The Geography of Equity Analysis

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ABSTRACT

I provide evidence that geographically proximate analysts are more accurate than other analysts. Stock returns immediately surrounding forecast revisions suggest that local analysts impact prices more than other analysts. These effects are strongest for firms located in small cities and remote areas. Collectively these results suggest that geographically proximate analysts possess an information advantage over other analysts, and that this advantage translates into better performance. The well-documented underwriter affiliation bias in stock recommendations is concentrated among distant affiliated analysts; recommendations by local affiliated analysts are unbiased. This finding reveals a geographic component to the agency problems in the industry.

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This paper investigates the effect of distance on the accuracy and investment value of equity analysts' forecasts and recommendations. Using a large panel of analyst data from 1994 to 2001, I provide evidence that geographically proximate analysts outperform their distant counterparts. Specifically, local analysts are significantly more accurate than other analysts. I find this effect to be strongest in small firms, in firms located outside of the most populated cities, and in firms located in remote areas. The magnitude of the accuracy advantage is small in the overall sample ($0.025 per share on average), but larger in various subsamples; for example, local analysts covering small stocks in remote areas are approximately $0.141 per share more accurate than distant analysts.

Abnormal returns surrounding large forecast revisions suggest that local analysts also impact stock prices more than other analysts. In tests controlling for the magnitude of the revision, firm size, and analyst affiliation status, I find that local analyst revisions are associated with an incremental average excess return of -0.128% per day (t=-2.46) in the three days surrounding strong negative revisions; this number grows to -0.164% (t=-2.54) for firms located in small cities. Following strong positive revisions, these figures are slightly smaller (0.095% for all stocks, and 0.102% for non-metro stocks), but still significant. As with the accuracy results, these effects are strongest in the most recent subperiod.

These findings are consistent with the hypothesis that geographically proximate analysts possess an information advantage over other analysts. As an information story would suggest, local analysts appear to have a decided advantage in covering small stocks and stocks located in remote areas, where the access to private information is likely to be strongest and competition for that information the weakest. By contrast, I find little support for the notion that the link between geography and performance is a natural by-product of the endogenous coverage decisions made by analysts. For example, local analysts do not outperform other analysts simply as a result of covering fewer stocks or through increased specialization. I present some evidence that local investor demand or visibility may be linked to local abnormal performance, perhaps through increased analyst attention or effort, but these results are based on a small subset of stocks for which advertising expense is nonzero.

Geography also provides an interesting viewpoint through which to explore the much-publicized affiliation effect. The affiliation effect refers to the perceived tendency of analysts from a brokerage house that has an underwriting relationship with a stock to issue more positive predictions than analysts from nonaffiliated houses. The implication is that brokerage houses reward optimistic affiliated analysts who generate investment banking business and trading commissions, at the expense of clients who trust analysts' research to be unbiased.[1] The extent to which affiliated analysts' research suffers from this potential agency problem is an open question, and one that has spawned a series of recent empirical papers.[2] Since I am able to separate out the effects of affiliation and geographic proximity throughout the paper, another contribution of this paper is that I am able to quantify both agency costs and information costs for a large sample of analysts.

In this context, I explore the effect of distance on the well-documented affiliation bias in analysts' stock recommendations. Lin and McNichols (1998) examine the effect of underwriting relationships on stock recommendations and report that lead underwriter analysts' recommendations are significantly more favorable than those made by unaffiliated analysts. They also report that three-day returns to lead underwriter hold recommendations are significantly more negative than those to unaffiliated hold recommendations.[3] This result suggests that affiliated analysts are more likely than other analysts to issue a hold recommendation when a sell recommendation is warranted. I re-examine these results by conditioning on the location of the analyst.

I am able to replicate the underwriter affiliation bias in stock recommendations in my sample, but find no evidence of a bias for affiliated analysts who are also located nearby the firm being covered. For example, the incremental three-day average excess return to distant affiliated hold recommendations is -1.74% (t=-3.87), while the incremental market reaction to local affiliated hold recommendations is only -0.25% (t=-0.78). I argue that this is because local affiliates are less likely to be working at high-status firms, where agency problems are intense due to the constant pressure to garner investment banking business. Thus, not only does this paper point to an informational link between proximity and equity analysis, but it also sheds light on the geographic nature of the agency problems in the industry.

The paper is organized as follows. Section I provides some background and a description of the data, while Sections II to IV present my empirical design and results. Section V concludes.

I. Methodology

A. Background and Motivation

Arguments supporting a link between proximity and information flow are presented in Coval and Moskowitz (2001), who analyze the role of geography in the context of mutual fund managers. They maintain that geographic proximity is inversely related to the cost of information acquisition. It also provides access to private information: "Investors located near a firm can visit the firm's operations, talk to suppliers and employees, as well as assess the local market conditions in which the firm operates" (p. 839). Similarly, I would argue that the ability of local analysts to make house calls rather than conference calls, during which time they can meet CEOs face-to-face and survey the firm's operations directly, provides them with an opportunity to obtain valuable private information. Following this logic, geographic proximity is a sensible proxy for the quality of analyst information.

On a more basic level, the advantage of focusing on equity analysts is that this industry offers an ideal testing ground for a number of theories of economic behavior. Since analyst data is available in large quantities and in relatively standardized formats, this industry is one of the few areas that allows precise estimation of the effects of asymmetric information, agency costs, herding, etc., for an important segment of the financial community. I use this testing ground to evaluate the idea that geographic proximity facilitates information flow. Further, since I can control for investment banking affiliations, I attempt to distinguish information effects from agency effects throughout the paper.

Evidence that affiliated analysts suffer from an agency problem caused by the inherent conflict of interest in the functions they perform is mounting (see Hong and Kubik (2003) for a summary). Although an analyst could conceivably gain inside information about a firm that has an underwriting relationship with the analyst's brokerage house, and hence deliver more accurate forecasts or recommendations, previous research (see, for example, Lin and McNichols (1998)) finds that this is usually not the case. This perceived agency problem is garnering widespread attention in the political arena as well. Congressional hearings to "analyze the analysts" have already taken place, and on May 8, 2002, the SEC approved proposed rule changes by the NASD and the NYSE to address security analyst conflicts of interest.

Another policy question that indirectly motivates my research is the recent passage of Regulation FD. Effective October 23, 2000, companies must reveal any “material” information to all investors and analysts simultaneously in the case of intentional disclosures, or within 24 hours in the case of unintentional disclosures. According to SEC Proposed Rule S7-31-99, regulators believe that allowing selective disclosure is “not in the best interests of investors or the securities markets generally.” And yet, empirical evidence is sparse on whether this type of disclosure even exists in the first place, or if the enactment of the law has actually reduced selective disclosure.[4] Since my tests explore a specific possible channel of selective disclosure, they are relevant to this debate.

My paper also contributes to the growing literature on the importance of geography in economics. Coval and Moskowitz (1999), Zhu (2002), and Huberman (2001) report strong preferences for geographically local equities among investors. Hong, Kubik, and Stein (2002) link geographic proximity and mutual fund managers as in Coval and Moskowitz (2001), but instead focus on the information that investors that are close together can pass on to one another; they document a word-of-mouth effect, whereby local mutual fund managers are more likely to hold a particular stock if other managers from different fund families in the same city are holding that same stock. Similarly, Grinblatt and Keloharju (2001) report that investors are more likely to buy, hold, and sell stocks of Finnish firms that are located close to the investor, while Portes and Rey (2002) find a strong geographic component in cross-border equity flows. Petersen and Rajan (2002) and Berger et al. (2002) also explore the concept of physical distance, but do so in the context of commercial banks’ lending to small companies.

And yet, only a few papers explore geography in the context of equity analysis. In addition, all of the existing studies focus on cross-border effects, rather than within-country effects. The international evidence appears to be mixed. For example, Chang (2002) explores the Taiwanese market and finds that expatriate analysts located outside the country (but whose firms have a local research group) outperform otherwise similar foreign analysts, but also outperform local analysts. Similarly, Bacmann and Bolliger (2001) find that foreign financial analysts outperform home-country analysts in Latin American emerging markets. On the other hand, Bolliger (2001) reports an accuracy advantage for home-country analysts at small and medium-size brokerage houses in Europe, and Orpurt (2002) finds that home-country analysts covering German-headquartered firms forecast earnings better in the short-run than foreign analysts. However, none of these papers tests a pure distance effect, since borders introduce a host of other issues. As such, by focusing solely on U.S. equity analysts covering domestic firms, this paper is the first to isolate the effect of physical distance on the accuracy and investment value of analysts' forecasts and recommendations.

B. Data

My sample merges several data sets, the most important of which is the I/B/E/S analyst forecast data. I use the I/B/E/S U.S. Detail History and the I/B/E/S Recommendation History data sets for this paper. The Detail History file contains individual analysts' earnings forecasts of U.S. companies between 1983 and 2002, while the Recommendations History file contains analysts' investment recommendations (translated by I/B/E/S into a common 1-5, 1="Strong Buy," 5="Strong Sell" scale) issued from 1993 to 2002.[5]

In particular, since I match each analyst with his firm, and then match each firm to its geographic location, I also use the Broker Translation File. Using this file, I match the analyst codes from the Detail File to the analyst's name and brokerage firm.[6] I then match the analyst's name and brokerage firm name (by hand) with entries from *Nelson's Directory of Investment Research*. This volume is available annually from 1994 to 2002, and contains each analyst's name, her phone number, which office she works in, and the address (city, state, and zip code) for each of the firm's offices (including all branch offices). Therefore I can distinguish between analysts who are located at the brokerage firm's headquarters and those who are not. Each volume of *Nelson's Directory of Investment Research* is published in December of year t using data as of November of year t. I classify an analyst's location starting in November of year t and lasting until October of year $t+1$ according to the information in *Nelson's Directory*.[7] I then find latitude and longitude data for each analyst's location (measured at the city center) using the U.S. Census Bureau's Gazetteer city-state (places.zip) file.

I obtain the location of each firm's headquarters each year from the Compact Disclosure database, and find the latitude and longitude data for each firm's location using the U.S. Census

Bureau's Gazetteer city-state file as before. Using the latitude and longitude data, I then compute the distance between each analyst and each firm that she covers.

In order to control for brokerage house affiliations, I use data from the SDC New Issues Database. Using SDC, I obtain information on initial public offerings (IPOs) and secondary equity offerings (SEOs) conducted during my sample period, including the date of the offering and the name of the lead underwriter. I merge information on these IPOs and SEOs with my analyst sample, so that I can categorize for each stock that an analyst covers whether her brokerage house has an underwriting relationship with that stock. More specifically, if an analyst issues an earnings forecast on a stock within a specified period (see below) after its IPO/SEO date and in which her brokerage house is the lead underwriter for the IPO/SEO, then I define that stock as having an underwriting relationship with the analyst's brokerage house.

Stock returns are drawn from the Center for Research in Securities Prices (CRSP) Daily Stocks Combined File, which includes NYSE, AMEX, and Nasdaq stocks. For some of the tests in the paper, firms must also have data in COMPUSTAT on book equity for the fiscal year ending in calendar year $t-1$. Since I only have data from the *Nelson's Directory of Investment Research* starting in November 1994, my main sample period extends from November 1994 to December 2001 in order to avoid a survivor bias that might be caused by applying locations backward to 1983 (the first year for which the I/B/E/S Detail data is available).

I also collect data on individual analysts' reputations by using the list of All-Americans drawn from the October issue of *Institutional Investor* magazine for 1994 to 2001. I classify an analyst as an All-American starting in November of year t and lasting until October of year $t+1$ if she appears in the list of All-Americans in October of year t. Although I collect data for First, Second, and Third Team All-Americans, as well as Honorable Mention All-Americans, the tests in this paper use the term All-American to refer to an analyst listed in any of these four categories. As in Hong and Kubik (2003), my primary measure of the brokerage house hierarchy is derived from a brokerage house ranking published by *Institutional Investor* magazine. Each year in the October issue, the ten or so brokerage houses with the most All-Americans are listed as "The Leaders." I classify the top ten houses in this annual poll as high-status starting in November of year t and lasting until October of year $t+1$ if the house appears in the list of "The Leaders" in October of year t; all other brokerage houses not on the list are classified as low-status using the same timing convention.

Table I provides descriptive statistics of my final sample, which is formed by merging these various datasets. Panel A presents the summary statistics for the one-year earnings forecasts sample, broken down by forecast type and firm type; Panel B presents the same statistics for the recommendations sample, broken down by recommendation type and firm type. The entire one-year earnings forecasts sample includes 4,344 firms located in all 48 states of the continental U.S. (firms and analysts located in Alaska and Hawaii are excluded from the sample so as not to exaggerate the effect of physical distance), plus the District of Columbia (classified as a state in Table I). The sample also includes 4,254 analysts located in 34 different states. While a large fraction of forecasts are issued by analysts located within 100 kilometers of New York City (Pct. NYC=56.47), my results are not affected when these observations are removed from the sample (see Table VI). Panel B shows a similar distribution of coverage for the stock recommendations sample.

Insert Table I about here

Similar to Coval and Moskowitz (2001), I place firms, forecasts, and recommendations into three categories: Metro - defined as being located in any of the 20 most populated cities; Non-metro - defined as not being located in any of the 20 most populated cities; and Remote - defined as being at least 400 kilometers away from any of the 20 most populated cities. The 20 most populated cities are defined by the U.S. Census Bureau at the beginning of each year. Not surprisingly, analysts in remote and non-metro areas tend to be associated with smaller brokerage firms (14.88 analysts on average for remote analysts, compared to 42.58 analysts on average for metro analysts). Similarly, remote firms tend to be smaller, with less analyst coverage. The recommendations sample shows similar patterns, although the analysts tend to work at slightly larger firms, and firms tend to have fewer outstanding recommendations.

A subtle shift also occurs between the two subperiods, as the fraction of forecasts issued by analysts located within 100 kilometers of New York City drops from 59.08% to 54.55%. This shift is not present in the recommendations sample, however. In addition, untabulated statistics indicate that in the early period (from November 1994 to December 1997), the share of forecasts issued by analysts within 100 kilometers of San Francisco was 6.44% of the total sample; by contrast, in the period from January 1998 to December 2001 this percentage jumped to 9.25%. A similar shift (6.51% of recommendations in the early period up to 9.41% of recommendations in the later period) shows up in the recommendations sample.

II. Relative Forecast Accuracy

A. Regression Methodology

I run cross-sectional regressions of relative forecast accuracy on a host of analyst characteristics in order to test the notion that geographically proximate analysts possess superior information. My framework is analogous to the one developed in Clement (1999), in that I control for firm-year variation in both the dependent and independent variables. Clement (1998) finds that controlling for firm-year effects increases the likelihood of identifying systematic differences in analysts' forecast accuracy, relative to a model that controls for firm fixed effects and year fixed effects. Firm-year effects result from factors that make a particular firm's earnings easier (or harder) to predict in some years than others. Examples of events that may give rise to firm-year effects are voluntary management disclosures, mergers, strikes, etc.

I measure performance by comparing an analyst's absolute forecast error to the average absolute forecast error of other analysts following the same stock during the same time period. Specifically, I calculate the de-meaned absolute forecast error ($DAFE_{i,j,t}$), which is equal to the absolute forecast error for analyst i's forecast of firm j for fiscal year t ($AFE_{i,j,t}$) minus the mean absolute forecast error for firm j for fiscal year t ($\overline{AFE_{j,t}}$). Absolute forecast error is equal to the absolute value of an analyst's latest forecast, minus actual company earnings (drawn from the I/B/E/S Actuals File), as a percentage of stock price 12 months prior to the beginning of the fiscal year. Negative values of $DAFE$ represent better than average performance, while positive values of $DAFE$ represent worse than average performance. Following Clement (1999), I also employ an alternate measure of performance, $PMAFE$ (proportional mean absolute forecast error), which is equal to $DAFE$ divided by $\overline{AFE}$; Clement (1998) shows that deflating $DAFE$ by $\overline{AFE}$ reduces heteroskedasticity.

I regress these performance measures (*DAFE* or *PMAFE*) on a variety of analyst characteristics. The model's independent variables are also adjusted by their related firm-year means to properly control for firm-year effects; the approach here is equivalent to estimating a model using firm-year dummy variables to control for firm-year effects. The specific characteristic of interest for the purposes of this paper is (the log of one plus) the physical distance (*LOGD*) between analyst i and the headquarters of each firm j she covers.[8] In addition to distance, I employ other measures of geographic proximity. For example, I compute a dummy variable (*LOCAL*) equal to 1 if the analyst is located within 100 kilometers of firm j, and equal to zero otherwise. This distance threshold is arbitrary; however, I have experimented with various distance thresholds, from 50 kilometers up to 200 kilometers, and the results are similar to those presented here. In addition, my conclusions are unchanged if I replace the *LOCAL* variable with a dummy variable equal to 1 if the analyst is located in the same state as firm j.

I control for several factors that previous research has identified as contributing to differences in relative forecast accuracy among analysts. Perhaps the most important of these factors is the age of the forecast. Clement (1999) reports that relative absolute forecast errors increase at the rate of 0.35% per day, and stresses the need for careful controls for age when comparing forecasts. As such, I include (the log of one plus) the number of days (*AGE*) between analyst i's forecast for firm j and the firm's announcement of actual earnings. Following Clement (1999), I also include controls for experience and available resources. I measure firm-specific experience (*FEXP*) as (the log of one plus) the number of days that analyst i has supplied a forecast for firm j.[9] I measure available resources by calculating the size of analyst i's brokerage firm (*BSIZE*), computed as (the log of one plus) the number of analysts working for the same firm as i and supplying forecasts during the same fiscal year t as i. I control for analyst and brokerage firm reputation by computing dummy variables that are equal to 1 if analyst i is an All-Star (*STAR*) or works for a high-status brokerage firm (*HIGH*).[10] I also control for the possibility that some analysts are systematically more optimistic than other analysts by computing the percent of companies (*PCTOP*) followed by analyst i at time t for which analyst i's last forecast is above actual earnings.

Although Lin and McNichols (1998) report that one-year earnings forecasts are unaffected by underwriting affiliations, I include a variable (*AFFIL*) designed to capture such an effect. Specifically, if an analyst issues an earnings forecast on a stock within five years after its IPO date or within two years after an SEO date for which her brokerage house is the lead underwriter for the IPO or SEO, then I define that stock as having an underwriting relationship with the analyst's brokerage house. Analysts may deliver upwardly biased estimates for stocks for which their brokerage house serves as the lead underwriter on a stock offering, since analysts' pay is sometimes tied to investment banking revenues. In this case, the variable $AFFIL$ would capture the effect of this agency problem.

Therefore, the final model takes the form:

$$\begin{aligned} DAFE_{i,j,t} \text{ (or } PMAFE_{i,j,t}) \quad = \quad & \beta_1 DAGE_{i,j,t} + \beta_2 DFEXP_{i,j,t} + \beta_3 DSTAR_{i,j,t} + \beta_4 DHIGH_{i,j,t} \\ & + \beta_5 DBSIZE_{i,j,t} + \beta_6 DPCTOP_{i,j,t} + \beta_7 DAFFIL_{i,j,t} \\ & + (\beta_8 DLOCAL_{i,j,t} \text{ or } \beta_8 DLOGD_{i,j,t}) + \varepsilon_{i,j,t}, \end{aligned} \tag{1}$$

where all variables are firm-year mean adjusted (the D preceding each variable stands for differenced or de-meaned). Note that while my test is stated in terms of forecast accuracy, the regression above analyzes analysts' forecast errors. Small forecast errors indicate a high level of accuracy. In addition, the regression equation does not require an intercept, since means are subtracted from all variables.

B. Full-Sample Regression Results

I estimate Fama and MacBeth (1973) cross-sectional regressions yearly and monthly, and report the time-series average of the estimated coefficients in Table II. My conclusions are unchanged if I estimate pooled regressions instead. I restrict the sample to only those firms with at least three forecasts and two unique analysts in a fiscal year, and I winsorize the extreme 2% of observations for the $DAFE$, $PMAFE$, and OPT variables.[11] Once again, neither of these restrictions significantly affects my results.

The estimated coefficients in Table II indicate that a local presence is negatively related to relative forecast error. Panel A shows that the average yearly coefficient on $DLOCAL$ is negative and significant in both the $DAFE$ and $PMAFE$ specifications (t=-2.13 and t=-2.28). Meanwhile, the coefficients on $DLOGD$ are positive for both specifications and significant for the $PMAFE$

specification (t=2.72). Geographically proximate analysts by either distance metric are thus significantly more accurate (i.e., have lower forecast errors) in their one-year earnings forecasts than their distant counterparts. This result supports the notion that geographic proximity facilitates information flow, and that distance is a plausible proxy for asymmetric information between analysts. The magnitude of the accuracy effect is small, however; multiplying the coefficient on *LOCAL* (=-2.77%) in the *PMAFE* specification by the mean absolute forecast error for the sample (=$0.890) reveals a $.025 per share accuracy advantage on average for local analysts.

Insert Table II about here

This main result is robust to a variety of permutations. For brevity, I only report a few such checks.[12] For example, Panel A also reports estimates for a more parsimonious model of *PMAFE*, with only *DAGE* and *DBSIZE* as control variables; the coefficients on both *DLOCAL* and *DLOG* are still significant. Univariate regressions and a variety of other untabulated specifications produce identical inferences. Panel B reports statistics for monthly regressions, which also produce similar results (t=-2.31 for *DLOCAL*, t=2.33 for *DLOGD*) in the baseline *PMAFE* specification. Finally, when I run the accuracy tests on two-year ahead earnings per share forecasts instead of one-year forecasts, the coefficient estimate on *DLOCAL* is again negative and significant, and the coefficient estimate on *DLOGD* is again positive and significant.

Since my results are similar for the two distance variables, I only report results for (and limit my discussion to) the *LOCAL* variable for the remainder of the paper. My conclusions are unchanged if I use the *LOGD* variable instead, or if I make small adjustments to the distance threshold used in calculating *LOCAL*.

Other results in Table II are worth noting. For example, the coefficient on *AFFIL* is negative and significant in the accuracy regressions, indicating that affiliated analysts are more accurate in their one-year earnings forecasts than other analysts. The magnitude of this effect is larger than the proximity effect discussed above. However, untabulated statistics indicate that this result is sensitive to the way *AFFIL* is defined. For example, if I change the year cutoff, or define affiliation only by IPO affiliation, this result is no longer significant. And yet, the coefficient is always negative, calling into question the typical assumption that affiliated analysts produce inferior forecasts.

Not surprisingly, the estimated coefficient on *DPCTOP* is always strongly positive, indicating that analysts who are systematically more optimistic than other analysts tend to be less accurate. I also find that analysts who work at high-status (*HIGH*) firms are more accurate than other analysts; note, however, that this coefficient is only significant in the *PMAFE* specification. All of the other control variables enter in the expected directions. For example, the coefficient on the age of the forecast is positive and strongly significant in all specifications. In addition, consistent with evidence presented in Clement (1999), analysts with more firm-specific experience and more available resources are more accurate than other analysts (i.e., the coefficients on *DFEXP* and *DBSIZE* are strongly negative in all specifications). As in Stickel (1992), I find that All-Star analysts are more accurate than other analysts, although this finding is fairly weak in the *DAFE* regressions.

C. Bias versus Informativeness

One problem with interpreting superior local accuracy as indicative of superior local information is that the aforementioned accuracy tests do not distinguish bias from informativeness. For example, local analysts may be more accurate simply because they are less optimistic, rather than better informed.

I investigate this possibility by re-running the baseline yearly regressions depicted in Table II after replacing the *PCTOP* variable with an explicit control for the analyst's relative optimism about the stock in question, rather than his overall tendency towards optimism. Specifically, in the *DAFE* specification, *OPT* is equal to the forecast error for analyst i's forecast of firm j for fiscal year t ($FE_{i,j,t}$) minus the mean forecast error for firm j for fiscal year t ($\overline{FE_{j,t}}$), where forecast error is equal to the analyst's latest forecast, minus actual company earnings (drawn from the I/B/E/S Actuals File), as a percentage of stock price 12 months prior to the beginning of the fiscal year. The variable *POPT* is simply *OPT* scaled by the absolute value of ($\overline{FE_{j,t}}$). Both of these variables thus capture the direction of the forecast error.

Panel A of Table III shows that even after explicitly controlling for the relative optimism of an analyst about a given stock, local analysts are still more accurate than other analysts. Specifically, the coefficient on *DLOCAL* is negative in both regressions, and significant (t=-2.17) in the

PMAFE specification. Further, when I replace the dependent variables *PMAFE* and *DAFE* with *POPT* and *OPT*, the coefficient on *DLOCAL* is no longer significant in either regression.

Insert Table III about here

Panel B reports additional evidence on this issue. Specifically, I run monthly Fama and MacBeth (1973) cross-sectional (and pooled) regressions of actual earnings on forecasted earnings separately for local analysts and nonlocal analysts. Panel B reveals strongly negative estimated intercepts in these regressions. This is the classic analyst optimism bias reported frequently in the literature (see, for example, DeBondt and Thaler (1990) and Abarbanell and Bernard (1992)). Importantly, however, the estimated intercepts for the two samples (locals and nonlocals) are not significantly different; locals are not significantly less (or more) optimistic than nonlocals. On the other hand, the coefficient estimates (β) *are* significantly different for the two groups, as are the R-squared values. In particular, the (β) estimate is closer to one and the R-squared value is larger for local forecasts. Since the coefficient and the R-squared value in this type of regression tell us about the informativeness of the forecast, these results indicate that local forecasts are more informative than nonlocal forecasts, but not necessarily less optimistic.

D. Sorting by Firm Characteristics

In this section I try to isolate areas and firm types for which the effect of geographic proximity is particularly important. Table IV reports the estimated coefficients for the *DLOCAL* variable only, for subsets of the Table II yearly regressions of *DAFE* on the eight analyst characteristics. I break down my original sample of 96,538 observations into a variety of subsamples. Each column presents a subsample broken down by firm characteristics. For example, my first sort is by size; each month I categorize stocks based on their one-month lagged market capitalization. *SMALL* refers to stocks below the median level of market capitalization, and *LARGE* refers to stocks above the median market capitalization.[13] I also sort stocks each month based on their lagged book-to-market ratio. Book equity (*BE*) is defined as the COMPUSTAT book value of stockholders' equity, plus balance sheet deferred taxes and investment tax credit (if available), minus the book value of preferred stock. Depending on availability, I use redemption, liquidation, or par value (in that order) to estimate the book value of preferred stock. I match the yearly book equity figure for all fiscal years ending in calendar year $t-1$ with returns starting in July of year t. This figure is then divided by market capitalization (ME) at month $t-1$ to form the BE/ME ratio, so that the BE/ME ratio is updated each month. $GROWTH$ stocks are those stocks below the median BE/ME, while $VALUE$ stocks are those above the median BE/ME. Finally, I sort stocks into three groups based on past returns from $t-12$ to $t-2$ (as in Fama and French (1996)). I classify $LOSERS$ as stocks below the median past return, and $WINNERS$ as stocks above the median past return.

Insert Table IV about here

Clear patterns emerge from these sorts. As seen in the first row of Table IV, the effect of geographic proximity is concentrated in small stocks, value stocks, and stocks with poor past returns. Local analysts covering these types of stocks are significantly more accurate than other analysts. Further sorting firms into Metro stocks, Non-metro stocks, and Remote stocks, as described in Section I, Table IV shows that local analysts are more accurate when covering stocks located in remote cities or smaller cities. Again, these effects are concentrated in small stocks and in value stocks.[14] The magnitude of the accuracy advantage is significantly larger for local analysts covering stocks in remote areas ($0.054=-0.0629*0.860), particularly small stocks in remote areas ($0.141=-0.0803*1.761).

Breaking the sample into two subperiods reveals that the effect of geographic proximity is stronger in recent years. While the Internet and improvements in information technology may have brought people closer together, they have not wiped out the advantage that local analysts gain by being closer to the firms they cover. Interestingly, in the post-Reg FD sample, the estimate for *DLOCAL* is no longer significant, but the direction and magnitude is similar to the overall estimate; thus the enactment of this law has not dramatically decreased the accuracy advantage that local analysts exhibit. Finally, the magnitude of the *DLOCAL* coefficient is similar when comparing a sample of forecasts issued within five days of a quarterly earnings announcement date to a sample of all other forecasts, although only the non-earnings-related sample result is significant.

E. Sorting by Analyst Characteristics

Table V provides another breakdown of the baseline *DLOCAL* result from Table II, this time focusing on analyst characteristics as opposed to firm characteristics. Each column in Table V represents a different analyst type, such as non-New York City analysts (*XNYC*), All-Star analysts (*STAR*), non-All-Stars (*NON*), analysts working at high-status firms (*HIGH*), and analysts not working at high-status firms (*LOW*). Meanwhile, the rows in Panel A group observations by the nature of the forecast, and the rows in Panel B classify analysts by the composition of their portfolio.

Insert Table V about here

Following Gleason and Lee (2003), Panel A categorizes forecast revisions according to their signal attributes (*SIGNAL*). If a revision is above the analyst's prior forecast *and* above the prior consensus forecast, I classify this forecast as signaling unambiguously good news (*SIGNAL*=1). By contrast, if a revision is below the analyst's prior forecast *and* below the prior consensus forecast, I classify this forecast as signaling unambiguously bad news (*SIGNAL*=-1). I place all other forecasts into a third category (*SIGNAL*=0).

Panel A shows that even after removing all New York analysts, the local accuracy advantage persists (*DLOCAL*=-0.0250, *t*=-2.76).[15] Meanwhile, the magnitudes of the *DLOCAL* coefficient suggest that the local effect is strongest among All-Star analysts and high-status analysts, although the coefficient is only significant for low-status analysts (*t*=-2.07). Finally, local analysts appear to be considerably more accurate when making unambiguous downward revisions. This last result is strongly confirmed in the subsamples of non-New York analysts, non-All-Stars, and low-status analysts, and present (but insignificant) in the samples of All-Stars and high-status analysts.

To form the coverage terciles depicted in Panel B, I simply compute the number of stocks that each analyst covers in a given year: Low-coverage analysts are those who fall below the 33rd percentile of number of stocks covered by an analyst in a given year, while High-coverage analysts are those above the 66th percentile. I further categorize analysts by the share of local stocks in their portfolio; specifically, *LOCMED* is a dummy variable equal to one if the share of local stocks in an analyst's portfolio is above the median share of local stocks over all analysts' portfolios. Panel B shows that high coverage analysts exhibit the strongest local advantage, particularly those whose portfolios are tilted towards local stocks (*DLOCAL*=-0.0512, t=-2.67). This result suggests that the local accuracy advantage may have more to do with private information (e.g., absorbing the local culture or running in the same social circles as local business leaders), and less to do with effort (i.e., the reduced cost of information gathering for local analysts).

F. Endogenous Coverage Decisions

Looking at coverage in the preceding way masks the fact that analysts are not randomly assigned to firms and how intensively to follow them. Analysts make their coverage decisions endogenously, and these coverage decisions could lead naturally to a link between performance and proximity in the absence of asymmetric information costs. I explore three versions of this hypothesis in this section.

One possibility is that local analysts, particularly those in remote areas, simply cover fewer firms, and hence benefit from the increased concentration of their attention relative to metropolitan analysts. Panel A of Table VI shows that local analysts and remote analysts do indeed cover fewer firms on average (11.87 and 11.10 firms per year, respectively, compared to 12.37 for the entire population of analysts). However, Panel B of Table VI shows that when I include a variable equal to the number of firms covered by each analyst (*DTASK*) in the original regression specification from Table II, local analysts still exhibit an accuracy advantage (t=-1.92 for the entire sample, and t=-2.51 for remote analysts). Further, as an information story would suggest, remote analysts actually cover more local firms (2.22 per year) than the average analyst (2.01). Due to the fixed costs of relocating and establishing local ties, it is likely to be too costly for analysts in large cities to simply relocate to remote areas.

Another possibility is that local analysts may be more specialized (e.g., by industry) than other analysts. Specialists might be more accurate than broader analysts, even if the cost of information is the same across locations, producing a relation between distance and accuracy. I explore this issue by examining the performance of Expert analysts, which I define as analysts covering firms in only one 2-digit SIC code in a given year.[16] Panel A of Table VI shows that expert analysts are actually located further from the firms they cover on average and tend to cover fewer local stocks (1.67 per year) than the average analyst (2.01). Both remote analysts and local analysts tend to be less specialized (31.63% and 32.51% are experts, respectively, compared to 32.66% for the entire sample), suggesting that specialization is not a major determinant of local analyst performance. As shown in Panel B of Table VI, including a dummy variable equal to one if the analyst is a specialist (*DSPEC*) in the regression from Table II does not diminish the predictive power of proximity.

Insert Table VI about here

A final version of the endogenous coverage hypothesis builds on the first story, but argues that what may drive the local analyst advantage is not the total number of firms covered, but rather the intensity with which certain individual firms are covered. Since fund managers and individual investors are biased towards holding local stocks (see Coval and Moskowitz (1999) and Zhu (2002)), the customers of local analysts may be driving the proximity patterns documented above. Local analysts may simply spend more time analyzing local stocks, since these are the stocks that their local clients demand. Testing this idea requires a measure that is correlated with local investor demand, but which does not necessarily provide information to analysts. One such measure is advertising intensity, since Zhu shows that individual investors tend to invest more in stocks that advertise heavily, and individuals may be more likely to gain exposure to the advertising of local companies (Nelson (1974)). Breaking the sample into firms with high and low advertising intensity (defined as advertising expense divided by sales), Panel B of Table VI shows that local analysts do perform much better when covering high advertising firms than when covering low advertising firms (*DLOCAL*=-0.0510 with *t*=-3.25 compared to *DLOCAL*=0.0377, *t*=1.63)), although this result is much weaker for remote firms. Conditioning on advertising intensity greatly reduces the sample size, however, as roughly 80% of the forecasts in the sample come from firms with zero advertising expense. Among firms with zero advertising expense, the local accuracy advantage persists. Nevertheless, I cannot completely rule out the possibility that visibility may play a role in the performance of local analysts; a full exploration of this issue would require merging individual or institutional investor holdings data with analyst forecast data, a task beyond the scope of this paper.

III. Stock Price Impact of Forecast Revisions

A. Test Design

Another way to test if local analysts bring new and valuable information to the market is by analyzing the stock price impact of forecast revisions. Since weak analysts can simply mimic the earnings forecasts of timely skilled analysts in order to improve the accuracy of their forecasts, relative forecast accuracy is not a sufficient statistic for testing if geographic proximity facilitates information flow.[17] Specifically, I test the hypothesis that forecast revisions made by local analysts have a greater impact on stock prices than revisions made by distant analysts.[18] Such a finding would provide strong evidence that local analysts have an information advantage over other analysts. This type of test also helps gauge the *economic* significance of my prior results.

I test the hypothesis that forecast revisions made by local analysts have a greater impact on stock prices by running Fama and MacBeth (1973) cross-sectional regressions of three-day average excess returns ($AAR_{i,-1,1}$) on the magnitude of the revision (see Beaver, Clarke, and Wright (1979)), firm size (see Atiase (1985)) and dummy variables indicating affiliation status and local status. The following regression is used:

$$AAR_{j,-1,1} = \beta_0 + \beta_1 LNME_{j,-30} + \beta_2 SUF_{i,j,0} + \beta_3 AFFIL_i + \beta_4 LOCAL_i + \varepsilon, \tag{2}$$

where $AAR_{j,-1,1}$ is the average size-adjusted announcement return for firm j, $LNME_{j,-30}$ is the (log of) market capitalization of the stock one month prior to the revision, $SUF_{i,j,0}$ is the standardized unexpected forecast made by analyst i for firm j, $AFFIL_i$ is a dummy variable equal to one if the analyst is an affiliated analyst as defined in Table II (and zero otherwise), and $LOCAL_i$ is a dummy variable equal to one if the analyst is a local analyst (and zero otherwise). The standardized unexpected forecast equals analyst i's forecast for firm j on day 0 minus analyst i's prior forecast for firm j (in fiscal year t), scaled by the cross-sectional standard deviation of all prior outstanding forecasts for firm j.[19]

I break the regressions down into two types of revisions, one group signaling good news and the other signaling bad news. As in Panel A of Table V, I categorize revisions according to their signal attributes (*SIGNAL*). However, for these tests I focus only on those forecasts signaling unambiguously good news (*SIGNAL*=1) or unambiguously bad news (*SIGNAL*=-1), and exclude all forecasts with conflicting signals (*SIGNAL*=0).[20] I also restrict these regressions to various subsamples according to the location of the firm being covered. As before, I group stocks located in large cities and small cities separately to see if local analysts have a greater impact on stock prices for particular types of firms. I also divide the sample up into different analyst types (as in Table V) and different subperiods (as in Table IV).

An advantage of equation (2) is that the estimated coefficient for the dummy variable represents the marginal abnormal return associated with unambiguous forecast revisions by local analysts. I run these regressions monthly, with revisions placed in the calendar month in which day 0 falls, and compute the time-series average of the 85 monthly estimated coefficients.

I also analyze the relation between forecast revisions and excess stock returns during the post-release period. For this test, I substitute the post-release average excess return from day 2 to day 64 ($AAR_{j,2,64}$) in place of the three-day average excess return in equation (2). This test provides direct evidence concerning the speed with which investors react to the information contained in analysts' forecast revisions.

B. Results

Table VII reports the time-series average of the estimated coefficients for Fama and MacBeth (1973) cross-sectional regressions of equation (2). Panel A reports the regression results for equation (2) and shows that both strong positive *and* strong negative revisions by local analysts affect stock prices more than revisions by other analysts. For the entire sample of stocks, covering 86,127 revisions, local revisions are associated with an incremental average excess return of 0.095% per day in the three days surrounding strong positive revisions, which is significant at the 5% level. This effect is slightly larger for stocks located in small cities (0.102% per day). For large negative revisions, the effects are even stronger (-0.128% and t=-2.46 for all stocks, -0.164% and t=-2.54 for non-metro stocks). The fact that the market's response to local revisions is stronger on the downside is consistent with the finding in Table V that the local accuracy advantage is strongest for large downward revisions. Overall, the finding that both positive and negative revisions by local analysts are met with significant price responses provides support for the notion that geographically proximate analysts are better informed than other analysts.

Insert Table VII about here

Panel C shows that the price responses associated with local revisions more than doubled in magnitude from the early period to the more recent period (0.057% up to 0.126% for positive revisions, and -0.058% to -0.180% for negative revisions). Again this finding is consistent with the result reported in Table IV that the local accuracy advantage has increased in recent years. The post-Reg FD results are even more dramatic, as negative revisions by locals in the 13 months after the enactment of the law were met with an incremental average excess return of -0.421% per day in the three-day announcement period.

The results in Panel D indicate that strong positive revisions by local non-New York analysts, local All-Star analysts, and local high-status analysts are met with significant incremental price responses, but strong negative revisions are not. This is the first piece of evidence that conflicts with the results reported in Table V, since the local accuracy advantage for all three of these analyst types was found to be strongest for downward revisions; note, however, that these particular accuracy results were insignificant for the All-Star and high-status samples. The large negative price response associated with local low-status analysts is again consistent with the finding in Table V that these particular analysts are more accurate on the downside. Finally, the incremental price responses for locals covering high advertising firms are insignificant (albeit for a much smaller sample), calling into question the robustness of the earlier finding that the local accuracy advantage is particularly strong among firms of this type.

The affiliation results in Table VII are striking. Panel A indicates that the market's response to unambiguously negative revisions by affiliated analysts is quite large. Consistent with an agency story, affiliated analysts are associated with an incremental average excess return of -0.555% per day (t=-3.67) in the three days surrounding strong negative revisions, but only 0.074% per day (t=0.68) around strong positive revisions. Since affiliated analysts may have an incentive to release positive news and a disincentive to issue negative forecasts, the market views positive revisions by affiliated analysts as uninformative and negative revisions as extremely informative.

Not surprisingly, these affiliation patterns are even stronger in recent years. For example, as shown in Panel C, strong positive revisions by affiliated analysts in the more recent subperiod were met with an incremental average excess return of -0.190% per day (albeit insignificant), indicating that the market was completely discounting good news by affiliated analysts during this period. However, the market still viewed strong negative revisions by affiliated analysts as informative during this period, as these were met with an incremental average excess return of -0.665% per day.

Panel D reports that this asymmetry in the market's response to affiliated analysts' revisions is *not* found among non-New York analysts, non-All-Stars, and low-status analysts. Since these are the three groups where one might expect agency problems to be less severe, this result is not surprising.

The regression results in Panel B indicate that the price responses documented above are short-term in nature. Although the signs are the same as in Panel A, there is no significant differential relation between forecast revisions and post-release excess returns for local analysts versus nonlocal analysts, or for affiliated analysts versus unaffiliated analysts.

In summary, both positive and negative revisions by local analysts have a greater stock price impact than revisions by nonlocals, and this difference is strongest for non-metro stocks, and in the more recent subperiod. This additional impact continues on for the next three months, but the long-run differential is not significant. By contrast, only negative revisions by affiliated analysts have a greater stock price impact than revisions by unaffiliated analysts. Consistent with an agency story, this asymmetry is strongest in metro stocks, for All-Star analysts, and for high-status analysts.

IV. Analysts' Stock Recommendations

In this section I analyze the effect of distance on analysts' stock recommendations. Rather than simply replicating the same (or similar) tests as those in Sections II to III on the recommendation data, I use geography to shed light on a specific phenomenon that has received considerable attention in recent years with respect to stock recommendations, namely the underwriter affiliation bias. Much of the empirical evidence documenting an affiliation bias in analysts' research has focused solely on stock recommendations, not on earnings forecasts. Indeed, my accuracy tests found no evidence of inferior performance by affiliated analysts in their one-year earnings forecasts.[21]

A. Stock Price Impact of Analyst Recommendations: Local versus Affiliated Analysts

My first test replicates a commonly cited example of the affiliation bias in stock recommendations, and re-examines this result by conditioning on the location of the analyst. As in Lin and McNichols (1998), I examine the market's reaction to affiliated and unaffiliated analysts' stock recommendations.

I analyze the stock price impact of affiliated analysts versus unaffiliated analysts by running Fama and MacBeth (1973) cross-sectional regressions each month of three-day average excess returns on a series of dummy variables. I estimate the relation between returns and recommendations as follows:

$$AAR_{j,-1,1} = \beta_1 SB_{j,A} + \beta_2 B_{j,A} + \beta_3 H_{j,A} + \beta_4 SB_j + \beta_5 B_j + \beta_6 H_j + \beta_7 S_j + \varepsilon, \tag{3}$$

where $AAR_{j,-1,1}$ is the average size-adjusted announcement return for firm j, SB is a dummy variable equal to one if the recommendation is a strong buy and equal to zero otherwise, B is dummy variable equal to one if the recommendation is a buy and equal to zero otherwise, H is a dummy variable equal to one if the recommendation is a hold and equal to zero otherwise, and S is a dummy variable equal to one if the recommendation is a sell or strong sell and equal to zero otherwise. The variables subscripted by A equal one if the respective recommendation is issued by an affiliated analyst, and equal zero otherwise. I define an affiliated analyst as an analyst who issues a recommendation on a stock in the two years after the issuance of an secondary equity offering (SEO) of that stock, where the lead underwriter for that SEO is also the brokerage firm that employs the analyst. As in Lin and McNichols (1998), I estimate the model for the sample of all recommendations issued in the two years after the SEO. Because there are very few affiliated sell recommendations, these are grouped together with affiliated hold recommendations.

Dropping this restriction changes no conclusions. The coefficients β_1, β_2, and β_3 thus capture the mean incremental returns associated with affiliated analysts' recommendations relative to the same recommendations by unaffiliated analysts, and the *t*-statistics for these coefficients indicate the significance of the difference in the mean market reactions for affiliated and unaffiliated analysts.

Panel A of Table VIII presents my results, which are very similar to those originally reported in Lin and McNichols (1998). The estimation results in the first row of Panel A indicate a significant positive response to unaffiliated strong buy recommendations (t=16.11), and a significant negative response to unaffiliated hold (t=-14.23) and sell recommendations (t=-7.37). This evidence indicates that investors view analysts' recommendations as informative. For example, investors seem to interpret hold recommendations as negative rather neutral information about a stock.

Insert Table VIII about here

The coefficient on β_1 is not significant, suggesting that investors do not view affiliated strong buy recommendations as less informative than unaffiliated strong buy recommendations. By contrast, the coefficient on β_2 is negative and significant, although the significance of this particular result is not robust to the cutoff period used (here I include all recommendations issued in the two years after the SEO). The coefficient β_3, however, is strongly significant in all of my specifications (t=3.90), and is large in magnitude (-1.76% per day). This is the central result in Lin and McNichols (1998), and it appears in my sample as well, namely that the market interprets an affiliated hold to mean sell to a greater degree than an unaffiliated hold. This result suggests that affiliated analysts are more likely to issue a hold recommendation when a sell recommendation is warranted.

I then explore the effect of geographic proximity in this context. Panel B of Table VIII reports that affiliated analysts that are also local are *not* associated with a significant negative incremental price response (-0.25% per day, t=-0.78) when they issue hold recommendations. Thus the market does not view an affiliated local hold recommendation as more informative than an unaffiliated hold recommendation. However, the reaction to strong buy recommendations is now positive and significant (0.32% per day, t=1.96), suggesting that local affiliated analysts do have an information advantage over other analysts when issuing these particular recommendations.

Panel C shows that distant affiliated hold recommendations are met with an incremental price reaction of -1.74% per day on average over the three-day announcement period. Thus the agency cost estimates implied by Panel A are only borne out for distant affiliated analysts. A possible explanation for this finding is explored in the next section.

I also examine the relation between pre-release excess returns and stock recommendations by substituting the pre-release average excess return from day -22 to day -2 ($AAR_{j,-22,-2}$) in place of the three-day average excess return in equation (5). The second row of Panel A reports a result similar in spirit to the "booster shot" result first reported in Michaely and Womack (1999). Using a sample of IPOs from 1990 to 1991, Michaely and Womack report that returns of firms with affiliated buy recommendations declined, on average, 1.6% in the 30 days prior to the buy recommendation, while firms receiving unaffiliated buy recommendations increased 4.1% over the same period. They conclude that affiliated analysts attempt to boost the stock prices of firms they have taken public at times when such a booster shot is really need, i.e., when the firm is performing poorly. Similarly, the second row of Panel A shows that in my sample of SEOs, affiliated buy recommendations are associated with an incremental average excess return of -0.11% per day (t=-1.88) in the pre-release period relative to buy recommendations by unaffiliated analysts.

As with the hold affiliated announcement effect, this booster shot effect is concentrated among distant affiliated analysts. The second row of Panel B reports an incremental average excess return of only 0.01% per day (t=0.07) in the pre-release period for local affiliated buy recommendations relative to buy recommendations by unaffiliated analysts. By contrast, Panel C shows that distant affiliated buy announcements are met with an incremental average excess return of -0.09% per day (t=-1.75) in the pre-release period.

B. Optimism in Analyst Recommendations: Local versus Affiliated Analysts

The evidence that local affiliates appear to issue more informative recommendations than distant affiliates in the strong buy category is consistent with an information advantage on the part of local affiliates. However, the finding that local affiliates' hold recommendations appear unbiased relative to their distant counterparts is puzzling, and motivates a more extensive analysis.

I examine the robustness of this last result by employing the matching procedure described in Lin and McNichols (1998) to compare the stock recommendations of affiliated analysts and unaffiliated analysts. I define an affiliated analyst as one who issues a recommendation on a stock in a designated time period around the issuance of a SEO of that stock, where the lead underwriter for that SEO is also the brokerage firm that employs the analyst.[22] I then find a recommendation for this same stock by an unaffiliated analyst within 60 days of the affiliated recommendation; only recommendations with an eligible match are included in the test. When there is more than one affiliated recommendation for an underwriter-offering observation, the recommendation made on the day closest to the offering date is included in the sample. Similarly, when there is more than one unaffiliated recommendation within 60 days of the respective affiliated recommendation, I choose the unaffiliated recommendation issued most closely to the date of the affiliated recommendation. This research design therefore controls for differences in the characteristics of firms that affiliated versus unaffiliated firms choose to cover.

Panel A of Table IX presents the mean differences between pairs of affiliated and unaffiliated recommendations. Using 247 matched pairs of recommendations, I find that the mean affiliated recommendation is 1.725 (1=strong buy, 5=strong sell), while the mean unaffiliated recommendation is 1.895; this difference (-0.170) is statistically significant at the 5% level, indicating that affiliated analysts are significantly more optimistic than unaffiliated analysts for this sample of recommendations. This result is very similar to the findings in Lin and McNichols (1998), although they analyze a different time period (SEOs from 1989 to 1994). Again, this result is typically interpreted as signaling an agency problem on the part of equity analysts; the argument is that analysts bias their recommendations upwards for stocks underwritten by their brokerage house, since analysts' pay is sometimes tied to investment banking revenues. The second column of Panel A shows that this affiliation bias is concentrated in stocks located in large cities: The difference between affiliated recommendations and unaffiliated recommendations is -0.390 for metro stocks, which is again strongly significant. By contrast, the sample of non-metro stocks reveals a much smaller bias (-0.120), which is no longer statistically significant. When I restrict the sample to include only affiliated analysts who produce recommendations within 90 days of the SEO, the mean difference is larger and still significant (-0.231, t=-2.81), but the number of matches is smaller (147).

Insert Table IX about here

After verifying that the affiliation bias documented in Lin and McNichols (1998) is again present in my sample, I examine the effect of geographic proximity in this setting. Specifically, I look to see if local affiliates' recommendations are unbiased, as my previous stock price tests suggest. To do so, I employ the same matching procedure described above, except that I now group local and nonlocal affiliated analysts separately. Panel A shows that affiliated analysts that are *also* local tend to issue recommendations that are *not* significantly more optimistic than a matched sample of unaffiliated analysts. The strongly significant affiliation bias documented in Panel A is not present for affiliated analysts who are also local. A similar result appears in the sample of metro stocks, although this finding is based on only 6 matched pairs. By contrast, the last few rows of Panel A show that the affiliation bias documented above is concentrated among distant affiliates (mean difference=-0.181, t=-2.41).

This result, coupled with the similar findings on price impact in the prior section, raises the question of why local affiliated recommendations are unbiased. Panel B provides some summary statistics to help explore this question. A few obvious differences between the sample of local affiliates and the sample of distant affiliates stand out, the first being the differing percentage of high-status analysts between the two groups (34.21% for local affiliates and 55.35% for distant affiliates). The large percentage differences in All-Star analysts (15.79% to 30.23%) and New York analysts (31.58% to 68.84%) between the two groups paint a similar picture. Clearly, local affiliated analysts are fundamentally different from nonlocal affiliated analysts.

Table X presents some summary statistics for the entire sample of recommendations to see if these patterns are widespread. Indeed, the percentage of high-status recommendations among distant affiliated recommendations (49.43%) is far higher than among local affiliated recommendations (30.56%). Further, the percentage of high-status recommendations among affiliated recommendations as a whole is far higher than the percentage in the entire population of recommendations (46.46% to 21.09%). These findings suggest that local affiliates may be unbiased simply because they tend to represent a different class of analyst, one that is less exposed to the agency problem often cited to explain the affiliation bias. Since local affiliates account for only 14.90% of all affiliated recommendations, and account for a higher percentage of first-time offerings than repeat offerings (15.81% to 14.36%), local affiliates may have less incentive to bias their recommendations upwards in the hopes of garnering additional investment banking business.

Insert Table X about here

Panel B of Table X explores this idea further. As before, I examine differences in mean recommendation codes. However, in this case the tests are performed on various subsets of the analyst population. The variable X is associated with the analyst characteristic in the designated column. For example, $AFFIL_{X=1}$ refers to affiliated recommendations where the affiliated analyst is also in category X; the first row of the $HIGH$ column thus reports the mean recommendation code for affiliated analysts who also work at high-status firms. All affiliated recommendations are matched to non-affiliated recommendations, as described in Table IX. The first few rows of Panel B show that the affiliation bias is concentrated among high-status analysts (mean difference=-0.252, t=2.64) and analysts located in New York City (mean difference=-0.194, t=-2.29); since 88.6% of high-status analysts are located in New York, these similarities are not surprising.

The last few rows of Panel B report an interesting result. In these rows I form matched pairs strictly by analyst characteristics. For example, instead of matching affiliated analysts with unaffiliated analysts, I simply match up high-status analysts (whether or not they are affiliated with the SEO) with low-status analysts using the same timing rules around SEO issuances as before. Using this approach, I find a mean recommendation difference equal to -0.083 (t=-1.24) in the high-status minus low-status match. High-status analysts thus appear to be more optimistic than low-status analysts around SEO issuances, even when they are not explicitly affiliated with the deal. Further, when I limit the sample to high-status analysts producing recommendations within 90 days of the SEO date, this mean recommendation difference climbs to -0.201 (t=-2.48), a figure almost as large as the original affiliation bias figure reported in Panel A of Table IX (=-0.231, t=-2.81). This finding suggests that the affiliation bias in stock recommendations is largely a high-status analyst phenomenon. Since high-status affiliates are more likely to be located in New York and further away from the stocks they cover, the results in Tables VIII and IX that local affiliates are unbiased is not surprising.

V. Conclusion

This paper is the first to connect the concept of geography to U.S. equity analysts' forecasts and recommendations. The effect of distance has already been documented in corporate governance, delegated portfolio management, and many other areas of economics, but has not been adequately examined in the context of equity analysis. I find evidence in support of the hypothesis that geographically proximate analysts possess an information advantage over other analysts. Local analysts are more accurate and have a greater impact on security prices than other analysts. These effects are strongest in recent years, for firms located in small cities and remote areas, and for analysts issuing downward revisions. I find little support for the notion that the link between geography and performance is a natural by-product of the endogenous coverage decisions made by analysts, although I do find that local investor demand or visibility may play a role in local abnormal performance. Analyzing the flow of information between investors and analysts by merging data on individual or institutional investor holdings with analyst forecast data would provide an ideal testing ground to evaluate this last possibility, and is a subject for future research.

I find no evidence of an underwriter affiliation bias for affiliated analysts that are also located nearby the firm being covered. I argue that this is because local affiliates are less likely to be working at high-status firms, where agency problems are intense due to the constant pressure to garner investment banking business. This suggests that there is an important geographic component to the agency problems in the analyst industry.

Since conflicts of interest appear to be particularly problematic when affiliated analysts are located far away from the firm, an interesting extension of this paper would be to analyze the effect of switching locations on analyst (particularly high-status analyst) performance. Broader questions, such as which factors drive location changes for analysts, and the extent to which knowledge is portable are also intriguing. These and other issues are left to future research.

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Table I
Summary Statistics for U.S. Equity Analysts

This table reports summary statistics for the two main samples used in this paper. Panel A reports statistics for the one-year earnings forecast sample, and Panel B reports statistics for the stock recommendation sample. Both samples are broken down by forecast as well as by firm characteristics. Forecasts are classified as Metro if the analyst is located in one of the 20 most populated cities, and Remote if the analyst is not located within 400 kilometers of any of the 20 most populated cities; firms are classified analogously, but according to the location of the firm's headquarters rather than the location of the analyst. A forecast is classified as Local if the analyst is located within 100 kilometers of the headquarters of the firm being covered. Analysts and firms located in Hawaii and Alaska are excluded from the sample, and the District of Columbia is counted as a state. For each forecast, the brokerage size (Brok. Size) is equal to the number of analysts working for the same firm in the same calendar year as the analyst producing the forecast. Analyst coverage (Coverage) is equal to the number of analysts covering the firm each fiscal year.

Panel A: One-Year Earnings Forecast Sample (n=348,571)						
Forecast Type	No. of Analysts	No. of States	Avg. Dist. To Firms (km)	Pct. Local	Pct. NYC	Mean (Median) Brok. Size
All	4254	34	1515.60	16.84	56.47	37.44 (35)
Metro	3358	13	1602.68	17.07	68.67	42.58 (41)
Non-metro	1141	33	1179.84	15.95	0.00	17.61 (12)
Remote	462	13	1054.66	22.25	0.00	14.88 (10)
199411-199712	2645	33	1474.77	17.02	59.08	30.11 (28)
199801-200112	3849	33	1545.60	16.71	54.55	42.83 (41)
Firm Type	No. of Firms	No. of States	Avg. Dist. To Analysts (km)	Avg. Size ($ mill.)	Avg. No. of Employees	Mean (Median) Coverage
All	4344	49	1394.69	1824.43	6947.3	5.79 (3.83)
Metro	877	14	1505.53	2290.81	8327.7	7.00 (4.60)
Non-metro	3544	49	1373.95	1711.51	6650.0	5.49 (3.63)
Remote	966	28	1415.26	1521.59	5927.2	5.26 (3.67)
199411-199712	2998	49	1326.44	1329.70	7523.8	5.47 (3.75)
199801-200112	3610	48	1419.47	2586.19	7902.2	6.01 (4)

Panel B: Stock Recommendation Sample (n=87,001)						
Recommendation Type	No. of Analysts	No. of States	Avg. Dist. To Firms (km)	Pct. Local	Pct. NYC	Mean (Median) Brok. Size
All	4054	34	1509.76	16.83	49.41	40.33 (27)
Metro	3183	13	1605.68	17.36	64.00	44.96 (34)
Non-metro	1101	33	1227.30	15.27	0.00	26.70 (13)
Remote	436	13	1168.93	21.85	0.00	23.22 (10)
199411-199712	2204	33	1447.99	17.14	49.86	28.88 (22)
199801-200112	3448	33	1545.67	16.65	49.14	46.99 (33)
Firm Type	No. of Firms	No. of States	Avg. Dist. To Analysts (km)	Avg. Size ($ mill.)	Avg. No. of Employees	Mean (Median) Coverage
All	4271	48	1403.62	2160.74	7632.0	3.26 (2.33)
Metro	903	14	1557.33	3037.81	8875.5	3.64 (2.67)
Non-metro	3457	48	1372.69	1931.27	7338.8	3.15 (2.33)
Remote	937	27	1405.00	1743.77	7199.6	3.07 (2.33)
199411-199712	2773	48	1321.33	1460.87	8002.4	3.04 (2)
199801-200112	3588	48	1436.47	3052.62	8868.1	3.67 (2.5)

Table II
Regressions of Relative Forecast Error on Analyst Characteristics

Fama and MacBeth (1973) cross-sectional regressions are run (yearly in Panel A, monthly in Panel B) from November 1994 to December 2001. This table reports the time-series average of the estimated coefficients from regressions of de-meaned absolute forecast error (*DAFE*) and proportional mean absolute forecast error (*PMAFE*) on a host of analyst characteristics. All variables are firm and fiscal-year mean adjusted (the *D* preceding each variable stands for differenced or de-meaned). For example, the variable *DAFE* equals the difference between the absolute forecast error for analyst i for firm j in fiscal year t and the mean absolute forecast error for firm j in fiscal year t; absolute forecast error is equal to the absolute value of an analyst's latest forecast, minus actual company earnings, as a percentage of stock price 12 months prior to the beginning of the fiscal year. The variable *PMAFE* equals *DAFE* divided by the mean absolute forecast error. The variable *DAGE* equals the log of one plus the age (in days) of analyst i's forecast; *DBSIZE* equals the log of one plus the number of analysts working for the same firm as i and supplying forecasts during the same year t; *DSTAR* is a dummy variable equal to one if the analyst was ranked as an All-Star the previous October by *Institutional Investor* magazine; *DHIGH* is a dummy variable equal to one if the analyst works at a high-status firm (where status is measured by the *Institutional Investor* magazine's rankings of brokerage firms); *DFEXP* equals the log of one plus the number of days that analyst i supplied a forecast for firm j; *DPCTOP* equals the percent of companies followed by analyst i in year t for which analyst i's last forecast is above actual earnings; *DAFFIL* is a dummy variable equal to one if the analyst works for the same brokerage firm that served as the lead underwriter for firm j's initial public offering within the past five years or for a secondary equity offering by firm j within the past two years; *DLOCAL* is a dummy variable equal to one if the analyst is located within 100 kilometers of firm j in fiscal year t; and *DLOGD* equals the log of one plus the distance (in kilometers) of the analyst i from firm j in fiscal year t; each variable is de-meaned as described above. The t-statistics are in parentheses.

Panel A: Yearly Regressions

Dep. Var.	*DAGE*	*DBSIZE*	*DSTAR*	*DHIGH*	*DFEXP*	*DPCTOP*	*DAFFIL*	*DLOCAL*	*DLOGD*
DAFE	0.2351	-0.0200	-0.0120	-0.0196	-0.0149	0.2567	-0.0609	-0.0197	
	(8.02)	(-2.53)	(-0.77)	(-1.20)	(-4.65)	(6.57)	(-2.80)	(-2.13)	
PMAFE	38.3639	-3.9581	-3.8302	-5.8194	-1.9194	56.0157	-3.9867	-2.7652	
	(10.26)	(-3.66)	(-3.83)	(-2.55)	(-4.20)	(7.92)	(-2.47)	(-2.28)	
DAFE	0.2350	-0.0201	-0.0119	-0.0198	-0.0149	0.2566	-0.0610		0.0025
	(8.01)	(-2.51)	(-0.76)	(-1.21)	(-4.68)	(6.56)	(-2.78)		(1.39)
PMAFE	38.3497	-3.9959	-3.8217	-5.8506	-1.9221	56.0218	-3.9836		0.4649
	(10.23)	(-3.71)	(-3.80)	(-2.57)	(-4.26)	(7.91)	(-2.46)		(2.72)
PMAFE	39.8310	-7.5650						-3.1704	
	(10.50)	(-16.32)						(-2.61)	
PMAFE	39.8150	-7.6174							0.5265
	(10.47)	(-16.03)							(2.88)

Panel B: Monthly Regressions

Dep. Var.	*DAGE*	*DBSIZE*	*DSTAR*	*DHIGH*	*DFEXP*	*DPCTOP*	*DAFFIL*	*DLOCAL*	*DLOGD*
PMAFE	43.8960	-3.0415	-5.6447	-5.4017	-2.1482	62.0247	-2.4224	-3.0436	
	(28.87)	(-4.68)	(-4.73)	(-2.53)	(-9.23)	(16.82)	(-0.36)	(-2.31)	
PMAFE	43.8679	-3.1004	-5.6187	-5.3863	-2.1520	62.1661	-2.7410		0.5754
	(28.80)	(-4.74)	(-4.73)	(-2.51)	(-9.25)	(16.87)	(-0.43)		(2.33)
					(Two-Year Ahead Forecasts)				
PMAFE	70.5991	-1.6526	-0.1789	-1.1433	-0.5202	10.5647	-1.6196	-1.5152	
	(20.10)	(-3.67)	(-0.27)	(-1.50)	(-3.99)	(7.73)	(-0.46)	(-2.41)	
PMAFE	70.5918	-1.6864	-0.1748	-1.1338	-0.5209	10.5731	-1.7182		0.3593
	(20.10)	(-3.77)	(-0.26)	(-1.48)	(-3.99)	(7.72)	(-0.50)		(2.60)

Table III
Bias versus Informativeness

Panel A reports the time series average of the estimated coefficients from yearly Fama and MacBeth (1973) regressions of de-meaned absolute forecast error (*DAFE*), proportional mean absolute forecast error (*PMAFE*), and relative optimism (*OPT*, *POPT*) on a host of analyst characteristics. The dependent variables *DAFE* and *PMAFE* and the independent variables *DAGE*, *DBSIZE*, *DSTAR*, *DHIGH*, *DFEXP*, *DAFFIL*, *DLOCAL*, and *DLOGD* are defined as in Table II; all variables are firm and fiscal-year mean adjusted (the *D* preceding each variable stands for differenced or de-meaned). The variable *OPT* equals the forecast error for analyst *i*'s forecast of firm *j* for fiscal year *t* ($FE_{i,j,t}$) minus the mean forecast error for firm *j* for fiscal year *t* ($\overline{FE_{j,t}}$); forecast error is equal to the analyst's latest forecast, minus actual company earnings, as a percentage of stock price 12 months prior to the beginning of the fiscal year. The variable *POPT* is simply *OPT* scaled by the absolute value of ($\overline{FE_{j,t}}$). Panel B reports the estimated coefficient, intercept, and R-squared values for Fama and MacBeth (1973) monthly cross-sectional regressions and pooled regressions of actual earnings on forecasted earnings; local analysts' forecasts and nonlocal analysts' forecasts are grouped separately. The *t*-statistics are in parentheses.

Panel A: Incorporating Relative Optimism into Table II Regressions

Dep. Var.	*DAGE*	*DBSIZE*	*DSTAR*	*DHIGH*	*DFEXP*	$(P)OPT$	*DAFFIL*	*DLOCAL*
PMAFE	38.8645	-4.6365	-4.2789	-6.2852	-1.9307	1.5073	-2.6597	-2.7440
	(10.19)	(-4.54)	(-4.28)	(-2.71)	(-4.16)	(3.22)	(-1.40)	(-2.17)
DAFE	0.1779	-0.01762	-0.0079	-0.0217	-0.0110	0.3648	-0.0705	-0.0153
	(7.13)	(-2.08)	(-0.67)	(-1.31)	(-4.61)	(5.90)	(-4.91)	(-1.66)
POPT	57.2318	-10.2860	-3.2889	0.1960	-2.1762		5.5544	-1.2317
	(8.76)	(-6.14)	(-0.75)	(0.32)	(-2.19)		(1.60)	(-0.85)
OPT	0.1638	-0.0148	-0.0107	0.0003	-0.0097		0.0991	-0.0095
	(10.91)	(-1.78)	(-1.02)	(0.02)	(-5.34)		(1.62)	(-1.11)

Panel B: Regressions of Actual Earnings on Forecasted Earnings
$(E_{i,j,t} = \alpha_i + \beta_i F_{i,j,t} + \varepsilon_{i,j,t})$

	Fama-MacBeth			Pooled		
Sample	β	α	R^2	β	α	R^2
$LOCAL = 1$	0.9904	-0.0008	0.8589	0.9940	-0.0006	0.8752
	(155.26)	(-3.20)	(81.10)	(206.07)	(-4.52)	
$LOCAL = 0$	0.9754	-0.0008	0.8215	0.9777	-0.0006	0.8342
	(206.07)	(-4.36)	(76.34)	(631.68)	(-7.71)	
$DIFF = (1-0)$	0.0150	-0.0000	0.0374			
	(1.95)	(-0.06)	(2.48)			

Table IV
Local Regression Coefficients Sorted by Firm Characteristics

Fama and MacBeth (1973) cross-sectional regressions are run each year from 1994 to 2001 for subsamples of Table II. This table reports only the coefficient estimates and associated t-statistics (in parentheses) for the *DLOCAL* variable in the Table II regression of de-meaned absolute forecast error (*DAFE*) on eight analyst characteristics. Stocks are classified as Metro if the firm's headquarters is located in one of the 20 most populated cities, and Remote if the firm's headquarters is not located within 400 kilometers of any of the 20 most populated cities. The abbreviation Earn. annc. refers to quarterly earnings announcement dates. *SMALL* stocks are those below the median market capitalization for all stocks in the sample, and *LARGE* stocks are those above the median market capitalization. *GROWTH* and *VALUE* stocks are sorted similarly but by book-to-market ratios; *LOSERS* and *WINNERS* are sorted similarly but by cumulative returns over the past year. The t-statistics and the number of observations (n) are in parentheses.

Sample	Dep. Var.	*ALL*	*SMALL*	*LARGE*	*GROWTH*	*VALUE*	*LOSERS*	*WINNERS*
All stocks (n=96,538)	*DAFE*	-0.0197	-0.0491	0.0017	-0.0109	-0.0335	-0.0287	-0.0150
		(-2.13)	(-2.79)	(0.31)	(-1.20)	(-1.74)	(-2.69)	(-1.25)
Metro stocks (n=23,261)	*DAFE*	-0.0046	-0.0327	0.0021	-0.0047	-0.0092	-0.0020	-0.0109
		(-0.33)	(-1.38)	(0.23)	(-0.30)	(-0.39)	(-0.13)	(-0.88)
Non-metro stocks (n=73,277)	*DAFE*	-0.0266	-0.0579	0.0020	-0.0125	-0.0487	-0.0441	-0.0188
		(-2.12)	(-2.44)	(-0.35)	(-0.82)	(-2.22)	(-3.48)	(-1.03)
Remote stocks (n=19,285)	*DAFE*	-0.0629	-0.0803	-0.0386	-0.0200	-0.1030	-0.0528	-0.0782
		(-2.68)	(-3.19)	(-1.03)	(-1.03)	(-2.21)	(-2.11)	(-3.07)
Subperiod: 199411-199712 (n=40,616)	*DAFE*	-0.0136	-0.0433	0.0109	-0.0203	-0.0037	-0.0205	-0.0070
		(-0.75)	(-1.22)	(1.26)	(-1.37)	(-0.13)	(-1.11)	(-0.30)
Subperiod: 199801-200112 (n=54,722)	*DAFE*	-0.0239	-0.0505	-0.0080	-0.0008	-0.0603	-0.0374	-0.0197
		(-3.50)	(-3.65)	(-2.65)	(-0.07)	(-3.59)	(-2.77)	(-2.58)
Post-Reg FD: 200011-200112 (n=62,612)	*DAFE*	-0.0162	-0.0733	0.0339	0.0270	-0.0900	-0.0122	-0.0153
		(-0.32)	(-1.18)	(0.88)	(0.41)	(-1.87)	(-0.20)	(-0.33)
Within 5 days of earn. annc. (n=25,915)	*DAFE*	-0.0263	-0.0506	-0.0043	-0.0060	-0.0527	0.0236	-0.0438
		(-0.94)	(-1.03)	(-0.56)	(-0.47)	(-1.05)	(0.80)	(-0.99)
Non-earn. annc. (n=61,886)	*DAFE*	-0.0249	-0.0627	-0.0012	-0.0028	-0.0549	-0.0381	-0.0148
		(-2.85)	(-4.95)	(-0.14)	(-0.29)	(-3.87)	(-3.00)	(-1.06)

Table V
Local Regression Coefficients Sorted by Analyst Characteristics

Fama and MacBeth (1973) cross-sectional regressions are run each year from 1994 to 2001 for subsamples of Table II. This table reports only the coefficient estimates and associated t-statistics (in parentheses) for the *DLOCAL* variable in the Table II regression of de-meaned absolute forecast error (*DAFE*) on eight analyst characteristics. $SIGNAL = 1$ if the analyst revises his forecast for firm j above his prior forecast for firm j and above the prior consensus forecast for firm j; $SIGNAL = -1$ if the analyst revises his forecast for firm j below his prior forecast for firm j and below the prior consensus forecast for firm j. All other forecasts are classified as $SIGNAL = 0$. The *XNYC* column excludes all forecasts issued by analysts located within 100 kilometers of New York City; the *STAR* column includes only those forecasts issued by All-Star analysts; the *NON* column excludes those forecasts issued by All-Star analysts; the *HIGH* column includes only those forecasts issued by analysts working at a high-status brokerage house; and the *LOW* column excludes those forecasts issued by analysts at high-status brokerage houses. Low-coverage analysts are those who fall below the 33rd percentile of number of stocks covered by an analyst in a given year; High-coverage are those above the 66th percentile. *LOCMED* is a dummy variable equal to one if the share of local stocks in an analyst's portfolio is above the median share of local stocks over all analysts' portfolios. The t-statistics and the number of observations (n) are in parentheses.

Panel A: By Forecast Signal

Sample	Dep. Var.	*ALL*	*XNYC*	*STAR*	*NON*	*HIGH*	*LOW*
All	*DAFE*	-0.0197	-0.0250	-0.0462	-0.0116	-0.0714	-0.0192
forecasts		(-2.13)	(-2.76)	(-1.64)	(-1.12)	(-1.17)	(-2.07)
(n=96,538)							
$SIGNAL = 1$	*DAFE*	-0.0207	-0.0192	0.0333	-0.0170	-0.0431	-0.0285
revisions		(-1.15)	(-0.91)	(1.81)	(-0.92)	(-1.39)	(-1.45)
(n=33,910)							
$SIGNAL = -1$	*DAFE*	-0.0601	-0.0614	-0.1479	-0.0489	-0.1603	-0.0429
revisions		(-3.31)	(-2.90)	(-1.53)	(-2.42)	(-0.64)	(-3.23)
(n=25,251)							
$SIGNAL = 0$	*DAFE*	-0.0081	-0.0318	-0.1742	0.0028	-0.0950	-0.0077
forecasts		(-0.89)	(-3.31)	(-2.07)	(0.31)	(-1.76)	(-0.94)
(n=37,344)							

Panel B: By Analyst Portfolio								
Sample	Subgroup	Dep. Var.	*ALL*	*XNYC*	*STAR*	*NON*	*HIGH*	*LOW*
Low-coverage analysts (*n*=11,107)		*DAFE*	0.0129	-0.0111	0.2851	0.0329	-0.0160	0.0216
			(0.45)	(-0.28)	(1.47)	(1.02)	(-0.17)	(1.01)
	LOCMED=1 (*n*=7,218)	*DAFE*	-0.0249	0.0174	0.1770	-0.014	-0.0458	0.00091
			(-0.81)	(0.21)	(1.06)	(-0.54)	(-0.61)	(0.28)
Mid-coverage analysts (*n*=26,532)		*DAFE*	-0.0181	-0.0329	0.0472	-0.0299	-0.0116	-0.0251
			(-1.26)	(-2.47)	(0.55)	(-1.24)	(-1.09)	(-1.96)
	LOCMED=1 (*n*=11,889)	*DAFE*	-0.0400	-0.0377	0.0342	-0.0180	-0.0249	-0.0382
			(-1.34)	(-1.71)	(0.52)	(-0.38)	(-0.56)	(-1.86)
High-coverage analysts (*n*=58,899)		*DAFE*	-0.0340	-0.0325	-0.1756	-0.0217	-0.1205	-0.0314
			(-2.78)	(-2.93)	(-3.10)	(-2.09)	(-1.29)	(-2.13)
	LOCMED=1 (*n*=18,734)	*DAFE*	-0.0512	-0.0626	-0.2070	-0.0471	-0.1616	-0.0572
			(-2.67)	(-2.16)	(-2.70)	(-2.61)	(-1.23)	(-2.42)

Table VI
Individual Analyst Portfolios and the Effect of Coverage Decisions

Panel A reports summary statistics broken down by analyst type. Analysts are classified each year as Metro and Remote each year as described in Table I. An analyst is classified as Expert in year t if the analyst covers only stocks falling within one 2-digit SIC code during year t. An analyst is considered Local to firm j in year t if the analyst is located within 100 kilometers of the headquarters of firm j in year t. No. of Obs. equals the number of unique analyst-firm-year observations. No. of Firms Cov. equals the average number of firms covered each year by each analyst type, and No. of Local Firms Cov. equals the average number of local firms covered each year by each analyst type. Pct. Expert equals the percent of expert analysts across analyst types each year, and No. of SIC Codes Cov. equals the average number of 2-digit SIC codes covered by each analyst type each year. Panel B reports only the coefficient estimates and associated t-statistics (in parentheses) for the *DLOCAL* variable in the Table II regression of de-meaned absolute forecast error (*DAFE*) on various analyst characteristics. The third column adds the variable *DTASK* to this regression, the fourth column adds the variable *DSPEC* instead, and the fifth column adds both *DTASK* and *DSPEC* to the regression. *DSPEC* equals the log of one plus the number of firms for which analyst i supplied a forecast in year t, and *DPSEC* is a dummy variable equal to one if the analyst is an Expert as defined above; both variables are firm and fiscal-year mean adjusted. The sixth, seventh, and eighth columns of Panel B report estimates of the *DLOCAL* variable from the Table II regression, this time for subsamples of firms broken down by advertising intensity (ADS/S). *LOW* (ADS/S) firms are those below the median level of advertising intensity for firms in the sample, and *HIGH* (ADS/S) are those above the median level. The t-statistics and the number of observations (n) are in parentheses.

Panel A: Summary Statistics for Individual Analyst Portfolios

Analyst type	No. of Obs.	No. of Firms Cov.	No. of Local Firms Cov.	Pct. Expert Analysts	No. of SIC Codes Cov.	Avg. Dist to Firms (km)
All analysts	113517	12.37	2.01	32.66	2.13	1480.36
Metro analysts	88440	12.34	2.05	33.21	2.03	1567.50
Remote analysts	9689	11.10	2.22	31.63	2.40	1098.63
Local analysts	19185	11.87	4.17	32.51	2.14	28.79
Expert analysts	37072	10.43	1.67	100.00	1.00	1521.07

Panel B: Task Complexity, Specialization, and Investor Demand

Sample	Dep. Var.	w/ *DTASK*	w/ *DSPEC*	w/ *DTASK* and *DSPEC*	*NO* (ADS/S)	*LOW* (ADS/S)	*HIGH* (ADS/S)
All stocks (n=96,538)	*DAFE*	-0.0186 (-1.92)	-0.0202 (-2.18)	-0.0188 (-1.94)	-0.0265 (-1.73)	0.0377 (1.63)	-0.0510 (-3.25)
Remote stocks (n=19,285)	*DAFE*	-0.0587 (-2.51)	-0.0612 (-2.65)	-0.0551 (-2.41)	-0.0658 (-2.58)	0.0075 (0.07)	-0.1184 (-1.51)

Table VII
Stock Price Impact of Forecast Revisions

Fama and MacBeth (1973) cross-sectional regressions are run every month from November 1994 to December 2001. This table reports the time-series average of the estimated coefficients from monthly regressions of percentage size-adjusted average daily returns around forecast revisions on firm size, the magnitude of the revision, the affiliation status of the analyst (*AFFIL*), and a measure of geographic proximity (*LOCAL*). Revisions are classified by signal type, where $SIGNAL = 1$ if the analyst revises his forecast for firm j above his prior forecast for firm j and above the prior consensus forecast for firm j; $SIGNAL = -1$ if the analyst revises his forecast for firm j below his prior forecast firm j and below the prior consensus forecast for firm j. Stocks are classified as Metro if the firm's headquarters is located in one of the 20 most populated cities. The variable $LOCAL_i$ is a dummy variable equal to one if the analyst is located within 100 kilometers of firm j. The variable $AFFIL_i$ is a dummy variable equal to one if the analyst works for the same brokerage firm that served as the lead underwriter for firm j's initial public offering within the past five years or for a secondary equity offering by firm j within the past two years. The variable $AAR_{j,a,b}$ is the percentage size-adjusted average daily return for firm j from event day a to b. The variable $SUF_{i,j,0}$ is the standardized unexpected forecast for analyst i, which is defined as the change in the forecast divided by the cross-sectional standard deviation of outstanding forecasts for firm j. The variable $LNME_{-j,-30}$ is the log of one plus the market capitalization of firm j one month prior to the revision. The t-statistics and the number of observations (n) are in parentheses.

Panel A: $AAR_{j,-1,1} = \beta_0 + \beta_1 LNME_{j,-30} + \beta_2 SUF_{i,j,0} + \beta_3 AFFIL_i + \beta_4 LOCAL_i + \varepsilon$

Sample	Signal Type	β_0	β_1	β_2	β_3	β_4
All	$SIGNAL = 1$	0.8469	-0.0539	-0.0769	0.0735	0.0946
stocks	(n=86,127)	(7.38)	(-4.41)	(-0.75)	(0.68)	(2.38)
	$SIGNAL = -1$	-0.5643	0.0492	0.9781	-0.5550	-0.1281
	(n=63,214)	(-2.56)	(1.75)	(5.54)	(-3.67)	(-2.46)
Metro	$SIGNAL = 1$	0.8836	-0.0586	-0.1172	0.1320	0.0395
stocks	(n=23,097)	(4.68)	(-2.59)	(-0.43)	(0.70)	(0.55)
	$SIGNAL = -1$	-0.7551	0.0816	0.5343	-0.4660	-0.1346
	(n=13,894)	(-2.70)	(2.39)	(3.26)	(-1.99)	(-1.61)
Non-metro	$SIGNAL = 1$	0.8368	-0.0538	-0.0494	0.0403	0.1019
stocks	(n=63,030)	(7.26)	(-4.17)	(-0.34)	(0.34)	(1.95)
	$SIGNAL = -1$	-0.4893	0.0345	0.9927	-0.3640	-0.1638
	(n=49,320)	(-1.90)	(1.08)	(5.72)	(-2.26)	(-2.54)

Panel B: $AAR_{j,2,64} = \beta_0 + \beta_1 LNME_{j,-30} + \beta_2 SUF_{i,j,0} + \beta_3 AFFIL_i + \beta_4 LOCAL_i + \varepsilon$

Sample	Signal Type	β_0	β_1	β_2	β_3	β_4
All	$SIGNAL = 1$	0.1205	-0.0091	-0.0475	-0.0102	0.0020
stocks	(n=86,127)	(4.88)	(-3.15)	(-2.80)	(-0.60)	(0.33)
	$SIGNAL = -1$	-0.0536	-0.0012	0.0455	-0.0005	-0.0188
	(n=63,214)	(1.74)	(-0.35)	(2.53)	(-0.02)	(-1.09)

Panel C: $AAR_{j,-1,1} = \beta_0 + \beta_1 LNME_{j,-30} + \beta_2 SUF_{i,j,0} + \beta_3 AFFIL_i + \beta_4 LOCAL_i + \varepsilon$						
Sample	Signal Type	β_0	β_1	β_2	β_3	β_4
Subperiod:	$SIGNAL = 1$	0.3888	-0.0196	0.0354	0.4245	0.0566
199411-199712	(n=34,067)	(3.43)	(-1.32)	(0.67)	(3.47)	(1.99)
	$SIGNAL = -1$	-0.4389	0.0320	0.4648	-0.4584	-0.0580
	(n=27,635)	(-3.65)	(2.37)	(5.97)	(-2.11)	(-1.19)
Subperiod:	$SIGNAL = 1$	1.2076	-0.0811	-0.1620	-0.1897	0.1256
199801-200112	(n=51,867)	(7.23)	(-4.63)	(-0.92)	(-1.20)	(1.87)
	$SIGNAL = -1$	-0.6677	0.0629	1.369	-0.6648	-0.1802
	(n=35,382)	(-1.75)	(1.29)	(4.63)	(-3.22)	(-2.15)
Post-Reg FD:	$SIGNAL = 1$	1.6342	-0.1121	-0.1149	-0.1653	0.1361
200011-200112	(n=5,506)	(3.42)	(-2.35)	(-0.20)	(-0.41)	(0.67)
	$SIGNAL = -1$	-1.8525	0.1976	1.4686	-1.0540	-0.4210
	(n=5,419)	(-1.54)	(1.27)	(1.50)	(-2.35)	(-1.93)

Panel D: $AAR_{j,-1,1} = \beta_0 + \beta_1 LNME_{j,-30} + \beta_2 SUF_{i,j,0} + \beta_3 AFFIL_i + \beta_4 LOCAL_i + \varepsilon$						
Sample	Signal Type	β_0	β_1	β_2	β_3	β_4
Excl. NYC	$SIGNAL = 1$	0.6949	-0.0344	0.0322	0.2245	0.2277
forecasts	(n=37,007)	(4.94)	(-2.22)	(0.23)	(1.10)	(2.35)
	$SIGNAL = -1$	-0.3176	0.01411	0.8140	-0.3294	-0.0779
	(n=26,179)	(-2.19)	(0.73)	(6.55)	(-1.21)	(-0.82)
All-Star	$SIGNAL = 1$	1.0315	-0.0704	-0.0764	-0.3586	0.2157
forecasts	(n=15,919)	(5.67)	(-3.29)	(-0.53)	(-1.84)	(2.09)
	$SIGNAL = -1$	-0.6176	0.0454	0.9746	-0.5244	-0.0551
	(n=12,070)	(-3.01)	(1.86)	(6.13)	(-1.95)	(-0.43)
Non-All-Star	$SIGNAL = 1$	0.8368	-0.0533	-0.0860	0.2068	0.0592
forecasts	(n=70,208)	(6.37)	(-4.08)	(-0.69)	(1.64)	(1.31)
	$SIGNAL = -1$	-0.6211	0.0616	1.0595	-0.4102	-0.1740
	(n=51,144)	(-2.22)	(1.62)	(4.35)	(-2.41)	(-2.88)
High-status	$SIGNAL = 1$	1.2245	-0.0927	-0.0321	-0.1690	0.3173
forecasts	(n=26,169)	(9.00)	(-5.73)	(-0.35)	(-1.20)	(3.81)
	$SIGNAL = -1$	-0.4255	0.0248	0.9827	-0.6068	-0.1443
	(n=20,067)	(-2.36)	(1.22)	(8.10)	(-2.41)	(-1.36)
Low-status	$SIGNAL = 1$	0.7667	-0.0458	-0.1014	0.2629	0.0387
forecasts	(n=59,958)	(6.26)	(-3.61)	(-0.80)	(1.64)	(0.83)
	$SIGNAL = -1$	-0.6231	0.0616	0.9441	-0.3707	-0.1437
	(n=43,147)	(-2.79)	(2.14)	(5.37)	(-1.72)	(-2.86)
High advertising	$SIGNAL = 1$	2.3208	-0.2110	0.4549	0.0887	0.1284
forecasts	(n=11,743)	(1.93)	(-1.69)	(1.56)	(0.81)	(1.47)
	$SIGNAL = -1$	-0.8234	0.0804	0.9164	-0.4191	-0.1393
	(n=6,964)	(-2.13)	(1.62)	(4.20)	(-1.53)	(-0.66)

Table VIII
Stock Price Impact of Affiliated Analyst Recommendations

Fama and MacBeth (1973) cross-sectional regressions are run every month from November 1994 to December 2001. This table reports the time-series average of the estimated coefficients from monthly regressions of percentage size-adjusted average daily returns around stock recommendations on a series of dummy variables. In Panels A, B, and C, the last four independent variables are equal to one if the recommendation is a strong buy (SB_j), buy (B_j), hold (H_j), or sell (S_j). In Panel A, the first three independent variables ($SB_{j,A}$, $B_{j,A}$, and $H_{j,A}$) are equal to one if the recommendation is a strong buy, buy, hold, or sell, and is issued by an analyst who works for the same brokerage firm that served as the lead underwriter for the firm's most recent secondary equity offering (within the past two years), and equal to zero otherwise. Thus β_1, β_2, and β_3 capture the mean incremental returns associated with affiliated analysts' recommendations relative to the same recommendations by non-affiliated analysts. In Panel B, the first three independent variables ($SB_{j,A,L}$, $B_{j,A,L}$, and $H_{j,A,L}$) are equal to one if the recommendation is a strong buy, buy, hold, or sell, and is issued by a lead underwriter analyst who is also local (local analysts reside within 100 kilometers of the firm's headquarters). The first three independent variables ($SB_{j,A,NL}$, $B_{j,A,NL}$, and $H_{j,A,NL}$) in Panel C are defined similarly; the subscript *NL* refers to a lead underwriter analyst who is *not* a local analyst. The variable $AAR_{j,a,b}$ is the percentage size-adjusted average daily return from event day *a* to *b*. The *t*-statistics and the number of observations (*n*) are in parentheses.

Panel A: Incremental Returns Associated with All Affiliates

$$AAR_{j,-a,b} = \beta_1 SB_{j,A} + \beta_2 B_{j,A} + \beta_3 H_{j,A} + \beta_4 SB_j + \beta_5 B_j + \beta_6 H_j + \beta_7 S_j + \varepsilon$$

	Dep. Var.	β_1	β_2	β_3	β_4	β_5	β_6	β_7
Announcement period:	$AAR_{j,-1,1}$	0.62211	-0.6862	-1.7602	0.7198	-0.0390	-1.1553	-1.0672
(*n*=86,824)		(1.26)	(-2.18)	(-3.78)	(16.11)	(-1.04)	(-14.23)	(-7.37)
Pre-release period:	$AAR_{j,-22,-2}$	-0.0741	-0.1071	-0.0751	0.0463	0.0335	-0.0689	-0.0557
(*n*=86,824)		(-1.30)	(-1.87)	(-1.17)	(4.99)	(2.70)	(-4.70)	(-2.06)

Panel B: Incremental Returns Associated with Local Affiliates

$$AAR_{j,a,b} = \beta_1 SB_{j,A,L} + \beta_2 B_{j,A,L} + \beta_3 H_{j,A,L} + \beta_4 SB_j + \beta_5 B_j + \beta_6 H_j + \beta_7 S_j + \varepsilon$$

	Dep. Var.	β_1	β_2	β_3	β_4	β_5	β_6	β_7
Announcement period:	$AAR_{j,-1,1}$	0.3322	-0.4214	-0.2463	0.7202	-0.0438	-1.1635	-1.0786
(*n*=86,824)		(1.96)	(-1.75)	(-0.78)	(16.18)	(-1.16)	(-14.26)	(-7.38)
Pre-release period:	$AAR_{j,-22,-2}$	0.0019	0.0050	-0.021	0.0457	0.0327	-0.0683	-0.0556
(*n*=86,824)		(0.04)	(0.07)	(-0.69)	(4.91)	(2.63)	(-4.74)	(-2.07)

Panel C: Incremental Returns Associated with Distant Affiliates

$$AAR_{j,a,b} = \beta_1 SB_{j,A,NL} + \beta_2 B_{j,A,NL} + \beta_3 H_{j,A,NL} + \beta_4 SB_j + \beta_5 B_j + \beta_6 H_j + \beta_7 S_j + \varepsilon$$

	Dep. Var.	β_1	β_2	β_3	β_4	β_5	β_6	β_7
Announcement period:	$AAR_{j,-1,1}$	0.0826	-0.6966	-1.738	0.7211	-0.0402	-1.1562	-1.0672
(*n*=86,824)		(0.46)	(-2.10)	(-3.87)	(16.12)	(-1.07)	(-14.27)	(-7.37)
Pre-release period:	$AAR_{j,-22,-2}$	-0.0673	-0.09106	-0.0676	0.0464	0.0335	-0.0680	-0.0557
(*n*=86,824)		(-1.16)	(-1.75)	(-0.98)	(4.98)	(2.70)	(-4.71)	(-2.06)

Table IX
The Affiliation Bias in Analyst Stock Recommendations

Panel A reports differences in mean recommendation codes (1=Strong Buy, 5=Strong Sell), where recommendations are matched in pairs using a (maximum) 60-day window between recommendation dates. The variable $AFFIL$ is a dummy variable equal to one if the analyst works for the same brokerage firm that served as the lead underwriter for the firm's most recent secondary equity offering (SEO), and zero otherwise; $NONAFF$=1 whenever $AFFIL$=0, and vice versa. The variable $LOCAFF$ is a dummy variable equal to one if $LOCAL$=1 and $AFFIL$=1, and equal to zero otherwise; the variable $DISTAFF$ is a dummy variable equal to one if $LOCAL$=0 and $AFFIL$=1, and equal to zero otherwise. In the ALL column, the affiliated recommendation nearest to the SEO date is chosen, while in the ALL90, ALL120, and ALL250 columns, the affiliated recommendation must be within 90, 120, or 250 days of the SEO date, respectively. The MET column includes only those stocks headquartered in one of the 20 most populated cities, while the $NMET$ column excludes those metropolitan stocks. Panel B presents summary statistics for the latter two samples of matched recommendations. MET_A refers to analysts located in a metro area, while MET_F refers to stocks located in a metro area; all other variables are described in Table V. The t-statistics and the number of matched pairs (n) are in parentheses.

Panel A: Matched Pair Mean Differences						
	ALL	MET	$NMET$	ALL90	ALL120	ALL250
$AFFIL$	1.7247	1.6098	1.7500	1.5238	1.5484	1.5912
$NONAFF$	1.8947	2.0000	1.8702	1.7551	1.7742	1.8343
$DIFF$	-0.1700	-0.3902	-0.1202	-0.2313	-0.2258	-0.2431
	(-2.45)	(-2.16)	(-1.61)	(-2.81)	(-2.78)	(-3.11)
	(n=247)	(n=41)	(n=206)	(n=147)	(n=155)	(n=181)
$LOCAFF$	1.7105	1.5000	1.7500	1.5000	1.5217	1.6000
$NONAFF$	1.7105	1.6667	1.7188	1.5909	1.5652	1.6000
$DIFF$	0.0000	-0.1667	0.0312	-0.0909	-0.0435	0.0000
	(0.00)	(-0.42)	(0.18)	(-0.48)	(-0.23)	(0.00)
	(n=38)	(n=6)	(n=32)	(n=22)	(n=23)	(n=25)
$DISTAFF$	1.7395	1.6486	1.7611	1.5280	1.5530	1.5897
$NONAFF$	1.9209	2.0270	1.88944	1.7840	1.8106	1.8718
$DIFF$	-0.1814	-0.3784	-0.1333	-0.2560	-0.2576	-0.2821
	(-2.41)	(-1.96)	(-1.64)	(-2.81)	(-2.87)	(-3.31)
	(n=209)	(n=35)	(n=174)	(n=125)	(n=132)	(n=156)

Panel B: Matched Pair Summary Statistics					
	$Pct.HIGH$	$Pct.STAR$	$Pct.NYC$	$Pct.MET_A$	$Pct.MET_F$
$LOCAFF$	34.21	15.79	31.58	76.32	15.79
$NONAFF$	14.29	5.26	31.58	76.32	15.79
(n=38)					
$DISTAFF$	55.35	30.23	68.84	91.63	16.74
$NONAFF$	19.90	9.77	42.33	69.30	16.74
(n=209)					

Table X
Why are Recommendations by Local Affiliates Unbiased?

Panel A reports summary statistics for the entire recommendation sample. The column *ALL* includes all recommendations, the *AFFIL* column includes only affiliated recommendations, etc.; the variables *AFFIL*, *LOCAFF*, and *DISTAFF* are defined as in Table VIII. *FIRST* further limits the sample to affiliated recommendations associated with first-time SEOs, while *REPEAT* includes all other affiliated recommendations. Panel B reports differences in mean recommendation codes, as in Panel A of Table VIII. The variable X is associated with the analyst characteristic in the designated column. For example, $AFFIL_{X=1}$ refers to affiliated recommendations where the affiliated analyst is also in category X; the first row of the *HIGH* column thus reports the mean recommendation code for affiliated analysts who also work at high-status firms. All affiliated recommendations are matched to non-affiliated recommendations as described in Table VIII. The t-statistics and the number of matched pairs (n) are in parentheses.

Panel A: Recommendation Sample Breakdown

	ALL	*AFFIL*	*AFFIL* (*FIRST*)	*AFFIL* (*REPEAT*)	*LOCAFF*	*DISTAFF*
Pct.HIGH	21.09	46.62	46.13	46.60	30.56	49.43
Pct.LOCAL	16.83	14.90	15.81	14.36	100.00	0.00

Panel B: Matched Pair Mean Differences

	(Nearest Affiliated Rec. to SEO)				(Affiliated Rec. Within 90 Days of SEO)			
$X=$ [	*HIGH*	*LOW*	*NYC*	*XNYC*	*HIGH*	*LOW*	*NYC*	*XNYC*
$AFFIL_{X=1}$	1.7333	1.7339	1.7212	1.7872	1.5190	1.5294	1.5319	1.5094
NONAFF	1.9852	1.8145	1.9152	1.8404	1.8481	1.6471	1.8404	1.6048
DIFF	-0.2519	-0.0808	-0.1939	-0.0532	-0.3291	-0.1176	-0.3085	-0.0943
	(-2.64)	(-0.84)	(-2.29)	(-0.47)	(-2.89)	(-0.99)	(-3.04)	(-0.67)
	(*n*=136)	(*n*=111)	(*n*=152)	(*n*=95)	(*n*=79)	(*n*=68)	(*n*=94)	(*n*=53)
$LOCAFF_{X=1}$	1.7692	1.6923	1.7500	1.6923	1.4286	1.5333	1.7500	1.3571
NONAFF	1.7692	1.6923	1.8333	1.6538	1.5714	1.6000	1.6250	1.5714
DIFF	0.0000	0.0000	-0.083	-0.0385	-0.1429	-0.0667	0.1250	-0.2143
	(0.00)	(0.00)	(-0.28)	(-0.02)	(-0.50)	(-0.26)	(0.40)	(-0.89)
	(*n*=12)	(*n*=26)	(*n*=12)	(*n*=26)	(*n*=7)	(*n*=15)	(*n*=8)	(*n*=14)
$DISTAFF_{X=1}$	1.7419	1.7549	1.7124	1.8406	1.5278	1.5283	1.5116	1.5641
NONAFF	2.0081	1.8333	1.9281	1.8986	1.8750	1.6604	1.8605	1.6154
DIFF	-0.2661	-0.0784	-0.2157	-0.0580	-0.3472	-0.1321	-0.3488	-0.0513
	(-2.66)	(-0.73)	(-2.44)	(-0.42)	(-2.85)	(-0.97)	(-3.26)	(-0.30)
	(*n*=124)	(*n*=85)	(*n*=140)	(*n*=69)	(*n*=72)	(*n*=53)	(*n*=86)	(*n*=39)
$X=1$	1.7669		1.8229		1.6169		1.7514	
$X=0$	1.8496		1.8192		1.8182		1.7946	
DIFF	-0.0827		0.0040		-0.2012		-0.0432	
	(-1.24)		(0.06)		(-2.48)		(-0.58)	
	(*n*=266)		(*n*=206)		(*n*=155)		(*n*=185)	

Notes

[1]See Hong and Kubik (2003) for evidence in support of this view.

[2]See, for example, Lin and McNichols (1998), Dechow, Hutton, and Sloan (1998), and Michaely and Womack (1999).

[3]Additionally, Michaely and Womack (1999) report that stocks for which underwriter analysts issue buy recommendations perform more poorly than buy recommendations by unaffiliated analysts prior to, at the time of, and subsequent to the recommendation date. By contrast, Dugar and Nathan (1995) find no evidence of stock price response to recommendations by affiliated or unaffiliated analysts.

[4]See Agrawal and Chadha (2002) and Bailey, Li, Mao, and Zhong (2003) for a discussion of these and related issues.

[5]I use the I/B/E/S adjusted earnings-per-share data in this paper, since I require a smooth time-series of earnings forecasts. This opens up the possibility that my results are affected by the adjustment bias discussed in Diether, Malloy, and Scherbina (2002) and in Payne and Thomas (2003). However, I can report that my results are slightly stronger when I use the raw, unadjusted data.

[6]I employ a similar procedure for the Recommendations History data set. I also eliminate all analyst teams from the data set, since it is not always possible to match them up to a unique location.

[7]My results are not sensitive to this timing convention. For example, if I classify an analyst's location starting in November of year t and lasting until October of year $t+1$ *only* if the analyst appears again in the *Nelson's Directory* in November of year $t+1$, my results are virtually identical.

[8]Since locations are identified by latitude and longitude, I calculate the arclength, d_{ij}, between each pair (see Coval and Moskowitz (1999) for details).

[9]I have also experimented with a measure of general in-sample experience rather than firm-specific experience, but the results are similar to those presented here.

[10]See Stickel (1992), Lamont (2002), and Chevalier and Ellison (1999) for insight into the importance of controlling for these factors.

[11]I winsorize by applying the 1st and 99th percentile breakpoints of the designated variable each month to observations falling outside these breakpoints in a given month. Using yearly breakpoints or one set of breakpoints for the entire sample changes no conclusions.

[12]Untabulated statistics indicate that local analysts also issue forecasts more frequently than nonlocal analysts, and issue a higher percentage of lead forecasts than nonlocals; these frequency and herding results are available on request.

[13]Here and elsewhere, using terciles or quartiles to form these group designations, instead of medians, changes no conclusions.

[14]Untabulated industry breakdowns reveal little connection between the geographic concentration of an industry and the local accuracy advantage; these results are available on request.

[15]Similarly, after dropping all analysts located within 100 kilometers of San Francisco, the coefficient on *DLOCAL* is comparable (-0.0230) and still significant (t=-2.17) in the $PMAFE$ yearly specification from Table II.

[16]These results are robust to a variety of definitions of analyst specialization, such as the number of 2-digit SIC codes covered or the dispersion in 2-digit SIC codes within an analyst's portfolio, and to different industry classifications (e.g., 4-digit SIC codes or the 17-48 industry portfolios of Fama and French (1997)).

[17]Cooper, Day, and Lewis (2001) provide evidence that ranking analysts solely on forecast accuracy can lead to misclassification errors.

[18]While Carleton, Chen, and Steiner (2002) conduct a similar test of the market's response to regional versus national analysts' announcements, they do not focus on geographic proximity as their unit of analysis, but rather on the perceived reputation of the analyst's employer.

[19]As in Stickel (1992), when computing $SUF_{i,j,0}$, I set a divisor less than \$.25 arbitrarily equal to \$.25 to mitigate small denominators, and scaled forecast revisions are truncated at -200% and +200%. Scaling forecast revisions by the absolute value of the prior forecast or by price changes no conclusions.

[20]I obtain similar results if I restrict the regressions to revisions of a relatively constant magnitude of SUF (e.g., the top 10% SUF or the bottom 10% SUF) as in Stickel (1992), rather than according to signal attributes.

[21]Similarly, Teoh and Wong (2002) find no evidence that the predictive power of accruals to explain forecast errors differs by affiliation status.

[22]Table IX presents results for a variety of different designated time periods.