity for financial analysts. If their estimates are more accurate than the conventional published forecasts of large institutions, there is ample opportunity for differentiating between the best and worst-performing companies" (p. 71). The methodology and the design of Regan and Neiderhoffer study were rather crude: Only the extreme 100 cases (out of 1,253 common stock) in a single year were examined.[12]

Gonedes, Dopuch, and Penman [1976], in a study on the value of mandatory disclosure of management forecasts, conducted an empirical analysis of the information content of FAF which they used as a proxy for management forecasts. They used a sample of 148 firms, each represented by 24 biweekly earnings forecasts in each of the years 1967 and 1968 (the forecasts were collected from the *Earnings Forecaster*). Each firm was reassigned, every two weeks, to one of four portfolios, depending on the ratio of its earnings forecast to its price (observed ten days earlier). The return of each portfolio in the ten days surrounding the forecast disclosure was measured and compared to that of a control portfolio of equal risk. The results showed that the portfolio of the firms with the highest E/P ratio had an average return somewhat above that of an equally risky portfolio and that, in particular, the portfolio of the firms with the lowest E/P ratio had an average return significantly below that of the control portfolio. They concluded that "forecasted earnings per share seem to reflect information pertinent to valuing firm. It seems that this information content can be almost entirely ascribed to the unfavorable implications of an extremely low (scaled) forecast" [p. 127].

While their test of information content is not very powerful (the portfolio affiliation of a particular stock might not constitute new information; there is also a publication lag of the source document), Gonedes, Dopuch, and Penman's findings are in accord with other studies in suggesting that FAF have information content.

In a more direct test on the information content of FAF, Givoly and Lakonishok [1979] examined the response of the market to revisions in FAF. Using a sample of 49 firms from the *Earnings Forecaster*, Givoly and Lakonishok observed the stock price response to 1,420 revisions in FAF during the years 1967-74. The results revealed significant abnormal returns in the expected direction (i.e., positive or negative abnormal return associated with upward and downward revisions, respectively) in the month of the forecast revision, as well as in the month preceding it and the two months following it. The abnormal returns were quite substantial and positively related to the size of the revision: In the revision month and the two following months the abnormal return was 2.2 percent for all revisions and 4.5 percent for revisions over 10 percent [see ibid, Table 7]. Refinements to the basic design (exclusion of revisions made concurrent with earnings releases; different procedures for computing abnormal returns) left the results intact. These results strongly suggest that FAF do have information content. Furthermore, the slow response of the market to analyst's revision is inconsistent with the semistrong efficiency of the market.

In a followup work, Givoly and Lakonishok [1980] directly tested the extent to which investment strategies could be designed to exploit the publicly available

---

[12] Due to the exclusive attention to the 100 extreme cases, the same results could be produced by a variety of models; that is, if extreme price fluctuations are indeed correlated with extreme changes in earnings (i.e., earnings have information content), then the forecast error, in such cases, of other prediction models beside FAF would very likely yield a similar correlation with price changes.

information on revisions of analysts' forecasts. Portfolios consisting of stocks whose earnings have recently been revised upward systematically outperformed an equally risky random portfolio. Depending on the particular strategy selected, such a portfolio was shown to yield over 15 percent annual abnormal return, net of transaction cost [see ibid, Table 4].

In a more recent paper, Elton, Gruber, and Gultekin [1981] evaluated the degree of excess return that could be generated by utilizing information on, separately, consensus mean earnings forecasts, prediction errors of earnings forecasts, and revisions in earnings forecasts. The expectational data consisted of a monthly file of one- and two-year earnings forecasts prepared by analysts in the years 1973, 1974, and 1975, which was compiled by Lynch, Jones and Ryan (the Institutional brokers Estimate System). The final sample consisted of 913 and 696 one- and two-year forecasts, respectively, made at two forecast dates, March and September. The results showed that

> (1) No excess return could be made by the knowledge of the existing forecast; firms for which a high earnings growth was forecasted performed as well as firms with a low forecasted earnings growth. This finding is consistent with the stock market being efficient with respect to the publicly available earnings forecasts.
> (2) Significant excess returns were associated with the earnings prediction error. Furthermore, the amount of excess returns that could be earned varied with the magnitude of the forecast error. These results suggest that FAF have information content.
> (3) Significant excess returns were associated with changes in the analysts estimates. In fact, the return from forecasting accurately future forecasts themselves were somewhat higher than the return from being able to forecast actual earnings. The result is consistent with other evidence showing that it is consensus forecasts that determine security prices.

Abdel-khalik and Ajinkya [1982] examined whether both early knowledge of FAF revisions (possessed by select clients and analysts themselves) and published FAF revisions are reflected in security prices. The sample consisted of estimates revisions made by Merrill, Lynch, Pierce, Fenner and Smith, Inc., for optionable stocks during the period August 1977 to December 1978. These revisions were first announced internally (and to select clients) and made public in the first weekly *Options Alert* issued by that firm. The research was designed so as to enable testing of both the strong form and the semistrong form of the efficient market hypothesis. Specifically, the existence of a significant association between the content of the revision and stock price movements during the few days between its internal distribution and public disclosure would lead to a rejection of the "strong-form" hypothesis while the existence of such association well after the public disclosure of the revision would lead to a rejection of the "semistrong" hypothesis. The results showed that while the "strong-form" hypothesis was rejected, no abnormal return could be earned after the week of publication, a finding consistent with the "semi-strong-form" hypothesis.[13]

---

[13] The results concerning the semistrong hypothesis conflict with those reported by Givoly and Lakonishok [1979]. The following points should, however, be borne in mind. (1) Abdel-khalik and

7.2 FAF AS A SURROGATE FOR MARKET EXPECTATION OF EARNINGS

The findings of the studies on the association between the content of FAF and stock price movements lead basically to the same conclusion, namely, that FAF do have information content. The fact that the content of analysts' forecasts of earnings is associated with stock returns does not necessarily mean that FAF are the preferred surrogate for the unobservable market expectation of earnings. Other expectation models might better explain stock price behavior and, hence, more properly be viewed as the true representative of market expectation.

Considering the fact that FAF are, on average, more accurate than other tested models, and assuming that investors are rational, it is reasonable to assume that FAF represents better than other models the earnings expectation of the market.

The question whether FAF are a better expectational surrogate is important for several reasons. First, many studies, particularly those dealing with the information content of earnings, used some naive, or mechanical, models to generate the expected earnings and to measure "unexpected earnings." These studies could become more powerful if a better surrogate for earnings is identified. Second, stock valuation models as well as P/E studies often rely on expected earnings as a basic parameter. Better identification of market expectation would improve these models. Finally, establishing that FAF provide a satisfactory surrogate for market expectation would underscore the importance of studies on various properties of FAF (accuracy; rationality; time-series behavior) and provides motivation for further research in the area.

Two of the first studies to examine the adequacy of FAF as a surrogate for market expectations of earnings, relative to predictions based on past accounting data, were by Malkiel [1970] and Malkiel and Cragg [1970]. These studies attempted to explain the P/E ratio by a regression is which the growth rate, dividend yield, and risk measures were the independent variables. The future growth rate was estimated, once from historical long-term growth rates and once from an average predicted future long-term growth rate, of earnings-per-share. The first study used a sample of 178 companies from a cross section of industries in the years 1961-65; the second study concentrated on public utilities of which 33 were included in the sample covering the years 1961-67. The design of the two studies was similar.

To select the representative of the historically based growth estimates, 40 alternative predictors of growth were examined to find those that showed the closest correlation with market price-earnings multiples over each of the years covered by the studies. These growth rates differed with respect to the period of calculation, the method of calculation, and the financial data upon which the

---

Ajinkya's work relates to one forecaster only. (1) Previous evidence by Givoly and Lakonishok [1979] indicates clustering or "waves" of revisions, all of which are positively correlated. Thus, Merrill and Lynch's forecasts might not necessarily constitute new information to which the stock market is expected to respond. (3) As was pointed out by Abdel-khalik and Ajinkya, "companies with optionable stocks are large and the generalizability of the results to other companies will need further testing."

calculation was made. The ten-year growth rate of cash earnings per share was either clearly superior to, or at least no worse than, any of the others in each of the years and was therefore used in the yearly regressions. Needless to say, this procedure introduced a selection bias in the results in favor of finding a greater explanatory power of the historically based estimates. The analysts' were gathered from nine security firms, and their average was calculated to produce a single predictor.

Despite the aforementioned bias, the results in both studies showed that the regression fits were much better using the expectational variables than the historical ones. The average R^2 in Malkiel and Cragg's study [1970] was 0.75 and .49 (across five years) for the FAF-based and historically based growth estimates, respectively. The corresponding values reported in Malkiel's [1970] study (and averaged over four years) were 0.83 and 0.59. Based on these findings, Malkiel concluded that "a reasonable proxy has been obtained for what might be considered the expectations of the 'representative investor'" [p. 152].

In a recent study, Fried and Givoly [1982] evaluated FAF against naive models as a surrogate for market expectation of earnings. The comparison was based on the relationship between stock price movements and the signals (both the sign and the magnitude of the prediction error) produced by alternative expectation models. The model whose signals were the most strongly associated with stock price behavior was considered the best surrogate.

Analysts' forecasts for the 11 years 1969-79 were collected from the *Earnings Forecaster*. Considered each year were the FAF of that year's earnings outstanding at the beginning of April. Almost all forecasts were first issued to the public between the release of the annual report for the previous year and the first quarterly report. Sampled each year were companies for which at least four FAF were available (so that a meaningful average could be computed). Two naive expectation models were chosen: the submartingale (with drift) and the index model (for a description of the models, see Section 5.2).

The results showed that abnormal returns were more strongly correlated with the prediction errors of FAF than with the prediction errors of the two naive models. For instance, an investment strategy under which stocks were added to the portfolio on the basis of a foreknowledge of the direction and magnitude of FAF error was superior to that based on a foreknowledge of the prediction errors of each of the naive models (the first strategy yielded an average annual abnormal return of over 14 percent, and the strategies based on the naive models achieved less than 9 percent).

Analysts' forecasts appear to represent the earnings expectations of market participants more adequately than naive models. Still, few studies so far have used FAF to surrogate for market expections (among the few are Ajinkya and Gift [1983] and Givoly and Palmon [1982]). The superiority of FAF as an expectation surrogate does not invalidate the results of studies which used time-series (naive) models to find the association between unexpected earnings and unexpected share price movements (the information content of earnings). Rather, it reinforces these results by indicating that the association might even be stronger. The results provide added motivation for studying other important properties of FAF such as time-series behavior and cross-section dispersion.

7.3 CAUSES OF FAF SUPERIORITY

Fried and Givoly [1982] also analyzed the causes for the superiority of FAF over the naive models. Two such causes were hypothesized. (1) FAF use a broader information set which includes nonaccounting information on the firm, its industry, and the general economy, while naive models (and particularly those examined) rely exclusively on accounting information. (2) FAF have a timing advantage in that they are issued some time within the year being forecasted. Thus, they can use more recent information about the firm's earnings which becomes available only after the end of the fiscal year.

To test the effect of broadness of information on the relative performance of FAF, Fried and Givoly used the partial correlation $r_{AP \cdot X}$ where A is the realized earnings, P is FAF and X is the earnings predicted by the naive model. Values of $r_{AP \cdot X} > O$ suggest that FAF contain predictive power based not only on extrapolation but also on an autonomous component.

The results showed relatively high positive partial correlation coefficients: The average coefficient of the correlation between realization and FAF, given the naive prediction, was 0.55 and 0.56 for the comparison with the submartingale and the index model, respectively. The values remain high, 0.51 on average, when the correlation was conditioned on the predictions of both naive models. These values, which are significantly greater than zero, suggest that FAF utilize a considerable amount of information that is independent of the time-series and cross-sectional properties of the earning series that are captured by the two naive models.

To test the effect of the timing of the forecast, the performance of different subsamples of forecasts, each initially released in a different month, was compared and analyzed. As expected, forecasts released earlier showed a stronger association with price movements during the forecast year. However, the improvement between "early" forecasts (defined in the study as those released in January and February) and "late" forecasts (those released in March and early April) was not significant.

The idea that the timing advantage of a few weeks possessed by FAF is inconsequential to their overall performance is echoed also in the results obtained by Brown and Rozeff [1978]. They correlated Value Line forecasting error with the time interval since the most recent quarterly earnings announcement. The correlation was essentially zero, leading them to conclude that "Value Line superiority can be attributed to its use of the information set available to it on a quarterly earnings announcement date, and not to the acquisition of information arriving after the quarterly earnings announcement date" [p. 73].

The insignificance of the difference in the performance of analysts' forecasts made several weeks apart should not be confused with a lack of improved forecasting as the year's end approaches. To the contrary, the evidence shows that as the year progresses, the accuracy of FAF improves [see, for example, Crichfield, Dyckman, and Lakonishok, 1978; Collins and Hopwood, 1980; and Elton, Gruber, and Gultekin, 1981].

8. DISPERSION OF FAF

Most of the research on FAF has centered around the properties of the consensus, or the mean, forecast. Recently, attempts have been made to explore the information content of financial analysts' divergence of beliefs about future earnings. This attention to dispersion parallels that observed in the research on the expectations of economic variables. In particular, the dispersion of economists' forecasts of the inflation rate was examined and found to be an important determinant of the interest rate [see, for example, Barnea, Dotan, and Lakonishok, 1979; Levi and Makin, 1979; and Bomberger and Williams, 1981].

8.1 DISPERSION OF FAF AS A MEASURE OF RISK

Dispersion of earnings expectations, as measured by the cross-sectional variance (or standard deviation) of FAF, can be interpreted as an earnings uncertainty measure. Another uncertainty measure that has long been employed by academicians and practitioners in their attempts to model investor's behavior and evaluate stocks is earnings variability [see, for example, the use of this measure by Litzenberger and Rao, 1971; and Ahlers, 1972].

The idea that past volatility is only partially related to uncertainty surrounding future expectations has been recently developed by Cukierman and Wachtel [1982a, 1982b] (for the inflation variable) and Cukierman and Givoly [1982]. Cukierman and Givoly developed a model for the formation of earnings expectations whereby each forecaster, in making a prediction, employs both information common to all other forecasters (e.g., past earnings) and specific information. They showed that under fairly general conditions (pertaining primarily to the stability of the variances of the series), the cross-sectional error in earnings forecasts is the correct empirical counterpart of uncertainty, that is, of the dispersion of the distribution of expected earnings. Their model also implies (and this implication is confirmed by empirical tests) that the cross-sectional error is positively associated with the dispersion of forecasts across forecasters.

The alternative risk measures seem to be correlated. Givoly and Lakonishok [1983] found that the dispersion of earnings forecasts, as well as the predictability of earnings forecasts, is related to traditional risk measures such as systematic risk (beta), total risk (standard deviation of returns), and earnings growth variability. Cukierman and Givoly [1982] and Elton, Gruber, and Gultekin [1982] found that dispersion of FAF is positively related to the error in the consensus forecast of earnings.

Dispersion of earnings forecasts and earnings unpredictability are apparently perceived by investors as valuable information and as proxies for risk. Value Line publishes regularly the unpredictability rating of companies earnings; Standard and Poor's provides in its *Earnings Forecaster* a number of earnings forecasts for each of the approximately 1,500 companies listed in the publication, and the firm of Lynch, Jones, and Ryan, supplies investors with such measures as range and standard deviation of a multitude of contemporaneous earnings forecasts made by different financial analysts.

Friend, Westerfield, and Granito [1978] and Malkiel and Cragg [1980] used dispersion of expectations as an additional measure of risk. Friend, Westerfield, and Granito tried to explain a consensus expected return by several risk measures. The expected return was computed as the mean forecast of seven financial institutions. Three independent risk variables were tested. The first two were the traditional risk variables, beta and the residual standard deviation of returns. The interesting variable was the third one, a measure of heterogeneity of expectations derived from expected stock returns from various institutions. The empirical results revealed that the measure of heterogeneity of expectations was the most consistent variable in explaining expected returns. When actual returns instead of expected returns were used as the dependent variable, the results remained qualitatively the same. The measure used by Friend, Westerfield, and Granito is conceptually similar to the dispersion measure based on earnings expectations. Malkiel [1981], in a test similar to the one performed by Friend, Westerfield, and Granito, used dispersion of earnings expectation as one of his explanatory variables. Additional explanatory variables were beta, economy risk, inflation risk, and interest rate risk. The last three variables measure the sensitivities of given stock to movements in National Income, CPI, and market interest rates. The dependent variable was defined as the expected rate of return and derived from the dividend valuation model. Malkiel concluded that

> The best single risk proxy is not the traditional beta calculation but rather the dispersion of analysts' forecasts. . . Companies for which there is a broad consensus with respect to future earnings and dividends seem to be less risky (and hence have lower expected returns) than companies for which there is little agreement among security analysts.

Givoly and Lakonishok [1983] examined the effect of earnings uncertainty, as measured by dispersion of earnings expectations and earnings unpredictability, on the information content of earnings. Their sample consisted of over 1,200 cases (company-years), each represented by at least four forecasts. The data source for FAF was the *Earnings Forecaster* in the years 1969-79. The methodology involved the testing of a regression in which the abnormal return in the period surrounding the earnings release was the dependent variable and the prediction error and the cross-sectional dispersion and forecast error of FAF the independent variables.

The results showed that the response to unexpected earnings depends on the dispersion (uncertainty) of the earnings forecasts. In general, when uncertainty concerning future earnings is great, the stock price movement triggered by a given prediction error (unexpected earnings) is relatively small.

8.2 THE PATTERN OF FAF DISPERSION OVER TIME

The pattern of the FAF dispersion during the forecast year was examined by Crichfield, Dyckman, and Lakonishok [1978] and by Elton, Gruber, and

Gultekin [1982]. The former reported a slight tendency of the cross-sectional standard deviation of FAF to decline as the end of the year is approached (though this tendency was in most years insignificant at 5 percent significance level). This finding is quite interesting since the accuracy of these estimates increased continuously as the year's end approached. They found no convincing explanation for this puzzling result. Collins and Hopwood [1980] suggested that the stability over time in the divergence of analysts' estimates is due to the very small number of outliers among FAF, which reflects analysts' ability to incorporate exogenous information in their forecasts.

Elton, Gruber, and Gultekin [1982] found a decline in FAF's dispersion over the first four months of the forecast year, but no further reduction in the remaining eight months. The apparent conflict with respect to FAF behavior over the first four months between Crichfield, Dyckman, and Lakonishok and by Elton, Gruber and Gultekin might be due to the different data sources. While the latter used processed data (the standard deviations) available from Lynch, Jones, and Ryan (the *IBES* Service), the former used raw data on individual forecasts (from S&P's *Earnings Forecaster*). Corrections to the data due to illogical values, etc., which would and probably have been done by the latter, could not be performed by Elton, Gruber, and Gultekin who used the ready statistics. On the other hand, they used a more comprehensive sample—over 400 companies—each represented by 3 to 20 concurrent forecasts each year, while Crichfield, Dyckman, and Lakonishok sampled only 46 companies with few concurrent forecasts for each company-year. Additional research in the area is necessary to resolve the conflicting findings.

9. CONCLUDING REMARKS

The last two decades have witnessed a growing interest in the formation and characteristics of expectations of economists and investors. Given the important role that earnings numbers should theoretically play in stock valuation, and the overwhelming empirical evidence that earnings do indeed possess an information content, it is clear why earnings forecasts have attracted much research effort.

The research on FAF in recent years has been stimulating with rich implications for the behavior of investors, the usefulness of earnings numbers, and the competence of analysts. The findings show that FAF performance is, in general, superior to that of naive models. This result is consistent with a rational market for forecasting services, where the higher cost of FAF is compensated by a better performance.

An important property of FAF is their rationality: FAF were found to incorporate the past history of realizations and predictions in an unbiased manner. It is interesting to note that this property is not exhibited by economists in their prediction of variables such as inflation, GNP, or unemployment.

Various studies provide evidence that investors use FAF and, in fact, behave as if they form their own expectations on the basis of FAF. The finding that FAF can serve as a reasonable surrogate for the (unobservable) market expectation of earnings may help future studies that rely on knowledge of earnings expectation.

The finding also underscores the importance of the research on FAF to our understanding of the operation of the market.

The study of the dispersion of FAF provides an interesting, yet not fully modeled, result: that divergence of earnings expectations is an important measure of risk, shadowing the traditional risk variables such as security beta or the variability of the return.

There are many questions important to our understanding of the way FAF are formed and used that have not yet been addressed. We do not have a good enough knowledge of the forecasting process. We know something about the revision process that takes place whenever new quarterly reports are published, but we do not know how extrapolative data are synthesized with other information nor how marketwide factors (inflation, interest rate, GNP, etc.) are incorporated in the earnings predictions. Little is also known about the degree of uniqueness of the information used by the individual analyst. Do analysts truly possess inside information or do they rely basically on a common body of knowledge? Do they use each other's forecast as an important input? An interesting work in this respect is that by Lees [1981], in which certain aspects of the symbiosis of analysts and corporate managers were analyzed.

An important dimension of the forecaster's behavior is his loss function. This function must relate to the way forecasts are evaluated. Do brokerage houses measure the performance of their forecasters? Given the complexity of this task (e.g., how to control for uncontrollable states of nature or how to compare performance of forecasts made for different firms), it is possible that many institutions do not even attempt to carry it out. The knowledge of the forecaster's loss function can provide us with an understanding of the nature of the point estimate provided by him—is it likely to be the mean, the median, or some other measure of the expected earnings distribution?

The analysis of the accuracy of FAF relied, in most studies on the performance of the mean forecast. No attempt has been made to explore quality differentials among analysts. Is there a superior forecaster? Such a finding might be inconsistent with rational behavior of investors. Another important question is whether brokerage houses specialize in certain industries or firms and, if so, does the specialization result in a better performance?

Another interesting issue is the degree by which the market index of earning and, indirectly, stock market movements could be accurately predicted from individual companies' forecasts of earnings. It was found, for example, that investors could benefit from the knowledge of revisions in FAF made for individual companies. Could they similarly benefit from the knowledge on the aggregate (cross-sectional) behavior of FAF?

These unresolved questions make this research area lively and rewarding for both theoreticians and empiricists interested in the operation of the financial analysts' industry, the formation of investors' expectations and the interaction between accounting numbers and stock behavior.

REFERENCES

Abdel-khalik, A., "Three Generations of Research on Quarterly Reports: Some Thoughts on the Research Process," *Perspective on Accounting Research*, ed. by Nair and Williams, College of Business, University of Wisconsin, 1980.

Abdel-khalik, A, "Overfitting Bias in the Models Assessing the Predictive Power of Quarterly Reports," working paper 82-2, Accounting Research Center, University of Florida (January 1982).

Abdel-khalik, A. and B. Ajinkya, "Returns to Informational Advantages: The Case of Analysts' Forecast Revisions," *Accounting Review* (October 1982), pp. 661-80.

Abdel-khalik, A. and J. Espejo, "Expectations Data and the Predictive Value of Interim Reporting," *Journal of Accounting Research* (Spring 1978), pp. 1-13.

Abdel-khalik, A. and R. Thompson, "Research on Earnings Forecasts: The State of the Art," *Accounting Journal* (Winter 1977-78), pp. 180-209.

Agmon, T. and Y. Amihud, "The Forward Exchange and the Prediction of the Future Spot Rate: Empirical Evidence," *Journal of Banking and Finance* (November 1981), pp. 425-43.

Ahlers, D., "SEM: A Security Evaluation Model," reprinted in Elton and Gruber, *Security Evaluation and Portfolio Analysis* (Englewood Cliffs, NJ: Prentice-Hall, 1972) pp. 227-44.

Ahlers, D. and J. Lakonishok, "A Study of Economists' Consensus Forecasting," *Management Science* (October 1983), pp. 1113-25.

Ajinkya, B. and M. Gift, "Corporate Managers' Earnings Forecasts and Symmetrical Adjustments of Market Expectations," *Journal of Accounting Research* (Autumn 1984) forthcoming.

Albrecht, S., L. Lookabill, and J. McKeown, "The Time Series Properties of Earnings," *Journal of Accounting Research* (Autumn 1977), pp. 226-44.

Ball, R. and P. Brown, "An Empirical Evaluation of Accounting Index Numbers," *Journal of Accounting Research* (Autumn 1968), pp. 159-78.

Barefield, R. and E. Comiskey, "The Accuracy of Analysts' Forecasts of Earnings Per Share," *Journal of Business Research* (July 1975), pp. 241-52.

Barnea, A., A. Dotan, and J. Lakonishok, "The Effects of Price Level Uncertainty on the Determination of Nominal Interest Rates: Some Empirical Evidence," *Southern Economic Journal* (October 1979), pp. 609-14.

Basi, B., K. Carey, and R. Twark, "A Comparison of the Accuracy of Corporate and Security Analysts' Forecasts of Earnings," *Accounting Review* (April 1976), pp. 244-54.

Beaver, W., *Financial Reporting, The Accounting Revolution* (Englewood Cliffs, N.J.: Prentice-Hall, 1981).

Bierwag, G. and M. Grove, "Aggregate Koyck Functions," *Econometrica* (October 1966), pp. 828-32.

Bomberger, W. and W. Frazer Jr., "Interest Rates, Uncertainty and Livingston Data," *Journal of Finance* (June 1981), pp. 661-75.

Brooks, L. and D. Buckmaster, "Further Evidence of the Time Series Properties of Accounting Income," *Journal of Finance* (December 1976), pp. 1359-73.

Brown, L. and M. Rozeff, "The Superiority of Analyst Forecasts as Measures of Expectations: Evidence from Earnings," *Journal of Finance* (March 1978), pp. 1-16.

Brown, L. and M. Rozeff, "Adaptive Expectations, Time Series Models and Analyst Forecasts Revisions," *Journal of Accounting Research* (Autumn 1979), pp. 341-51.

Brown, L. and J. Hughes, M. Rozeff, and J. Vanderweide, "Expectation Data and the Predictive Value of Interim Reporting: A Comment," *Journal of Accounting Research* (Spring 1980), pp. 278-88.

Brunner, K., A. Cukierman, and A. Meltzer, "Stagflation, Persistent Unemployment and Economic Shocks," *Journal of Monetary Economics* 6 (1980), pp. 467-92.

Cargill, T., "Anticipated Price Changes and Nominal Interest Rates in the 50's," *Review of Economics and Statistics* (August 1976), pp. 1-16.

Chang, L. and K. Most, "Financial Statements and Investment Decisions," (manuscript, Florida International University, Miami, 1980).

Collins, W. and W. Hopwood, "A Multivariate Analysis of Annual Earnings Forecasts Generated from Quarterly Forecasts of Financial Analysts and Univariate Time Series Models," *Journal of Accounting Research* (Autumn 1980), pp. 340-406.

Cragg, J. and B. Malkiel, "The Consensus and Accuracy of Some Predictions of the Growth of Corporate Earnings," *Journal of Finance* (March 1968), pp. 67-84.

Crichfield, T., T. Dyckman, and J. Lakonishok, "An Evaluation of Security Analysts' Forecasts," *Accounting Review* (July 1978), pp. 651-68.

Cukierman, A. and D. Givoly, "Heterogeneous Earnings Expectations and Earnings Uncertainty—Theory and Evidence," working paper, Tel Aviv University (1982).

Cukierman, A. and P. Wachtel, "Differential Inflationary Expectations and the Variability of the Rate of Inflation: Theory and Evidence," *American Economic Review* (September 1976), pp. 595-609.

Cukierman, A. and P. Wachtel, "Relative Price Variability and Non-Uniform Inflationary Expectations," *Journal of Political Economy* (February 1982), pp. 146-58.

Cukierman, A. and P. Wachtel, "Inflationary Expectations: Reply and Further Thoughts on Inflation Uncertainty," *American Economic Review* (forthcoming).

Elton, E. and M. Gruber, "Earnings Estimate and the Accuracy of Expectational Data," *Management Science* (April 1972), pp. 409-24.

Elton, E., M. Gruber, and M. Gultekin, "Expectations and Share Prices," *Management Science* (September 1981), pp. 975-87.

Elton, E., M. Gruber, and M. Gultekin, "Professional Expectations: Accuracy and Diagnosis of Errors," *Journal of Financial and Quantitative Analysis* (forthcoming).

Fama, E., "Short Term Interest Rates as Predictors of Inflation," *American Economic Review* (June 1975), pp. 269-82.

Fama, E., "Forward Rates as Predictors of Future Spot Rates," *Journal of Financial Economics* (October 1976), pp. 361-77.

Figlewski, S., "Optimal Price Forecasting Using Survey Data," working paper 167, New York University, Solomon Brothers Center for the Study of Financial Institutions (revised May 1980).

Figlewski, S. and P. Wachtel, "The Formation of Inflationary Expectations," *Review of Economics and Statistics* (September 1981), pp. 1-10.

Foster, G., "Quarterly Accounting Data: Time Series Properties and Predictive Ability Results," *The Accounting Review* (January 1977), pp. 1-21.

Frenkel, J., "Inflation and the Formation of Expectations," *Journal of Monetary Economics* I (October 1975), pp. 403-21.

Fried, D. and D. Givoly, "Financial Analysts' Forecasts of Earnings: A Better Surrogate for Earnings Expectations," *Journal of Accounting and Economics* (October 1982), pp. 85-107.

Friedman, B., "Optimal Expectations and the Extreme Information Assumptions of 'Rational Expectations' Macro Models," *Journal of Monetary Economics* (January 1979), pp. 23-41.

Friedman, M., *A Theory of the Consumption Function* (New York, NY: National Bureau of Economic Research, 1957).

Friend I., R. Westerfield, and M. Granito, "New Evidence on the Capital Asset Pricing Model," *Journal of Finance* (June 1978), pp. 903-20.

Gibson, W., "Interest Rates and Inflationary Expectations: New Evidence," *American Economic Review* (December 1972), pp. 845-65.

Givoly, D., "The Formation of Earnings Expectations," working paper, Faculty of Management, Tel Aviv University (October 1982).

Givoly, D. and J. Lakonishok, "The Information Content of Financial Analysts' Forecasts of Earnings," *Journal of Accounting and Economics* (Winter 1979), pp. 165-85.

Givoly, D. and J. Lakonishok, "Financial Analysts' Forecasts of Earnings: Their Value to Investors," *Journal of Banking and Finance* (September 1980), pp. 221-33.

Givoly, D. and J. Lakonishok, "Divergence of Earnings Expectations: The Effect on Market Response to Earnings Signals," working paper 768/83, Faculty of Management, Tel Aviv University (February 1983).

Givoly, D. and D. Palmon, "Timeliness of Annual Earnings Announcements: Some Empirical Evidence," *Accounting Review* (July 1982), pp. 486-508.

Gonedes, N., "Properties of Accounting Numbers: Models and Tests," *Journal of Accounting Research* (Autumn 1973), pp. 212-37.

Gonedes, N., "Capital Market Equilibrium and Annual Accounting Numbers: Empirical Evidence," *Journal of Accounting Research* (Spring 1974), pp. 26-62.

Gonedes, N., N. Dopuch, and S. Penman, "Disclosure Rules, Information-Production, and Capital Market Equilibrium: The Case of Forecast Disclosure Rules," *Journal of Accounting Research* (Spring 1976), pp. 89-137.

Granger, C. and P. Newbold, *Forecasting Economic Time Series* (New York, NY: Academic Press, 1977).

Griffin, P., "The Time-Series Behavior of Quarterly Earnings: Preliminary Evidence," *Journal of Accounting Research* (Spring 1977), pp. 71-83.

Imhoff, E. Jr., and P. Paré, "Analysis and Comparison of Earnings Forecast Agents," *Journal of Accounting Research* (Autumn 1982), pp. 429-39.

Jaggi, B., "Further Evidence on the Accuracy of Management Forecasts vis-à-vis Analysts' Forecasts," *Accounting Review* (January 1980), pp. 96-106.

Lahiri, K., "Inflationary Expectations: Their Formation and Interest Rate effects," *American Economic Review* (March 1976), pp. 124-31.

Lakonishok, J., "Stock Market Expectations: Some General Properties," *Journal of Finance* (September 1980), pp. 921-31.

Lees, F., "Public Disclosure of Corporate Earnings Forecasts," Research Report (New York, NY: The Conference Board, 1981).

Levi, M. and J. Makin, "Inflation Uncertainty and the Phillips Curve: Some Empirical Evidence," *American Economic Review* (December 1980), pp. 1022-27.

Litzenberger, R. and C. Rao, "Estimates of the Marginal Rate of Time Preferences and Average Risk Aversion of Investors in Electric Utility Shares: 1960-1966," *The Bell Journal of Economics and Management* (Spring 1971), pp. 365-77.

Lynch, Jones, and Ryan, *Institutional Brokers Estimate System* (IBES) (a monthly publication, New York).

McDonald, C., "An Empirical Evaluation of the Reliability of Published Predictions of Future Earnings," *Accounting Review* (July 1973), pp. 502-10.

Malkiel, B., "The Valuation of Public Utility Equities," *The Bell Journal of Economics and Management* (Spring 1970), pp. 143-60.

Malkiel, B., "Risk and Return: A New Look," working paper,Lynch, Jones, and Ryan, New York (April 1981).

Malkiel, B. and J. Cragg, "Expectations and the Structure of Share Prices," *American Economic Review* 60 (1970), pp. 601-17.

Malkiel, B. and J. Cragg, "Expectations and the Valuation of Shares," working paper 471 (April 1980), National Bureau of Economic Research.

Mincer, J. and V. Zarnowitz, "The Evaluation of Economic Forecasts," ed. by J. Mincer, *Economic Forecasts and Expectations* (New York, NY: National Bureau of economic Research, 1969).

Mussa, M., "Adaptive and Regressive Expectations in a Rational Model of Inflationary Process," *Journal of Monetary Economics* (October 1975), pp. 299-306.

Muth, J., "Rational Expectations and the Theory of Price Movement," *Econometrica* (July 1961), pp. 315-35.

Neiderhoffer V. and P. Regan, "Earnings Changes, Analysts' Forecasts and Stock Prices," *Financial Analysts Journal* (May-June 1972), pp. 65-71.

Nerlove, M., "Adaptive Expectations and Cobweb Phenomena," *Quarterly Journal of Economics* (May 1958), pp. 227-40.

Pesando, J., "A Note on the Rationality of the Livingston-Price Expectations," *Journal of Political Economy* (February 1975), pp. 849-58.

Pyle, D., "Observed Price Expectations and Interest Rates," *Review of Economics and Statistics* (August 1972), pp. 275-80.

Richards, M., "Analysts Performance and the Accuracy of Corporate Earnings forecasts," *Journal of Business* (July 1976), pp. 350-57.

Ruland, W., "The Accuracy of Forecasts by Management and by Financial Analysts," *The Accounting Review* (April 1978) pp. 439-47.

Solow, R., *Price Expectations and the Behavior of the Price Level* (Manchester, England: Manchester University Press, 1969).

Standard and Poor's *Earnings Forecaster* (A weekly publication, New York).

Theil, H., *Applied Economic Forecasting* (Amsterdam, Holland: North-Holland Publishing Company, 1966).

Turnovsky, S., "Some Empirical Evidence on the Formation of Price Expectations," *Journal of the American Statistical Association* (December 1970), pp. 1441:54.

Watts, R., "The Time-Series Behavior of Quarterly Earnings," working paper, Department of Commerce, University of Newcastle (April 1975).

Watts, R. and R. Leftwich, "The Time-Series of Annual Accounting Earnings," *Journal of Accounting Research* (Autumn 1977), pp. 253-71.

Zacks Investment Research Inc.: *The Icarus Services* (a data base service) Chicago.

Zarnowitz, V., "An Analysis of Annual and Multiperiod Quarterly Forecasts of Aggregate Income, Output and the Price Level," *Journal of Business* (January 1979), pp. 1-34.

Are Financial Analysts' Forecasts of Corporate Profits Rational?

Michael P. Keane and David E. Runkle

University of Minnesota and Federal Reserve Bank of Minneapolis

This paper develops generalized method-of-moments tests for the rationality of earnings per share forecasts made by individual stock analysts. We fail to reject the hypothesis of rationality as long as we take into account two complications: (1) the correlation in a given period of analysts' forecast errors in predicting earnings for firms in the same industry and (2) discretionary asset write-downs, which affect earnings but are intentionally ignored by analysts when they make earnings forecasts. Our results challenge earlier work by De Bondt and Thaler and by Abarbanell and Bernard that found irrationality in analysts' forecasts.

I. Introduction

A substantial literature exists in accounting and finance that examines the properties of financial analysts' forecasts of corporate earnings. Researchers have been interested in analysts' forecasts for a variety of reasons, and we consider three here.

One reason is that asset pricing and cost-of-capital models generally involve earnings expectations variables for which proxies must be provided if these models are to be tested empirically or imple-

We thank the Institutional Brokers Estimate System (I/B/E/S) for the use of the I/B/E/S analyst data and Dan Chin and Joe Piepgras for superb research assistance. We are grateful for the insightful comments of an anonymous referee, Lars Hansen, Lane Daley, and Patricia McKernon Runkle and seminar participants at the Federal Reserve Bank of Minneapolis, the University of Minnesota, and the University of British Columbia. The views expressed herein are those of the authors and not necessarily those of the Federal Reserve Bank of Minneapolis or the Federal Reserve System.

[*Journal of Political Economy*, 1998, vol. 106, no. 4]
 0022-3808/98/0604-0007$02.50

mented in practice. Using time-series models to provide such proxies is common, but these proxies suffer from two problems. First, they may be less accurate than actual market expectations because they incorporate only a small set of information (i.e., lagged values of earnings and other variables). Second, when time-series models are used to generate expectations, any test of the asset pricing or cost-of-capital model under consideration becomes a joint test of the model of interest and the time-series model of expectations.

Given these two problems, a number of authors have shown interest in the properties of analysts' forecasts both because they may provide a superior proxy for market expectations and because, if one accepts their validity, one may construct direct tests of the asset pricing or cost-of-capital model that are of interest, while treating expectations as given. Examples of papers motivated by this line of interest are the following: (1) studies that have examined the accuracy of analysts' forecasts and, in particular, whether they are more accurate than forecasts from simple time-series models, such as those by Cragg and Malkiel (1968), Elton and Gruber (1972), Brown and Rozeff (1978), Crichfield, Dyckman, and Lakonishok (1978), Collins and Hopwood (1980), Fried and Givoly (1982), Elton, Gruber, and Gultekin (1984), and O'Brien (1988, 1990); and (2) studies that have examined the extent to which share price movements are associated with analysts' forecast revisions and forecast errors, such as those by Ball and Brown (1968), Beaver, Clarke, and Wright (1979), Givoly and Lakonishok (1979), Fried and Givoly (1982), Brown et al. (1987), Hughes and Ricks (1987), O'Brien (1988), and Lys and Sohn (1990).

A second reason for interest in analysts' forecasts is that if these forecasts do measure market expectations, then evidence of excess volatility or irrationality in analysts' expectations may help to explain what some researchers argue are excessively volatile asset price movements or anomalous market behavior. This line of research is exemplified by the work of De Bondt and Thaler (1985, 1990) and, later, by the work of Klein (1990), Abarbanell (1991), Mendenhall (1991), Abarbanell and Bernard (1992), and Ali, Klein, and Rosenfeld (1992).

A third reason for interest in analysts' forecasts is that they may provide a rare opportunity to test the rational expectations hypothesis. We doubt that data on expectations measure agents' true expectations unless those data are subject to some type of market test (see Keane and Runkle 1990). But since financial analysts' livelihoods depend on the accuracy of their forecasts and since we observe the same forecasts that the analysts sell, we can plausibly argue that these numbers accurately measure the analysts' expectations. Studies that examine whether analysts' forecasts have the properties of rational forecasts (i.e., that test for unbiasedness or efficiency or both) are those by Crichfield et al. (1978), Fried and Givoly (1982), Givoly (1985), O'Brien (1988), De Bondt and Thaler (1990), Klein (1990), Abarbanell (1991), Mendenhall (1991), Abarbanell and Bernard (1992), Ali et al. (1992), and Xiang (1992). There is also a related literature in economics on testing the rationality of forecasts, as illustrated by Brown and Maital (1981), Figlewski and Wachtel (1981), Zarnowitz (1985), Frankel and Froot (1987), and Keane and Runkle (1990).

In this paper, we provide a new analysis of analysts' forecasts that is most closely connected to the second and third lines of research. Specifically, we test the rationality of individual analysts' earnings forecasts as reported in the Institutional Brokers Estimate System (I/B/E/S) data set. Although many studies have already examined this issue, we justify yet another on the basis that the issue of cross-sectional correlation in analysts' forecast errors has not yet been fully addressed.

Several authors (esp. Crichfield et al. 1978; Bernard 1987; O'Brien 1988; Abarbanell 1991; Abarbanell and Bernard 1992) have noted that statistical inference about the properties of analysts' forecasts is very difficult if forecast errors are correlated across forecasters or firms. If, at time t, multiple analysts forecast time $t + 1$ earnings for a firm, their forecast errors will tend to be positively correlated as long as unanticipated shocks to earnings occur between t and $t + 1$. The same is true if these analysts forecast earnings for multiple firms and if shocks occur between t and $t + 1$ that affect all firms similarly. Any test of unbiasedness or efficiency that makes use of data on multiple forecasters or multiple firms will tend to overreject the null hypothesis if such positive correlations are ignored.

In this paper, we develop a generalized method-of-moments (GMM) estimator that gives correct statistical inference in the presence of complex patterns of correlation across analysts in their forecast errors. We show that failure to account for these correlations leads to overwhelming rejections of unbiasedness and efficiency in the I/B/E/S data but that a correct statistical inference (accounting for these correlations) is that unbiasedness and efficiency cannot be rejected. Note that we cannot reject the hypothesis that analysts fully incorporate into their earnings forecasts the information contained in both lagged earnings reports and lagged stock price behavior. Thus many of the rejections of rationality of analysts' forecasts that have been published appear to be due solely to downward-biased standard errors.

II. Our Work versus Related Literature

Some previous studies have attempted to deal with the problem of correlated errors across forecasters or firms. To our knowledge, Crichfield et al. (1978) first noted the problem. They stated that "at any point in time, forecasts for all companies may be cross-sectionally correlated due to aggregate market events" and that "a relatively long time span is required to test the ability of SA's [security analysts] to estimate the mean of the EPS (earnings per share) distribution" (p. 653). In their empirical work, Crichfield et al. used data on the mean of analyst forecasts of annual earnings for 46 firms in the years 1967–76 from the *Standard and Poor's Earnings Forecaster.*

Such a short time period may not be adequate for tests of rationality if large aggregate shocks occur that affect many companies. If aggregate shocks are important, then mean forecast errors (defined as actual EPS minus the mean EPS forecast) will tend to be positive or negative for individual years and will have mean zero only over time (not over firms at a point in time). This is why Crichfield et al. stated that "studies based on a comparison of realizations with forecasts over a short time horizon are likely to be deficient" (p. 653). At the time they did their analysis, the *Earnings Forecaster* data were available for only 10 annual observations. Even with this short a time period, they could not reject unbiasedness of the mean forecast. However, as we shall show below, with only 10 time periods, even one large aggregate shock could cause a rejection of unbiasedness. Considerably longer time spans are necessary to avoid sensitivity to this type of problem. Fried and Givoly (1982) also studied unbiasedness of the mean forecasts of annual earnings from the *Earnings Forecaster,* using data on 424 firms for the 1969–79 period. They found that the mean forecast is biased upward. However, since the number of time periods is only 10, this result may be due to aggregate shocks during the sample period, as Crichfield et al. suggest.

O'Brien (1988) studied annual EPS forecasts of analysts in the I/B/E/S data set for the 1975–81 period, which gave seven annual observations. The sample in her analysis has data on 184 firms and 1,260 firm years. O'Brien was apparently the first to deal with aggregate shocks by allowing for random period-specific shocks when testing for unbiasedness, a procedure that we generalize below. She finds weak evidence that forecasts are upward-biased (i.e., too optimistic) but correctly observes that

> an alternative explanation consistent with these results is that analysts issue unbiased forecasts, but this seven-year period, 1975 through 1981, is one with primarily negative

> unanticipated EPS. Unfortunately, the most obvious way to distinguish between the hypothesis of deliberate optimistic bias and this alternative is to collect data for a longer span of years. This is not possible with the I/B/E/S detail data. [P. 65]

In this paper, we extend O'Brien's work on the I/B/E/S data in three ways. First, we use the I/B/E/S data on quarterly earnings forecasts from the fourth quarter of 1983 to the fourth quarter of 1991 in order to achieve a time-series length of 33 periods.[1] This greater time span should reduce the sensitivity of our results to aggregate shocks. As an example, suppose that analysts' annual EPS forecasts were generally overly optimistic for 1975 because the severity of the recession was not anticipated in late 1974. Nevertheless, by the end of the first quarter of 1975, the severity of the recession was apparent, so the quarterly earnings forecasts for the second through fourth quarters should not have been overly optimistic. Second, we allow for firm-specific as well as aggregate shocks. Third, we develop a GMM estimator that allows us to test for efficiency as well as unbiasedness while taking into account both aggregate and firm-specific shocks.[2]

In a pair of recent papers examining analyst forecast rationality, Abarbanell (1991) and Abarbanell and Bernard (1992) both test for unbiasedness and efficiency using the most recent analyst forecast from the Value Line Investment Survey. Abarbanell studied quarterly forecasts for the years 1981–84 for 100 firms and found that the mean forecast error is negative (an overestimate) and that a positive correlation exists between prior share price changes and analysts' forecast errors (i.e., a positive [negative] price change increases the probability of a low [high] earnings forecast). Abarbanell and Bernard studied quarterly forecasts for 178 firms in the 1976–86 period, giving a time-series length of 44 periods. They found that the Value Line analysts' forecast errors are positively autocorrelated for the first three quarterly lags (i.e., they do not efficiently utilize the information in their lagged errors), that unbiasedness can be rejected because analysts are overly optimistic, and that analysts' errors are positively correlated with the lagged change in earnings (i.e., analysts underreact to earnings changes). However, as Abarbanell and Bernard state,

[1] Quarterly I/B/E/S data started in 1983, even though annual data were available earlier.

[2] The problems for statistical inference created by aggregate shocks have also been discussed by Bernard (1987).

> the standard errors should be interpreted with caution, given that the assumption of independence across firms is almost certainly violated. . . . Cross-sectional dependence is of concern . . . because all firms are affected by economy-wide movements. However, given the limited number of time series observations available here, relative to the number of firms, standard techniques for adjusting for cross-sectional dependence are not feasible. [P. 1188]

One contribution of our paper is to provide a GMM technique to adjust for cross-sectional dependence that is feasible for this type of data.

III. Econometric Issues

Suppose that analyst n makes a forecast in time t of EPS for firm j in period $t + 1$. We shall denote that forecast as ${}_t\mathrm{EPS}^j_{n,t+1}$. We wish to test whether such an analyst's predictions are rational in Muth's (1961) sense, that is, that they are equal to the mathematical expectation of actual EPS, conditional on the information available to analyst n at time t. In other words,

$$ {}_t\mathrm{EPS}^j_{n,t+1} = E(\mathrm{EPS}^j_{t+1} | I_{n,t}), \tag{1}$$

where EPS^j_{t+1} is actual EPS for firm j in period $t + 1$, $I_{n,t}$ is the information available to analyst j at time t, and E is the mathematical expectations operator.

Note that if all analysts have the same loss function, private information accounts for the differences in forecasts among analysts. Under that condition, if analysts all had exactly the same information, they would make the same forecast. Otherwise their forecasts would not be rational.

For an individual analyst, a test of forecast rationality can be performed by running the regression

$$ \mathrm{EPS}^j_{t+1} = \alpha_0 + \alpha_1 {}_t\mathrm{EPS}^j_{n,t+1} + \alpha_2 X_{n,t} + \epsilon^j_{n,t+1}, \tag{2}$$

where $X_{n,t}$ is any variable known to analyst n at time t. *Unbiasedness* implies that in a regression without $X_{n,t}$ variables, the coefficients in equation (2) may be restricted to $\alpha_0 = 0$ and $\alpha_1 = 1$. Efficiency requires that any variable known by n at time t should have no predictive power in the regression; that is, $\alpha_2 = 0$ (in addition to $\alpha_0 = 0$ and $\alpha_1 = 1$).

At least two reasons can be given to explain why regression tests for unbiasedness and efficiency could lead to rejections, even if analysts were rational in forming their expectations. First, analysts may not have symmetric loss functions. They may be penalized more for a large overprediction than for a large underprediction. Second, aggregate shocks may cause the sample mean forecast error for an individual to be nonzero for a finite T. In either of these cases, we could reject forecast rationality, even though analysts made optimal forecasts given their information sets and their loss functions.[3]

We could test analyst forecast rationality by randomly selecting one analyst and one firm. If we did so, we could estimate equation (2) by ordinary least squares (OLS). This sampling method will give test statistics that are consistent in T, the number of time periods for which analysts' forecasts are observed. But we may want to improve the power of our tests by including forecasts from multiple analysts for multiple firms. However, if we include these additional observations, our statistical inference will be invalid unless we correctly model the covariance of forecast errors across analysts and across firms.

We address this issue of error covariance in two parts. First, we discuss the individual analyst's information set and the intertemporal correlation of forecast errors for the individual analyst. Second, we discuss how forecast errors are correlated across analysts and across firms.

We shall now consider what is contained in the information set of analyst n in period t. Certainly, any public information known at time t, such as previous earnings announcements by the firm, should be known to the analyst. Such public information should certainly be orthogonal to $\epsilon^j_{n,t+1} \equiv \text{EPS}^j_{t+1} - {}_t\text{EPS}^j_{n,t+1}$, the analyst's one-step-ahead forecast error in predicting EPS for firm j. In addition to public information, equation (1) implies that any private information that the analyst had at time t, such as the analyst's own prior forecasts and forecast errors, should also be orthogonal to $\epsilon^j_{n,t+1}$. And if other analysts' forecasts or the average of other analysts' forecasts is announced publicly, they should also be orthogonal to analyst n's forecast error.

A key issue is whether an analyst knows his or her previous forecast error at the time he or she forecasts EPS. In the I/B/E/S data we use, the release of information happened in the sequence shown in figure 1, where the solid vertical lines represent the end of each time period. Figure 1 shows that, in each period, EPS for the previous period is announced before analyst n makes a forecast of EPS for the current period. In this case, analyst n's previous forecast error ($\text{EPS}^j_t - {}_{t-1}\text{EPS}^j_{n,t}$) is known when the analyst makes the prediction

[3] We also do not consider in this paper whether analysts are making their predictions strategically, on the basis of predictions made by other analysts.

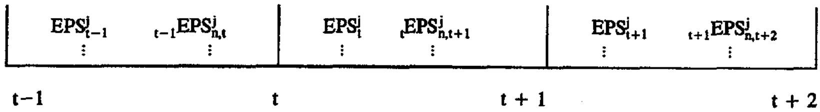


FIG. 1

${}_tEPS^j_{n,t+1}$. Therefore, the previous forecast error should be orthogonal to the current forecast error.[4] All private or public information known by the analyst when the analyst makes the forecast could be included in (2) to conduct a valid test of forecast rationality.

We next discuss how forecast errors are correlated *across* analysts and firms. If we understand this issue, we can increase the power of our tests of rationality by including observations on multiple analysts and on multiple firms.

We start by considering the case in which multiple analysts make forecasts for the same firm. As we noted previously, if the analysts all had exactly the same information (and the same loss function), they would make exactly the same forecast. In this case, the analysts' forecast errors would be exactly the same, and considering multiple analysts would produce no efficiency gain. The only gain to considering multiple analysts would come from the differences in analysts' forecasts that arise from an individual analyst's private information. But, even in this case, we would expect a very high correlation among analysts' forecasts (and forecast errors) because of the public information that they share.

Suppose that N analysts make one-step-ahead forecasts for firm j. Under the null hypothesis of forecast rationality, we assume that the variances and covariances of the analysts' forecast errors are

$$E(\epsilon^j_{n,t+1}\epsilon^j_{n,t+1+s}) = \begin{cases} a, & s = 0 \\ 0, & s \neq 0 \end{cases} \tag{3}$$

and

$$E(\epsilon^j_{n,t+1}\epsilon^j_{m,t+1+s}) = \begin{cases} c, & s = 0,\ m,\ n \leq N,\ m \neq n \\ 0, & s \neq 0. \end{cases} \tag{4}$$

There are two sources of these restrictions. First, the variance of an analyst's forecast error, equation (3), differs from the covariance of two different analysts' forecast errors, equation (4), because each

[4] If we had used k-step-ahead forecasts, each analyst's forecast errors would be MA($k - 1$), as discussed by Hansen and Hodrick (1980).

analyst possesses private information about the firm. Second, forecast errors are uncorrelated across time, if the forecasts are rational, because we use only forecasts that are made after EPS for the previous quarter is released. We have shown how to conduct statistical inference in this case in Keane and Runkle (1990).

We further increase the power of our tests for rationality by including observations on forecasts for different firms.[5] This step requires additional assumptions and a new estimation procedure, and it is the focus of our paper. Just as forecast errors across analysts for one firm are correlated because of public information, forecast errors for a single analyst for multiple firms in an industry are correlated because of unforeseen events that affect all firms in an industry. Of course, because information about industry conditions is public, forecast errors will be correlated across analysts for different firms in the industry as well.

Suppose now that an industry has N analysts and J firms. In each time period, each analyst makes predictions about EPS for each firm. Assume that at period t each analyst makes a one-step-ahead prediction for EPS for each firm. Under the null hypothesis of forecast rationality, equations (3) and (4) hold. However, we make two additional sets of assumptions about the covariances of analysts' forecasts across firms:

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{n,t+1+s}) = \begin{cases} b, & s = 0, j, l \leq J, j \neq l \\ 0, & s \neq 0 \end{cases} \tag{5}$$

and

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{m,t+1+s}) = \begin{cases} d, & s = 0, m, n \leq N, m \neq n, j, l \leq J, j \neq l \\ 0, & s \neq 0. \end{cases} \tag{6}$$

Equation (5) allows an individual analyst's forecast errors for different firms in an industry to be correlated. This correlation occurs because of unforeseen events that affect all firms in the industry. Note that the covariance of an analyst's forecast errors for different firms, equation (5), differs from the variance of the analyst's forecast error for a single firm, equation (3), because some unforeseen events are firm specific. Therefore, $b < a$.

Equation (6) allows different analysts' forecast errors for different firms in an industry to be correlated. This correlation occurs because of unforeseen events that affect all firms in the industry. However,

[5] The power of the tests will increase as long as analysts' forecast errors across firms are not perfectly correlated.

the covariance of forecast errors across firms for different analysts, equation (6), differs from the covariance of forecast errors across firms for a single analyst, equation (5), because each analyst can have private information about industry conditions. Therefore, $d < c$.

As with equations (3) and (4), equations (5) and (6) do not allow serial correlation in the errors. Again, this restriction stems from our use of forecasts that are made after EPS for the previous quarter is released.

Finally, note that this error structure, by assuming *homoskedasticity*, assumes that variances and covariances do not differ across forecasters or firms. In Section IV we normalize EPS across firms by dividing EPS by the stock price at the end of the previous quarter. This normalization is crucial to justify our assumption of homoskedasticity.

With covariance structure (3)–(6), errors are not independent across forecasters or across firms. Thus any attempt to estimate equation (2) by OLS will yield inconsistent test statistics since OLS standard errors are constructed under the assumption that all errors are independent and identically distributed. In Appendix A we propose a feasible GMM estimator for equation (2). Our estimator uses exactly the same orthogonality restrictions as OLS, so the coefficient estimates are the same as those of OLS. However, our estimator uses the information in the error covariance structure (3)–(6) to correctly compute the standard errors for the coefficient estimates. It differs from the GMM estimator used in Keane and Runkle (1990) because that earlier estimator can be used only when forecasters make predictions for only one time series. That estimator would not let us test the rationality of forecasts made by analysts for multiple firms within an industry.

Unlike OLS, the feasible GMM estimator will yield test statistics that are consistent in T. Consistency is in T rather than the number of analysts or the number of firms in an industry because forecast errors that arise from shocks affecting an entire industry will not cancel out across analysts or firms. That is, the sample version of the orthogonality condition $E(\epsilon^{j}_{n,t+1} | I_{n,t})$ converges to zero as the number of time periods increases, but not as the number of analysts or firms increases, if the number of time periods is held fixed.[6]

We now consider our five specific tests of rationality, all of which test the rationality of one-step-ahead forecasts. First, we test for unbiasedness. (If analysts' forecasts are biased, conducting further tests of efficiency is pointless.) Second, we test whether the analyst's previous one-step-ahead forecast is correlated with the analyst's current

[6] This point was first noted, in a different context, by Chamberlain (1984).

one-step-ahead forecast error. Third, we test whether the earnings announcement from period t is correlated with the analyst's current one-step-ahead forecast error. (This test shows whether analysts either underreact or overreact to the most recent earnings announcement.) Fourth, we test whether the analyst's lagged one-step-ahead forecast error is correlated with the analyst's current one-step-ahead forecast error. (This test shows whether an analyst learns from his or her own past forecast errors.) Fifth, we test whether the average lagged one-step-ahead forecast error by all analysts covering a firm is correlated with the analyst's current one-step-ahead forecast error.

IV. Data

The data for our study come from three sources. We use individual analyst predictions from I/B/E/S, earnings data from Compustat, and data about the timing of stock splits and stock dividends from the Center for Research in Security Prices (CRSP).

We believe that the I/B/E/S individual analyst data set is one of only two potential sources of data on individual analyst forecasts that satisfy two criteria necessary for implementing our econometric methods.[7] First, a unique code identifies each analyst. This identification is necessary to allow us to test the hypotheses about private information. Second, the date on which the forecast was made can be identified with reasonable accuracy. This dating is necessary so that our assumptions about the analysts' information sets are correct. We return to this issue later in the paper.

We choose six four-digit Standard Industrial Classification (SIC) industries to analyze, on the basis of the number of firms in the industry and analyst coverage. Within each industry we choose those firms for which a minimum of 100 quarterly forecasts were made in at least 25 different quarters from the fourth quarter of 1983 to the fourth quarter of 1991.[8] We choose industries for which at least three firms satisfied these criteria. We also restrict our sample to firms having a December 31 fiscal year end. Table B1 in Appendix B shows a list of the industries we use.

Since we want to ensure that the forecasts were made by professional earnings analysts rather than analysts who had made just a

[7] The other data set that could be used is the Zacks individual forecast database (see Stickel 1990). Value Line does not contain multiple individual forecasts. In addition, since Value Line does not publish how it computes its "actual" earnings numbers, there is no way to independently verify their construction from the raw financial reports.

[8] The average firm had observations for 29 quarters. The I/B/E/S quarterly data are not available before the fourth quarter of 1983.

couple of forecasts, we restrict our sample to the predictions of analysts who made forecasts in at least five different quarters. We use only forecasts designated as predictions of primary EPS, so that forecasts are comparable across analysts.[9]

Finally, we restrict our sample to those forecasts for which we have reasonable assurance that the firm's earnings announcement from the previous quarter was known at the time the analyst made the forecast. We do this by restricting our sample to those forecasts recorded at least 7 days after the firm's earnings announcement for the previous quarter.[10]

The mechanics of this restriction deserve further explanation. The I/B/E/S records the date on which a forecast is entered into the database rather than the date on which the forecast was made. But we have three reasons to think that the entry date is within a week of the date on which the forecast was made. First, since 1983, I/B/E/S has recorded the forecasts quite quickly.[11] Second, the vast majority of analysts work in New York, where I/B/E/S is located, so postal time is likely to be short.[12] Finally, the empirical distribution of forecast entry dates shows that virtually no forecasts are entered in the 7 days before an earnings announcement but that a large number of forecasts are entered after 7 days. Since analysts are more likely to make a new forecast immediately after the earnings announcement than immediately before, this pattern in the empirical distribution of entry dates suggests that a 7-day cutoff is sufficient to ensure that the analyst made the new prediction after the firm's earnings announcement.

Our data for actual EPS come from Compustat. We use Compustat earnings data rather than I/B/E/S earnings data because of the well-known problems with data alignment in the I/B/E/S earnings data (see Philbrick and Ricks 1991). We use primary EPS before extraordinary items as our measure of earnings because that is the measure of EPS that corresponds best to what I/B/E/S states the analysts are trying to predict (see Institutional Brokers Estimate System

[9] If sufficient stock options or convertible bonds are outstanding, firms are required to report fully diluted EPS, taking into account potential share dilution, in addition to primary EPS. We exclude forecasts of fully diluted EPS.

[10] We use Compustat's earnings announcement dates.

[11] In private conversations, I/B/E/S officials reported that from the fourth quarter of 1983 to the first quarter of 1985, forecasts were recorded within 5 days of receipt. Since the second quarter of 1985, I/B/E/S has done all of its data entry in-house. Forecasts are now entered within 2 days of receipt. Throughout the sample, we find no problems with delays in I/B/E/S data entry, such as those noted for earlier periods by Brown, Foster, and Noreen (1985) and O'Brien (1988).

[12] In fact, by the end of the sample, almost all the forecasts were sent electronically to I/B/E/S, so that they were entered into the database on the same day they were made.

1987). Even this measure of earnings may not be perfect in all cases, however; we discuss it in further detail below. To eliminate heteroskedasticity in forecast errors, we normalize both predicted EPS and actual EPS by dividing both by the stock price on the last day of the previous quarter.

All the data we use are corrected for stock splits, as listed on the CRSP master tape. If a split is announced and occurs between the time in which a forecast is made and the earnings announcement, the actual EPS is adjusted to conform to the presplit forecast. If a split is announced between the end of the previous quarter and the time in which the forecast is made, the previous quarter's stock price is adjusted to conform to the postsplit forecast and earnings announcement.

V. Empirical Results

We now consider our tests for unbiasedness and efficiency of individual analysts' forecasts for each of the six industries in our sample. Since we use quarterly data in our study, the one-step-ahead forecasts discussed in Section IV are one-quarter-ahead forecasts. All our tests are based on these one-quarter-ahead forecasts.

The first set of tests is based on analysts' one-quarter-ahead earnings forecasts in the chemical industry. Panel A of table 1 shows tests of the unbiasedness and efficiency of those forecasts. Row 1 of this panel shows that if OLS is used to estimate equation (2), the value of the test statistic for the null hypothesis of unbiasedness is 40.31. Since this statistic should be distributed asymptotically as a χ_2^2 random variable if the null hypothesis is true, that hypothesis is rejected overwhelmingly. This rejection should not be surprising. We argued in Section III that OLS standard errors will understate the true amount of parameter uncertainty because OLS ignores the dependence of analysts' forecast errors within a given time period.

Row 2 of panel A shows what happens to the test statistic for unbiasedness when our new GMM estimator is used. Since the model is exactly identified, the parameter estimates are exactly the same as for OLS, but the standard errors are much larger. This increase in the standard errors causes the test statistic for the null hypothesis of unbiasedness to drop from 40.31 to only 6.51. However, the null hypothesis of unbiasedness can still be rejected at the 5 percent level.

At this point, we might appear to have fairly strong evidence that analysts' earnings forecasts for the chemical industry are biased. But this is not so. Figure 2 shows that a few outlying observations are responsible for the rejection of unbiasedness.

In panel *a* of figure 2, the analyst's forecast is on the *X*-axis and

TABLE 1

Tests of Forecast Unbiasedness and Efficiency: SIC 2800

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0128	1.3214	. . .	40.3103	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	588
	(.0025)	(.1035)		(.0000)			
2	−.0128	1.3214	. . .	6.5188	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	588
	(.0062)	(.2646)		(.0384)			
3	−.0039	1.1064	−.0987	4.9198	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	229
	(.0047)	(.2005)	(.1924)	(.1778)		${}_{t-1}\mathrm{EPS}^j_{n,t}$	
4	−.0130	1.3581	−.0347	39.4815	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	575
	(.0026)	(.2740)	(.0343)	(.0000)		EPS^j_t	
5	−.0130	1.3581	−.0347	4.9198	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	575
	(.0064)	(.2740)	(.0651)	(.1778)		EPS^j_t	
6	−.0052	1.0589	−.0163	4.7962	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	229
	(.0042)	(.1733)	(.0507)	(.1873)		$(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	
7	−.0130	1.3254	.0001	6.2411	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	552
	(.0066)	(.2783)	(.0686)	(.1005)		$(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	
				B. Special-Charge Censoring			
1	−.0068	1.1612	. . .	35.1502	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	572
	(.0016)	(.0644)		(.0000)			
2	−.0068	1.1612	. . .	3.6655	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	572
	(.0050)	(.2165)		(.1600)			
3	.0003	1.0763	−.1509	3.6097	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	223
	(.0034)	(.1474)	(.1411)	(.3068)		${}_{t-1}\mathrm{EPS}^j_{n,t}$	
4	−.0069	1.1809	−.0193	30.9333	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	559
	(.0016)	(.0685)	(.0212)	(.0000)		EPS^j_t	
5	−.0069	1.1809	−.0193	3.3757	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	559
	(.0051)	(.2263)	(.0538)	(.3372)		EPS^j_t	
6	−.0021	.9970	−.0118	2.7014	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	223
	(.0032)	(.1302)	(.0382)	(.4400)		$(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	
7	−.0068	1.1575	−.0012	3.4108	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1},$	536
	(.0053)	(.2282)	(.0561)	(.3325)		$(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	

Note.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

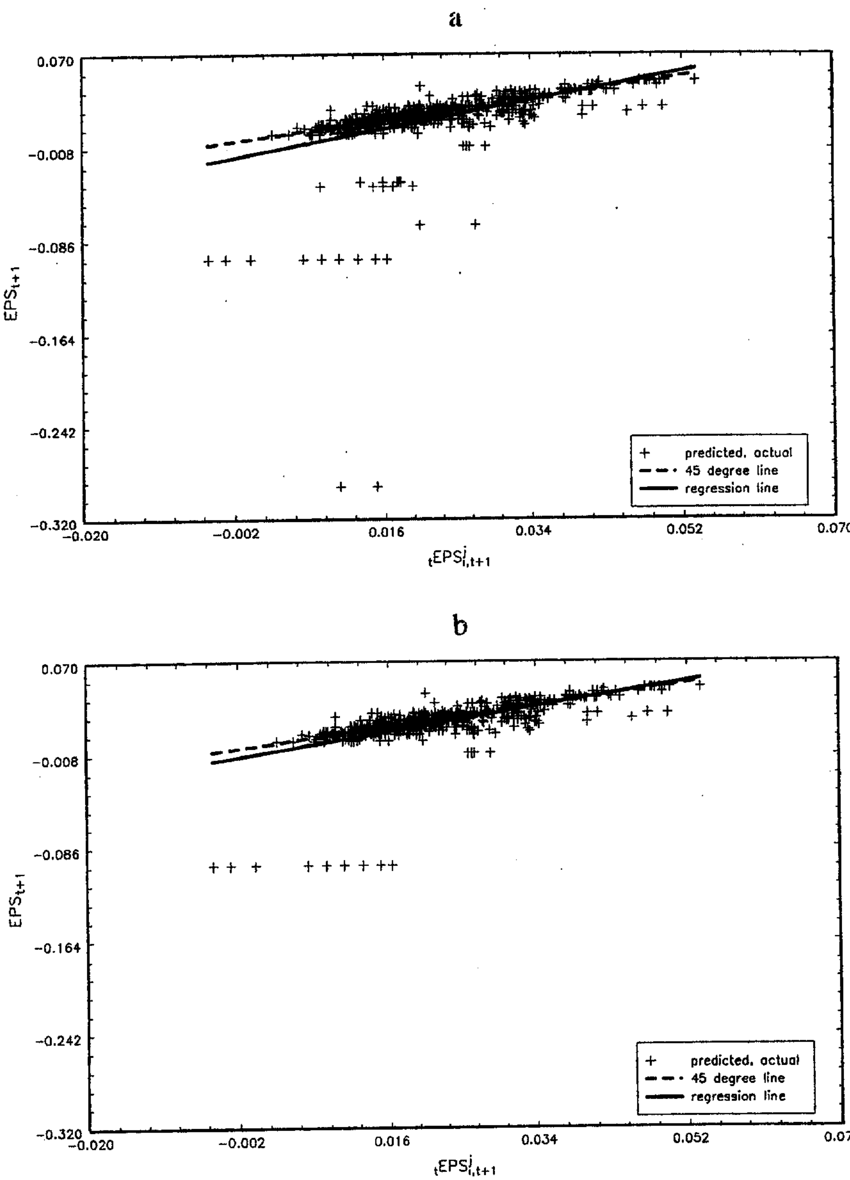


FIG. 2.—EPS forecasts and realizations (SIC 2800). *a*, All observations. *b*, Special-charge censoring.

the actual earnings announcement is on the *Y*-axis. As before, both the forecast and the announcement are normalized. The crosses represent one analyst's forecast and the subsequent earnings announcement for one firm in one quarter, that is, the observations we use in our regressions. The dashed line is the 45-degree line. Its slope in the panel is different from 45 degrees because of the different scales of the *X*- and *Y*-axes. The solid line is the fitted regression line from the test of unbiasedness.

The slope of the fitted regression line is clearly greater than that of the 45-degree line, as the earlier regression coefficients showed. But figure 2 shows that this steep slope is caused by a few outlying observations. These observations have very large negative values for actual earnings. For example, the two observations with the lowest values of actual earnings represent quarterly losses per share that are more than one-fourth of the stock price at the end of the previous quarter.

Several of the observations plotted in panel *a* of figure 2 are cases in which the firm had a large above-the-line asset write-down or other special accrual. However, there are good theoretical reasons for deleting such observations. Philbrick and Ricks (1991, p. 401) note that "I/B/E/S refers to extraordinary items as 'write downs which are at the discretion of management,' while according to generally accepted accounting principles, not all discretionary write-downs qualify as extraordinary items. Therefore, the earnings components included in an I/B/E/S forecast may not be the same as in the corresponding Compustat actuals."[13] Thus the standard measure of actual earnings that we use—EPS, before discontinued operations and extraordinary items—will not accurately reflect what analysts are trying to predict if a large above-the-line asset write-down or other special charges occur in a given quarter.[14]

We solve this problem in panel *b* of figure 2 by eliminating the observations for which the discretionary special charge[15] per share

[13] They also note that Value Line generally excludes special above-the-line items that Compustat includes in pretax EPS before extraordinary items and discontinued operations.

[14] Philbrick and Ricks (1991) discuss this issue in detail. However, they attempt to adjust for the tax effects of these discretionary accruals so that they can still include these observations in their analysis. We do not think that a researcher could come up with an unbiased estimate of the after-tax earnings that analysts are trying to predict if such a discretionary accrual occurs. If biased estimates of after-tax earnings were used, the resulting regression coefficients and test statistics would be inconsistent. Thus we believe that omitting these observations is the only way to prevent invalid statistical inference.

[15] Although generally accepted accounting principles specify a uniform terminology and set of qualifications for extraordinary items and discontinued operations, there are no such restrictions for discretionary asset write-offs and other before-tax special charges. Compustat lumps these items under the description "special

(normalized by the beginning-of-quarter share price) was larger than four standard deviations from the average, price-normalized analyst forecast error for the industry for all periods.[16] When these observations are eliminated, the slope of the fitted regression line becomes almost exactly the same as the 45-degree line.

In each of the cases omitted in panel *b* of figure 2, the firm had a large discretionary special charge. American Cyanamid reported a special charge of $291.9 million in the third quarter of 1990. Dow Chemical reported a special charge of $592 million during the fourth quarter of 1985. Olin reported a charge of $303 million to nonoperating income in the third quarter of 1985 and a special charge of $80 million in the first quarter of 1991. Details on these charges from the relevant annual reports are included in Appendix B. Including observations with these charges would result in incorrect statistical inference since I/B/E/S specifies that such charges are not to be included in the analysts' earnings forecasts. We dropped each of the forecasts made by analysts for Olin and Monsanto in these cases.[17]

Panel B of table 1 shows the regression results that correspond to observations shown in panel *b* of figure 2 when we eliminate the effects of the previously mentioned large discretionary special charges. Row 1 of this panel shows the results of estimating equation (2) using OLS. Note that the test statistic for unbiasedness is still so large (35.15) that the null hypothesis of unbiasedness is rejected. This rejection is suspect, however, since it assumes that all the observations are independent.

Row 2 of panel B shows the results of estimating equation (2) on the smaller sample using the GMM estimator. Here the test statistic for the null hypothesis of unbiasedness is small enough (3.67) that the hypothesis is not rejected.

By comparing the first two rows of both panels, we can see the importance of correctly selecting our data sample and correctly selecting our estimator for correct statistical inference about the unbiasedness of analysts' one-quarter-ahead forecasts in the chemical industry. If we either included observations containing large discretionary special charges or used OLS, we would incorrectly decide

charges." However, in annual reports they could also be called nonrecurring charges, restructuring charges, or asset write-offs, or whatever the firm wants to call them. We shall refer to them as discretionary special charges in this paper.

[16] We validated the special charges using variable 32 on both the quarterly Compustat tapes and annual reports. We chose a cutoff based on the standard deviation of average industry forecast error because the standard deviation should measure how big the earnings surprise was that was caused by the special charge.

[17] This restriction reduces the number of observations in our unbiasedness tests from 588 to 572.

that the analysts' forecasts were biased. Only when we use both a correct sample and an estimator that accounts for correlation among analysts' forecast errors do we fail to reject the hypothesis of unbiasedness.

Panel B of table 1 also shows that the hypothesis of forecast efficiency is not rejected as long as the GMM estimator is used. Rows 3–7 show efficiency tests. In each of these tests, a single variable in the forecasters' time *t* information set was included as the extra regressor in equation (2). The tests were conducted separately, rather than jointly, because a given observation could not be included in the sample if any single variable were missing. Hence, an unacceptably small number of observations would have been included in the joint test.

Row 3 shows the effect of adding to equation (2) the analyst's own previous one-step-ahead forecast. The χ^2 test statistic shows that that variable has no additional explanatory power in predicting actual earnings beyond that of the current one-step-ahead forecast.

Rows 4 and 5 show the effect of adding to equation (2) the earnings announcement that was released shortly before the analyst's forecast was made. Row 4 shows that if the previous earnings announcement is included and OLS is used, the hypothesis of efficiency is rejected. Row 5 shows that if the same equation is estimated using the GMM estimator, the hypothesis of efficiency is not rejected.

Row 6 shows that analysts learn from their own past forecast errors. An analyst's immediate past one-step-ahead forecast error does not significantly help to predict firm earnings, conditioned on the analyst's current one-quarter-ahead forecast. Row 7 shows that the average immediate past one-step-ahead forecast error of all analysts covering the firm also makes no significant incremental contribution in predicting earnings.

All these tests show that we fail to reject either unbiasedness or efficiency of analysts' one-quarter-ahead forecasts in the chemical industry if we use the GMM estimator and we eliminate observations with large discretionary above-the-line write-downs and accruals.

The remaining tables and figures in the paper show the results of similar investigations for the other industries in our sample. For each of the next four industries, in the top panel of the tables and figures, we present the results of using all the observations in the sample. In the bottom panel of the tables and figures, we present the results of eliminating all analyst forecasts that contained large discretionary special charges, using the four-standard-deviation criterion discussed above. Appendix B contains the details of the large special charges, as discussed in the firms' annual reports. Note that these additional tests for analyst forecast rationality in different industries are not additional independent observations because aggregate economic shocks can cause correlation in analysts' forecast errors across industries. At best, the analysis of these different industries can give us some indication of whether the results we found for the chemical industry were representative of all industries.

Tables 2–5 and figures 3–6 tell a consistent story. As long as we use the GMM estimator and exclude observations with large discretionary above-the-line write-downs or accruals, no evidence disputes the hypothesis that analysts' earnings forecasts are rational. Using the GMM estimator, we reject neither unbiasedness nor efficiency. All these estimates provide additional support for concluding that analysts' forecasts are rational.

The only industry in which analysts' forecasts do not appear to be rational is the airline industry. Table 6 shows that no matter which estimator or sample is used, both the unbiasedness and the efficiency of analysts' forecasts are rejected. In addition, there is no difference between panels A and B of table 6 because none of the airlines included had a large discretionary special charge during the sample period. But this result should not be too surprising. In 1990 and 1991 the airline industry suffered historically unprecedented losses. Figure 7 shows exactly how bad the losses were in that industry. In fact, airlines lost more money in those two years than they made in the previous 60 years. For any analyst to have accurately assessed the combined effects of the Gulf War and the recession on the airline industry in those years would have been almost impossible. Claiming that analysts' forecasts were not rational simply because they could not accurately predict the magnitude of the earnings catastrophe that hit the airline industry seems far-fetched. The airline results are an excellent illustration of how large aggregate shocks can cause inconsistent estimates for a small T.

One potential criticism of our study is that we arbitrarily chose a four-standard-deviation cutoff to eliminate observations with large special charges. At the suggestion of the referee, we reestimated each of the regressions using both a 3.5- and a 4.5-standard-deviation cutoff. The results were very similar. When we used the 3.5-standard-deviation cutoff, none of the tests for tables 1–5 using the truncated sample rejected forecast rationality. When we used the 4.5-standard-deviation cutoff, rationality was rejected only for a single test (eq. 7 in table 3). We believe that a 4.5-standard-deviation cutoff is quite extreme. Since the sensitivity tests change our results in only one extreme case in which the sample contains observations that we believe should be excluded, those tests reinforce our conclusions that the analysts' forecasts are rational.

TABLE 2

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3330

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0225	1.5038	…	24.7697	OLS	$1,\ {}_{t}EPS^{j}_{n,t+1}$	302
	(.0048)	(.1113)		(.0000)			
2	−.0225	1.5038	…	7.2193	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1}$	302
	(.0091)	(.2223)		(.0271)			
3	−.0228	1.4909	.1072	12.4818	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	116
	(.0074)	(.2684)	(.2505)	(.0059)		${}_{t-1}EPS^{j}_{n,t}$	
4	−.0235	1.5511	−.0245	25.6337	OLS	$1,\ {}_{t}EPS^{j}_{n,t+1},$	300
	(.0050)	(.1455)	(.1033)	(.0000)		EPS^{j}_{t}	
5	−.0235	1.5511	−.0245	7.4214	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	300
	(.0093)	(.2599)	(.1033)	(.0596)		EPS^{j}_{t}	
6	−.0267	1.6841	−.1787	16.7461	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	116
	(.0075)	(.1788)	(.0999)	(.0008)		$(EPS^{j}_{t} - {}_{t-1}EPS^{j}_{n,t})$	
7	−.0263	1.5927	−.0605	7.8688	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	295
	(.0101)	(.2440)	(.1140)	(.0488)		$(EPS^{j}_{t} - {}_{t-1}\overline{EPS}^{j}_{t})$	
				B. Special-Charge Censoring			
1	.0045	.8840	…	5.3620	OLS	$1,\ {}_{t}EPS^{j}_{n,t+1}$	296
	(.0022)	(.0514)		(.0685)			
2	.0045	.8840	…	1.6900	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1}$	296
	(.0039)	(.0977)		(.4296)			
3	.0048	.8001	.0995	2.4898	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	114
	(.0040)	(.1406)	(.1235)	(.4771)		${}_{t-1}EPS^{j}_{n,t}$	
4	.0051	.8395	.0305	6.5497	OLS	$1,\ {}_{t}EPS^{j}_{n,t+1},$	294
	(.0023)	(.0659)	(.0294)	(.0877)		EPS^{j}_{t}	
5	.0051	.8395	.0305	2.1217	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	294
	(.0041)	(.1169)	(.0447)	(.5475)		EPS^{j}_{t}	
6	.0038	.9209	−.0488	3.0587	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	114
	(.0042)	(.0993)	(.0453)	(.3827)		$(EPS^{j}_{t} - {}_{t-1}EPS^{j}_{n,t})$	
7	.0058	.8545	.0150	2.0245	GMM	$1,\ {}_{t}EPS^{j}_{n,t+1},$	289
	(.0045)	(.1107)	(.0489)	(.5673)		$(EPS^{j}_{t} - {}_{t-1}\overline{EPS}^{j}_{t})$	

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

TABLE 3

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3334

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0122 (.0035)	1.2734 (.1144)	. . .	14.6069 (.0007)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	326
2	−.0122 (.0055)	1.2734 (.1831)	. . .	5.7692 (.0559)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	326
3	−.0048 (.0043)	1.1495 (.1814)	−.0945 (.1984)	3.9481 (.2671)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	118
4	−.0117 (.0035)	1.2144 (.1244)	.0478 (.0375)	16.1505 (.0010)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	322
5	−.0117 (.0054)	1.2149 (.1873)	.0478 (.0494)	6.8431 (.0771)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	322
6	−.0035 (.0037)	1.0232 (.1271)	.1842 (.1095)	6.0758 (.1080)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	118
7	−.0122 (.0052)	1.2521 (.1715)	.0521 (.0508)	9.4325 (.0241)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	313
				B. Special-Charge Censoring			
1	−.0086 (.0035)	1.1700 (.1131)	. . .	8.3672 (.0152)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	322
2	−.0086 (.0051)	1.1700 (.1714)	. . .	3.6245 (.1633)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	322
3	−.0015 (.0043)	1.0846 (.1768)	−.1271 (.1886)	2.4367 (.4868)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	117
4	−.0080 (.0035)	1.1054 (.1228)	.0516 (.0364)	10.2934 (.0162)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	318
5	−.0080 (.0052)	1.1054 (.1791)	.0516 (.0473)	4.8198 (.1855)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	318
6	−.0005 (.0036)	.9343 (.1236)	.1938 (.1034)	5.2603 (.1537)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	117
7	−.0084 (.0049)	1.1441 (.1623)	.0579 (.0485)	7.0569 (.0701)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	309

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

TABLE 4

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 3711

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0064	.9790	...	19.1318	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	557
	(.0018)	(.0361)		(.0001)			
2	−.0064	.9790	...	2.0252	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	557
	(.0063)	(.1049)		(.3633)			
3	−.0087	.9206	.0598	2.8733	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	241
	(.0069)	(.1426)	(.1418)	(.4116)			
4	−.0069	.9248	.0759	23.3465	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	549
	(.0019)	(.0444)	(.0354)	(.0000)			
5	−.0069	.9248	.0759	2.2593	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	549
	(.0064)	(.1313)	(.1165)	(.5204)			
6	−.0085	.9613	−.0595	2.9268	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	241
	(.0066)	(.1273)	(.1563)	(.4031)			
7	−.0070	1.0027	−.0123	1.7771	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	538
	(.0066)	(.1227)	(.1708)	(.6199)			
				B. Special-Charge Censoring			
1	.0001	.9414	...	7.7374	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	516
	(.0013)	(.0246)		(.0209)			
2	.0001	.9414	...	1.1871	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	516
	(.0041)	(.0660)		(.5224)			
3	−.0007	.9038	.0271	1.7610	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	216
	(.0045)	(.0844)	(.0830)	(.6235)			
4	−.0002	.8277	.1446	48.6952	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	508
	(.0012)	(.0296)	(.0225)	(.0000)			
5	−.0002	.8277	.1446	6.4412	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	508
	(.0036)	(.0758)	(.0653)	(.0920)			
6	.0020	.8726	.1689	5.6326	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	216
	(.0043)	(.0760)	(.0882)	(.1309)			
7	.0024	.9073	.2187	6.4048	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	497
	(.0039)	(.0715)	(.0905)	(.0934)			

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

TABLE 5

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 4011

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0292 (.0097)	1.7256 (.4049)	. . .	26.0697 (.0000)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	277
2	−.0292 (.0204)	1.7256 (.8245)	. . .	3.9868 (.1362)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	277
3	−.0668 (.0268)	2.0176 (1.0459)	1.2577 (1.1144)	8.3297 (.0397)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	86
4	−.0319 (.0099)	1.7004 (.4195)	.1709 (.1024)	29.3671 (.0000)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	271
5	−.0319 (.0212)	1.7004 (.8643)	.1709 (.2097)	4.8245 (.1851)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	271
6	−.0497 (.0220)	2.6641 (.9268)	.2075 (.2034)	7.8439 (.0494)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	86
7	−.0337 (.0250)	1.8983 (1.0469)	.1174 (.2232)	4.4499 (.2168)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	248
				B. Special-Charge Censoring			
1	−.0007 (.0023)	.9309 (.0942)	. . .	15.9156 (.0003)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	262
2	−.0007 (.0036)	.9309 (.1474)	. . .	5.1829 (.0749)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$	262
3	−.0080 (.0077)	1.0909 (.2948)	.1079 (.2938)	4.7149 (.1939)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, ${}_{t-1}\mathrm{EPS}^j_{n,t}$	81
4	−.0016 (.0023)	.9558 (.0978)	.0124 (.0230)	15.5673 (.0014)	OLS	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	256
5	−.0016 (.0038)	.9558 (.1547)	.0124 (.0359)	5.1629 (.1603)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, EPS^j_t	256
6	−.0067 (.0064)	1.1541 (.2689)	.0346 (.0529)	5.1530 (.1609)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	81
7	−.0028 (.0044)	.9974 (.1878)	−.0025 (.0381)	5.2716 (.1530)	GMM	1, ${}_t\mathrm{EPS}^j_{n,t+1}$, $(\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	233

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

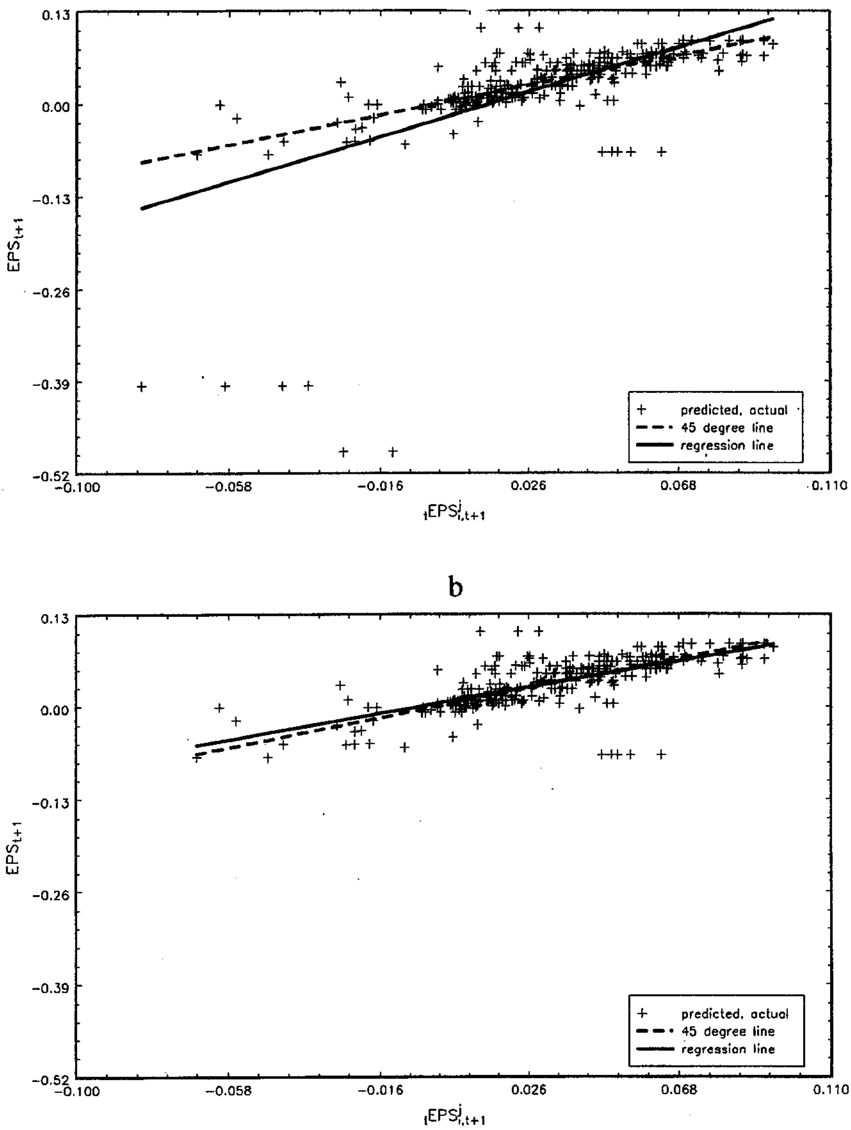
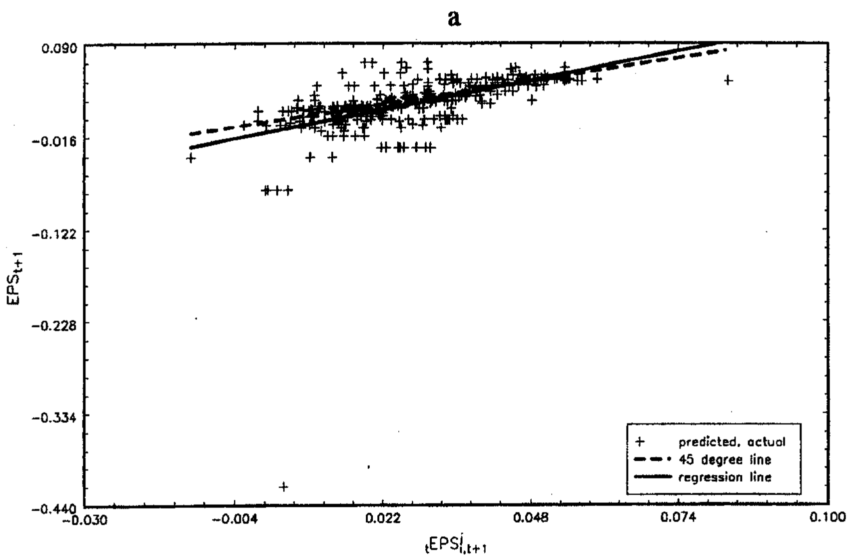


FIG. 3.—EPS forecasts and realizations (SIC 3330). *a*, All observations. *b*, Special-charge censoring.



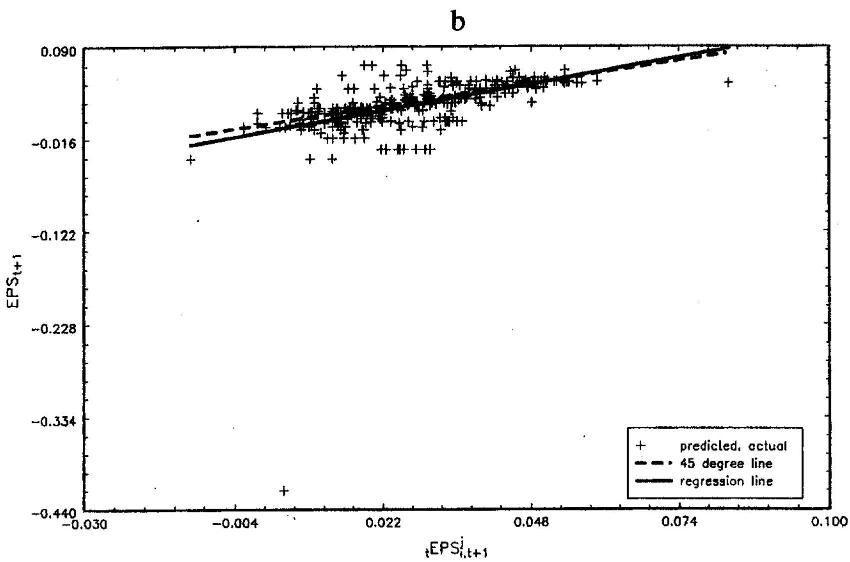


FIG. 4.—EPS forecasts and realizations (SIC 3334). *a,* All observations. *b,* Special-charge censoring.

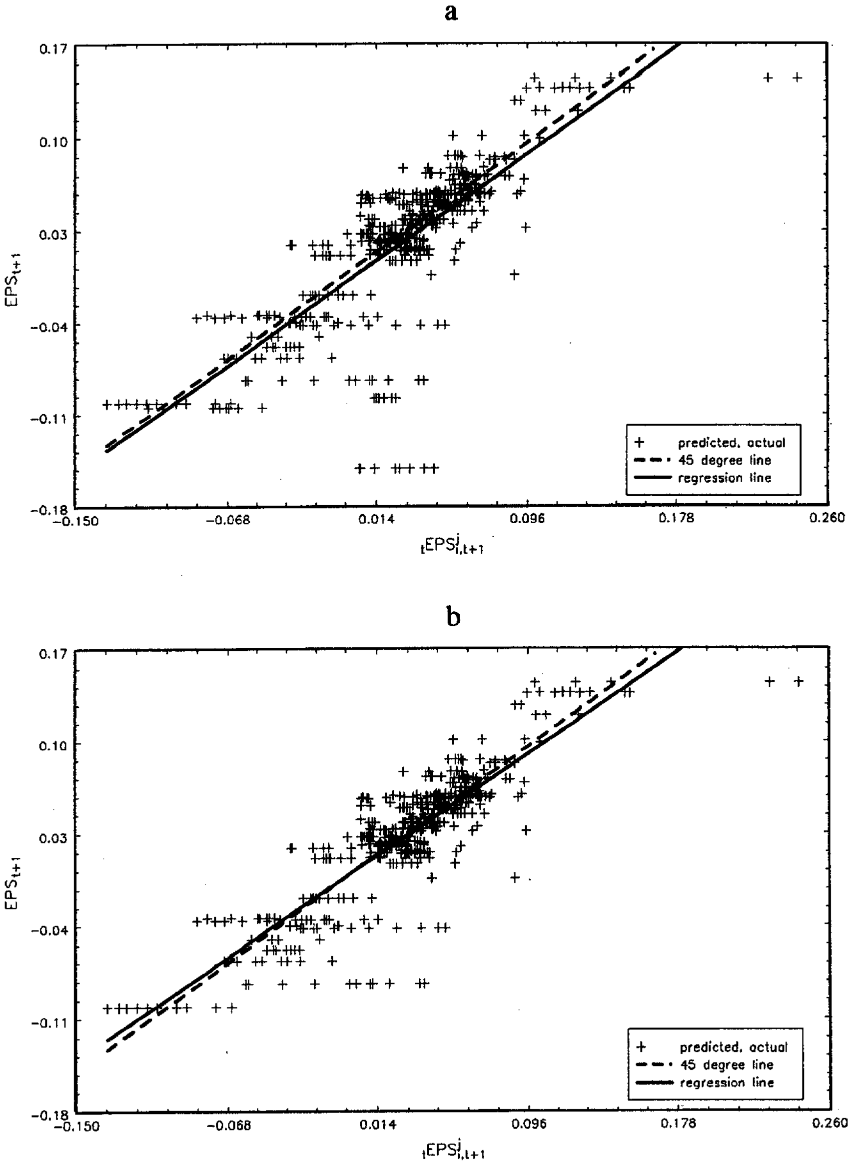


FIG. 5.—EPS forecasts and realizations (SIC 3711). *a*, All observations. *b*, Special-charge censoring.

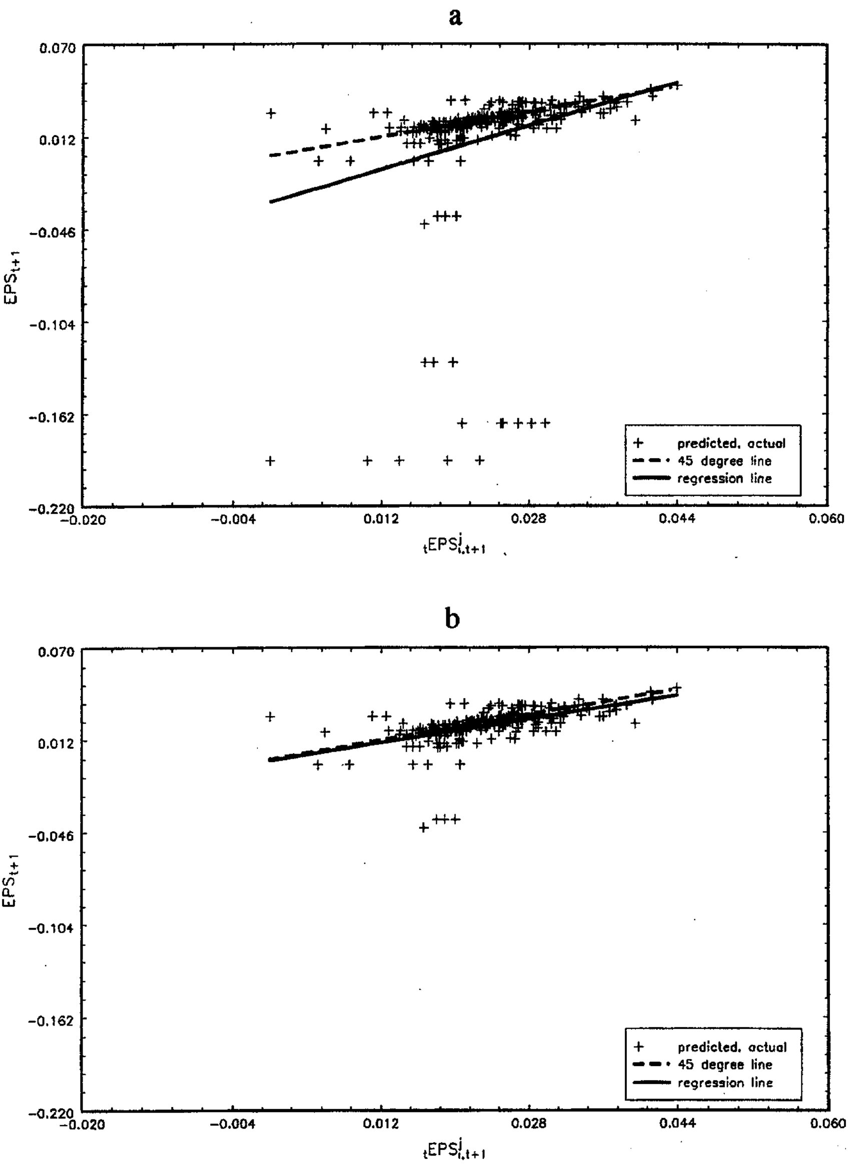


FIG. 6.—EPS forecasts and realizations (SIC 4011). *a*, All observations. *b*, Special-charge censoring.

TABLE 6

TESTS OF FORECAST UNBIASEDNESS AND EFFICIENCY: SIC 4512

Equation	α_0	α_1	α_2	χ^2 for H_0	Method	Regressors	Observations
				A. All Observations			
1	−.0117	1.2553	. . .	139.3637	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	501
	(.0014)	(.0271)		(.0000)			
2	−.0117	1.2553	. . .	20.7546	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	501
	(.0033)	(.0673)		(.0000)			
3	−.0130	1.2091	.0306	22.3149	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, {}_{t-1}\mathrm{EPS}^j_{n,t}$	195
	(.0037)	(.0804)	(.0900)	(.0001)			
4	−.0016	.9558	.0124	228.5348	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}, \mathrm{EPS}^j_t$	493
	(.0013)	(.0412)	(.0324)	(.0000)			
5	−.0104	.9871	.2656	24.8202	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, \mathrm{EPS}^j_t$	493
	(.0038)	(.1076)	(.0865)	(.0000)			
6	−.0046	.9330	.6455	83.0216	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, (\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	195
	(.0033)	(.0562)	(.0795)	(.0000)			
7	−.0004	.8091	.9308	55.2606	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, (\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	479
	(.0034)	(.0833)	(.1516)	(.0000)			
				B. Special-Charge Censoring			
1	−.0100	1.1797	. . .	64.6148	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	491
	(.0014)	(.0332)		(.0000)			
2	−.0100	1.1797	. . .	9.6614	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}$	491
	(.0035)	(.0796)		(.0080)			
3	−.0114	1.0449	.1341	10.7822	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, {}_{t-1}\mathrm{EPS}^j_{n,t}$	188
	(.0038)	(.0884)	(.0914)	(.0130)			
4	−.0082	.8775	.2676	150.4435	OLS	$1, {}_t\mathrm{EPS}^j_{n,t+1}, \mathrm{EPS}^j_t$	483
	(.0013)	(.0468)	(.0313)	(.0000)			
5	−.0082	.8775	.2676	17.0831	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, \mathrm{EPS}^j_t$	483
	(.0036)	(.1056)	(.0825)	(.0007)			
6	−.0048	.9136	.5489	37.8368	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, (\mathrm{EPS}^j_t - {}_{t-1}\mathrm{EPS}^j_{n,t})$	188
	(.0033)	(.0693)	(.1049)	(.0000)			
7	−.0007	.8116	.8283	34.8176	GMM	$1, {}_t\mathrm{EPS}^j_{n,t+1}, (\mathrm{EPS}^j_t - {}_{t-1}\overline{\mathrm{EPS}}^j_t)$	469
	(.0034)	(.0878)	(.1637)	(.0000)			

NOTE.—Standard errors are in parentheses under coefficients. Significance levels are in parentheses under test statistics.

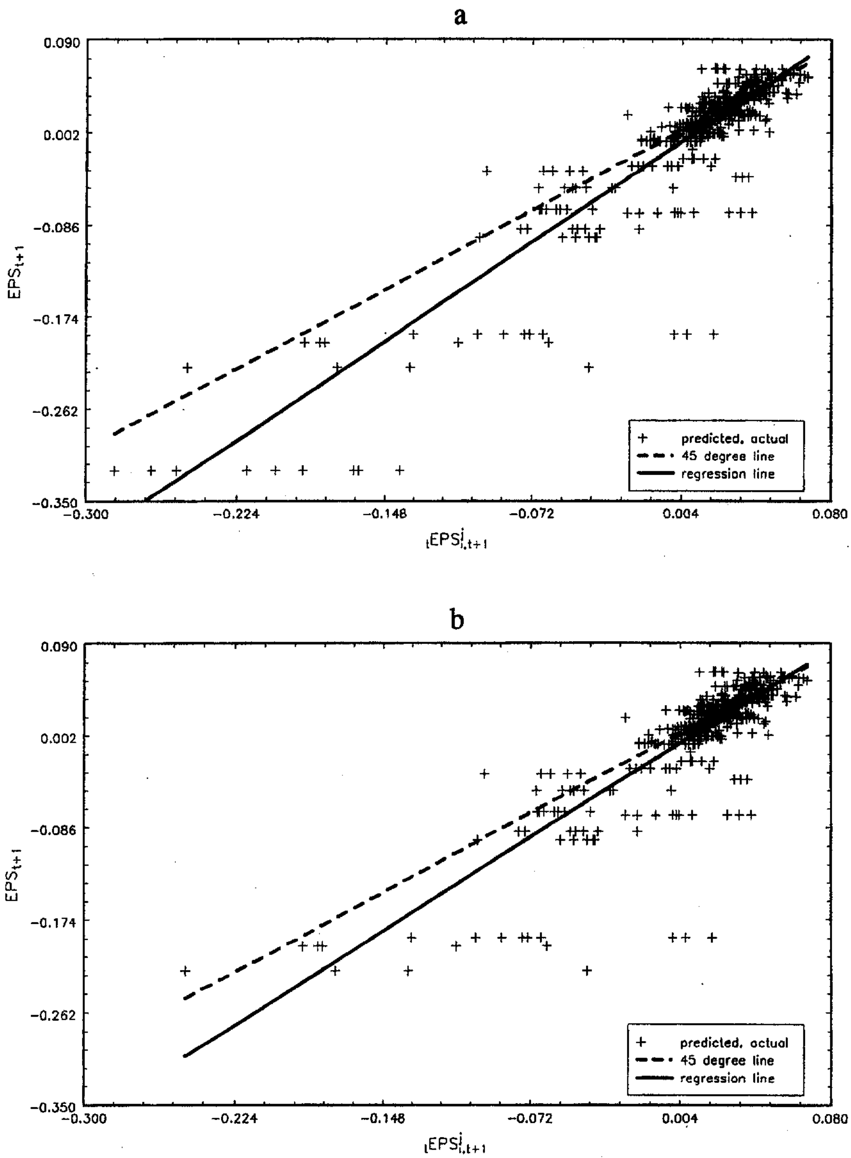


FIG. 7.—EPS forecasts and realizations (SIC 4512). *a*, All observations. *b*, Special-charge censoring.

Another potential criticism of our study is that our time series are short and that therefore our estimates are unreliable. Although we wish we had longer time series, the time series we have are much longer than are commonly used in panel data applications in the profession. They are certainly longer than those for other tests of analyst forecast rationality. And it is difficult for us to believe that analysts' forecasts are actually biased, but that we failed to reject forecast rationality for every single case in tables 1–5.

VI. Conclusion

The evidence in this paper strongly supports the view that professional stock market analysts make rational forecasts of earnings per share for the companies they follow. This result supports the view that current financial disclosures, in addition to other financial information gathered by analysts, provide intelligent users of financial statements with enough information to predict the current condition of firms with reasonable accuracy. It also suggests, contrary to popular opinion, that analysts do not systematically shade their forecasts; rather, their forecasts are unbiased. Our results also indicate that one will tend to falsely conclude that earnings forecasts are upward-biased if one fails to account for discretionary special charges. The seeming bias that occurs is simply a function of the conservative bias of accounting: that management can take large discretionary write-downs of assets, but assets cannot be written up.

We have also demonstrated the importance of careful data selection and statistical inference to our analysis. Future researchers should carefully consider how analyst forecast errors are correlated across analysts and firms. They should also consider whether discretionary write-downs and accruals will cause reported EPS to inaccurately measure what analysts were trying to predict.

Appendix A

Econometric Methods

We now propose a feasible generalized method-of-moments (GMM) estimator for equation (2) in the text. First, we must specify the structure of $\mathbf{\Omega}$, the covariance matrix of all the errors from the regression equation. Second, we must specify how to consistently estimate $\mathbf{\Omega}$ to arrive at a feasible GMM estimator.

As we discussed in the text, we assume that the covariance structure for the forecast errors, equations (3)–(6), is

$$E(\epsilon^{j}_{n,t+1}\epsilon^{j}_{n,t+1+s}) = \begin{cases} a, & s = 0 \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{j}_{m,t+1+s}) = \begin{cases} c, & s = 0,\ m,\ n \leq N,\ m \neq n \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{n,t+1+s}) = \begin{cases} b, & s = 0,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0, \end{cases}$$

$$E(\epsilon^{j}_{n,t+1}\epsilon^{l}_{m,t+1+s}) = \begin{cases} d, & s = 0,\ m,\ n \leq N,\ m \neq n,\ j,\ l \leq J,\ j \neq l \\ 0, & s \neq 0. \end{cases}$$

Suppose that we order our forecast observations as follows,

$${}_1\mathrm{EPS}^1_{1,1+k} \cdots {}_T\mathrm{EPS}^1_{1,T+1}\ {}_1\mathrm{EPS}^2_{1,1+k} \cdots {}_T\mathrm{EPS}^J_{1,T+1}\ {}_1\mathrm{EPS}^1_{2,1+k} \cdots {}_T\mathrm{EPS}^J_{N,T+1},$$

and order the observations for EPS and $X_{n,t}$ accordingly. Then $\mathbf{\Omega}$ will have the following structure:

$$\underset{JNT \times JNT}{\mathbf{\Omega}} = \begin{bmatrix} \mathbf{E} & \mathbf{F} & \cdots & \mathbf{F} \\ \mathbf{F} & \mathbf{E} & \cdots & \mathbf{F} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{F} & \mathbf{F} & \cdots & \mathbf{E} \end{bmatrix},$$

when

$$\underset{JT \times JT}{\mathbf{E}} = \begin{bmatrix} \mathbf{A} & \mathbf{B} & \cdots & \mathbf{B} \\ \mathbf{B} & \mathbf{A} & \cdots & \mathbf{B} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{B} & \mathbf{B} & \cdots & \mathbf{A} \end{bmatrix},$$

$$\underset{JT \times JT}{\mathbf{F}} = \begin{bmatrix} \mathbf{C} & \mathbf{D} & \cdots & \mathbf{D} \\ \mathbf{D} & \mathbf{C} & \cdots & \mathbf{D} \\ \vdots & \vdots & \ddots & \vdots \\ \mathbf{D} & \mathbf{D} & \cdots & \mathbf{C} \end{bmatrix},$$

and

$$\underset{T \times T}{\mathbf{A}} = a \cdot I_T, \quad \underset{T \times T}{\mathbf{C}} = c \cdot I_T,$$

$$\underset{T \times T}{\mathbf{B}} = b \cdot I_T, \quad \underset{T \times T}{\mathbf{D}} = d \cdot I_T.$$

We can consistently estimate the elements of **A**, **B**, **C**, and **D** as follows.

First, estimate equation (2) using OLS, which will give consistent estimates of parameters $\hat{b}_{\text{GMM}}$.[18] Use these estimates to construct an estimated residual vector. Then construct the elements of **A**, **B**, **C**, and **D**:

$$\hat{a} = \frac{1}{TJN} \sum_{t=1}^{T} \sum_{j=1}^{J} \sum_{n=1}^{N} \hat{\epsilon}^{j}_{n,t+1} \times \hat{\epsilon}^{j}_{n,t+1},$$

$$\hat{b} = \frac{1}{TJ(J-1)N} \sum_{t=1}^{T} \sum_{j=1}^{J} \sum_{\substack{l=1 \\ l \neq j}}^{J} \sum_{n=1}^{N} \hat{\epsilon}^{j}_{n,t+1} \times \hat{\epsilon}^{l}_{n,t+1},$$

$$\hat{c} = \frac{1}{TJN(N-1)} \sum_{t=1}^{T} \sum_{j=1}^{J} \sum_{n=1}^{N} \sum_{\substack{m=1 \\ m \neq n}}^{N} \hat{\epsilon}^{j}_{n,t+1} \times \hat{\epsilon}^{j}_{m,t+1},$$

$$\hat{d} = \frac{1}{TJ(J-1)N(N-1)} \sum_{t=1}^{T} \sum_{j=1}^{J} \sum_{\substack{l=1 \\ l \neq j}}^{J} \sum_{n=1}^{N} \sum_{\substack{m=1 \\ m \neq n}}^{N} \hat{\epsilon}^{j}_{n,t+1} \times \hat{\epsilon}^{l}_{m,t+1}.$$

Given the assumption we have made about the structure of the errors in equation (2), we can then construct a consistent estimate of the covariance matrix of $\hat{b}_{\text{GMM}}$, namely,

$$V(\hat{b}_{\text{GMM}}) = [\mathbf{X}'\mathbf{X}(\mathbf{X}'\hat{\boldsymbol{\Omega}}\mathbf{X})^{-1}\mathbf{X}'\mathbf{X}]^{-1}.$$

If some observations are missing, the estimates of **A**, **B**, **C**, and **D** can be constructed using all nonmissing observations on the residuals. Missing observations create no additional problems for inference.

Appendix B

Disclosures on Observations with Above-the-Line Special Items Eliminated in the Truncated Sample

Table B1 shows the industries we use.

SIC 2800

American Cyanamid 90:3 (1990 Annual Report)

"During 1990, the company provided, on a pre-tax basis, $291.9 [million] primarily for special costs associated with plans to curtail and consolidate certain product lines; to reduce the carrying value of certain assets to estimated realizable amounts, including investments in subsidiaries and affiliates; and for increased environmental remediation costs."

[18] The terms $\hat{b}_{\text{OLS}}$ and $\hat{b}_{\text{GMM}}$ are identical in this case because they use the same orthogonality conditions. This new estimator correctly specifies $V(\hat{b}_{\text{GMM}})$.

TABLE B1

INDUSTRIES EXAMINED IN THIS STUDY

SIC Code	Industry	Number of Firms	Number of Analysts
2800	Chemicals	5	49
3330	Smelters and refiners—nonferrous	3	34
3334	Smelters and refiners—aluminum	3	28
3711	Motor vehicles and car bodies	3	35
4011	Railroads, line-haul operating	3	29
4512	Air transportation, certified	4	37

Dow Chemical 85:4 (1985 Annual Report)

"The fourth quarter of 1985 included a special pretax charge of $471 [million] for asset-related writeoffs and writedowns and $121 [million] for personnel related costs."

Olin 85:3 (1985 Annual Report)

"The total provision made to cover all costs of the restructuring was $330 million pre-tax, or $230 million after-tax. The reserve provides for permanently decommissioning certain chemical facilities, writing down facilities and assets impaired by changed worldwide economic conditions."

Olin 91:1 (1991 Annual Report)

"The 1991 first-quarter loss includes a[n] $80 million special charge to cover losses on disposition and writedown of certain business assets and costs of personnel reductions."

SIC 3330

ASARCO 84:4 (1984 Annual Report)

"The 1984 results included an unusual pre-tax charge of $254 million reflecting the closing or shutdown of certain facilities and the writedown in value of properties no longer considered economic in view of reduced price expectations."

Phelps Dodge 84:4 (1984 Annual Report)

"In view of the exceedingly difficult conditions currently prevailing in the copper market, the company . . . is implementing a program to further restructure certain of its operations. As part of this program, the company recorded a $195 million non-recurring pre-tax charge in the fourth quarter of 1984, $110 million of which was charged against continuing operations."

SIC 3334

Alcan Aluminum 85:4 (1985 Annual Report)

"Approximately one half of the charge of $416 [million] reflects the estimated long-term impairment in economic value of the company's bauxite and alumina operations arising from a large excess of production capacity in the world compared with existing and anticipated demand. The remainder of the special charges and rationalization expenses relates to a program to reduce levels of management and the total number of employees, to costs associated with the sale and restructuring of a number of small businesses, to the reduction in value of certain overseas investments, and to the write-down of certain raw materials."

Reynolds Metals 85:3 (1985 Annual Report)

"Our company reported . . . a revised after-tax charge of $322 million for the writedown and other costs associated with various uneconomic assets."

SIC 3711

Chrysler 89:3 (1989 Annual Report)

"In September 1989, Chrysler sold 75 million shares of its equity investment in Mitsubishi Motors Corporation (MMC) for approximately $598 [million]. . . . The sale resulted in a gain before taxes of $503 million."

Chrysler 89:4 (1989 Annual Report)

"The results of operations for the year ended December 31, 1989 include a provision of $931 million for costs associated with a restructuring of Chrysler's automotive operations. The restructuring charge includes: the estimated costs of the discontinuation and curtailment of certain manufacturing operations and the elimination of certain product lines; the write-down of certain long-term assets; and the recognition of pension costs, unemployment benefits and other related costs for separated employees."

Chrysler 91:1 (1991 Annual Report)

"The results of operations for the year ended December 31, 1991 included a non-cash, nonrecurring credit provision of $391 million which is the result of a reduction in the planned capacity adjustments related to facilities acquired by the company in connection with its purchase of AMC in 1987."

General Motors 90:3 (1990 Annual Report)

"In 1990, a special restructuring charge of $3,314.0 million was included in the results of operations to provide for the closing of four previously idled U.S. assembly plants, as well as provide for other North American manufacturing and warehouse operations which will be consolidated or cease operating over the next three years."

General Motors Annual Report 1991

"In 1991, a special restructuring charge of $2,820.8 million was included in the results of operations to provide for the idling of six North American assembly, four powertrain, and 11 component plants."

SIC 4011

Burlington Northern 86:2 (1986 Annual Report)

"Our [1986] restructuring program was designed to adjust to the fundamental changes in our environment and to position the corporation to increase the utilization of its transportation, energy and real estate assets. We expect these actions to have a very positive effect on rates of return, cash flow and earnings in the years ahead.

"The principal items covered by the special charge of $1.7 billion before-tax include:

"A $600 million reserve to cover corporate-wide workforce reductions and costs associated with early retirements, severances, relocations, and elimination and consolidation of excess facilities.

"A $577 million writedown of some developed and non-producing oil and gas properties, reflecting their diminished value as a result of the rapid and unprecedented drop in energy prices. These properties represent a relatively small portion of our holdings and will not have a significant effect on our extensive hydrocarbon reserves.

"A $305 million writedown of Champlin's Corpus Christi refinery and its related marketing and distribution system in anticipation of completing our joint-venture agreement with Peteroleos de Venezuela, S. A. We are optimistic that the venture, which represents a good business opportunity for both parties, will be finalized in the near future. This transaction will free up cash and position the business to be a more consistent income and cash contributor.

"A $261 million writedown to cover excess rail equipment, probable future losses in a petrochemical venture and certain other items."

Burlington Northern 91:2 (1991 Annual Report)

"Included in 1991 results is a pre-tax special charge of $708 million related to railroad restructuring costs and increases in liabilities for casualty claims and environmental clean-up costs. The special charge is comprised of the following components:

"Restructuring—This program provides for workforce reduction of employees. The restructuring program and related charge has two components:

"$40 million to provide for employee related costs for a separation program.

"$185 million to provide for employee related costs for the elimination of surplus crew positions.

"Other—$350 million to increase casualty reserves based on an actuarial valuation and escalations in both the cost and number of projected hearing loss claims.

"$133 million to increase environmental reserves based on recently completed studies and analysis of potential environmental clean-up and restoration costs."

Union Pacific 86:2 (1986 Annual Report)

"In June 1986, the corporation announced a major restructuring program, which included a special charge against second quarter results. The special charge, which amounted to $1.7 billion, recognized the diminished value of certain assets and covered costs associated with reductions in employee levels throughout the corporation."

SIC 4512

USAir 90:4 (1990 Annual Report)

"Results for 1990 include special charges aggregating approximately $138 [million]."

USAir 91:4 (1991 Annual Report)

"Operating expenses for 1991 included a one-time gain of $107 million related to freezing of the fully funded non-contract employee pension plan."

References

Abarbanell, Jeffery S. "Do Analysts' Earnings Forecasts Incorporate Information in Prior Stock Price Changes?" *J. Accounting and Econ.* 14 (June 1991): 147–65.

Abarbanell, Jeffery S., and Bernard, Victor L. "Tests of Analysts' Overreaction/Underreaction to Earnings Information as an Explanation for Anomalous Stock Price Behavior." *J. Finance* 47 (July 1992): 1181–1207.

Ali, Ashiq; Klein, April; and Rosenfeld, James. "Analysts' Use of Information about Permanent and Transitory Earnings Components in Forecasting Annual EPS." *Accounting Rev.* 67 (January 1992): 183–98.

Ball, Ray, and Brown, Philip. "An Empirical Evaluation of Accounting Income Numbers." *J. Accounting Res.* 6 (Autumn 1968): 159–78.

Beaver, William H.; Clarke, Roger; and Wright, William F. "The Association

between Unsystematic Security Returns and the Magnitude of Earnings Forecast Errors." *J. Accounting Res.* 17 (Autumn 1979): 316–40.
Bernard, Victor L. "Cross-Sectional Dependence and Problems in Inference in Market-Based Accounting Research." *J. Accounting Res.* 25 (Spring 1987): 1–48.
Brown, Bryan W., and Maital, Shlomo. "What Do Economists Know? An Empirical Study of Experts' Expectations." *Econometrica* 49 (March 1981): 491–504.
Brown, Lawrence D.; Griffin, Paul A.; Hagerman, Robert L.; and Zmijewski, Mark E. "Security Analyst Superiority Relative to Univariate Time-Series Models in Forecasting Quarterly Earnings." *J. Accounting and Econ.* 9 (April 1987): 61–87.
Brown, Lawrence D., and Rozeff, Michael S. "The Superiority of Analyst Forecasts as Measures of Expectations: Evidence from Earnings." *J. Finance* 33 (March 1978): 1–16.
Brown, Philip; Foster, George; and Noreen, Eric. *Security Analyst Multi-year Earnings Forecasts and the Capital Market.* Studies in Accounting Research, no. 21. Sarasota, Fla.: American Accounting Assoc., 1985.
Chamberlain, Gary. "Panel Data." In *Handbook of Econometrics*, vol. 2, edited by Zvi Griliches and Michael D. Intriligator. Amsterdam: North-Holland, 1984.
Collins, William A., and Hopwood, William S. "A Multivariant Analysis of Annual Earnings Forecasts Generated from Quarterly Forecasts of Financial Analysts and Univariate Time-Series Models." *J. Accounting Res.* 18 (Autumn 1980): 390–406.
Cragg, John G., and Malkiel, Burton G. "The Consensus and Accuracy of Some Predictions of the Growth of Corporate Earnings." *J. Finance* 23 (March 1968): 67–84.
Crichfield, Timothy; Dyckman, Thomas R.; and Lakonishok, Josef. "An Evaluation of Security Analysts' Forecasts." *Accounting Rev.* 53 (July 1978): 651–68.
De Bondt, Werner F. M., and Thaler, Richard H. "Does the Stock Market Overreact?" *J. Finance* 40 (July 1985): 793–805.
———. "Do Security Analysts Overreact?" *A.E.R. Papers and Proc.* 80 (May 1990): 52–57.
Elton, Edwin J., and Gruber, Martin J. "Earnings Estimates and the Accuracy of Expectational Data." *Management Sci.* 18 (April 1972): B409–B424.
Elton, Edwin J.; Gruber, Martin J.; and Gultekin, Mustafa N. "Professional Expectations: Accuracy and Diagnosis of Errors." *J. Financial and Quantitative Analysis* 19 (December 1984): 351–63.
Figlewski, Stephen, and Wachtel, Paul. "The Formation of Inflationary Expectations." *Rev. Econ. and Statis.* 63 (February 1981): 1–10.
Frankel, Jeffrey A., and Froot, Kenneth A. "Using Survey Data to Test Standard Propositions Regarding Exchange Rate Expectations." *A.E.R.* 77 (March 1987): 133–53.
Fried, Dov, and Givoly, Dan. "Financial Analysts' Forecasts of Earnings: A Better Surrogate for Market Expectations." *J. Accounting and Econ.* 4 (October 1982): 85–107.
Givoly, Dan. "The Formation of Earnings Expectations." *Accounting Rev.* 60 (July 1985): 372–86.
Givoly, Dan, and Lakonishok, Josef. "The Information Content of Financial Analysts' Forecasts of Earnings: Some Evidence on Semi-strong Inefficiency." *J. Accounting and Econ.* 1 (December 1979): 165–85.

Hansen, Lars Peter, and Hodrick, Robert J. "Forward Exchange Rates as Optimal Predictors of Future Spot Rates: An Econometric Analysis." *J.P.E.* 88 (October 1980): 829–53.

Hughes, John S., and Ricks, William E. "Associations between Forecast Errors and Excess Returns Near to Earnings Announcements." *Accounting Rev.* 62 (January 1987): 158–75.

Institutional Brokers Estimate System. "Ordinary and Extraordinary Accuracy." New York: Inst. Brokers Estimate System, 1987.

Keane, Michael P., and Runkle, David E. "Testing the Rationality of Price Forecasts: New Evidence from Panel Data." *A.E.R.* 80 (September 1990): 714–35.

Klein, April. "A Direct Test of the Cognitive Bias Theory of Share Price Reversals." *J. Accounting and Econ.* 13 (July 1990): 155–66.

Lys, Thomas, and Sohn, Sungkyu. "The Association between Revisions of Financial Analysts' Earnings Forecasts and Security-Price Changes." *J. Accounting and Econ.* 13 (December 1990): 341–63.

Mendenhall, Richard R. "Evidence on the Possible Underweighting of Earnings-Related Information." *J. Accounting Res.* 29 (Spring 1991): 170–79.

Muth, John F. "Rational Expectations and the Theory of Price Movements." *Econometrica* 29 (July 1961): 315–35.

O'Brien, Patricia C. "Analysts' Forecasts as Earnings Expectations." *J. Accounting and Econ.* 10 (January 1988): 53–83.

———. "Forecast Accuracy of Individual Analysts in Nine Industries." *J. Accounting Res.* 28 (Autumn 1990): 286–304.

Philbrick, Donna R., and Ricks, William E. "Using Value Line and IBES Analyst Forecasts in Accounting Research." *J. Accounting Res.* 29 (Autumn 1991): 397–417.

Stickel, Scott E. "Predicting Individual Analyst Earnings Forecasts." *J. Accounting Res.* 28 (Autumn 1990): 409–17.

Xiang, B. "On the Rationality of Value Line's Quarterly Earnings Forecasts: Evidence from Alternative Methods." Ph.D. dissertation, Univ. Alberta, 1992.

Zarnowitz, Victor. "Rational Expectations and Macroeconomic Forecasts." *J. Bus. and Econ. Statis.* 3 (October 1985): 293–311.

The effect of the SEC's regulation fair disclosure on analyst forecast attributes

Rong Yang, Yaw M Mensah. **Journal of Financial Regulation and Compliance**. London: 2006. Vol. 14, Iss. 2; pg. 192, 18 pgs

Abstract (Summary)

This study aims to examine the effect of the Securities and Exchange Commission's regulation fair disclosure (Reg FD) on analyst forecast performance for pre-Reg FD closed-call (CLC) and open-call (OPC) firms compared with the non-conference-call (NCC) firms in the post-Reg FD period. Specifically, it examines whether Reg FD influenced the earnings forecast accuracy and forecast dispersion of financial analysts for the previous-CLC firms in the post-Reg FD period compared with the previous-OPC firms, and both sets of conference call firms relative to the NCC firms in the same period. The main findings indicate that forecast accuracy improved for both OPC and CLC firms compared with the NCC firms in the post-Reg FD period. More importantly, the differences in earnings forecast performance between the pre-Reg FD OPC and CLC firms had disappeared in the post-Reg FD period.

Full Text (5635 words)

[Headnote]

Abstract

Purpose - This study aims to examine the effect of the securities and Exchange Commission's regulation fair disclosure (Reg. FD) on analyst forecast performance for pre-Reg. FD closed-call (CLC) and open-call (OPC) firms compared with the non-conference-call (NCC) firms in the post-Reg. FD period.

Design/methodology/approach - Specifically, it examines whether Reg. FD influenced the earnings forecast accuracy and forecast dispersion of financial analysts for the previous-CLC firms in the post-Reg. FD period compared with the previous-OPC firms, and both sets of conference call firms relative to the NCC firms in the same period.

Findings - The main findings indicate that forecast accuracy improved for both OPC and CLC firms compared with the NCC firms in the post-Reg. FD period. More importantly, the differences in earnings forecast performance between the pre-Reg. FD OPC and CLC firms had disappeared in the post-Reg. FD period.

Originality/value - These results offer further confirmation of previous findings that Reg. FD has contributed to leveling the playing field for financial analysts and investors.

Keywords Financial institutions, Earnings, Forecasting, Disclosure, Conferencing

Paper type Viewpoint

1. Introduction

On October 23, 2000, the US securities and Exchange Commission (sec) issued regulation fair disclosure (hereafter Reg. FD) which prohibits selective disclosure of material nonpublic information to certain financial analysts, institutional investors and others prior to making it available to the general public. Information is considered material if it is important enough to persuade an investor to buy or sell a stock. Before the implementation of Reg. FD, most conference calls were accessible only to certain analysts and institutional investors. It has been argued that conference calls, because they were predominantly closed, may have contributed to an information gap between analysts privy to the call and analysts and other investors excluded from the call. The intent of Reg. FD was to prevent this selective disclosure of information.

A number of published studies have already examined the impact of Reg. FD on various aspects of the capital markets and investment climate, including the effect on analyst forecast accuracy and dispersion, although the findings have been contradictory. Using data from the first three quarters after the release of Reg. FD, Agarwal and Chadha (2003) report that sell-side analysts' forecasts were less accurate and more dispersed than before its adoption, where Heflin et al (2003) report no change in analysts' earnings forecast bias, accuracy or dispersion compared to the pre-Reg. FD period. Furthermore, Shane et al. (2001), also using data from the same period, find that analysts gathered more information between earnings announcements so that their forecasts are ultimately as accurate as those made in the period before Reg. FD was adopted.

This study has two main objectives. The first is to examine if there were changes in analyst earnings forecast errors (FE) and forecast dispersion (FD) in the pre- and post-Reg. FD period between the "closed-call" (henceforth referred to as CLC) firms and "open-call" (OPC) firms. The second objective is to determine if there were any changes in analyst earnings forecast attributes between the CLC and OPC firms as a group (labeled CC - conference call firms), and the non-conference-call (NCC) firms in the post-Reg. FD environment.

Thus, this study contributes to the existing literature by differentiating between firms in the pre-Reg. FD period that held closed conference calls, firms that held open conference calls, and other firms which held NCCs. By limiting the study only to OPC and NCC firms in the post-Reg. FD period, we are able to control for extraneous factors such as changing group membership in our analyses. second, because the study covers the period from October 1998 to September 2002, more quarterly observations are available to conduct the tests than in previous research.

The remainder of this study is organized as follows. section 2 presents a brief summary of previous studies focused on only the main sources, and an outline of the hypotheses examined in the paper. section 3 describes the sample selection and a brief outline of our research methodology. section 4 presents the major results of the study. section 5 presents the conclusions and suggestions for future research. In the Appendix, we provide details on the research methodology and the regression equations used to analyze the data.

2. Literature review and hypothesis development

2.1 Brief review

Economic theory suggests that expanded disclosures can reduce information asymmetry arising between the firm and its shareholders or among potential buyers and sellers of firm shares and benefit firms by correcting any firm mis-valuation and increasing institutional interest and liquidity for the firm's stock. For example, Diamond and Verrecchia (1991) find that credible commitments by managers to improve disclosure increasing the precision of public information about firm value results in higher current stock prices due to reduced information asymmetry and increased liquidity. Frankel et aL (1999) provide evidence that firms holding conference calls as a voluntary disclosure medium tend to be relatively larger, more profitable, more heavily followed by analysts, and access the capital markets more often than other firms.

In other related findings, Bowen et al (2002) provide evidence that regular use of earnings-related conference calls could present a selective disclosure problem if the public is not privy to these calls, even if conference calls tend to reduce both FE and FD. Bushee and Noe (2000) find that firms with greater analyst following and greater institutional ownership are less likely to have conference calls that provide open access to all investors. Core (2001) presents evidence consistent with the intuition that informed investors prefer less disclosure, and that analysts and institutions produce information that reduces information asymmetry and the need for conference calls.

As cited previously, some of the research focused on the effect of Reg. FD on financial analyst behavior have yielded mixed results. In general, however, the majority of these studies conclude that Reg. FD has had the intended benefit of diminishing the information advantage of analysts with previously exclusive access to management, although some anecdotal stories in the press still hint at the continued exclusive disclosure of material non-public information (Wall Street Journal, 2004). Interested readers can contact the lead author for a more detailed reference list.

2.2 Expected effects of Reg. FD on analysts forecast performance and related stock market

The arguments surrounding Reg. FD revolve around two major themes:

(1) its potential to level the playing field for all investors; and

(2) its potential to increase the cost of capital by restricting the availability of information to investors.

The first of these themes relies on the rationale that, by providing equal access to firm information, Reg. FD can reduce the level of information asymmetry, leading stock prices to be less dependent on private information. This logic implies that any loss of accuracy in earnings forecasts by analysts would be offset by the wider dissemination of information and hence, a more informed general investor population. In addition, Reg. FD may enhance the accuracy and precision of analysts' earnings forecasts, if it succeeded in opening up new sources of information to analysts, or if analysts could substitute the information obtained directly from companies with the information gathered from customers, suppliers, competitor's industry observers, and other sources of information. That is consistent with Mohanram and Sunder's (2006) finding, analysts may substitute privately acquired information for public-disclosed information for firms after the enactment of Reg. FD.

The counter-argument relies on the possibility that Reg. FD could have an adverse effect on certain analysts' forecast accuracy through denying them the sometimes-exclusive access to management that they previously enjoyed. Given the important role of financial analysts as intermediaries who provide professional investment to the capital markets, the decreased accuracy may have deleterious capital market consequences. In addition, it has been argued that Reg. FD induce firms to reduce the level of information and guidance that they may have provided originally in the closed conference calls, but which they may be unwilling to impart in open conference calls.

Recently, Bushee et al (2004) find that Reg. FD had a significantly negative impact on managers' decisions to continue hosting conference calls even though this impact was not large. Hence, the level of specialty guidance may have decreased in the post-Reg. FD period. At the same time, Gintschel and Markov (2004) report that the informativeness of analysts output has dropped in the post-FD environment. Specifically, they found that the absolute price impact of information disseminated by financial analysts dropped by 28 percent in this period. Eleswarapu et aL (2004) also report that the return volatility around mandatory announcements had decreased, and the impact was more pronounced for smaller and less liquid stocks. Taken together, these results suggest a strong impact of Reg. FD on the functioning of capital markets.

2.3 Hypothesis development

Extant studies assume that public information is common across all analysts and private information is idiosyncratic and uncorrelated across analysts. They have used FE and FD as proxies for analyst forecast attributes. Both FE and FD capture the extent to which private information differs across analysts, which also represents the level of actual past selective disclosure. For instance, Barron et aL (1998) present a model that expresses two properties of their forecasts, proxied by both dispersion in individual forecasts and the squared error in the mean forecast, as functions of the amount or "precision" of analysts' public and private information in forecasting firms' earnings. Sunder (2001) further find that "restricted-call" firms faced higher information asymmetry compared to "open-call" firms in the pre-Reg. FD period, while in the post-Reg. FD period, the differences in information asymmetry between two groups do not persist.

In summary, analysts should make more FE for OPC firms than for CLC firms if open conference calls do not provide as much information as closed conference calls. The first objective of Reg. FD was to level the playing field among all investors and analysts with respect to access to corporate information. If this objective were achieved with the implementation of Reg. FD, then one observable effect should be no difference in analysts' earnings forecast attributes between the previous-OPC and previous-CLC firms. This line of reasoning leads to the following set of hypotheses (stated in null form):

H^sub o^1.1. Analysts' quarterly earnings FE for the previous-CLC firms are not significantly different from those for the previous-OPC firms in the post-Reg. FD period (i.e. FE^sup CLC^^sub POST^ [asymptotically =] FE^sup OPC^^sub POST^).

H^sup o^1.2. Analysts' quarterly earnings FD for the previous-CLC firms is not significantly different from that for the previous-OPC firms in the post-Reg. FD period (i.e. FD^sup CLC^^sub POST^ [asymptotically =] FD^sup OPC^^sub POST^).

Using the same line of reasoning, it can be argued that the earnings FE and FD of NCC firms should be greater than those of both CLC and OPC firms (if they remained conference call firms) in the post-Reg. FD period. In other words, Reg. FD's exclusive effect should be on closing the information gap between the OPC and CLC firms, but should have no effect on the greater informativeness of conference calls as a means of communicating more information to investors (as demonstrated by prior research). This leads to the following set of hypotheses (in alternative form):

H^sub a^1.3. Analysts' quarterly earnings FE for NCC firms are significantly greater than those for both previous-CLC and OPC firms in the post-Reg. FD period (i.e. FE^sup NCC^^sub POST^ > (FE^sup CLC^^sub POST^, FE^sup OPC^^sub POST^)).

H^sub a^ 1.4. Analysts' quarterly earnings FD for NCC firms is significantly greater than that for both previous-CLC and OPC firms in the post-Reg. FD period (i.e. FD^sup NCC^^sub POST^ > (FD^sup CLC^ ^sub POST^, FD^sup OPC^^sub POST^)).

In addition to the effects hypothesized above, the effectiveness of Reg. FD can be further evaluated by its effect on changes in the forecast attributes. That is, if the equality of the earnings forecast attributes between the CLC and OPC firms in the post-Reg. FD period is to be attributed to the adoption of Reg. FD, then the change in the forecast attributes from the pre- to the post-FD period should reflect this. So the absolute change in both FE and FD for the previous-CLC firms should be bigger than those for the OPC firms. These hypotheses can be stated in alternative form as follows:

H^sub a^2.1. The absolute change in analysts' quarterly earnings FE for the previous-CLC firms is significantly higher than that for the previous-OPC firms in the post-Reg. FD period (i.e. |ΔFE^sup CLC^| > |ΔFE^sup OPC^|).

H^sub a^ 2.2. The absolute change in analysts' quarterly earnings FD for the previous-CLC firms is significantly higher than that for the previous-OPC firms in the post-Reg. FD period (i.e. |ΔFD^sup CLC^| > |ΔFD^sup OPC^|).

3. Brief description of research methodology

3.1 Sampk selection

Following the Bushee et al. (2003) approach, firms on the Bestcalls.com list are considered to be "open-call" firms (i.e. calls that allow unlimited real time access), while the firms provided by First Call Corporation but not included on the Bestcalls.com list are considered to be "closed-call" firms (i.e. calls that restrict access to invited professionals) in the pre-Reg. FD period. According to Bowen et al. (2002, p. 286, footnote 1), Bestcalls.com launched a web site in March 1999 publicizing the dates and times of conference calls open to individual investors. However, some firms did not allow individuals access to their calls. Meanwhile, other firms began live broadcasts of their conference calls using internet web casts. So it is reasonable to assume that after March 1999, all firms on the Bestcalls.com list had OPCs. Therefore, we divide the samples into three groups, OPC, CLC and NCC (where no disclosures are made via conference calls) firms in the pre-Reg. FD period. More specifically, the firms listed by the Bestcalls.com are regarded as OPC firms, while the firms listed by First Call Corporation but not included in the Bestcalls.com list are regarded as CLC fims. Firms listed in CRSP and the I/B/E/S databases but not included in either Bestcalls.com or First Call Corporation lists are regarded as pre-NCC firms.

To obtain better control of extraneous factors, the sample is restricted to firms which retained their status in both pre- and post-Reg. FD environments. We exclude fims that Bestcalls.com lists as NCC firms, as well as NCC firms now listed as CC firms. The analyst forecast data used are obtained from I/B/E/S database, and earnings announcement dates and other control variables from quarterly Compustat data sets. To ensure the meaningful computation of dispersion, the minimum number of analysts following a firm is set to four. All firms are required to have non-missing quarterly I/B/E/S forecast data during the period of October 1998 through September 2002 and non-missing quarterly Compustat data. After applying this screening process, the surviving sample consists of 1,697 firms (521 OPC, 990 CLC, and 186 NCC fims). The total final sample consists of 12,806 firm-quarter observations in the pre-Reg. FD period, and 13,104 firm-quarter observations in the post-Reg. FD period.

3.2 Research methodology

Empirical accounting research frequently utilizes the properties of analyst forecasts, such as accuracy, dispersion, bias, etc. to construct proxies for variables of interest. For instance, FD and errors in the mean forecast are used to proxy for the uncertainty or the degree of consensus among analysts or market expectations. Based on prior research, we estimated the effect of Reg. FD on analysts' forecast attributes by running a series of regression equations. Technical details on the regressions estimated are provided in Appendix. The description below is a brief summary of the approach used in the paper.

To control for factors that have been shown in prior research to be highly related to the levels of analyst FE and FD, we include in our regressions proxies measures for firm size, industry effect, earnings predictability, earnings surprise, and age of the forecast. Firm size and the level of FE or the level of FD are proxies for the richness of the firm's information environment. The ability of analysts to forecast the current quarter's earnings depends on both earnings surprise in the prior quarter and any information disclosed during the conference call. Forecast age is also an important determinant of forecast accuracy.

We estimate two regression equations, with the dependent variable in the first equation the absolute FE, and in the second equation, the FD. The independent variables in both equations include the dummy variables to represent the CLC and OPC firms, interaction terms to control for the presence of high-technology firms in the sample, forecast age (AGE), the number of analysts which follow a given firm (ANA), the size of earnings surprise in the previously released quarterly earnings (SURP), and firm size (SIZE).

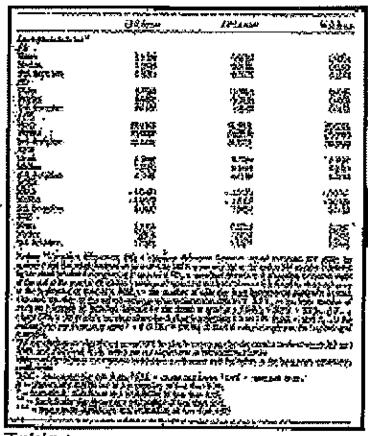
The interaction terms for high-technology firms are designed to evaluate whether forecast attributes are consistently different for firms in the high technology sector. Barron et al (2002) find that lower levels of analyst consensus are associated with high-tech firms because of their relatively high R&D expenditures. Therefore, a significantly positive coefficient on HighTech is consistent with the belief that analysts make more FE and dispersion for high-technology firms due to a higher information asymmetry as compared to non-high-technology firms.

4. Empirical results

4.1 Descriptive statistics

Tables I-III present some descriptive statistics on the post-Reg. FD period variables. Panel A reveals that both the mean and the median of analyst FE for NCC firms are greater than those for CLC and OPC firms in the post-Reg. FD period. Also the median of FD for NCC firms is greater than the median for both OPC and CLC firms in the post-Reg. FD period. Panel B presents the significant difference in means of FE and FD using statistical tests for the differences (specifically, Scheffe's tests and Wests) in the post-Reg. FD period.

The first part of panel B shows that the means of OPC and CLC firms are not statistically different (at the 0.05 probability level), whereas the means for the other two groups, NCC and OPC, NCC and CLC are significantly different in the post-Reg. FD period. On the other hand, the second part of panel B shows the means between NCC and CC, and between CLC and OPC in the post-Reg. FD period are statistically different. All the t-values are significant for each comparison except for the comparison of FD between NCC and CC in the post-Reg. FD period. These preliminary results are generally consistent with H1.1-H1.4.



Enlarge 200%
Enlarge 400%

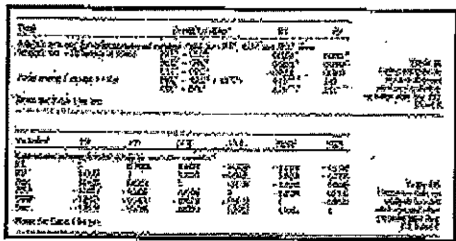
Table I.
Univariate tests on analysts forecast attributes and other variables after Reg. FD: Panel A

Panel C presents correlation coefficients (both the Pearson product-moment and Spearman rank-order correlations) between analyst forecast attributes and their determinants in the post-Reg. FD period. All the correlation coefficients have signs consistent with those expected for the regression coefficients and all are significant except for the correlation coefficient between the number of analysts following (ANA) and forecast age (AGE), and between ANA and earnings surprise (SURP). The correlation coefficients between the number of analysts following (ANA) and the firm size (SIZE) is the highest among all coefficients, which is consistent with the previous research findings that large firms usually have a large group of analysts following regardless of the implementation of Reg. FD.

4.2 Regression results

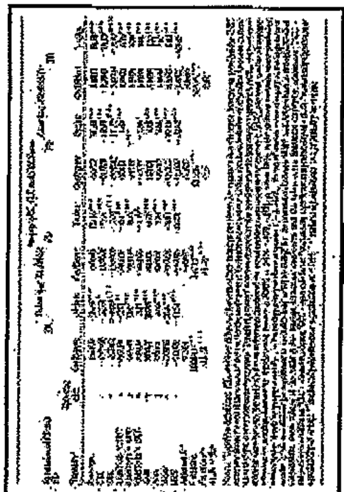
Table FV presents the results of regressing analyst FE and FD in the pre- and post-Reg. FD periods by using equations (1) and (2). As expected, the coefficients of two dummy variables, CLC and OPC, are significantly negative. Moreover, the coefficients of CLC are greater than the coefficients of OPC for both regressions of FE and FD in both preand post-Reg. FD periods. Also as expected, forecast age (AGE), the number of forecasts (ANA) and high-tech firms (HighTech) are positively associated with FE and FD, while earnings surprise (SURP) and firm size (SIZE) are negatively associated with FE and FD.

Focusing on the tests of H1-H4, the results in Table IV (PRE period) indicate that conference calls did provide additional information to financial analysts, with both OPC and CLC firms having fewer earnings FE than NCC firms prior to the implementation of Reg. FD. This conclusion can be drawn from the differences in the values of the intercepts terms for the NCC and CLC dummy variables. The intercept of the regression of FE in the pre-Reg. FD period is 0.0169 for NCC firms, 0.0163 (i.e. 0.0169 - 0.0006) for CLC firms, and 0.0158 (i.e. 0.0169 - 0.0011) for OPC firms. The intercept of the regression of FD in the pre-Reg. FD period is 0.0049 for NCC firms, 0.0045 (i.e. 0.0049 - 0.0004) for CLC firms, and 0.0044 (i.e. 0.0049 - 0.0005) for OPC firms.



Enlarge 200%
Enlarge 400%

Table II.
Univariate tests on analysts forecast attributes and other variables after Reg. FD: Panel B
Table III.
Univariate tests on analysts forecast attributes and other variables after Reg. FD: Panel C



Enlarge 200%
Enlarge 400%

Table IV.
Regression of analyst FE and dispersion on both pre- and post-Reg. FD variables

Further examination of the regressions results in Table IV (POST period) supports the inference that analysts still made more FE and had higher FD for the NCC firms as compared to the OPC and CLC firms after the release of Reg. FD. In the post-Reg. FD period, the intercept of the regression of FE is 0.0877 for NCC firms, 0.0652 (i.e. 0.0877 - 0.0225) for CLC firms, and 0.0645 (i.e. 0.0877 - 0.0232) for OPC firms. The intercept of the regression of FD in the post-Reg. FD period is 0.0203 for NCC firms, 0.0158 (i.e. 0.0203 - 0.0045) for CLC firms, and 0.0157 (i.e. 0.0203 - 0.0046) for OPC firms.

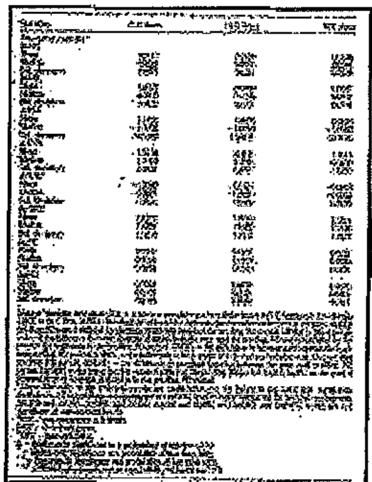
To determine if Reg. FD has any impact on analysts FE, it is necessary to compare the coefficients across CLC and OPC firms within each period which can be done using the standard F-test. The F-tests performed show that the observed differences between the coefficients of interest (α^sub 1^ and α^sub 2^ in equation (1) and β^sub 1^ and α^sub 2^ in equation (2) in the Appendix) support the hypotheses presented earlier. In the pre-Reg. FD period, the F-value for FE (FD) is 31.73 (11.28), and the Rvalue is significant at the 0.001 level. Thus, these two null hypotheses that α^sub 1^ = α^sub 2^, and β^sub 1^ = β^sub 2^ can both be rejected. However, in the post-Reg. FD period, the F-value for FE (FD) is 0.37 (0.11) with an insignificant probability level. Thus, the null hypotheses that α^sub 1^ = α^sub 2^ in equation (1) and β^sub 1^ = β^sub 2^ in equation (2) cannot be rejected.

In summary, there are observable differences in the regression coefficients between CLC and OPC firms in the PRE period, and these statistically significant differences in coefficients disappear in the POST period. These results thus support both H 1.1 and H1.2, and provide evidence that differences in analyst forecast performance between the previous-CLC and previous-OPC firms do not persist after Reg. FD went into effect[t].

4.3 Univariate analyses of change in analyst forecast attributes

Tables V-VII present some descriptive statistics on the absolute change in analyst FE (|ΔFE|) and FD (|ΔFD|). From panel A, it can be observed that the means of |ΔFE| and |ΔFD| for CLC firms are smaller than those for OPC firms. Panel B presents the significant difference in means of the absolute change in FE and FD using both Scheffe's tests and the pairwise t-tests.

The results from Scheffe's tests show the comparisons in means are significantly different at the 0.05 level among three groups except for one comparison, |ΔFE| between CLC and OPC firms. At the same time, the results from the t-tests show that there is no significant difference in mean levels of |ΔFE| or |ΔFD| for the comparison between NCC and CC (including CLC and OPC firms) firms and the comparison between CLC and OPC firms. Panel C presents the Pearson and Spearman correlation coefficients between the absolute change in analyst forecast attributes and their determinants.



Enlarge 200%
Enlarge 400%

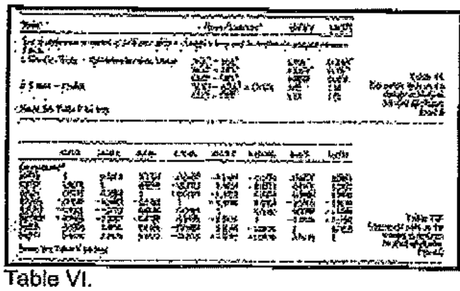
Table V.
Univariate tests on the change in analysts forecast attributes: Panel A

4.4 Regression results for change in analyst forecast attributes

Table VIII presents the regression results obtained when the absolute changes in analyst quarterly FE (|ΔFE|) and FD (|ΔFD|) are regressed on the hypothesized independent variables (as presented in equations (3) and (4) in Appendix). The sign of coefficients on the dummy variable, OPC, for both regressions of |ΔFE| and |ΔFD| is not significant, a result which contradicts H2.1 and H2.2. In addition, the sign of coefficients on the dummy variable, NCC, is significantly positive for both regressions of ΔFE and |ΔFD|.

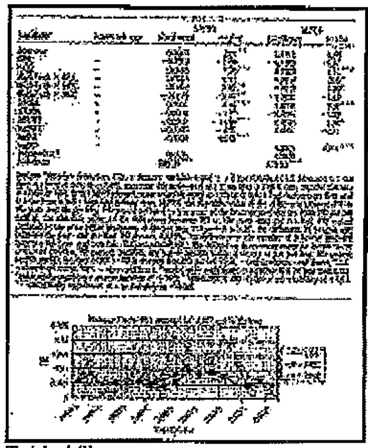
Because we adopt October 23,2000 as the boundary between the pre-Reg. FD period and the post-period, it is possible that the failure to support H2.1 and H2.2 may be due to the choice of the cut-off date. Previous research by Mac (2003) finds that firms had already changed their voluntary disclosure policy in the pre-enactment period (December 20,1999-October 22, 2000), before Reg. FD became effective on October 23, 2000. Thus, if some firms in the sample have already changed their voluntary disclosure policy prior to the release of Reg. FD because they anticipate the passage of Reg. FD, the tests may not be sufficiently powerful.

Figures 1 and 2 show the graph of the means of FE and FD among three groups, CLC, OPC and NCC firms, from the third quarter of 1998 to the third quarter of 2002. Both Figures 1 and 2 show that FE and FD for NCC firms are higher than those for both OPC and CLC firms in both pre- and post-Reg. FD periods. However, the means of FE (FD) for CLC firms are greater than those for OPC firms in the pre-Reg. FD period (before the third quarter of 2000), but generally indistinguishable in the post-Reg. FD period.



Enlarge 200%
Enlarge 400%

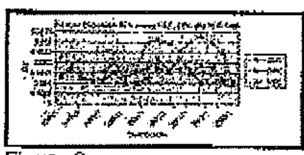
Table VI.
Univariate tests on the change in analysts forecast attributes: Panel B
Table VII
Univariate tests on the change in analysts forecast attributes: Panel C



Enlarge 200%
Enlarge 400%

Table VII.
Regression of the change in analyst forecast attributes
Figure 1.

The statistical tests performed earlier show that the difference in OPC and CLC means for FE and FD are not statistically significant (when the control variables are accounted for) in the post-Reg. FD period. However, CLC firms have statistically significant (and positive) intercepts compared to NCC firms in both pre- and post-Reg. FD periods. This finding indicates that both FE and FD for NCC firms increase relative to those of OPC and CLC firms (both of which held conference calls). Thus, the overall view conveyed is that conference calls continue to be useful in helping analysts to produce accurate forecasts during a period when NCC firms experience a huge jump in earnings FE and FD.



Enlarge 200%
Enlarge 400%

Figure 2.

4.5 Additional analysis and robustness tests

It can be argued that FE is another factor which affects FD. To evaluate this possibility, we use a recursive two-stage regression approach by allowing FE to be included as an explanatory variable for the FD equation. The regression results of FE and FD are qualitatively consistent with the previous results without adding FE in the regression of FD.

To evaluate the robustness of these results to possible outliers, we apply four diagnostic tests recommended by Belsley et al (1980):

(1) the diagonal of the projection matrix (Hat matrix);

(2) the studentized residuals (RSYUDENT);

(3) the change in the determinants of the covariance matrix of the estimates (CovRatio); and

(4) the change in the predicted value (DFFITS).

The filters are applied by setting observations exceeding the cutoffs recommended by Belsley et al. (1980) to missing values. Qualitatively, the results are the same regardless of whether the outliers are eliminated or not.

5. Conclusion

Prior to the release of Reg. FD, CLC firms were accustomed to disclosing material nonpublic information to certain analysts and institutional investors while not concurrently releasing the information to the general public. There is considerable anecdotal evidence indicating that managers penalize analysts based on the content of their forecasts by limiting or cutting off analysts' future contact with management. Since, voluntary disclosures (e.g. conference calls) put individual investors at a larger informational disadvantage, it has been of concern to the sec that the effect of selective disclosure is similar to insider trading. The primary purpose of Reg. FD is to curtail analysts' private channels to companies that they had previously enjoyed.

The results of this study are somewhat mixed. On one hand, there is support for the inference that, at least with respect to closing the information gap between analysts privy to the closed conference calls and those not privy to these calls, Reg. FD succeeded in that no statistical difference in earnings FE and FD between the previous-CLC and previous-OPC firms remained in the post-Reg. FD period. Moreover, in the post-Reg. FD period, conference calls continue to lead to lower FE and FD for both previous-OPC and previous-CLC firms, despite a huge jump in the earnings forecast attributes for firms which do not hold conference calls.

Against these favorable findings may be offset the contrary finding that no change in the earnings forecast attributes centered on the actual date of adoption of Reg. FD could be detected. Moreover, the findings reported by Gintschel and Markov (2004) that the informativeness of analysts' information output have declined in the post-Reg. FD period suggests that analysts' forecast attributes may no longer play as vital a role in the capital markets as in the pre-Reg. FD period. To the extent that this was the intent of the sec in adopting Reg. FD, then the policy may be deemed to be a success[2].

[Footnote]
Notes
1. These results are consistent with the findings reported by Shane et al (2001). They provide evidence that analysts gather relatively more uncertainty-relieving information between earnings announcements and by the end of the quarter, their forecasts are as accurate as they were in the prior year. That is to say, the previous-CLC firms may have changed their selective disclosure policy, and Reg. FD may have contributed to the leveling of such information asymmetry.
2. It is not clear what the implication of the findings of Clement and Tse (2003) that investors respond more strongly to the earlier forecasts than to the later forecasts (despite the greater accuracy of the later forecasts) are to the findings reported by Gintschel and Markov (2004). Presumably, analysts forecasts may be more useful when released early that later. The effect of Reg. FD on analyst behavior in terms of earlier or later revisions of forecasts have yet to be examined.

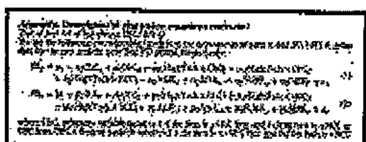
[Reference]
References

Agarwal, A. and Chedha, S. (2003), "Who is afraid of Reg. FD? The behavior and performance of sell-side analysts following the sec's fair disclosure rules", working paper, University of Alabama, Tuscaloosa, AL.
Barron, O.E., Byard, D., Kile, C., Riedl, EJ. and Demers, E. (2002), "High-technology intangibles and analysts' forecasts/discussion", Journal of Accounting Research, Vol. 40, pp. 289-313.
Barron, O.E., Kim, 0., Lim, S.C. and Stevens, D.E. (1998), "Using analysts' forecasts to measure properties of analysts' information environment", The Accounting Review, Vol. 73, pp. 421-33.
Belsley, D.A., Kuh, E. and Welsch, R. (1980), Regression Diagnostics, Wiley, New York, NY.
Bowen, R.M., Davis, A.K. and Matsumoto, D.A. (2002), "Do conference calls affect analysts' forecasts?", The Accounting Review, Vol. 77, pp. 285-316.
Bushee, B. and Noe, C. (2000), "Disclosure quality, institutional investors, and stock return volatility", Journal of Accounting Research, pp. 171-202.
Bushee, B., Matsumoto, D. and Miller, G. (2003), "Open versus closed conference calls: the determinants and effects of

broadening access to disclosure", Journal of Accounting and Economics, Vol. 34, pp. 149-80.
Bushee, B., Matsumoto, D. and Miller, G. (2004), "Managerial and investor responses to disclosure regulation: the case of Reg. FD and conference calls", The Accounting Review, Vol. 79, pp. 617-743.
Clement, M. and Tse, S. (2003), "Do investors respond to analysts' forecast revisions as if forecast accuracy is all the matters?", The Accounting Review, Vol. 78, pp. 227-50.
Core, J.E. (2001), "A review of the empirical disclosure literature: discussion", Journal of Accounting and Economics, Vol. 31, pp. 441-56.
Diamond, D.W. and Verrecchia, R.E. (1991), "Disclosure, liquidity and the cost of capital", The Journal of Finance, Vol. 46, pp. 1325-60.
Dugan, I.J. (2004), "In the loop: how inside stock tips still flow despite regulatory crackdowns; some hedge funds, especially, find ways around efforts to level the playing field; heads-up on a rating change", Wall Street Journal, August 27, p. A.I.
Eleswarapu, V.R., Thompson, R. and Venkataraman, K. (2004), "The impact of regulation fair disclosure: trading costs and information asymmetry", Journal of Financial and Qualitative Analysis, Vol. 39, pp. 209-25.
Frankel, R., Johnson, M. and Skinner, DJ. (1999), "An empirical examination of conference calls as a voluntary disclosure medium", Journal of Accounting Research, Vol. 37, pp. 133-50.
Gintschel, A. and Markov, S. (2004), "The effectiveness of regulation FD", Journal of Accounting and Economics, Vol. 37, pp. 293-305.
Heflin, F., Subramanyam, K.R. and Zhang, Y. (2003), "Regulation FD and the financial information environment: early evidence", The Accounting Review, Vol. 78, pp. 1-38.
Mac, C. (2003), "The effects of regulation fair disclosure on information leakage", working paper, Columbia University, New York, NY.
Mohanram, F. and Sunder, S. (2006), "How has regulation FD affected the functioning of financial analysts?", Contemporary Accounting Research, Vol. 23 No. 2.
Shane, P., Soderstorm, N. and Yoon, S. (2001), "Earnings and price discovery in the post-Reg. FD information environment: a preliminary analysis", working paper, University of Colorado, Boulder, CO.
Sunder, S.V. (2001), "Investor access to conference call disclosures: impact of regulation fair disclosure on information asymmetry", working paper, New York University, New York, NY,

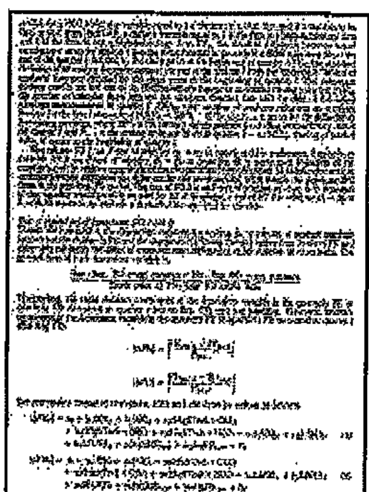
[Author Affiliation]
Rong Yang
Department of Business Administration, SUNY-College at Brockport, Brockport, New York, USA, and
Yaw M. Mensah
Rutgers Business School, Rutgers University, Piscataway, New Jersey, USA

[Author Affiliation]
Corresponding author
Rong Yang can be contacted at: ryang@brockport.edu



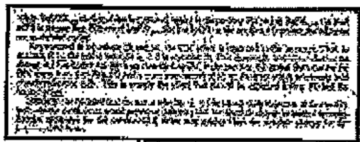
Enlarge 200%
Enlarge 400%

Appendix. Description of regression equations estimated



Enlarge 200%
Enlarge 400%

Appendix. Description of regression equations estimated



Enlarge 200%
Enlarge 400%

Appendix. Description of regression equations estimated

Indexing (document details)

Subjects:	Regulation FD, Earnings forecasting, Accuracy, Analysts, Impact analysis, Studies
Classification Codes	9190, 3100, 4310, 9130
Locations:	United States--US
Author(s):	Rong Yang, Yaw M Mensah
Author Affiliation:	Rong Yang Department of Business Administration, SUNY-College at Brockport, Brockport, New York, USA, and Yaw M. Mensah Rutgers Business School, Rutgers University, Piscataway, New Jersey, USA Corresponding author Rong Yang can be contacted at: ryang@brockport.edu
Document types:	Commentary
Document features:	Tables, Graphs, References, Equations
Section:	*FEATURE ARTICLE*
Publication title:	Journal of Financial Regulation and Compliance. London: 2006. Vol. 14, Iss. 2; pg. 192, 18 pgs
Source type:	Periodical
ISSN:	13581988
ProQuest document ID:	1043602421
Text Word Count	5635
Document URL:	http://proquest.umi.com/pqdlink?did=1043602421&sid=1&Fmt=4&clientId=58117&RQT=309&VName=PQD