Sarbanes-Oxley and IPO Analyst Bias

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I. Introduction

The new millennium ushered in a period of financial turmoil. In addition to the dot-com bubble bursting in early 2000, a number of high-profile corporate accounting scandals shook investor confidence over the next few years. The Enron accounting scandal, revealed in late 2001, resulted in the largest bankruptcy filing in U.S. history at the time, and an estimated loss to shareholders of over $10 billion[1]. The 'big five' accounting firm Arthur Andersen LLP, having declared Enron's 2000 financial statements to be accurate, was forced to relinquish its auditing license as a result, going from 85,000 employees in 2002 to only 200 in 2007. WorldCom's then-CEO Bernard Ebbers and his management team employed fraudulent accounting procedures between 1999 and 2002 to inflate the company's reported assets by $11 billion, topping Enron with an even larger Chapter 11 bankruptcy filing in late 2002.[2]

The result was a loss of public and investor confidence that lawmakers sought to correct. The Sarbanes-Oxley Act of 2002, formally known as the Public Company Accounting Reform and Investor Protection Act, established tighter standards for U.S. accounting firms, U.S. corporate boards, and white-collar crime; but it also placed more restrictions on brokerage analyst conflict of interest (henceforth 'analyst conflict of interest'). It is the effect of this last regulation that I will examine in this paper.

Simply put (as brokerage analysts will soon be discussed at greater length), brokerage analysts are charged with interpreting firms' financial reports to issue recommendations on whether to buy these firms' stock. Brokerage analyst conflict of interest can arise when the bank an analyst works for — often a full-service investment bank — has other dealings with these

[1] Benston (2006)
[2] SEC (2008)

firms, and when the bank can profit from the analyst giving the firm a higher rating than the firm's financials would otherwise merit.

Brokerage analyst conflict of interest is a long-documented phenomenon in the economic literature, particularly with respect to initial public offerings[3]. However, because changes in federal regulations — such as the Sarbanes-Oxley Act — are frequent, the nature and degree of analyst conflict of interest is often varying. This in turn necessitates that the literature re-asses new industry data over time to capture these changes, as well as to evaluate the efficacy of new regulations. For example, the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 has since strengthened some of the conflict of interest provisions of the Sarbanes-Oxley Act. However, the Dodd-Frank Act was passed so recently that it would be difficult to rigorously examine its effects, especially given the lack of IPOs in the last year: there are simply not enough data.

This paper aims to describe the degree to which brokerage analysts covering IPOs were biased towards those IPOs underwritten by their firm's investment banking division in the 2004-2005 period directly after the Sarbanes-Oxley Act was fully implemented. I will then compare these findings to an identical analysis of data taken from the 1997-1998 period before Sarbanes-Oxley was passed. I use 1997-1998 period because my analysis also uses 2-year IPO returns, and using any period later than 1997-1998 would risk misinterpreting the dot-com bubble

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[3] An Initial Public Offering, or IPO, is the first sale of ownership (stock) in a company to the public. The decision to 'go public' is often influenced by a need to raise investor capital, but can also be driven by the want for increased publicity that comes with being a publically traded company.

It is common for investment banks to be retained by the company going public in order to help with the IPO; in this context, they are called the 'underwriter' of the IPO. These underwriters are often involved in most or all of the complicated IPO process, from writing the prospectus, to marketing the firm to institutional investors, to setting the initial offer price for the IPO date. The choice of an underwriter is therefore extremely important in whether a company's IPO goes smoothly.

bursting with a variable I am trying to describe. In other words, 1997-1998 is the most recent pre-Sarbanes-Oxley period free of major market busts for which I could not account in my model.

Analysts

Brokerage analysts work for the brokerage services division of an investment bank. It is their job to analyze and interpret firm-specific and macro-level data in order to produce analyst reports on specific stocks. These analyst reports synthesize a company's publically reported financial metrics such as net margin, earning per share, return on assets, etc. into a recommendation to investors ranging from 'buy' to 'sell'. Implicit in these recommendations is the analyst's expectation for future earnings and stock price, for they would only issue a 'buy' recommendation if they expected the stock's price to increase in the future.

These analyst reports are then sold to hedge funds, money managers, institutional investors, and sometimes even private liberal arts colleges: they become a product the investment bank sells. The perceived quality — and therefore demand — for recommendations written by a given analyst is a function of the analyst's proven skill in interpreting stocks' future returns. However, this product is viewed differently by the corporate finance arm of investment banks than by the brokerage services branch, and therein lies analysts' conflict of interest.

While the brokerage arm of investment banks make money from analysts by selling their recommendations to investors, the corporate finance division also attracts IPO customers due to the implicit agreement of their brokerage research analysts to cover the stock post-IPO. Indeed, Krigman, Shaw, and Womack (1999)[4] attempt to qualify the role of analyst reputation in choice

[4] See Michaely (1999), p. 659.

of IPO underwriter. They find through a CEO and CFO survey in the 1990s that quality and reputation of the underwriting bank's research analysts were a "key factor" in their choice of an underwriter. With U.S. investment banks charging an average of 6.45% fees (Abrahamson 2011) on IPOs reaching into the hundreds of millions USD, the reputation of a brokerage analyst can be by far the most important factor describing that analyst's worth to their bank.

It is appropriate then that research analyst compensation be based on two factors at most firms. First, it is often based on the analyst's reputation as measured by the *Institutional Investor* All-American Research Teams poll[5]. This annual ranking of research analysts is computed from surveys of over 8,000 investment professionals. Secondly, analyst compensation and promotion is strongly linked to their ability to bring in underwriting clients. For example, Anita Raghavan argues in a *Wall Street Journal* article that an analyst's promotion from vice president to partner can be largely predicted by their history of contribution to underwriting revenue.

Analyst conflicts of interest are therefore especially strong in the case of IPOs. The incentive for analysts to positively exaggerate their recommendations of IPOs their bank underwrites is huge not only for their own compensation, but also for the present and future income of their firm. Pressure on analysts from banks can be so strong that the Sarbanes-Oxley Act added explicit protection from retaliation (termination of employment, for example) against analysts who give less than favorable reports to same-firm IPOs (see S-O, Sec. 15D, part 1ac). The fact remains, however, that the single biggest factor determining analyst compensation is contribution to underwriting fees. In choosing a lead underwriting bank, firms will naturally seek out the firm whose analysts will be most likely to give them a 'buy' recommendation, all else held equal.

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[5] Stickel (1992), cited in Michaely (1999).

On the other hand, analysts have a personal interest in preserving their reputation. Consistently overstating income expectations will in theory be met with a decrease in analyst ranking, and because analyst compensation depends so much on this factor, this could be a threat to an analyst's livelihood. Analysts also have ethical considerations to take into account in making recommendations they don't truly believe, solely because it would benefit them and their firm.

A factor that could otherwise explain higher recommendations by underwriter analysts is that analysts who work for the bank underwriting an IPO may have marginally more information about a firm than non-underwriter analysts. The concept of the 'Chinese Wall' between the brokerage analyst and corporate finance branches of a single investment bank states that, to ensure client confidence in the bank, information not be shared between these two arms of the bank. The idea is to theoretically prevent insider information from leaking out. If there is information spillover, however, this could indicate analyst selection bias: analysts might be more prone to initiate coverage on IPOs they can truthfully recommend as 'buys', based on this extra information. This could be a driving force behind the phenomenon of home bias in equity analysts, but we will be able to correct for this issue in our analysis.

Sarbanes-Oxley imposes three new conditions most relevant to this analysis:

i) "limiting the supervision and compensatory evaluation of securities analysts to officials employed by the broker or dealer who are not engaged in investment banking activities"

ii) "establishing structural and institutional safeguards within registered brokers or dealers to assure that securities analysts are separated by appropriate informational partitions within the firm from the review, pressure, or oversight of those whose involvement in investment banking activities might potentially bias their judgment or supervision"

iii) "requiring that a broker or dealer and persons employed by a broker or dealer who are involved with investment banking activities may not, directly or indirectly, retaliate against or threaten to retaliate against any securities analyst employed by that broker or dealer or its affiliates as a result of an adverse, negative, or otherwise unfavorable research report that may

adversely affect the present or prospective investment banking relationship of the broker or dealer with the issuer that is the subject of the research report"

In this paper, I will determine whether the Sarbanes-Oxley Act of 2002 did indeed decrease research analyst bias in evaluating IPOs. My main basis of comparison will be Michaely and Womack's 1999 paper 'Conflict of Interest and the Credibility of Underwriter Analyst Recommendations', published in *The Review of Financial Studies*. Michaely and Womack find that underwriter analysts are significantly more likely to initially issue a 'buy' recommendation on a given IPO than unaffiliated analysts. Furthermore, they find that affiliated analyst recommendations return on average 50% *less* over a 2-year holding period than their unaffiliated counterparts, across all industries studied.

II. Models

I will construct two models overall: two for the 1997-1998 'pre-Sarbanes-Oxley' period, and one for the 2004-2005 'post-Sarbanes-Oxley period'. It is the comparison of my results from these two periods that will inform my conclusion on the efficacy of this Act.

I will examine two effects of possible analyst home bias: inflated analyst recommendations and exaggeration of long-run returns. By not only looking at analysts' initial recommendation, but also at whether their estimates of future stock returns are accurate, I will be able to correct for the effect of information sharing within the investment bank.

If I find that underwriter analysts issue 'buy' recommendations more often for a given IPO, but that they are also more accurate with respect to long-run returns, then at least some of the perceived analyst bias may be attributed to enhanced information. If I find they exaggerate their recommendations but are no more accurate than their nonaffiliated counterparts, then this

will be clearer evidence that underwriter analysts are biased. If I find no evidence for either phenomenon, then I can simply treat affiliated and nonaffiliated analysts interchangeably, and conclude there is no significant bias associated with these affiliated analysts.

A preliminary model attempts to regress analyst recommendation on market capitalization, affiliation, and the EPS estimate corresponding to the recommendation in question. Apart from suffering from endogeneity between EPS estimates and analyst recommendation, this model also severely cuts down my sample size. To account for subsequent earnings reports being released and influencing analyst recommendations, I can only consider recommendations made before a company's first quarterly report in this model. In short, I cannot compare analyst recommendations when these recommendations are made using different financial statements: this would amount to serious omitted variable bias. So I do not employ this model specification, instead opting for a model that better describes the incentives behind brokerage analyst bias.

Pre-event Price Performance

My first model seeks to determine whether an analyst's "buy" recommendation is motivated by a stock's previous performance, rather than by other factors such as fundamental analysis. The intuition is that the conflict of interest for a brokerage analyst will be the most severe when their bank's IPO is performing poorly relative to the market, and is in need of price support. If this conflict of interest were ever to cause an analyst to issue a positive recommendation of one of their firm's IPOs, it would be when that IPO needs it the most — when its stock price is down. I therefore seek to describe the empirical relationship between

analyst affiliation and pre-recommendation stock returns. In quantifying this relationship, I only include "buy" and "strong buy" analyst recommendations, as a less enthusiastic recommendation of "hold" or "sell" could be correlated with fundamental analysis of observed pre-recommendation low returns. In other words, I only consider "buy" and "strong buy" recommendations in order to be sure that low returns influence these recommendations uniquely through the analyst conflict of interest relationship I am trying to evoke.

This is an especially tricky relationship to model due to the nature of the variable I am trying to describe, affiliation. To predict analyst affiliation, I would expect to regress this variable on a stock's pre-recommendation price performance and a host of control variables affecting affiliation through other channels, in order to parse out this conflict of interest relationship. However, doing so severely limits my ability to control for many factors influencing stock returns, such as market capitalization and sector. As these aspects of a stock affect returns but not necessarily analyst affiliation, their inclusion on the right-hand side of a regression might result in the estimation of spurious relationships, if any significant relationships at all. In order to resolve this issue, I estimate the following equation:

$$Perf_{(pre)} = C + \beta_1 * Affiliated + \beta_2 * MarketCap + \beta_3 * Time + \beta_4 * IndustryDummies \quad (1)$$

where $Perf_{(pre)}$ is the 56-day[6] value-weighted pre-recommendation stock returns excess to the market (further discussed in the pages following), *Affiliated* is a dummy variable for analyst

[6] I use a 56-day period because it is an even eight weeks: it is close to two months, and maps recommendation dates to the same day of the week eight weeks prior. Using a period that is not integer divisible by seven would result in the beginning of this pre-recommendation returns period landing on a weekend for some observations, thereby restricting and perhaps biasing my sample.

affiliation, *MarketCap* is the stock's market capitalization in thousands on the day of recommendation, *Time* is the number of days elapsed between the IPO date and the recommendation date, and *IndustryDummies* is a vector of industry dummy variables. This is an adaptation of Michaely and Womack's 1999 methodology[7]. I include market capitalization[8] to control for the well-documented[9] relationship between stock and index returns, and market capitalization. Industry dummies are included to account for relative differences in industry returns to the market over time. This decreases the "noise" in the excess returns due to some industries performing better relative to the market than others.

The coefficient of interest in equation (1) is β_1. Finding this coefficient to be negative and significant would mean that I observe pre-recommendation returns to be significantly more negative — by β_1 percentage points — for affiliated analysts, than for unaffiliated analysts. On the other hand, finding β_1 to be statistically not different from zero would imply that pre-recommendation returns are not statistically different for affiliated and nonaffiliated analysts. Finally, a positive and significant β_1 would mean that pre-recommendation returns are in fact higher — again, by β_1 percentage points — for affiliated analysts, than for unaffiliated analysts.

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[7] See Michaely and Womack (1999) p. 670 for their specification of this model, which does not include industry dummies.

[8] I do not use the natural logarithm of market capitalization because — in my opinion — taking the log of this variable (in thousands) would compress the data. For example, a stock with a $20 million market capitalization would have a log(MarketCap) value of log(20,000) = 9.903; doubling the market cap to $40 million, however, results in a log(MarketCap) of log(40,000) = 10.597, a relatively small increase in relation to *doubling* the non-logged market capitalization. This intuition is confirmed, in my opinion, by the fact that log(MarketCap) does not enter significantly when I substitute it for MarketCap in each regression I estimate, whereas MarketCap is significant in almost every regression I run. Although taking the log of market capitalization would allow me to interpret the coefficient on this variable (β_3) as a semi-elasticity, I only use market capitalization as a control variable, and am therefore not interested in the already well-documented effects of market capitalization on returns.

[9] See Dimson and Marsh (1986), for example.

I will therefore test the following hypothesis:

H_0: $\beta_1 < 0$

H_1: $\beta_1 \geq 0$ (not H_0)

If I do not reject H_0, this would show that pre-recommendation returns are statistically lower for affiliated recommendations than for nonaffiliated recommendations. This would in turn be evidence for the conflict of interest relationship I have described. Conversely, rejecting H_0 would be evidence against brokerage analyst bias.

This regression seems quite unorthodox. It appears that I am using the dummy variable *Affiliated* to estimate the variable $Perf_{(pre)}$, while the former variable is observed *after* the latter! Michaely and Womack do not address this objection in their discussion. It seems that the direction of the causal relationship between *Affiliated* and $Perf_{(pre)}$ is put into question, whereas we do not expect endogeneity between $Perf_{(pre)}$ and any of the control variables. In spite of the seemingly ambiguous direction of causality between *Affiliated* and $Perf_{(pre)}$, I submit that this relationship can in theory only work in one direction, with pre-recommendation returns influencing analyst affiliation for a given observation. Affiliation cannot affect pre-recommendation returns because it is observed strictly after returns are observed. I conceive of this regression as a quantitative thought experiment: first, I observe an analyst's "buy" recommendation of a stock, but do not yet observe the stock's pre-recommendation returns. Given the control variables I have included and whether the analyst is affiliated with the stock's IPO underwriter, I try to predict the stock's returns excess to the market in the 56-day period leading up to the recommendation. If analyst affiliation helps in this estimation (if it is

statistically significant), then I can conclude that pre-recommendation excess returns may indeed influence affiliated analysts through the conflict of interest channels described earlier in this paper. Note also that the relationship estimated here is not how pre-recommendation returns influence recommendations, because I am holding recommendation constant by only considering "buy" and "strong buy" recommendations. Rather, β_1 describes how affiliated analyst "buy" recommendations differ from nonaffiliated "buy" recommendations in their response to pre-recommendation returns.

Value-Weighted Excess Returns

I calculate 56-day pre-recommendation excess returns for a recommendation made on date *b* on stock *i* according to the following equation:

$$Perf^{i}_{(pre),a,b} = \prod_{t-a}^{b}(1 + r_i) - \prod_{t-a}^{b}(1 + r_{S\&P}) \quad (2)$$

where Pi denotes the product of the sequence, r_i is the value-weighted return of the stock on day *t*, $r_{S\&P}$ is the value-weighted return on the S&P on day *t*, *b* is the recommendation date, and *a* is the date 56 days before *b*. $Perf_{(pre)}$ is therefore the compounded value-weighted 56-day excess return to stock *i* leading up to the date of recommendation. This metric is recalculated for each recommendation, as recommendations are made on different days. Value-weighted returns are used to avoid the bias associated with compounding equal-weighted returns over time, described in Canina et al.'s 1998 paper.

Post-Recommendation Performance

If I do find evidence for a relationship between analyst affiliation and more enthusiastic recommendations, this could still be for legitimate reasons. As previously mentioned, the reason could be that affiliated analysts simply have better information about companies taken public by their investment bank, and therefore can be more discerning in their "buy" recommendations. This would discredit an allegation of conflict of interest, favoring a hypothesis according to which affiliated analysts recommendations differ from nonaffiliated analyst recommendations simply due to better information.

This would imply, however, that these analysts, having legitimately better information, are also more accurate in their recommendations than nonaffiliated analysts. I therefore test the two-year post-recommendation returns of the same analyst "buy" recommendations considered in my first model. The equation I estimate is:

$$Perf_{(post)} = C + \beta_1 * Affiliated + \beta_2 * MarketCap + \beta_3 * Time + \beta_4 * IndustryDummies \quad (3)$$

where $Perf_{(post)}$ is the two-year compounded value-weighted excess returns to the stock in the question, calculated according to equation (2), but over the two years following the recommendation date. I think of $Perf_{(post)}$ as the returns to a two-year "buy-and-hold" strategy: if affiliated analysts have better information, then we expect a "buy-and-hold" strategy based on their recommendations to have statistically higher returns than the same strategy based on nonaffiliated recommendations.

As in my first model, the coefficient of interest here is β_1. If I find that β_1 greater than zero and significant, this would imply that affiliated analysts have better information than their nonaffiliated counterparts, and that their recommendations have higher returns than nonaffiliated recommendations. If I find β_1 to be statistically no different from zero, then I would consider affiliated and nonaffiliated recommendations to have the same two-year return, on average. If I find β_1 to be less than zero and significant, this would be evidence that affiliated recommendations have lower returns than nonaffiliated recommendations.

I therefore test the following hypothesis:

H_0: $\beta_1 > 0$

H_1: $\beta_1 \leq 0$ (not H_0)

If I do not reject H_0, this is evidence that affiliated analysts are more accurate in their recommendations, perhaps because of better information. If I reject H_0, this will mean that affiliated analysts do not in fact have better information than their nonaffiliated counterparts, and are not statistically better at making stock recommendations. Joining the inference made on the results of this model, with my conclusion on analyst conflict of interest from my first model, will help to pinpoint the source of recommendation differences between affiliated and nonaffiliated analysts.

III. Data

I consider IPO dates during 1997 for my 'pre-Sarbanes-Oxley' group, and in 2004 for my 'post-Sarbanes-Oxley' group. First, I use the Hoover's database to determine which companies went public in 1997 and 2004. Then, the analyst reports come from the Wharton Research Data Services, which packages analyst recommendations and sells subscription access to their publication. IPO underwriter information is obtained from the Security and Exchange Commission's EDGAR online database as well as from Thompson One Banker Online. Finally, stock and index return data are collected from the Center for Research in Securities Prices at the University of Chicago.

In normalizing the data, I convert analyst recommendations to a 1-5 scale (strong buy, buy, attractive, hold, sell). Another issue I encounter is that of multiple underwriters, as almost every major IPO represents a combination of investment banks. In making this distinction, it is important to understand that it is the lead underwriter who stands to collect the most fees from the IPO, and it is their analysts who will follow the newly minted stock on the exchanges. Ellis, Michaely, and O'Hara (1998) confirm this intuition in finding that it is the lead bank that is responsible for post-IPO price support and continuing coverage, not supporting banks. Therefore, I only consider an analyst to be 'affiliated' if they work for the lead underwriter. The only exception to this rule is if an analyst from the lead bank is *not* covering the IPO, but an analyst from another underwriting bank is; in this case, I consider that latter analyst to be indeed 'affiliated'. My 1997 data include 200 recommendations made by 79 brokerage houses on 70 different stocks. My 2004 data include 483 recommendations made by 111 brokerage houses on 76 different stocks. All recommendations come from the year following the stock's IPO date.

IV. Results

The tables on the following pages indicate no statistical analyst bias or informational gain in either 1997 or 2004. The variable *Affiliated* does not enter significantly in any of the four regressions that I estimate, and I therefore conclude that the these samples demonstrate neither the conflict of interest relationship I modeled in equation (1), nor the information bias I specified with equation (3). In the following tables, starred coefficients are significant at the 5% level, and t-statistics are reported in parentheses.

Table 1: Dependent Variable: Pre-Recommendation Returns

	1997	**2004**
C	-0.072	-0.008
	-1.264	-1.337
Affiliated	0.011	0.001
	0.579	0.126
MarketCap	0.00000135*	0.000000000554
	3.047	1.909
Time	-0.000269*	0.0000543*
	-2.814	2.033
Capital Goods	0.098	0.088
	1.444	1.200
Consumer Nondurables	0.072	0.069
	1.200	1.320
Consumer Durables	0.234*	0.304*
	2.933	3.607
Consumer Services	0.082	0.069
	1.478	1.179
Energy	0.083	0.109
	1.052	1.157
Finance	0.048	0.077
	0.810	1.053
Healthcare	0.123	0.148
	1.954	1.915
Technology	0.099	0.138
	1.800	1.890
Transportation	0.049	0.044
	1.080	1.296
Observations	200	483
R-Squared	0.130	0.153

It seems that the effect of affiliation is not significantly different from zero, and also that it has the "wrong" sign in both years: I initially hypothesized that affiliation would be negatively related to pre-recommendation returns in the presence of the conflict of interest relationship I have described. In this case, I would expect the coefficient on affiliation to be negative, but this is not the case in my regression. This "wrong" sign means it is not simply that the standard errors of *Affiliated* are too high to not reject H_0, but that the relationship estimated in this equation is the opposite of what I expected. This is further evidence for a lack of biased, price-supporting recommendations on the part of affiliated analysts.

Table 2: Dependent Variable: Post-Recommendation Returns

	1997	**2004**
C	-0.134	0.038*
	-1.795	2.065
Affiliated	0.014	-0.00029
	0.548	-0.236
MarketCap	0.00000166*	0.00000000003930
	2.867	0.797
Time	-0.0001280	-.0000444*
	-1.024	-9.723
Capital Goods	0.162	0.138
	1.816	1.125
Consumer Nondurables	0.088	0.082
	1.125	1.350
Consumer Durables	0.273*	0.327*
	2.610	3.340
Consumer Services	0.091	0.082
	1.261	0.996
Energy	0.109	0.147
	1.044	1.200
Finance	0.063	0.091
	0.809	1.093
Healthcare	0.193*	0.234*
	2.334	2.294
Technology	0.132	0.176*
	1.834	1.981
Observations	200	483
R-Squared	0.110	0.159

In these estimations of equation (3), I find results consistent with my estimations of equation (1). Analyst affiliation does not enter significantly, and it is so close to zero that its sign switches between 1997 and 2004. I expect the coefficient on affiliation to be positive in this model: affiliation and the additional information that comes with it would in theory increase the returns of affiliated recommendations. However, this is only true for my 1997 model, while the coefficient on affiliation is negative in 2004. Based on these results, I find that analyst affiliation has zero statistical effect on a recommendation's two-year excess returns. This implies that affiliated brokerage analysts' information is no better than that of their nonaffiliated counterparts.

V. Conclusion

Although I set out to compare changes in brokerage analyst bias from 1997 to 2004, the data show no significant evidence for such bias in either time period. Not only has there been no statistically significant change in the degree of this conflict of interest as I measure it, but according to these data, this brokerage analyst bias did not significantly affect affiliated analyst recommendations in the first place. In addition, these data show evidence that affiliated analyst recommendations are statistically no more accurate than nonaffiliated analyst recommendations, as measured by the returns to a long-term buy-and-hold trading strategy. This supports the hypothesis that affiliated analysts do not benefit from significantly more information about an IPO. It follows that — according to this analysis — we can treat affiliated and nonaffiliated brokerage analysts entirely equally.

There are two possible causes of this result. The most obvious is that there did not exist a conflict of interest relationship for affiliated brokerage analysts during the time periods I have studied. This is indeed the simplest explanation of these results.

The other is that my data or my model (or both) has caused me to make incorrect inference about this brokerage analyst bias. It is possible that 1997 and 2004 were outliers for analyst conflict of interest; perhaps this is why I do not observe significant evidence of conflict of interest or increased information in these data.

However, I am wary to write off my conclusion as the result of a test with low power or as the result of a poorly specified model. If my conclusions were incorrect due to a lack of test power (Type I error, rejecting H_0 when it is in fact true), then the coefficient on affiliation — however insignificant — would at least be of the expected sign. This is not the case in three of my four regressions, as I have previously discussed. Only in my estimation of 1997 post-recommendation returns is affiliation estimated to have the expected sign. This is strong evidence that my lack of significant results is not caused by a low-power test.

Although I cannot fully speak to the correctness of my model specification, I do know that Michaely and Womack estimate a similar regression using 1990 data taken from many of the same sources, and find a significant result demonstrating analyst conflict of interest. In fact, I believe I slightly improved on their model by introducing sector dummy variables in my estimation of pre-recommendation returns. This improvement can be seen in the fact that my model explains roughly three times as much variance in the data as Michaely and Womack's (my R^2 of 0.130 against theirs of 0.0466). Decreasing the "noise" in the data in this way would in theory allow me to better estimate the true relationship between analyst affiliation and pre-recommendation returns.

The comparison to Michaely and Womack, who find significant evidence of this conflict of interest relationship, gives me hope that my model is not misspecified, but rather that by 1997 (and through 2004), this type of brokerage analyst conflict of interest had all but disappeared in U.S. investment houses. If we believe Michaely and Womack's results to be correct for their 1990 data, it follows that my somewhat similar specification would correctly estimate the relationship (or lack thereof) for my 1997 and 2004 data.

At best, this paper shows strong evidence against the existence of significant affiliated analyst bias or information gain. At the least, this paper is still a step forward in modeling and understanding the opposing forces that govern brokerage analyst incentives in the complicated and sometimes ethically questionable world of finance.

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