Rewriting History [*] [†]

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Abstract

Comparing two snapshots of the entire I/B/E/S analyst stock recommendations database, taken in 2002 and 2004 but each covering the same time period 1993-2002, we identify nearly twenty thousand changes of an unusual nature: the selective removal of analyst names from historic recommendations ("anonymizations"). This practice turns out to be pervasive and non-random: Bolder recommendations are more likely to be anonymized, as are recommendations from more senior analysts, *Institutional Investor* "all-stars," and those who remain in the industry beyond 2002. Abnormal stock returns following subsequently anonymized buy recommendations are significantly lower (by up to 11.0% p.a.) than those following buy recommendations that remain untouched, suggesting that particularly embarrassing recommendations are most likely to be anonymized. Analysts whose track records appear brighter due to anonymizations experience more favorable career outcomes over the 2003-2005 period than their track records and abilities would otherwise warrant.

Key words: Security analysts; Stock recommendations; Global Settlement; Career concerns; Forensic finance.

JEL classification: G21, G24, G28, J24, J44.

We provide evidence that nearly twenty thousand records in I/B/E/S, a database of research analyst recommendations widely used by investment professionals and academics, were manipulated between September 2002 and May 2004, a period that not only coincided with close scrutiny of Wall Street research by regulators, Congress, and the courts, but also saw a substantial downsizing of research departments at most major brokerage houses in the U.S. The manipulation in question took the form of the selective, ex post removal of an analyst's name from some of her historical recommendations which had originally been available in I/B/E/S under her name. These ex post "anonymizations" do not appear to be random; instead, they are concentrated among the worst performing recommendations. Removal of an analyst's name obviously makes it hard for investors, academics, and the labor market to assess an individual analyst's track record, and our tests show that analysts associated with anonymizations experience more favorable career outcomes than their true track records and abilities would otherwise warrant.[1]

Our findings are primarily based on a comparison of two snapshots of the I/B/E/S database, one taken in September 2002, and another taken in May 2004, but both covering the *same* period of data, namely October 1993 to July 2002. Absent manipulation, differences between the two snapshots should reflect only corrections of true data errors. Manipulation will show up in selective *changes* in certain data items over time which cannot be traced to such corrections.

This research design yields a series of interesting and disturbing findings. Our first contribution is to demonstrate that anonymizations are pervasive, and appear to have been done selectively. As many as 19,892 of the 280,463 recommendations on the 2002 tape (7.1%) have had the analyst's

[1] Anonymizations may also affect investment decisions and compensation schemes, to the extent that investors and employers rely on analyst rankings and track records that are tainted by partially altered I/B/E/S data. A prominent example is StarMine, which uses I/B/E/S data to rank analysts based on their forecasts and stock picking skills. While aimed at institutions, StarMine rankings are also available to individual investors, e.g. through Yahoo! Finance. StarMine is also used by some brokerage firms "to assist in the tracking of analyst performance with an eye towards utilizing the results as a component of analyst compensation." (StarMine press release dated June 5, 2002)

name removed on the 2004 tape. Recommendations that were bolder (in the sense of diverging more from the prevailing consensus at the time) are more likely to be anonymized, as are recommendations from more visible analysts (industry veterans and those ranked "all-stars" by buy-side investors in recent *Institutional Investor* polls). Among the group of analysts whose track record has been partly anonymized, we find that abnormal returns following subsequently anonymized buy recommendations are significantly lower than are abnormal returns following buy recommendations *by the same analysts* that remained untouched. The magnitude of this effect is large, ranging from 3.3% to 4.3% annualized. The effect is even larger for recommendations made in the post-bubble (March 11, 2000-July 18, 2002) period (as high as 8.0% annualized), and for the sub-sample of bolder recommendations (as high as 5.4% and 11.0% annualized in the full-sample and post-bubble periods, respectively).

We explore a series of possible explanations for these findings. I/B/E/S itself is the most obvious culprit. Unfortunately, despite repeated attempts, we have to date been unable to obtain any kind of clarification from Thomson Financial, which owns I/B/E/S.[2] However, we view it as highly unlikely that the anonymizations simply reflect innocent data cleanups by I/B/E/S. When comparing the 2002 snapshot to various other sources of data on analyst recommendations, including the research reports on which I/B/E/S is based, we find almost no cases where the original 2002 tape incorrectly identified an analyst.[3]

We then turn to the brokerage firms and the analysts. None of the 89 brokerage firms that quit sell-side research prior 2002 is associated with anonymization, while more than 85% of firms that continue producing research are. What might a brokerage firm gain from anonymizing some

[2] We first approached I/B/E/S on March 27, 2006. As of July 17, 2006, I/B/E/S "are working […] to try to figure out what exactly occurred and whether these [names] were deliberately taken out by request."
[3] Of course, we cannot rule out that I/B/E/S mistakenly removed analyst names in a way that randomly produced the patterns we document, though our tests suggest the probability of this is vanishingly small.

recommendations while still allowing I/B/E/S to disclose the identity of the brokerage firm itself? Perhaps it hoped to boost the reputations of its analysts, and thereby indirectly its own prestige. But if that was the hope, it had an unintended consequence: Anonymizing analysts are more likely to leave their employers, often for a more prestigious house. Or perhaps banks sought to make the data less transparent in the face of securities lawsuits (many litigation consultancies use I/B/E/S data in support of litigation), except the sheer number of companies for which recommendations have been anonymized makes this unlikely: 44.5% of the 11,708 unique CUSIPs on the 2002 tape are associated with one or more anonymized recommendation, which surely exceeds the number of lawsuits in the last few years. Of course, an apparent lack of motive does not rule out the possibility that brokerage firms are behind the anonymizations.

The motive for analysts to anonymize is probably the easiest to see. If analysts are evaluated on past performance, and this performance information can be manipulated, anonymization is consistent with analysts' career incentives. Indeed, comparing further snapshots of the I/B/E/S database, we find that there were virtually no anonymizations before or after the September 2002 through May 2004 period that we focus on.[4] During that period, large-scale restructuring of equity research departments and mass redundancies on Wall Street may have created immense pressure on some analysts to take extraordinary measures to keep their jobs. Discussions with several analysts and competitors to I/B/E/S suggest that data revisions and corrections may involve only a phone call and thus require minimal compliance in some brokerage firms.[5] However, we have no way of knowing exactly how nearly 20,000 recommendations came to be anonymized; no academic test

[4] We have four I/B/E/S recommendation tapes, dated January 2002, September 2002, May 2004, and February 2006. Between January and September 2002, there were nine anonymizations, i.e. one per month on average. Between September 2002 and May 2004, the sample period which we focus on, there were 995 anonymizations per month on average. Between May 2004 and February 2006, there were 591 anonymizations, or 28.1 per month on average.

[5] See the Appendix for further details on how data vendors collect and revise analyst data. We also asked a small number of analysts why some of their records were anonymized, and each of them claimed to "have no idea."

will be able to shed light on the mechanism, though interestingly we find that anonymized recommendations are virtually absent among analysts who had quit the industry at some point before 2002. This suggests that a necessary condition for anonymization is that an analyst has continued access to in-house databases or data vendors.

Whether or not individual analysts or their employers are the culprits, an undeniable consequence of the changes we document is that the track records of the analysts concerned now look better than they should. Our second contribution is to show that these analysts apparently benefited from anonymizations, at least in the sense of experiencing more favorable career outcomes than their track records and abilities would otherwise warrant. Following the career path modeling in Hong, Kubik, and Solomon (2000) closely, we find that analysts associated with anonymizations experience a nearly 70% increase in the likelihood of subsequently moving from a low-status to a high-status brokerage firm. This effect is much larger than any other in our career outcome models. For instance, even an analyst who was able to improve her forecast accuracy by five standard deviations relative to her sector peers still could not improve her job prospects by as much as an anonymizing analyst.

The idea that labor markets assess an agent's ability by observing her past performance is a common feature of many models of career concerns (see, for example, Holmström (1982), Ottaviani and Sorensen (2004, 2005)). In this setting, an agent's reputation is the outcome of a dynamic process whereby the market continually updates its assessment of ability by observing an agent's behavior over time. Agents have an incentive to maintain their reputation because wages later in their career are reputation-dependent (Fama (1980)). More recent, multi-agent theories of career concerns (e.g., Scharfstein and Stein (1990), Trueman (1994), Zwiebel (1995), and Prendergast and Stole (1990)) argue that agents may alter their behavior to influence the way the labor market

assesses their ability; for example, career concerns may at times induce herding behavior.[6] Our results suggest another possibility: Instead of changing behavior, agents may be able to maintain their reputations by manipulating ex post the *data* that markets use to learn about their ability.

Our findings also suggest an additional layer to the well-documented agency problem in the equity research industry. While analysts have incentives to maintain their reputations, recent evidence suggests that analysts may also respond to institutional incentives such as pressure from investment banking departments (see Lin and McNichols (1998), Michaely and Womack (1999), Hong and Kubik (2003)), a desire to maintain ties to senior management at the companies they cover (Lim (2001)), or a desire to generate commissions (Jackson (2005)). These alleged conflicts of interest have led to several new policy measures enacted by the SEC, NASD, and NYSE encouraging analysts and their banks to transparently report information on the success of past recommendations.[7] The joint press release of the SEC, NASD, NYSE, state prosecutors and other regulators outlining the Global Research Analyst Settlement, dated April 28, 2003, states that "to enable investors to evaluate and compare the performance of analysts, research analysts' historical ratings will be disclosed. Each firm will make its analysts' historical ratings and price target forecasts publicly available," typically by disclosing a stock-specific history at the end of a research report.[8] While this information may be of some use to investors wishing to understand the ratings history of a stock they are considering investing in, it is of more limited use in evaluating an analyst's track record across all the securities she has covered. In practice, most institutional investors and buy-side clients (as well as prospective employers) obtain historical data not from individual reports but from large-scale data repositories such as I/B/E/S or First Call. Therefore, the practical efficacy of the new transparency requirement relies on datasets such as I/B/E/S

[6] See Banerjee (1992), Bikhchandani, Hirshleifer, and Welch (1992), and Welch (1992) for other theories of herding.
[7] See Boni and Womack (2002) for a discussion of these new regulations.
[8] See http://www.sec.gov/news/press/2003-54.htm.

maintaining accurate and unbiased historical records of past analyst performance. Our findings suggest they do not.[9]

The paper is organized as follows. Section I reviews related literature. Section II describes the data we use. Sections III and IV present our results, and Section V concludes.

I. Related Literature

Since analysts plausibly have the most to gain from having their names removed from embarrassing recommendations, we first discuss related work from the labor economics literature. Fama (1980) argues that while the labor market uses past performance to assess an agent's ability, the market's assessment is also likely to include "extraneous noise which has little to do with his talents and efforts." However, the dynamic wage revision process imposed by the labor market generally eventually leads to ex post settling up. The power of this disciplining mechanism within any given labor market is of course an empirical question. In the case of sell-side research, analysts have an incentive to produce unbiased forecasts and recommendations for investors only if they are compensated for such behavior. Due to a lack of data on direct compensation, however, the literature generally tests this idea by linking analyst behavior to measures of implicit incentives or career concerns. Stickel (1992) finds that highly rated "all-star" analysts (who are typically better compensated than other analysts) make more accurate earnings forecast than other analysts, suggesting that accuracy is rewarded. Poorly performing analysts, on the other hand, generate less brokerage commission and are ultimately associated with higher job turnover (Mikhail, Walther, and Willis (1999)).

An important strand of the labor literature, however, suggests that analysts' career concerns and

[9] It is also interesting that the new regulatory measures under the Global Settlement do not require earnings forecast transparency, and we find almost no ex post anonymizations on the I/B/E/S earnings tape. This is in line with prior evidence that analysts' earnings forecasts are more easily verifiable than investment recommendations, implying a greater cost for analysts to manipulate their forecasts relative to their recommendations. See Ljungqvist et al. (2006).

the conflicts of interest inherent in equity research create an agency problem, potentially at the expense of investors who trust analyst research to be unbiased. Hong, Kubik, and Solomon (2000) find that younger analysts deviate less from the consensus than their older counterparts, consistent with the predictions of reputation-based herding models.[10] Hong and Kubik (2003) report that controlling for accuracy, analysts who are optimistic relative to the consensus are more likely to experience favorable job separations. They also find that analysts are judged less on accuracy than optimism when it comes to stocks underwritten by their employers, supporting allegations that analysts suffer from a conflict of interest when covering stocks affiliated with their brokerage firms.[11] We add to this literature on agency problems in the analyst industry by exploring a potentially new form of strategic behavior.

More generally, we contribute to the labor literature by examining an important channel through which agents may add noise to the labor market's assessment of their ability. This is similar in spirit to Jacob and Levitt's (2003) investigation of cheating by teachers in the Chicago public school system, to Oyer's (1998) study of how salesmen manipulate prices and the timing of sales to hit performance benchmarks tied to the fiscal year, and to Cooper, Gulen, and Rau's (2005) finding that mutual funds often change their names to take advantage of "hot" investment styles.[12]

Our paper is also relevant to the debate about the impact of the Global Research Analyst Settlement. On December 20, 2002, New York attorney general Eliot Spitzer, the SEC, NASD, North American Securities Administrators Association, NYSE, and state regulators announced the terms of an agreement, the so called "Global Settlement," concluding a joint investigation into the "undue influence of investment banking interests on securities research at brokerage firms." The

[10] Chevalier and Ellison (1999) and Lamont (2002) find similar results for mutual fund managers and macroeconomic forecasters, respectively. See Holmström (1999) and Scharfstein and Stein (1990) for related work on career concerns.
[11] Lin and McNichols (1998) and Michaely and Womack (1999) also report evidence in support of this view.
[12] See also Christie and Schultz's (1994) study of the absence of odd-eights quotes on NASDAQ and the possibility of implicit collusion by dealers.

settlement was finalized during 2003 and eventually involved 12 brokerage firms: Bear Stearns, CS First Boston, Deutsche Bank, Goldman Sachs, JP Morgan Chase, Lehman Brothers, Merrill Lynch, Morgan Stanley, Piper Jaffray, Salomon Smith Barney, Thomas Weisel, and UBS Warburg. As part of the settlement, the 12 brokerage firms agreed to pay $1.4 billion in "penalties, restitution, and monies to be used for investor education," to insulate their research analysts from investment banking pressure, and to disclose their analysts' historical ratings and price target forecasts (to enable investors to evaluate and compare analysts' past performance).

Boni and Womack (2002) argue that these provisions speak to many of the concerns voiced by the majority of buy-side professionals they survey, but that they will be difficult to credibly enforce in practice. Kadan et al. (2005) present evidence consistent with the hypothesis that these new provisions have curbed conflicts of interest in the industry. Jackson (2005) argues, however, that plans to split brokerage firms may be unsuccessful in eliminating analysts' optimism bias, since investment banking incentives are likely to be replaced by trade generation incentives (which ultimately may have a similarly distorting effect on research). Jackson analyzes the tradeoff between an analyst telling the truth to build her reputation versus misleading investors via optimistic forecasts to generate short-term increases in trade commissions. He argues that policies that make an analyst's reputation more transparent and increase the implicit penalty for opportunistic behavior (such as the new regulatory provisions discussed above) may be an effective tool for ameliorating analysts' incentives to issue optimistic forecasts. However, this argument relies on the assumption that the level of transparency in the market cannot be manipulated, a possibility we consider in this paper.[13]

Finally, our paper contributes to a small but growing literature detailing problems in the I/B/E/S

[13] Nor are analysts the only ones who may be able to "rewrite history." Michael Crowley notes that the wording of Senator Orrin Hatch's contribution to the June 2006 debate on a Constitutional ban on gay marriage was subsequently cleaned up in the official Congressional Record. See http://www.tnr.com/blog/theplank?pid=19614.

database. Payne and Thomas (2003) and Diether, Malloy, and Scherbina (2002) document a problematic rounding procedure on the part of I/B/E/S in the split-adjusted earnings-per-share data which induces a bias in commonly computed measures of analyst dispersion. While our key contribution is to document the pervasive and selective nature of analyst anonymizations in the data, we also document significant gaps between the First Call and the I/B/E/S databases. These are the two most commonly used databases for analyst research and are often viewed by researchers as substitutes, and yet we find surprisingly little overlap between the two sources. These findings have potentially important ramifications for existing and future empirical studies of equity analysts.

II. Data

II.A Sample Construction

The I/B/E/S recommendations database contains investment ratings issued since October 29, 1993 by most of the brokerage firms active in the U.S. To examine the *decision* to anonymize, our analysis compares two snapshots of the *same* set of recommendations for U.S. companies issued over the period October 1993 to July 18, 2002. The snapshots are taken at two different points in time: September 2002 and May 2004. We will refer to these snapshots as the 2002 and 2004 tapes, respectively. It is important to note that both tapes cover the same set of companies and the same time period, October 29, 1993 through July 18, 2002 (i.e., we ignore recommendations on the 2004 tape that were issued after July 18, 2002). Except for data cleanups, one would expect these two tapes to be identical. However, as we noted in the introduction, they are not: Nearly twenty thousand records were masked at some point between September 2002 and May 2004.

The 2002 tape contains 280,463 investment recommendations by 7,886 analysts working for

385 brokerage firms.[14] A typical record includes the analyst's name and her six-digit *amaskcd* identifier assigned by I/B/E/S; the name of the brokerage firm for which the analyst worked when issuing the recommendation; the ticker and historical CUSIP of the company concerned; the date the recommendation was issued; and the recommendation itself. Different brokerage firms use different wordings for their recommendations. I/B/E/S translates each recommendation into a numerical score on the following scale: Strong buy=1, buy=2, hold=3, sell=4, strong sell=5.

When analysts submit recommendations anonymously, I/B/E/S codes *amaskcd* to be zero, though all other information remains available, including the name of the brokerage firm employing the anonymous analyst. On the 2002 tape, there were 7,735 anonymous recommendations (2.8%). These are *not* the focus of our study. Instead, we focus on *newly anonymized* recommendations, i.e., a record on the 2004 tape that matches a record on the 2002 tape (same ticker, brokerage firm, and recommendation date) except that the *amaskcd* of the analyst that was named on the 2002 tape is now zero. We identify 19,892 such anonymized records (7.1% of the total).

Table I presents a breakdown of sample recommendations by I/B/E/S rating level. Of the 19,892 anonymized recommendations, 29.5% are strong buys, 36.3% are buys, 30.7% are holds, 1.9% are sells, and 1.7% are strong sells. Compared to non-anonymized recommendations, there are proportionately more strong buys and sells and relatively fewer holds among the anonymized recommendations. They thus tend to be bolder.

II.B Data Integrity

It is possible that records were anonymized simply because I/B/E/S discovered that analyst

[14] There are in fact 546 separate broker codes, but this overestimates the number of distinct brokerage firms since I/B/E/S sometimes uses multiple codes to identify the same brokerage firm (e.g., NOMURA and NOMURAUS both denote Nomura Securities). It also ignores the fact that many brokerage firms have merged over the sample period (e.g., the recommendations of SALOMON and SMITH are all associated with Citigroup's Salomon Smith Barney unit, as of June 2002). After allocating the historic recommendations of the predecessors to the surviving brokerage firm as of June 30, 2002 using the bank merger data described in Asker and Ljungqvist (2005), and consolidating multiple broker codes, the 2002 tape contains 385 distinct brokerage firms.

names had been assigned incorrectly on the 2002 tape.[15] To rule out this possibility, we investigate the data integrity of the 2002 tape against a variety of sources: The I/B/E/S earnings forecast tape; the First Call recommendations database; the analyst report collection available through Thomson's Investext service; and news sources accessed through Factiva. Collectively, these indicate that the 2002 tape was largely accurate. We discuss each in turn.

We first compare the 2002 and 2004 I/B/E/S *earnings forecast* tapes. Interestingly, we find virtually no cases where analyst names have been removed. Ex post anonymizations are hence a feature of the recommendations tape only. Moreover, we find a named earnings forecast by the same analyst for the same company within six months either side of the recommendation date in 17,882 (89.9%) of the 19,892 anonymized cases. Figure 1 shows a kernel density estimate of the (absolute) time between each anonymized recommendation and the nearest earnings forecast by the same analyst for the same stock, using an Epanechnikov kernel. Clearly, the distribution is very tight, peaking at zero: In 9,187 of the 19,892 anonymized cases (46.2%), the analyst named on the 2002 tape and whose name was removed from the recommendation on the 2004 tape made a *non-anonymous* earnings forecast on the exact same day of the recommendation, according to the 2004 earnings forecast tape. If the anonymizations reflected cleanups of data errors, it is unclear why I/B/E/S would have cleaned up only the recommendations database and not also the corresponding entries in the earnings forecast database.

First Call was developed by Thomson Financial as a direct competitor to I/B/E/S, which Thomson later acquired. First Call provides an alternative source of recommendations, with the

[15] We can rule out two possible scenarios. 1) I/B/E/S may have discovered an incorrect name on a rating, removed the name from the record, and then added a new record on the 2004 tape duplicating the original entry with the correct name. However, we find only three cases among the 19,892 anonymized records where I/B/E/S has added a new record for the same ticker, brokerage firm, and date but under a new name. 2) I/B/E/S may have discovered that the wrong brokerage firm had been credited with a recommendation, removed the analyst's name from the recommendation, and then added a new record on the 2004 tape duplicating the entry with the correct brokerage firm. However, we find only 91 cases in which I/B/E/S added a new record for the same ticker and date but under a different brokerage firm.

important difference that analyst names are never disclosed. Thus, a comparison of the two databases can only shed light on whether a given brokerage firm was associated with a certain recommendation on a given day. Of the 19,892 anonymized recommendations, 4,707 (23.7%) match a First Call recommendation for the same stock by the same brokerage firm on the same day. Further investigation using a randomized sample of analyst reports accessed through Thomson's Investext service reveals that I/B/E/S recommendation dates are not always accurate. First Call flags whether a recommendation was recorded in real time or in a weekly batch, and real-time records line up perfectly with our sample of Investext reports. If we widen the window for the First Call-I/B/E/S match, we find that 8,290 (41.7%) of the 19,892 anonymized I/B/E/S recommendations have a corresponding First Call entry within 14 calendar days either side of the I/B/E/S recommendation date, with most of the additional First Call matches preceding the I/B/E/S recommendations by a few days; see Table II.

A 41.7% match rate sounds low. For comparison, we also merge the 260,571 *non*-anonymized recommendations from the 2002 I/B/E/S tape with the First Call database. As Table II shows, 122,243 (46.9%) of these match up, which is significantly better statistically than the 41.7% rate among anonymized recommendations, but is still astonishingly low economically.[16,17]

The First Call comparison is inconclusive, not only because of the low degree of overlap, but also because even a positive match does not confirm that it was the analyst named on the 2002 tape who actually made the recommendation – it could have been someone else at the same brokerage

[16] Another data source that is sometimes used in academic research is briefing.com. This website requires tickers to be entered one by one, so it is not feasible to extract data for a large sample. We have, however, investigated the 562 anonymized recommendations by analysts at a well-known, randomly selected bulge-bracket firm. Of these, 140 can be found in briefing.com within a two-week window of the I/B/E/S recommendation date (88.6% are on the same day). Briefing.com has 49 recommendations that are not in First Call, while First Call has 97 recommendations that are not in briefing.com. None of these sources appears to be fully comprehensive.

[17] Note that whereas I/B/E/S uses historic CUSIPs, First Call updates the CUSIP field following name changes, reorganizations, and mergers. We undo these changes using information from CRSP and Factiva to maximize the hit rate. Note also that First Call recently "recycled" the codes of brokerage firms that had disappeared due to mergers in such way that the code translation file available from WRDS is highly misleading. We undo these changes as well.

firm. Ruling this out would require identifying the author of every anonymized I/B/E/S recommendation. Because the large number of cases makes this impracticable, we focus on six randomly selected brokerage firms, namely three bulge-bracket investment banks and three medium-sized brokerage firms. (The terms governing our use of these data prevent us from naming the firms we selected.) Using Investext and news wires accessed through Factiva, we are able to verify the analyst's identity in 1,144 cases. In only 43 of these 1,144 cases (3.8%) does the name of the analyst writing the report *not* match the name on the 2002 I/B/E/S tape.

We conclude that it appears extremely unlikely that the anonymizations on the 2004 I/B/E/S tape are the result of innocuous data cleanups. Other than data cleanups, we can think of no reason why I/B/E/S would have removed names of its own accord, which leads us to believe that anonymization requests likely came either from brokerage houses or the analysts concerned.

III. Empirical Analysis of Anonymized Recommendations

In this section we first describe what anonymized recommendations have in common and how anonymizers differ from non-anonymizers, and then examine the stock return performance of anonymized and non-anonymized recommendations.

III.A Brokerage Firm Characteristics

Which brokerage firms are associated with anonymized recommendations? Among the 385 brokerage firms, 16 reported exclusively anonymous recommendations on the 2002 tape, which leaves 369 brokerage firms that could potentially experience changes to their unmasked data between 2002 and 2004. Of these 369 firms, a remarkable 239 (64.8%) see one or more of their analysts' recommendations anonymized on the 2004 tape; Table III reports their salient characteristics.[18]

[18] We obtain qualitatively similar results if we require a substantial fraction of a brokerage firm's recommendations (such as 5%) to have been anonymized, rather than "at least one."

Brokerage firms with anonymized records are substantially larger than those without, both in terms of the size of their investment banking operations (proxied by their 2002 share of the equity underwriting market) and the size of their research departments (proxied by the number of analysts employed in 2002). Specifically, they have a 0.4% versus 0% equity underwriting market share and employ 18.4 versus 0.8 analysts on average. The curiously low average headcount among the 130 non-anonymizing brokerage firms reflects the fact that 89 of them have ceased publishing sell-side research by 2002, according to I/B/E/S.[19] Furthermore, continuing to publish research appears to be a pre-condition for a brokerage firm's recommendations to have been anonymized: Not a single recommendation associated with one of the 89 brokerage firms that have ceased publishing investment research by 2002 has been altered. By contrast, an astonishing 85.4% of the 280 brokerage firms that continue publishing research had some recommendations anonymized.

Beyond size and continued production of sell-side research, brokerage firms with anonymized records have two further characteristics in common: They include all twelve firms sanctioned in the Global Settlement and they tend to be integrated securities firms, offering both investment banking and research services. Of the 65 brokerage firms that lead-managed at least one underwritten equity transaction in 2002 (our proxy for the presence of an investment banking operation), 61 (93.8%) saw some anonymizations, whereas of the 304 brokerage firms without investment banking operations in 2002, only 178 (58.6%) did.

Finally, Table III shows that the average (median) brokerage firm saw 38.4% (27.6%) of its analysts and 16.4% (8.4%) of its outstanding recommendations go anonymous. These averages are lower among the (generally larger) sanctioned brokerage firms than among non-sanctioned ones. Though not shown, we also find that it is relatively rare for a bank's recommendations to be

[19] Recall that we consolidate brokerage firms' recommendation histories to reflect mergers through July 2002. Thus, the group of brokerage firms that have ceased publishing research does not consist of takeover targets.

anonymized *after* the analyst has left for another bank.

III.B Analyst Characteristics

Table IV shows that of the 7,886 analysts on the 2002 tape, 2,138 or 27.1% go anonymous on at least one of their recommendations. The average (median) analyst anonymizes 9.3 (2) recommendations, representing 29.7% (13.2%) of her recommendations. As the kernel density estimate of the number of anonymized recommendations per analyst in Figure 2 shows, most analysts anonymize only a handful of recommendations. At the other end of the distribution, a small handful of analysts are associated with a substantial number of anonymized recommendations, with one analyst's name having been removed from 308 observations.

How do anonymizers differ from non-anonymizers? According to Panel B of Table IV, both groups work for similar types of employers in terms of analyst headcount in 2002, investment banking market share, and sanctioning under the Global Settlement. But as Panel C shows, they differ considerably in their individual characteristics and track records. In fact, anonymizers appear to have the very best job prospects (Hong and Kubik (2003)) and hence perhaps the most to lose by disclosing poor performance. Relative to non-anonymizers, these analysts are not only more likely to be *Institutional Investor* all-stars, they are also associated with more accurate earnings forecasts compared to their sector peers, as defined in Hong, Kubik, and Solomon (2000).[20,21] Anonymizers are also more senior as judged by the average time they have been in the profession and thus, not surprisingly, issue significantly more recommendations than non-anonymizers. All of these characteristics point to anonymizers being analysts of relatively high visibility who may be more

[20] We measure analyst characteristics either as of 2002 (the putative date at which an analyst may have contemplated anonymizing some of her recommendations) or as of the time of the analyst's retirement, if this was earlier.

[21] We compute the absolute forecast error of each analyst *i* covering company *k* in year *t* as the difference between the analyst's most recent forecast of year-end earnings per share (issued between January 1 and July 1 of that year) and subsequent realized earnings. Absolute forecast errors are then scaled so that the most accurate analyst scores one and the least accurate zero. The analyst's relative forecast accuracy in year *t* then is her average score across the *k* stocks she covers over years *t*-2 to *t*.

likely to be scrutinized and hence are more affected by the increased transparency requirements of the new NASD regulations.[22]

Arguably the most remarkable result of Table IV is shown in Panel D. Earlier, we found no anonymized recommendations at brokerage firms that had ceased publishing research by 2002. We find a parallel at the analyst level. Specifically, 1,505 of the 2,138 anonymizers (70.4%) continue contributing recommendations and earnings forecasts to I/B/E/S after 2002 while only 46.4% of the 5,748 non-anonymizers stay in the industry. Arguably, continued employment in sell-side research is a precondition for an analyst to be able to clean-up her track record, and so the remarkable difference in post-2002 employment patterns is consistent with possibly strategic behavior.

However, what about the 29.6% of anonymizers who had apparently retired by 2002? Because we define continued employment in the industry based on an analyst contributing data to I/B/E/S, the 29.6% "retirees" figure is strictly an upper bound. Analysts moving to brokerage firms (such as boutiques) that do not contribute data to I/B/E/S will be classified as retirees, as will analysts who continue to work in research but no longer have their name listed first on research reports (perhaps because a more senior analyst has been recruited to the team) or analysts who have been promoted to the position of research director (and so stop publishing). At any rate, the 29.6% apparent retirees account for only 15.5% of the anonymized recommendations, and as we will show, our results are robust to excluding these observations.

III.C Which Recommendations are Anonymized?

In Table V, we ask whether conditional on the types of brokerage firm and analyst characteristics investigated above, recommendations are anonymized randomly or selectively. Unlike in Tables III and IV, the unit of observation in Table V is thus a recommendation. We use

[22] Analysts with these characteristics tend to publish more, and Table IV shows that more prolific analysts are more likely to be associated with anonymized records. However, this correlation does not drive the summary statistics. For example, we find the same patterns if we focus on analysts who make an above-median number of recommendations.

multivariate probit models to estimate the determinants of the probability that a given recommendation from among an analyst's set of recommendations is anonymized. We relate the probability that an analyst anonymizes one or more of her recommendations to her own characteristics as well as those of her brokerage firm,[23] and include random brokerage effects to control for otherwise omitted heterogeneity arising from differences across brokerage firms.

We also control for certain recommendation-level attributes. Specifically, we ask whether recommendations for investment banking clients (companies for which the analyst's current employer lead-managed one or more securities issues in the five years to July 2002) or frequent issuers of securities (by aggregate dollars raised in the same five years) are treated differently; and whether particularly bold recommendations (those that diverge more in absolute terms from the consensus recommendation for the stock that year) are more likely to be anonymized.

Table V reports the results of five different probit models. Each has good explanatory power, in view of the pseudo-R^2 of between 31.4% and 47.5%. The first two models use the entire sample of recommendations and differ only in how they treat an analyst's relative forecast accuracy: The first controls for the *level* of relative forecast accuracy as of 2002 (or the year the analyst last appeared on the I/B/E/S tape), while the second instead includes the *change* in the analyst's relative forecast accuracy between 1997 and 2002. The coefficient estimates indicate that recommendations by analysts with a history of relatively low forecast accuracy, and those whose accuracy has declined the most since 1997, are significantly more likely to be anonymized. Economically, these effects are relatively modest, however: One standard deviation decreases in the level or change of forecast accuracy are associated with 0.2 and 0.3 percentage point increases in the probability of anonymization from the mean of 7.1%, holding all other covariates at their sample means.

[23] Whether the brokerage firm has ceased publishing research predicts anonymization perfectly and so cannot be controlled for in these models.

The conditional likelihood of recommendations being anonymized increases by 44.5% (i.e., 3.2 percentage points) in column (2) if they were made by all-star rather than unrated analysts. More senior analysts and those having issued fewer recommendations by 2002 are also significantly more likely to end up anonymizing records, but these two effects are economically more modest. All else equal, the conditional likelihood of anonymization is 7.5 percentage points greater – i.e., more than twice as large – if the analyst remains in the industry as of 2002. By implication, the conditional likelihood of a recommendation made by an analyst who has retired by 2002 being anonymized is zero. The *t*-statistic for this variable is 32.8, making it easily the most significant determinant of anonymization.

While the size of the brokerage firm's investment banking operation has no consistent effect in our models, the size of the research department does: Recommendations are 34.5% (i.e., 2.4 percentage points) more likely to be anonymized for a one standard deviation increase in the log number of analysts employed in 2002. Interestingly, recommendations made by analysts who in 2002 worked at non-sanctioned brokerage firms are twice as likely to be anonymized ($p<0.001$).

Turning to the recommendation-level characteristics, bolder recommendations are more likely to be anonymized, even controlling for analyst and broker characteristics. As we will see in the next section, bold recommendations tend to poor performers. Whether the company is an investment banking clients or a large issuer of securities has no effect on the anonymization likelihood.

In sum, anonymized recommendations predominantly come from analysts at non-sanctioned brokerage firms with large research departments who remain in the industry as of 2002, rank as all-stars, are industry veterans, make relatively few recommendations, and whose forecast accuracy in 2002 is relatively low having deteriorated since 1997. The specifications shown in columns (3) through (5) show that these findings are robust to excluding from the sample analysts anonymizing fewer than 5% (column (3)) or more than 50 of their recommendations (column (4)) and those who

by 2002 appear to have left the industry (column (5)).

III.D Portfolio Returns to Anonymized Recommendations

In this section we examine the stock return performance of anonymized and non-anonymized recommendations, for the sample of analysts for whom some but not all of their past recommendations are anonymized. If anonymization of past recommendations is selective, it is plausible to expect that the most "embarrassing" recommendations, namely the worst performers, will be anonymized. We formally test the hypothesis that anonymized recommendations are likely to be the worst performers by computing calendar-time buy-and-hold portfolio returns as in Barber, Lehavy, and Trueman (2005) and Barber, Lehavy, McNichols, and Trueman (2005). For each type of recommendation (anonymized or non-anonymized) we form two portfolios: (1) A buy portfolio, consisting of all stocks that at least one analyst in that category upgraded to buy, or strong buy, or initiated coverage on with a buy or strong buy rating; and (2) a hold/sell portfolio, comprised of all stocks that at least one analyst in that category either downgraded to hold, sell, or strong sell, or initiated/resumed/reiterated coverage on with a hold, sell, or strong sell rating.[24] We include holds in the sell portfolio since the distribution of recommendations suggests that many analysts effectively rated stocks on a three-point scale (hold/buy/strong buy).

The construction of these portfolios closely follows the methodology in Barber, Lehavy, and Trueman (2005) and Barber et al. (2005). For each recommendation that is eligible for the buy portfolio, for example, the recommended stock enters the portfolio at the close of trading on the day the recommendation is announced. This approach explicitly excludes the announcement day return, on the assumption that many investors likely become aware of recommendation changes only with a delay. Each recommended stock remains in the portfolio until either the stock is downgraded or

[24] Restricting the buy (hold/sell) portfolios to only those stocks that were recently upgraded to buy or strong buy (downgraded to hold, sell, or strong sell) does not affect our conclusions, nor does altering the composition of the portfolios in minor ways (e.g., dropping resumptions, reiterations, etc.).

dropped from coverage by the analyst, for up to one calendar year (after which time the stock is automatically removed from the portfolio). If more than one analyst recommends a particular stock on a given date, the stock will appear multiple times in the portfolio on that date (once for each buy or strong buy recommendation).

Assuming an equal dollar investment in each recommendation, the portfolio return on date *t* is given by $\sum_{i=1}^{n_t} R_{it} x_{it} / \sum_{i=1}^{n_t} x_{it}$, where R_{it} is the date *t* return on recommendation *i*, n_t is the number of recommendations in the portfolio, and x_{it} is the compounded daily return of recommended stock *i* from the close of trading on the day of the recommendation through day *t*-1. (The variable x_{it} equals one for a stock recommended on day *t*-1.) The portfolio is updated daily, so that stocks that are downgraded or dropped from coverage are taken out of the portfolio at the close of trading on the day of the downgrade/drop. Calendar-time daily returns for the hold/sell portfolio are computed in the same manner.

We calculate abnormal portfolio returns in two ways: 1) By estimating a "four-factor" alpha (as in Carhart (1997)), and 2) by estimating a "four-factor plus industry" alpha. Four-factor alpha returns (α_j) are computed from the following daily time-series regression for each portfolio *j*:

$$R_t^j - R_{ft} = \alpha_j + \beta_j (R_{mt} - R_{ft}) + s_j SMB_t + h_j HML_t + u_j UMD_t + \varepsilon_{jt}, \qquad (1)$$

where R_t^j is the date *t* return on portfolio *j*, R_{ft} is the date *t* risk-free rate, R_{mt} is the date *t* return on the CRSP value-weighted index, SMB_t is the date *t* return on a value-weighted portfolio of small stocks minus the date *t* return on a value-weighted portfolio of big stocks, HML_t is the date *t* return on a value-weighted portfolio of high book-to-market stocks minus the date *t* return on a value-weighted portfolio of stocks with low book-to-market, and UMD_t is the date *t* return on a value-weighted portfolio of stocks with high returns from month *t*-12 to month *t*-2 minus the date *t* return on a value-weighted portfolio of stocks with low returns from month *t*-12 to month *t*-2. We compute

the "four-factor plus industry" alpha by adding ten additional value-weighted industry excess returns to the original four factors included in the time-series regression in equation (1).[25]

Panel A of Table VI presents the average daily abnormal returns to the buy portfolios of anonymized and non-anonymized recommendations. Over the entire sample period October 29, 1993 to July 18, 2002, anonymized recommendations significantly underperform both a four-factor and a four-factor plus industry benchmark. The magnitude of the underperformance is 1.1 basis points a day (2.9% annualized) after controlling for the typical four factors, and 1.3 basis points a day (3.4% annualized) after also controlling for industry composition. By contrast, non-anonymized recommendations slightly outperform these benchmarks.[26] The abnormal return on a long-short portfolio that goes long non-anonymized recommendations and short anonymized recommendations is statistically significant and economically important, earning alphas of between 3.3% and 3.6% annualized. This evidence is consistent with the hypothesis that analysts selectively anonymize their worst performing buy recommendations.

We next divide the sample into two sub-periods: (1) The period of the late 1990s bull market (i.e., the period ending March 10, 2000, the date of the NASDAQ market peak), and (2) the period of the bear market (i.e., the period beginning on March 10, 2000 and extending to the end of our sample). This division reveals an interesting picture, and one that complements recent evidence that analysts were issuing overly optimistic recommendations and forecasts with little regard to fundamentals at the height of the dot-com frenzy (see Hong and Kubik (2003) and Barber, Lehavy,

[25] All daily factors, including the ten industry factors, are obtained from Kenneth French's website. In addition to the factors listed in equation (1), we also include one lag of each of the independent variables in all regressions to address the possibility that nonsynchronous trading affects our results (see Scholes and Williams (1977)).

[26] This finding is *not* inconsistent with prior evidence that analyst buy recommendations add value, since we are restricting the sample here to only those analysts who anonymize at least one recommendation. When we run tests using the entire sample of analysts, we find modest evidence that buy recommendations add value, consistent with the evidence in Barber, Lehavy, McNichols, and Trueman (2003) that including the years 2000 and 2001 dampens the prior evidence that analysts' buy recommendations outperform.

and Trueman (2005)). As columns (5) and (6) of Panel A show, the poor performance of anonymized buy recommendations is particularly pronounced in the post-bubble period, with daily risk-adjusted underperformance ranging from an economically large 3.1 to 3.2 basis points (7.9% to 8.0% annualized). Meanwhile, underperformance by non-anonymized buy recommendations in this period is minimal, and statistically insignificant. Abnormal returns on long-short portfolios designed to capture this differential performance between anonymized and non-anonymized buy recommendations are again statistically significant and economically large: up to 7.7% annualized.

We next refine our tests by focusing on analysts who remain active in the profession after 2002 (i.e., by excluding potential retirees, as in Table V), and are thus more likely to care about their reputations. Panel B of Table VI reports results for this sub-sample of analysts and indicates that, once again, anonymized buy recommendations significantly underperform their benchmarks and their non-anonymized counterparts.[27] The magnitude of their benchmark underperformance is similar to the full sample of analysts, averaging between 1.1 and 1.6 basis points for the full sample period (2.7% to 3.9% annualized), and between 2.7 basis points and 3.0 basis points for the bear market period (6.9% to 7.7% annualized). Again, the non-anonymized recommendations slightly outperform their benchmarks, but not in a statistically or economically important way. The corresponding long-short portfolio earns between 1.4 and 1.7 basis points in daily abnormal returns over the full sample period (3.5% to 4.3% annualized), and between 2.7 and 3.2 basis points in the bear market period (6.9% to 8.0% annualized); the *t*-statistics range between 2.53 and 3.52.

Since our results in Table V indicate that bolder recommendations are more likely to be anonymized, we also examine the return performance of a sub-sample of "bold" buy recommendations. Specifically, we restrict the sample to recommendations that deviate in absolute

[27] Further restricting the sample as in Table V to analysts who anonymize at least 5% of their historical records, or anonymize fewer than 50 of their recommendations, yields similar results.

terms from the average recommendation for the stock that year by at least one notch (e.g., if the consensus is a hold, a buy recommendation by an analyst would be flagged as bold). Panel C of Table VI shows that anonymized buy recommendations that are bolder in nature are particularly poor performers, with average underperformance relative to the benchmarks as high as 3.3 basis points in the full sample period (8.2% annualized) and 5.9 basis points in the bear market period (14.7% annualized). Non-anonymized bold buy recommendations also underperform their benchmarks, but not nearly as dramatically; as a result, the corresponding long-short portfolio earns up to 2.1 basis points in the full sample period (5.4% annualized), and 4.4 basis points in the bear market period (11.0% annualized).

Panel D presents a different picture. In the full sample, and within each sub-period, a portfolio of anonymized hold/sell recommendations does *not* perform in a statistically or economically different manner from a portfolio of non-anonymized holds/sells.[28] This result is potentially consistent with widespread claims that many analysts issued buys/strong buys (particularly at the end of the dot-com bubble) when holds/sells were deserved instead, suggesting that abnormally poor performance is most likely to be concentrated in portfolios of buy recommendations. Possible reasons for the overly optimistic behavior of analysts range from undue pressure from investment banking divisions to maintain an optimistic outlook (Michaely and Womack (1999), Lin and McNichols (1998)) or the desire to increase trading commissions for their employers (Jackson (2005)) to the desire to maintain access to the management of covered firms (Lim (2001)).

IV. Career Outcomes

The results in Section III confirm that it is the boldest, most embarrassing recommendations that were most prone to be anonymized. While we have no proof that individual analysts or their

[28] Results (not shown) for the analyst sub-sample that excludes potential retirees, and the recommendation sub-sample of bold hold recommendations, are similar: The performance of anonymized hold/sell portfolios is not significantly different from that of non-anonymized hold/sell portfolios.

employers manipulated their I/B/E/S entries, an undeniable consequence of the changes on the I/B/E/S tape is that the track records of the analysts concerned now look better than they should. A plausible motivation for the changes might then have been the desire to manipulate the information on which an analyst's current and prospective future employers evaluate her performance. In this section, we investigate what role anonymizations have played in analysts' career progressions since 2002.

Our analysis closely follows previous work by Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003) which relates career outcomes (such as changes of employer, promotions, demotions, and exit from the profession) to a total of five measures of analyst ability (forecast accuracy, forecast optimism, forecast boldness, all-star ranking, and seniority).[29] We add to this list of explanatory variables an indicator identifying analysts who anonymized 5% or more of their 1993-2002 recommendations.[30] To the extent that anonymizations make an analyst's track record less transparent, we expect some analysts to experience more favorable career outcomes than their characteristics and abilities would ordinarily warrant.

We focus on the group of analysts who contributed forecasts to I/B/E/S in 2002 (that is, the individuals most likely to have anonymized some of their recommendations between the 2002 and 2004 tapes) and follow their career progressions through December 2005. This results in 8,098 analyst-years. Like Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003), we do not observe whether an analyst is fired, demoted, or promoted. We can, however, observe breaks in an

[29] Relative forecast accuracy is defined as in Section III.B, following Hong, Kubik, and Solomon (2000). Relative forecast boldness is the scaled rank of the analyst's average *absolute* deviation from the earnings forecast consensus. It is constructed analogously to relative forecast accuracy (averaging values of years *t*-2 through *t*) as in Hong, Kubik, and Solomon (2000). To calculate relative forecast optimism, we follow Hong and Kubik (2003). First, a dummy variable is set to one if an analyst's forecast for a given stock between January 1 and July 1 of year *t* exceeds the consensus forecast for that stock over the same time period; and zero otherwise. The analyst's relative forecast optimism is then defined as the average of the dummy variables over all stocks followed by the analyst in years *t*-2 through *t*.

[30] Our results are not sensitive to how we code this indicator variable. For instance, we obtain nearly identical results if we do not impose the 5% filter.

analyst's contributions to I/B/E/S (which Hong, Kubik, and Solomon interpret as an adverse career outcome) as well as moves to another employer. We follow Hong, Kubik, and Solomon in calling a promotion a move from a small (fewer than 25 analysts) to a large (25 or more analysts) brokerage firm, and vice versa for a demotion.[31]

We code an analyst as leaving the profession if she contributed forecasts to I/B/E/S in year *t* but not in year *t*+1. (Our coding allows the analyst to return in, say, year *t*+2 and so identifies periods of unemployment as temporary exits from the profession.) We similarly track job moves by comparing the identity of the analyst's employer at the end of years *t* and *t*+1. The dataset thus consists of an unbalanced annual panel tracking analysts across brokerage firms over the years 2003 to 2005. To ensure we are tracking the right individuals, we correct thousands of errors in the I/B/E/S Broker Translation or "bran" file using the NASD *BrokerCheck* service as well as reports obtained through Investext and brokerage firms' press releases announcing new hirings. The most common error involves an analyst being assigned a new identifier after she has moved to a new employer.[32] Without our corrections, the number of exits from the industry would be vastly overstated while the number of job moves would be understated. We are also careful to take mergers among brokerage firms into account. Mergers result in analysts at the target being assigned to the acquirer's I/B/E/S broker code after the merger, giving the false appearance of a job move.

The aforementioned five measures of analyst ability that Hong, Kubik, and Solomon (2000) and

[31] Hong and Kubik report that results are generally robust to other definitions of "high status" employers (including the number of all-star analysts employed and the reputation of the brokerage firm in the IPO market).

[32] We make four types of corrections. First, we consolidate 2,872 cases where I/B/E/S assigns multiple identifiers to the same analyst, which often happens when an analyst moved brokerage firms or changed her name following marriage or divorce. Second, we correct 203 cases where a single identifier in fact pertained to more than one person, usually with a common name ("J. SMITH", "M. DAVIS", "P. JONES", etc). Third, 819 analysts have data backfills credited to their current (and so incorrect) broker code, which creates the impression that they moved back and forth between two or more brokerage houses on daily or weekly basis. Finally, we correct cases involving 57 analysts where the wrong individual was credited with a certain analyst report. In addition, we ensure that the analyst identifiers are consistent between the earnings forecast and recommendations tapes. None of these data clean-ups in any way cause anonymizations, though they allow us to measure analyst characteristics and analyst job moves cleanly.

Hong and Kubik (2003) explore are practically orthogonal to each other, with two exceptions: Relatively more accurate analysts tend to be bolder (correlation: 24.0%) and all-stars tend to have been in the industry for longer (correlation: 20.3%). We thus depart from Hong, Kubik, and Solomon by simultaneously including all five measures in our empirical specifications rather than including them one by one. We do so to conserve space; our results are not sensitive to this choice. We estimate multivariate probits that in addition control for year fixed effects and the log number of stocks the analyst covered in year *t*, based on Hong, Kubik, and Solomon's argument that analysts who cover few companies are more likely to score in the tails of the distribution of accuracy, boldness, and optimism.

Table VII reports the results. The dependent variable in the first column is an indicator variable equal to one if the analyst changed employers between *t* and *t*+1. The unconditional probability of a job move is 10.9% in our sample, and increases by a statistically and economically significant 22.9%, from 10.9% to 13.4%, for analysts whose track record through July 2002 has been partly anonymized (holding the other covariates constant). Analysts who are bolder relative to their peers in the same sector are significantly less likely to move jobs. The predicted probability of an all-star moving employers in our time period is close to zero (2.6% versus 10.9% for the sample as a whole). The pseudo-R^2 of 2.4% indicates that a substantial portion of the variation in outcomes remains unexplained, though the R^2 rises to 10.8% when we include random brokerage firm effects (not tabulated).[33]

We conclude that anonymizers are more likely to move jobs, but a job move could be good or bad news depending on whether it amounts to a promotion or a demotion. In the next two columns

[33] Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003) include a full set of brokerage firm fixed effects. As fixed effects probit results in biased coefficient estimates, we have instead investigated random-effects panel probits. With the exception of the demotion models, where none of the coefficients are statistically significant, our results are virtually identical statistically and economically whether we include random effects or not.

we focus on moves to a large brokerage firm as a proxy for a favorable career outcome. Controlling for the fact that analysts are less likely to move to a large brokerage firm if they most recently worked for a small brokerage firm (p=0.041), the specification in column (2) shows that the probability of being hired by a large brokerage firm is positively and significantly related to anonymizations (p<0.001). Economically, anonymization status is the most significant determinant in this specification (after all-stars, who we saw earlier hardly move anywhere during our sample period): It increases the likelihood of moving to a large brokerage firm by 2.5 percentage points, an increase of 50% from the unconditional likelihood of 5%.[34] More senior analysts, those who make relatively bolder forecasts, and those covering fewer stocks are significantly less likely to move to a large brokerage firm.

In column (3), we restrict the sample to analysts who work for a small brokerage firm in year t and ask whether anonymization helps them move up to a large brokerage firm in t+1. The relevant sample size here is 2,500 analyst-years. Because all-star status perfectly predicts such a move (every all-star is "promoted") we cannot include it in this specification. If anything, anonymization status now has an even greater effect than before. Analysts whose I/B/E/S records were changed see their chances of moving up to a large brokerage firm improve by close to 70%, from 4.6% to 7.7% (p=0.007). At the 5% level, anonymization status is the only significant determinant of an analyst's promotion in this model. Compared to the other (imprecisely estimated) coefficients in the model it is larger even than a five-standard deviation improvement in the analyst's relative forecast accuracy.

Columns (4) and (5) repeat the analysis shown in columns (2) and (3) for moves to small brokerage firms. The coefficient estimated for anonymization status is slightly positive but both statistically and economically insignificant in either model. Thus, while a scrubbed track record is

[34] In unreported tests, we find that the probability of moving to a large brokerage firm peaks when 20 to 30 of an analyst's records have been anonymized. This suggests that our results are not driven by the outliers evident in Fig. 2.

associated with promotions, it is unrelated to demotions. Among the controls, we find that all-stars virtually never move to a small brokerage firm whereas analysts at a later stage in their careers and those who are more optimistic and cover fewer stocks are more likely to end up at a smaller house.

Our final specification, shown in column (6), investigates temporary or permanent departures from the industry. Analysts are significantly more likely to suffer unemployment or retire from Wall Street if they are not all-stars or have a reputation for relatively inaccurate research, consistent with the findings reported in Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). On the other hand, relatively more productive analysts, measured by the number of companies covered, are more likely to remain in employment. Controlling for these factors, we find that anonymization status has no effect on the likelihood of leaving the profession.

V. Conclusions

We provide evidence that nearly twenty thousand records in I/B/E/S, a database of research analyst recommendations widely used by investment professionals and academics, were manipulated between September 2002 and May 2004. Comparing two snapshots of the entire I/B/E/S analyst stock recommendations database, taken in 2002 and 2004 but each covering the *same* time period 1993-2002, we document thousands of changes. Our focus is the selective, ex post removal of an analyst's name from some of her historical recommendations which had originally been available in I/B/E/S under her name. Our tests reveal that such "anonymizations" are pervasive and non-random: Bolder recommendations are more likely to be anonymized, as are recommendations from senior analysts, all-star analysts, and those who remain in the industry beyond 2002. These are precisely the types of analysts with the best job prospects, who have the most to lose by disclosing their past performance. Abnormal stock returns following anonymized buy recommendations are significantly lower than those following non-anonymized buy recommendations. The magnitude of this effect is economically large, ranging from 3.3% to 11.0%

annualized, depending on the sample and time period examined. These findings suggest that analysts may attempt to distance themselves from poorly performing past recommendations.

Though our empirical results point that way, we cannot prove that individual analysts actually altered their I/B/E/S records. It is possible that the brokerage firms were in fact the culprits, although the patterns we document seem at odds with this interpretation: Anonymizations look quite indiscriminate at the brokerage firm-level, in that more than 85% of all brokerage firms in I/B/E/S that continue to publish sell-side research after 2002 are associated with anonymizations.

It is also possible that I/B/E/S cleaned up the data in such a way that it randomly produced the patterns we document, but this seems even less likely given the non-random nature of the results. Whether or not analysts were in fact behind these changes, however, their track records undeniably look better than they should, and we show that the analysts concerned apparently benefited in the sense of experiencing more favorable career outcomes than their track records and abilities would otherwise warrant: Anonymizers are more likely to move jobs, to be hired by a large brokerage firm, and to move from a small to a large firm (Hong, Kubik, and Solomon's (2000) measure of a promotion). Anonymization easily has the largest economic effect in our career outcome models.

Overall, our findings add more fuel to the debate over conflicts of interest in the equity research industry. Policies that make an analyst's reputation more transparent and increase the implicit penalty for opportunistic behavior, such as the new SEC/NASD/NYSE provisions that require analysts to disclose their prior ratings in their research notes, may be an effective tool for ameliorating analysts' incentives to issue optimistic opinions. However, this argument relies crucially on the assumption that the level of transparency in the market cannot be manipulated. Our findings suggest that agency problems in the analyst industry are not easily overcome, and that policies designed to increase transparency need to explicitly consider the practical issues surrounding the way in which analysts and banks are currently monitored by market participants.

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Appendix:
How Data Vendors Collect Analyst Recommendations and Discussions with Analysts

Brokerage firms disseminate their research reports, recommendations, and earnings forecasts through a number of different channels. Currently most have an intranet that clients can access, but firms also use commercial database vendors as "shop windows" for their research. For example, "In addition to the UBS Warburg web site our research products are available over third-party systems provided or serviced by: Bloomberg, First Call, I/B/E/S, IFIS, Multex, QUICK, and Reuters."

I/B/E/S was long the leader in this market. Thomson Financial developed First Call as a rival to I/B/E/S. Since Thomson's acquisition of Primark in 2000, Thomson has owned both databases. According to Thomson Financial, the two databases are intended to be complementary: First Call focuses on "research distribution" of reports and morning-meeting notes while I/B/E/S is "stronger on quantitative data."[35]

Commercial database vendors obtain research in two main ways: As an electronic feed from the brokerage firm's in-house database; or in the form of a research report in hardcopy or .pdf from which earnings forecasts and recommendations are transcribed into a database. The database vendors obtain research for free (in keeping with the idea that they provide a shop window for brokerage firms) and charge their clients subscription fees.

Every database vendor we have talked to employs filters to flag possibly erroneous data and has procedures in place for verifying suspect records. For instance, analysts we have interviewed confirm that database vendors will occasionally call to verify certain estimates or recommendations. The process also works in reverse. When alerted to obvious errors, perhaps by their clients, analysts will contact database vendors to have corrections made. Both database vendors and analysts we have talked to indicate that this is a simple process which does not involve compliance. We have been unable to ascertain whether changing data in a brokerage firm's in-house database (from which database vendors obtain electronic feeds) would automatically result in those data being changed ex post in commercial databases as well.

We have also asked a small number of analysts why some of their records were anonymized. We stress that these analysts do not represent a random sample, for two reasons: We could only locate analysts who remain in the industry as of Q1 2006; and most analysts were unwilling to return our phone calls, and those who did mostly worked for small brokerage houses. The analysts we talked to indicated they had no idea how their records became anonymized.

[35] See P.J. Hane, "Thomson Corp to Acquire Primark", Information Today, June 12, 2000. (Accessed at http://www.infotoday.com/newsbreaks/nb000612-1.htm on July 12, 2006.)

Figure 1. Kernel Density Estimate of Absolute Time Between Anonymized Recommendation and Closest Earnings Forecast for the Same Stock by the Same Analyst.

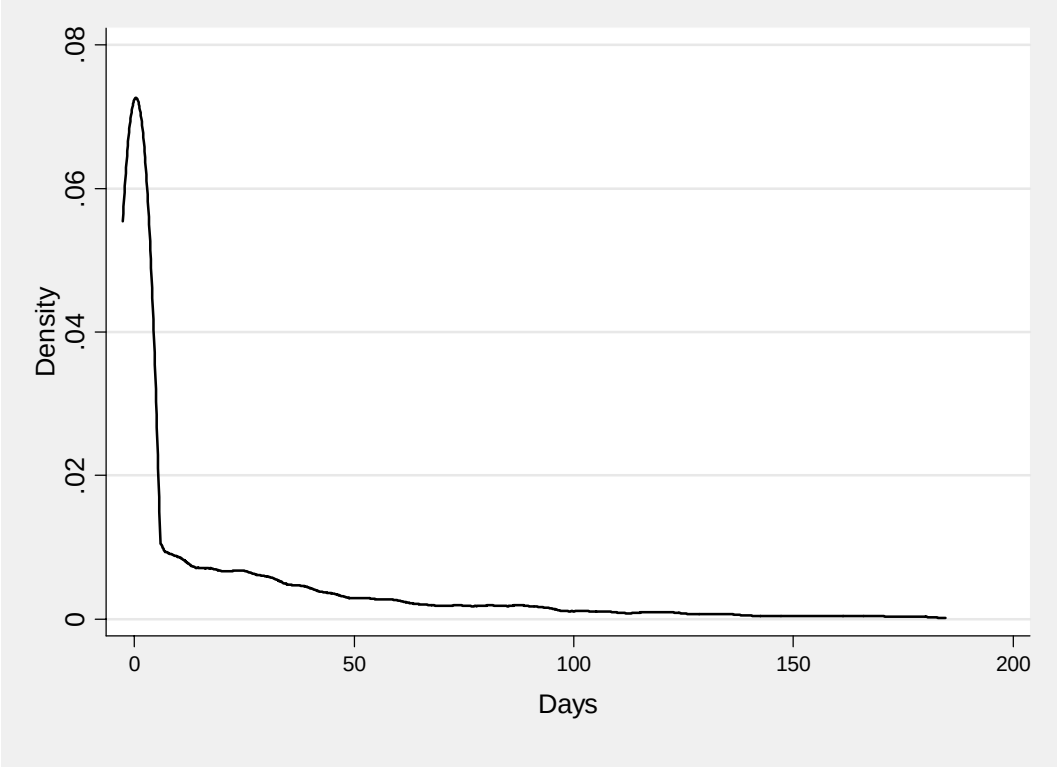

Figure 2. Kernel Density Estimate of Number of Anonymized Recommendations per Analyst.

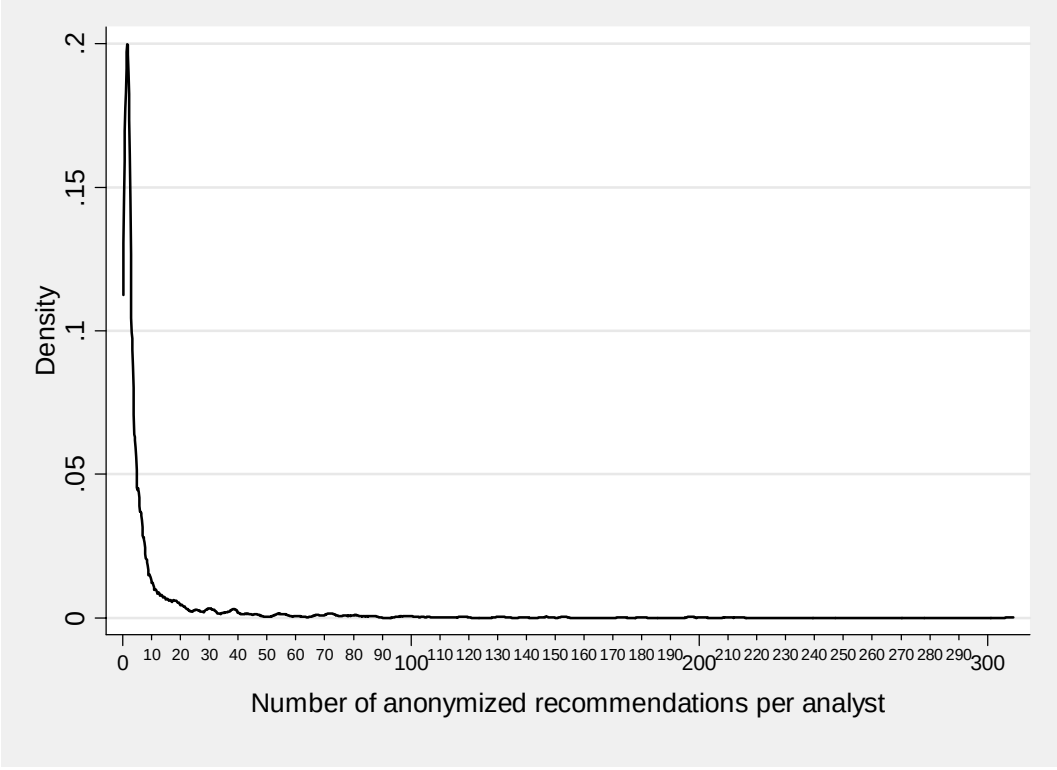

Table I. Distribution of Anonymized and Non-anonymized Recommendations.

The 2002 I/B/E/S tape contains 280,463 investment recommendations issued between October 1993 and July 18, 2002. Of these, 19,892 have been anonymized since they were first issued, in the sense that the name of the recommending analyst was disclosed on the 2002 I/B/E/S tape but no longer appears on the 2004 I/B/E/S tape. The table provides a breakdown of the number of recommendations by the strength of the rating using the I/B/E/S five-point scale. The asterisks indicate significance of difference of proportions at each rating level. We use ** and * to denote significance at the 1% and 5% level, respectively.

	No. of recommen-dations on 2002 tape	Distribution of rating levels (1 = strong buy)				
		1	2	3	4	5
All recommendations	280,463	28.6%	36.7%	31.4%	1.7%	1.6%
Anonymized recommendations	19,892	29.6%	36.2%	30.7%	1.8%	1.7%
Non-anonymized recommendations	260,571	28.5%	36.7%	31.5%	1.6%	1.6%
Significance of difference in proportions		**		*	*	
Fraction anonymized	7.1%					

Table II. Matching First Call and I/B/E/S.

The 2002 I/B/E/S tape contains 280,463 investment recommendations issued between October 1993 and July 18, 2002. Of these, 19,892 have been anonymized since they were first issued, in the sense that the name of the recommending analyst was disclosed on the 2002 I/B/E/S tape but no longer appears on the 2004 I/B/E/S tape. To examine whether names were removed purely to correct errors resulting from the wrong analyst or broker being credited with a recommendation, we match the recommendations on the 2002 I/B/E/S tape to the recommendations recorded in the First Call recommendations database. We match by the identity of the brokerage firm and the historic CUSIP of the company concerned as well as the recommendation date (which we allow to differ by up to 14 calendar days to allow for differences in the way the two databases record information).

	No. of I/B/E/S recommen-dations	No. of matched recommen-dations in First Call	Timeliness of match: First Call date – I/B/E/S date (in calendar days)						
			[-14,-11]	[-6,-10]	[-1,-5]	0	[1,5]	[6,10]	[11,14]
Anonymized recommendations	19,892	8,290	350	519	2,031	4,707	328	201	154
Non-anonymized recommendations	260,571	122,243	4,217	7,357	30,707	71,170	4,419	2,552	1,821

Table III. Summary Statistics: Brokerage Firm Characteristics.

The 2002 I/B/E/S tape contains 280,463 investment recommendations issued between October 1993 and July 18, 2002 by 7,886 separate named analysts working for 385 distinct brokerage firms. Sixteen firms already only had anonymous recommendations on the 2002 tape, leaving 369 brokerage firms whose characteristics are described in this table. These 369 firms are split into two groups depending on whether any of their recommendations have been anonymized since first issued, that is, if the name of the recommending analyst was disclosed on the 2002 I/B/E/S tape but no longer appears on the 2004 I/B/E/S tape. Equity underwriting market share is used as a proxy for the size of the firm's investment banking operations and is based on data from Thomson Financial/SDC's U.S. New Issues database. The number of analysts at each brokerage firm is used as a proxy for the size of the firm's research department and is based on the number of separate named analysts who contribute recommendations to I/B/E/S in 2002. A brokerage firm is deemed to have exited sell-side research if by 2002 it no longer contributes data to I/B/E/S. Sanctioned brokerage firms are the 12 firms that were subject to the 2003 Global Settlement.

	Some anonymized records	No anonymized records	Some anonymized records Row %	No anonymized records Row %
Number of brokerage firms	239	130		
Brokerage firm size in 2002				
mean equity underwriting market share	0.4%	0.0%		
mean # analysts at brokerage firm	18.4	0.8		
Exited sell-side research by 2002?				
Yes	0	89	0.0	100.0
No	239	41	85.4	14.6
Sanctioned in Global Settlement?				
Yes	12	0	100.0	0.0
No	227	130	63.6	36.4
Investment banking operations in 2002?				
Yes	61	4	93.8	6.2
No	178	126	58.6	41.4
Fraction of workforce with newly anonymous rec's				
mean	38.4%	n.a.		
median	27.6%	n.a.		
mean among sanctioned brokerage firms	21.8%	n.a.		
mean among non-sanctioned brokerage firms	39.3%	n.a.		
Fraction of broker's rec's that have been anonymized				
mean	16.4%	n.a.		
median	8.4%	n.a.		
mean among sanctioned brokerage firms	6.4%	n.a.		
mean among non-sanctioned brokerage firms	16.9%	n.a.		

Table IV. Summary Statistics: Analyst Characteristics

The 2002 I/B/E/S tape contains 280,463 investment recommendations issued between October 1993 and July 18, 2002 by 7,886 separate named analysts working for 385 distinct brokerage firms. The 7,886 analysts are split into two groups depending on whether any of their recommendations has been anonymized since first issued, that is, if the name of the recommending analyst was disclosed on the 2002 I/B/E/S tape but no longer appears on the 2004 I/B/E/S tape. The number of analysts at each brokerage firm is used as a proxy for the size of the firm's research department and is based on the number of separate named analysts who contribute recommendations to I/B/E/S in 2002. Equity underwriting market share is used as a proxy for the size of the firm's investment banking operations and is based on data from Thomson Financial/SDC's U.S. New Issues database. Sanctioned brokerage firms are the 12 firms that were subject to the 2003 Global Settlement. Analyst characteristics are reported as of 2002 or the date of the analyst's retirement (exit from I/B/E/S) if earlier. All-stars are analysts ranked as a top-three or runner-up analyst in their sector by *Institutional Investor*. Relative forecast accuracy is a measure of the analyst's average forecast accuracy across the stocks she has covered in the three years to July 2002 (or in the three years to the date of her retirement, if she has left the industry before July 2002) relative to the other analysts covering the same stocks. It is constructed as in Hong, Kubik and Salomon (2000) and ranges from 0 to 1, with a higher number indicating greater forecast accuracy. As a proxy for seniority, we compute the number of years since the analyst first appeared in the I/B/E/S database. Analysts are deemed to remain in the profession post-2002 if they contribute recommendations to I/B/E/S in 2003 through 2005. This may undercount the number of analysts who remain in the profession. We use *** and ** to denote significance at the 0.1% and 1% level, respectively. NM = "not meaningful".

	Some anonymized records	No anonymized records	*t*-test of difference in mean or fraction
Number of analysts	2,138	5,748	
Panel A: Number of recommendations			
Number of anonymized recommendations per analyst			
Mean	9.3	0	NM
Min	1	0	NM
Median	2	0	NM
Max	308	0	NM
Fraction of analyst's recommendations that have been anonymized			
Mean	29.7%	0	NM
Median	13.2%	0	NM
Panel B: Employer characteristics			
Mean # analysts at brokerage firm in 2002	96.9	94.8	-
Mean 2002 equity underwriting market share	3.6%	3.6%	-
Fraction who in 2002 or at retirement work at a sanctioned brokerage firm	39.7%	40.6%	-
Panel C: Analyst characteristics			
Fraction of analysts ranked all-star by *Institutional Investor* in Oct 2001	16.3%	13.8%	**
Mean relative forecast accuracy as of 2002 or retirement date	48.7%	47.1%	**
Mean seniority (years in I/B/E/S database) as of 2002 or retirement date	7.9	6.8	***
Mean # recommendations in I/B/E/S through 2002	53.8	27.4	***
Panel D: Post-2002 career paths			
Fraction of analysts who remain in the profession post-2002	70.4%	46.4%	***
Fraction of anonymized recommendations originally made by analysts who remain in the industry post-2002	84.5%	n.a.	NM

Table V. Characteristics of Recommendations that are Anonymized.

The unit of observation in this table is a recommendation, issued between October 1993 and July 18, 2002. The dependent variable is an indicator equal to 1 if the recommendation has been anonymized (i.e., if the name of the recommending analyst was disclosed on the 2002 I/B/E/S tape but no longer appears on the 2004 I/B/E/S tape), and zero otherwise. Explanatory variables are defined in Tables III and IV. All models are estimated using probit MLE and include random brokerage firm effects. The specification in column (3) excludes 1,100 observations by analysts who anonymize less than 5% of their recommendations, to allow for the possibility that these low-frequency anonymizations are innocuous data cleanups. Column (4) excludes 8,299 observations by analysts who anonymize more than 50 recommendations each. Column (5) limits the sample to analysts who by 2002 had not left the industry. Intercepts and dummies identifying the year in which the recommendation was originally made are not shown. Standard errors are shown in italics. Note that random-effects probit does not support a White or cluster adjustment for heteroskedasticity. We use ***, **, and * to denote significance at the 0.1%, 1%, and 5% level (two-sided), respectively.

	Whole sample (1)	Whole sample (2)	Exclude if <5% anon. (3)	Exclude if >50 anon. (4)	Exclude if retired (5)
Analyst characteristics					
Relative forecast accuracy	-0.467***		-0.538***	-0.242***	-0.680***
	0.059		*0.059*	*0.055*	*0.083*
Change in relative forecast accuracy since 1997		-0.135**			
		0.053			
=1 if analyst is ranked *II* all-star in Oct 2001	0.262***	0.231***	0.384***	0.007	0.448***
	0.038	*0.037*	*0.035*	*0.034*	*0.034*
seniority (log years in I/B/E/S)	0.051**	0.048**	0.009	0.040*	-0.015
	0.019	*0.019*	*0.019*	*0.018*	*0.020*
ln(analyst's no. of recommendations)	-0.104***	-0.105***	-0.110***	-0.254***	-0.002
	0.012	*0.012*	*0.012*	*0.011*	*0.014*
=1 if analyst still active post-2002	0.676***	0.677***	0.738***	0.587***	
	0.021	*0.021*	*0.021*	*0.021*	
Brokerage firm characteristics					
investment banking size (% eq. underwriting mkt share, 2002)	-0.394	-0.008	-0.629***	-0.059*	-0.170
	0.268	*0.205*	*0.149*	*0.025*	*0.108*
size of research department (log no. analysts at broker in 2002)	0.187***	0.160***	0.208***	0.107***	0.085***
	0.037	*0.030*	*0.025*	*0.021*	*0.026*
=1 if brokerage firm sanctioned in Global Settlement	-0.535***	-0.615*	-0.487***	-0.442**	-0.487***
	0.071	*0.259*	*0.151*	*0.144*	*0.050*
Characteristics of company/recommendation					
boldness	0.038**	0.034**	0.034**	0.031*	0.020
	0.013	*0.013*	*0.013*	*0.013*	*0.015*
=1 if company has IB relationship with brokerage firm	-0.014	0.058	0.087	0.076	0.124
	0.081	*0.077*	*0.077*	*0.057*	*0.069*
ln(aggregate amount of capital company raised in prior 5 years)	0.002	0.002	0.005	0.005*	0.002
	0.003	*0.003*	*0.003*	*0.003*	*0.003*
Diagnostics					
Pseudo R^2	47.5 %	47.5 %	47.1 %	31.4 %	42.8 %
Wald test: all coefficients=0 (χ^2)	1,562***	1,591***	1,817***	1,210***	1,363***
Likelihood ratio test: all random effects = 0 (*p*-vale of χ^2 test)	<0.001	<0.001	<0.001	<0.001	<0.001
Number of observations	259,714	259,714	258,614	251,415	169,174

Table VI. Average Daily Portfolio Buy-and-Hold Abnormal Returns to Portfolios of Anonymized and Non-Anonymized Stock Recommendations

This table reports the average daily percentage buy-and-hold abnormal returns for portfolios of buy recommendations (which includes upgrades to buy or strong buy, or initiations with a buy or strong buy rating) and portfolios of hold/sell recommendations (which includes downgrades to hold, sell, or strong sell, or initiations/resumptions/reiterations with a hold, sell, or strong sell rating), for all anonymized recommendations and non-anonymized recommendations by analysts who anonymized at least one of their recommendations (but not all). Corresponding *t*-statistics are in parentheses. Panel A reports the results for the full sample of buy recommendations, Panel B reports the results for the sub-sample of recommendations by those analysts who by 2002 had not left the industry, and Panel C reports the results for the sub-sample of recommendations that are "bold" (i.e., they deviate in absolute terms from the consensus by at least 1 notch). Panel D reports the results for the full sample of hold/sell recommendations. Columns (1) and (2) report the average daily abnormal returns for the entire sample period (October 29, 1993 to July 18, 2002); Columns (3) and (4) and columns (5) and (6) report the average daily abnormal returns for the period through March 10, 2000 (the date of the NASDAQ market peak) and the period subsequent to March 10, 2000, respectively. The "4-factor alpha'' is the intercept from a regression of the daily portfolio excess return on (i) the excess of the market return over the risk-free rate, (ii) the difference between the daily returns of a value-weighted portfolio of small stocks and one of large stocks (SMB), (iii) the difference between the daily returns of a value-weighted portfolio of high book-to-market stocks and one of low book-to-market stocks (HML), and (iv) the difference between the daily returns of a value-weighted portfolio of high price momentum stocks and one of low price momentum stocks (UMD). The "4-factor plus Industry alpha'" is the intercept from a regression of the daily portfolio excess return on (i)-(iv) plus (v) industry excess returns (value-weighted excess returns for each of ten industry segments as defined by Kenneth French). To control for non-synchronous trading, all regressions also include one-day lags of each of the independent variables.

	10/29/1993 to 07/18/2002		10/29/1993 to 03/10/2000		03/11/2000 to 07/18/2002	
	4-factor α	4-factor plus Industry α	4-factor α	4-factor plus Industry α	4-factor α	4-factor plus Industry α
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Buy recommendations, all analysts who anonymized at least one recommendation						
Anonymized (a)	-0.0117	-0.0134	-0.0012	-0.0023	-0.0316	-0.0312
	(-1.93)	(-2.29)	(-0.19)	(-0.39)	(-2.44)	(-2.42)
Non-Anonymized (b)	0.0015	0.0009	0.0058	0.0047	-0.0033	-0.0007
	(0.36)	(0.21)	(1.37)	(1.21)	(-0.36)	(-0.08)
(b) – (a)	0.0132	0.0143	0.0070	0.0070	0.0282	0.0304
	(2.73)	(2.99)	(1.42)	(1.43)	(2.66)	(2.89)
Panel B: Buy recommendations, all analysts who anonymized at least one recommendation (excluding possible retirees)						
Anonymized (a)	-0.0107	-0.0156	-0.0025	-0.0059	-0.0274	-0.0304
	(-1.76)	(-2.69)	(-0.40)	(-1.01)	(-2.13)	(-2.41)
Non-Anonymized (b)	0.0032	0.0013	0.0064	0.0046	-0.0001	0.0013
	(0.73)	(0.32)	(1.41)	(1.13)	(-0.01)	(0.15)
(b) – (a)	0.0139	0.0169	0.0089	0.0105	0.0273	0.0318
	(2.82)	(3.52)	(1.78)	(2.11)	(2.53)	(3.02)

Table VI. Continued.

	10/29/1993 to 07/18/2002		10/29/1993 to 03/10/2000		03/11/2000 to 07/18/2002	
	4-factor α	4-factor plus Industry α	4-factor α	4-factor plus Industry α	4-factor α	4-factor plus Industry α
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: "Bold" Buy recommendations, all analysts who anonymized at least one recommendation						
Anonymized (a)	-0.0279	-0.0326	-0.0212	-0.0246	-0.0469	-0.0585
	(-3.84)	(-4.67)	(-3.03)	(-3.70)	(-2.86)	(-3.64)
Non-Anonymized (b)	-0.0066	-0.0121	-0.0095	-0.0103	-0.0055	-0.0149
	(-1.35)	(-2.68)	(-1.89)	(-2.26)	(-0.54)	(-1.56)
(b) – (a)	0.0213	0.0205	0.0117	0.0142	0.0414	0.0436
	(3.04)	(2.94)	(1.71)	(2.10)	(2.63)	(2.74)
Panel D: Hold/Sell recommendations, all analysts who anonymized at least one recommendation						
Anonymized (a)	-0.0063	-0.0130	-0.0079	-0.0121	-0.0144	-0.0217
	(-0.99)	(-2.14)	(-1.26)	(-2.06)	(-1.06)	(-1.62)
Non-Anonymized (b)	-0.0035	-0.0066	-0.0071	-0.0079	-0.0043	-0.0104
	(-0.81)	(-1.64)	(-1.88)	(-2.29)	(-0.43)	(-1.10)
(b) – (a)	0.0028	0.0064	0.0008	0.0042	0.0101	0.0113
	(0.54)	(1.24)	(0.14)	(0.80)	(0.92)	(1.02)

Table VII. The Effect of Anonymization on Subsequent Career Outcomes.

This table investigates the role anonymizations play in analysts' subsequent career progressions. The unit of observation is an analyst-year and the time period spans 2003 through 2005. We focus on analysts who appeared on the 2002 I/B/E/S tape (i.e., we ignore analysts who entered the profession after 2002). The dependent variable equals one if the analyst that year changed jobs (col. (1)), moved to a 'large' brokerage firm employing 25 or more analysts (cols. (2) and (3)), moved to a 'small' brokerage firm employing fewer than 25 analysts (cols. (4) and (5)), or exited the industry (in the sense of ceasing to contribute research to I/B/E/S the following year; col. (6)). Relative forecast accuracy and boldness are defined as the analyst's scaled ranks (relative to other analysts covering the same stocks) of deviations between earnings forecast and subsequent earnings realization and absolute deviation from the earnings forecast consensus, respectively, while relative forecast optimism is defined using a dummy variable = 1 if the analyst's forecast for a stock is greater than the consensus of other analysts. In each case (accuracy, boldness, optimism), measures are averaged across stocks she covers in years t-2 through year t; see Hong, Kubik, and Solomon (2000) and Hong and Kubik (2003). The other explanatory variables are defined as in Tables III through V. All models are estimated using multivariate probit. To conserve space, intercepts and year fixed effects are not shown. All results are robust to including random brokerage firm effects to control for otherwise omitted heterogeneity arising from differences across brokerage firms. Heteroskedasticity-consistent standard errors are shown in italics. They are clustered by analyst (i.e., observations are assumed to be independent across analysts but not necessarily within). We use ***, **, and * to denote significance at the 0.1%, 1%, and 5% level (two-sided), respectively.

	Change jobs	Promotion		Demotion		Exit industry
		Move to large brokerage firm	Move from small to large brokerage firm	Move to small brokerage firm	Move from large to small brokerage firm	
	(1)	(2)	(3)	(4)	(5)	(6)
=1 if analyst's recommendations anonymized	0.135**	0.227***	0.298**	0.021	0.016	0.034
	0.050	*0.061*	*0.110*	*0.060*	*0.078*	*0.056*
=1 if analyst is ranked *II* all-star	-0.712***	-0.340***		-1.385***	-1.371***	-0.473***
	0.096	*0.102*		*0.304*	*0.304*	*0.098*
seniority (log years in I/B/E/S)	-0.021	-0.159***	-0.140	0.091*	0.081	0.021
	0.033	*0.043*	*0.074*	*0.039*	*0.052*	*0.034*
relative forecast accuracy	-0.088	0.263	0.543	-0.297	-0.289	-0.457***
	0.141	*0.179*	*0.359*	*0.160*	*0.173*	*0.141*
relative forecast optimism	0.169	-0.014	-0.085	0.232*	0.263	-0.025
	0.098	*0.129*	*0.242*	*0.113*	*0.137*	*0.096*
relative boldness	-0.466**	-0.503**	-0.514	-0.312	-0.145	0.063
	0.152	*0.187*	*0.376*	*0.178*	*0.198*	*0.157*
ln(analyst's no. of stocks covered)	-0.020	0.125**	0.126	-0.138***	-0.170***	-0.693***
	0.030	*0.040*	*0.076*	*0.034*	*0.044*	*0.032*
= if analyst worked at small brokerage firm *t*-1		-0.110*		0.304***		
		0.054		*0.047*		
Diagnostics						
Pseudo R^2	2.4 %	2.3 %	3.0 %	5.2 %	5.6 %	13.9 %
Wald test: all coefficients=0 (χ^2)	101.3***	69.6***	31.4***	119.7***	64.5***	628.3***
Number of observations	8,098	8,098	2,500	8,098	5,598	8,098