How Quiet is the Quiet Period? Broker Geography and Retail Investor Activity around IPOs

Daniel Bradley[a], William Gerken[b], and Jared Williams[c]

[a]*Department of Finance, University of South Florida, Tampa, FL 33620, 813.974.6358, danbradley@usf.edu*

[b]*Gatton College of Business, University of Kentucky, Lexington, KY 40515, 859.257.2774, will.gerken@uky.edu*

[c] *Department of Finance, University of South Florida, Tampa, FL 33620, 813.974.6316, jwilliams25@usf.edu*

Abstract

We examine the relation between the geographic dispersion of an IPO underwriter's financial advisors and retail investor activity during the IPO quiet period. We find that a one-standard deviation increase in the percentage of an area's financial advisors that are employed by an underwriter is associated with a 18.5% increase in the area's buying activity during the quiet period. This effect is strongest in larger metro areas, for small IPOs and for IPOs with higher underpricing. Further, it is increasing in the optimism of the underwriter's subsequent (post-quiet period) equity research, consistent with the underwriter's opinions and projections reaching the financial advisors' clients, seemingly in violation of IPO quiet period restrictions.

Keywords: Broker geography, IPOs, quiet period, financial advisors, retail investor behavior

JEL classifications: G24, G50

We thank Terry Odean for providing the retail trading data. We also thank Jack Bao, Jess Cornaggia, Joey Engelberg, Laura Field, Matt Gustafson, Peter Iliev, Kevin Mullally, Christo Pirinsky, Ana-Maria Tenekedjieva (discussant), and seminar participants at Penn State, the University of Central Florida, the University of Delaware, and the University of South Florida for their helpful comments.

How Quiet is the Quiet Period? Broker Geography and Retail Investor Activity around IPOs

1. Introduction

When firms go public, there is a mandatory "quiet" period that begins no later than the date it enters an agreement with its lead underwriter and extends for 40 days beyond the issue date.[1] During this quiet period, the firm and its underwriters are restricted from making statements containing predictions, projections, forecasts, estimates or opinions regarding the value of the company's newly-issued stock. While it is easy for regulators to observe whether the IPO firm or its underwriters violate these restrictions in their public statements and equity research reports, it is far more difficult to monitor their non-public communications. For example, financial advisors might learn of the opinions of their firm's analysts, and this information might affect the recommendations they give to their investor clients. Because the quiet period does not apply to investors, there is nothing preventing them from sharing their opinions with other investors such as their friends and family.

How might a researcher examine whether such information flow occurs? The internal communications of financial institutions or the investment advice that advisors give their clients are clearly unobservable. However, we can observe the geography of the underwriter's investment advisor workforce and the geography of retail investor trading during the IPO quiet period. If analysts' projections of an IPO firms' future performance reach the firm's financial advisors, we should expect this to affect the investment advice they give their clients, and we should expect them to share their stock picks with friends and family who might have a trading account with a discount brokerage (Pool, Stoffman, and Yonker, 2015). Thus, we should expect to see elevated retail trading activity in local areas whose financial advisor workforce is dominated by the IPO firm's underwriter, and we should expect this relation to be strongest among IPO-underwriter pairs such that the underwriter's analysts

[1] Although the quiet period currently extends 40 days after the date of the issuance, it lasted 25 days past the issuance date during our 1991-1996 sample period, and the 2012 JOBS Act removed most of the quiet period restrictions for emerging growth companies.

subsequently (after the quiet period) reveal themselves to be unusually optimistic about the IPO firm (Bradley, Jordan, and Ritter, 2003).

The main idea of this study can be easily explained via the following example. Human Genome Sciences Inc. (HGS) was a biopharmaceutical corporation headquartered in Rockville, Maryland, that went public in 1993. Its underwriters were Bear Stearns, Lehman Brothers, and Smith Barney. In 1993, Smith Barney and Lehman Brothers employed the majority of the financial advisors in Medford, Oregon: Smith Barney employed 22 of the 60 advisors in Medford, while Lehman employed 16. Conversely, in Chico, California, the three IPO underwriters did not employ any of the 26 financial advisors: the Chico market was dominated by AG Edwards and Paine Webber. We therefore expect investors in Medford to be relatively more knowledgeable about HGS due to the possible information flow to Smith Barney's and Lehman Brothers' financial advisors, whereas we expect investors in Chico to be less informed about HGS because the underwriters employed no financial advisors in that area. It follows that we expect it relatively more likely there is IPO-quiet-period retail trading in Medford than in Chico. Consistent with this prediction, a retail investor in Medford bought shares of HGS during its quiet period, while none of the investors in Chico did. The purpose of this study is to examine whether this example generalizes to the larger sample of all IPOs and core-based statistical areas (CBSAs).

Of course, the example above – even if representative of our large sample – does not suggest that anyone violates IPO quiet period restrictions. First, it is possible that there is simply more retail trading activity in Medford than in Chico, e.g., perhaps there is more retail investing activity in cities whose financial advisor workforce is dominated by bulge bracket banks. We control for these differences by including CBSA fixed effects in all our regressions to account for differences in population, trading activity, and financial advisors in cities like Chico and Medford. Second, the quiet period does not ban all discussion about IPO firms: firms and their underwriters are allowed to make

statements of fact regarding business developments and answer questions regarding factual matters. Thus, there might be more information flow (the dissemination of simple factual information) to areas dominated by the IPO's underwriter even if there were no discussion of projections or opinions. According to this view, there should not be a relationship between the optimism of the underwriter – as revealed by their subsequent, post-quiet-period earnings forecasts – and the retail trading activity in local areas dominated by the underwriter's financial advisors. However, we find that the relation between quiet-period trading activity and an underwriter's presence in an area is significantly stronger when the underwriter's analysts later reveal themselves to be relatively optimistic about the IPO firm's long-term earnings growth. We therefore conclude that some of this optimism makes its way to investors during the quiet period, when underwriters are prohibited from sharing such opinions and projections.

Our paper contributes to several strands of literature. First, our paper adds to the broad literature on the importance of financial and social networks on information diffusion.[2] Perhaps more closely related to our work, we add to recent research that shows the importance of within-brokerage networks to facilitate information flow on the trading desk side of the broker (Di Maggio, Francesco, Kermani, Sommavilla, 2019), connections among analysts in the research department to produce better output (Bradley, Gokkaya, and Liu, 2020; Cohen, Frazzini, and Malloy, 2010) and networks among the investment banking division to facilitate information production in IPOs (Bajo, Chemmanur, Simonyan, and Tehranian, 2016). Our paper adds to this by showing that information likely flows through networks tied to brokers' financial advisors.

[2] Social networks are shown to be important in impacting behavior in various financial settings. For instance, Fracassi (2016) and Hoi, Wu, and Zhang (2019) find that executive networks are important in shaping corporate policies. Engelberg, Gao, and Parsons (2013) suggest that CEOs' networks impact their salary. Cai and Sevilir (2012) show that board connections are related to M&A returns. Hong, Kubik, and Stein (2005) and Pool, Stoffman, and Yonker (2015) find that social networks among institutional money managers influence their trades and holdings.

Second, we contribute to the literature on the IPO quiet period, which is unique because information from underwriters and insiders to financial markets is restricted by the SEC during a time when uncertainty regarding the newly public firm is extreme. Bradley, Jordan, and Ritter (2003, 2008) show that during the late 1990s and early 2000, underwriter analysts almost always issue favorable recommendations on the firms they took public immediately when the quiet period expires, which is the first time they are free to do so. More closely aligned to our work, Bushee, Cedergren, and Michels (2019) also examine retail trading during the quiet period. However, their focus is the role that media (which is not bound by quiet period restrictions) plays in drawing attention to retail investors for IPOs. Indeed, they find a significant relation between media coverage and retail purchases.[3] Similarly, our results suggest that information regarding an IPO filters from the broker to individual investors during the quiet period.

Finally, we contribute to the nascent literature on financial advisors. According to FINRA, there were 618,000 registered representatives in the U.S. in 2020 working for 3,435 firms. The vast majority are financial advisors who primarily face retail clients. Financial advisors thus represent a large proportion of the makeup of the financial services industry and are an important component of a brokerage firm's workforce. Yet, while there is extant literature on the influence of underwriters and analysts working for these brokerage firms, there is scant research on connecting these functions to the sizable workforce of financial advisors working at the same firm. Foerster, Linnainmaa, Melzer and Previtero (2017) examine the impact of financial advisors on retail investor portfolios and show they have considerable influence, but provide little tailoring to clients' characteristics. Linnainmaa, Melzer and Previtero (2021) show that advisors' personal investments match those largely of their clients suggesting that advisors do not knowingly push unsuitable investments to their clients. Our

[3] They identify retail trading with the TAQ database using the algorithm developed by Boehmer et al. (2021).

paper takes a different approach by focusing on how financial advisors disseminate information produced internally by their colleagues.

2. Institutional background and testable hypotheses

When firms go public, they typically choose an investment bank to purchase the shares of stock at some predetermined price, and the investment bank then sells the shares to the public, a process known as underwriting. In addition to underwriting, the investment bank also assists with the preparation of registration statements that are filed with the SEC. An important part of the registration statement is the prospectus, which provides information regarding "the company's business, financial condition, management and other matters that are key to deciding whether the offering is a good investment."[4]

During the registration and underwriting process, there are several ways that conflicts of interest can lead to a distorted and/or unlevel information environment. First, there might be a quid pro quo agreement whereby the investment bank offers overly optimistic equity research in exchange for winning the firm's underwriting business. Relatedly, the investment bank might attempt to influence the share price of the firm immediately after it goes public by convincing their clients and the general public to buy shares of the stock before the market can settle on an equilibrium price that accurately reflects the firm's fundamental value. Finally, the firm might selectively disclose important details about the firm's financial condition that are not described in the IPO prospectus, creating an unlevel information field. For all these reasons, there is a so-called IPO "quiet period" that lasts for roughly a month after the firm's shares are available to the public. Currently, the quiet period lasts 40 calendar days past the IPO date but it was 25 days during our sample period.

[4] Source: https://www.sec.gov/files/ipo-investorbulletin.pdf

The SEC states that it is not feasible for them to "promulgate an all inclusive list of permissible and prohibited activities" during the IPO quiet period, but they mandate that issuers and their representatives "should not initiate publicity when in registration" and that "care should be exercised so that, for example, predictions, projections, forecasts, estimates and opinions concerning value are not given with respect to such things, among other, as sales and earnings and value of the issuer's securities." (SEC release 33-5180).[5] Consistent with the SEC's mandate, FINRA rule 2241.b.1 specifies that each member institution must "establish, maintain and enforce written policies and procedures reasonably designed to identify and effectively manage conflicts of interest related to" equity research reports, public appearances by analysts, and "the interaction between research analysts and those outside of the research department, including investment banking and sales and trading personnel, subject companies and customers."

While it is easy for regulators to observe whether financial institutions publish equity research during the IPO quiet period, it is far more difficult to determine if they informally share "opinions concerning value" (SEC release 33-5180). It is also difficult to know if they effectively manage potential conflicts of interest regarding the "interaction between research analysts and those outside of the research department, including investment banking and sales and trading personnel" during this period (FINRA rule 2241.b.1).

We hypothesize that conflicts of interest affect investment banks' activity in ways that are difficult for regulators to observe. For example, during the quiet period, they will likely refrain from issuing equity research during the quiet period (which is easy for regulators to observe) but they might disseminate predictions and opinions about IPO firms' value to their financial advisors, who might then pass this information along to their retail investor clientele. If this occurs, we further expect that

[5] The 2012 JOBS Act has reduced many of the quiet period restrictions for so-called "emerging growth companies" which have annual revenues of $1 billion or less.

the information flows to the full-service brokers' friends and family who have accounts at large discount brokerages. Thus, we hypothesize:

H1: *There will be a positive relation between retail investor trading activity in a given area during the IPO quiet period and the percentage of the area's financial advisors who work for one of the IPO underwriters.*

Moreover, even though SEC release 33-5180 prohibits banks from sharing "predictions, projections, forecasts, estimates and opinions concerning value" of things such as the IPO firm's sales, earnings and the value of the IPO stock, we hypothesize that such information will indirectly flow to the financial advisors, which will then flow to their retail investor clients and their friends and family. In other words, we expect that when the analysts are most optimistic about the IPO firm (or at least, want the public to be optimistic about it), the empirical support for H1 will be stronger. Thus, we hypothesize:

H2: *The effect described in hypothesis H1 is stronger when the underwriters' initial long-run earnings growth forecasts are relatively optimistic than when they are relatively pessimistic.*

3. Data

3.1. IPO data

Our sample of IPOs comes from the Securities Data Corporation (SDC). We consider the sample period January 1, 1991 – October 31, 1996, because the retail trading data (described below) ends in November 1996, and we analyze the retail trading activity in the 25 days following each IPO. We collect information regarding the IPOs' zip code, proceeds, underwriters, and bookrunners. To remain in our sample, we require that we can merge the IPO to CRSP and IBES and that we can estimate the latitude and longitude of the zip code using a walkthrough provided by the US Census

Bureau.[6] After applying these filters, our IPO sample consists of 1,795 US IPOs with average proceeds of $33.0 million and 2.5 underwriters. We report the 10th, 50th, and 90th percentiles of these variables and the degree to which the IPO is underpriced in Panel A of Table 1.

[Insert Table 1 here]

3.2. Financial advisor data

Our data on financial advisors' locations comes from historical filings of the Uniform Application for Securities Industry Registration or Transfer (Form U4), which provides detailed accounts of financial advisor registrations and work histories including the street address of office location. The Form U4 is filed by an employer when the advisor joins the firm and must be updated upon material changes such as changing jobs. The Form U4 data are aggregated in a database called the Central Registration Depository (CRD), which is jointly operated by FINRA and state securities regulators.

We obtain Form U4 data from a series of Freedom of Information Act requests to state regulators. Our universe of financial advisors consists of those registered in any of the states that respond to our requests during some point of their career. Advisors may register in multiple states, so we have data for many advisors in the states that do not supply information. We do not observe location data only for those advisors who never register in a reporting state. Although our sample of office locations is not comprehensive, it covers all major financial centers and populous states. As the selection mechanism for inclusion into the sample is a state regulator's interpretation of The Privacy Act of 1974 as it relates to the FOIA request, it is unlikely selection would systematically bias the

[6] Source: https://www2.census.gov/geo/docs/maps-data/data/gazetteer/2023_Gazetteer/2023_Gaz_zcta_national.zip

correlation between our variables of interest which are time-varying within core-based statistical areas (CBSAs). We then use this geographic employment data to determine the composition of financial advisor affiliations in each CBSA.

We report summary statistics for this sample of 148 financial institutions in Panel B of Table 1.

3.3. Retail investor data

Our retail investor data come from a large discount brokerage and is the same dataset that is analyzed by Barber and Odean (2000). We restrict attention to accounts that match to households who have zip codes that can be matched to a CBSA using a walkthrough provided by the US Census Bureau.[7]

3.4. Analyst earnings forecasts

Analyst earnings forecasts come from I/B/E/S. To match the analysts to brokerage filings, we use an old vintage of the detailed recommendation file that contains brokers' names along with the first initial and last name of the analyst. When matching, we use a combination of name and career history. Initially, we link firm identifiers between the Form U4 and IBES database. We then match analysts using a combination of first initial, last name, and career history. We eliminate observations with ambiguous information in the name fields such as missing or team reports. As career history is available in both databases, we can use the timing of job switches to match when name strings are ambiguous (e.g., "L. Smith" appears in both databases, however only one "L. Smith" in each database

[7] Source: https://www2.census.gov/geo/docs/maps-data/data/rel/zcta_cbsa_rel_10.txt. Note that some zip codes can cover relatively wide geographic areas, some of which belong to one CBSA and others which belong to another CBSA. In other words, in some instances, a single zip code can map to more than one CBSA. When aggregating to the CBSA level, we keep these households, i.e., if a household's zip code maps to two different CBSAs, its trading activity will be counted towards both of the CBSA's trading activity.

moves from Citigroup to Sutro & Co. in 1998 allowing an unique match). Finally, we verify matches, by validating their career start and end dates and FINRA licensing required of analysts.[8]

3.5. Sample construction

As described in Section 2.3 ("Retail investor data"), we use US census data to map the zip codes in the discount brokerage dataset to CBSAs. This mapping results in 365 distinct CBSAs with households in the discount brokerage dataset. The median CBSA has 189,415 people based on the 1990 census and it has 21 households with an account in the discount brokerage dataset. We report additional statistics from these variables' distributions in Panel C of Table 1.

To conduct our analysis, we consider the sample consisting of the 655,175 distinct CBSA-IPO (=365 CBSAs × 1,795 IPOs) pairs. For each CBSA-IPO pair, we examine how many buy transactions occur in the given CBSA for the given IPO during the quiet period, the dollar value of these purchases, and the number of distinct households that make these purchases. Recalling that the median CBSA only has 21 households with an account in the discount brokerage dataset, it is not surprising that there is generally very little buying activity in this setting: the average observation has 0.0054 quiet-period buy transactions for IPO stocks for a value of $49.44 purchased by 0.0047 distinct households.

For each IPO *i* and CBSA *j*, we construct a measure of the IPO underwriters' share of the financial advisor workforce in CBSA *j* as follows:

$$Underwriter\ local\ advisors_{ij} = \frac{A_{ij}}{N_{ij}}, \qquad (1)$$

[8] Research analysts must pass either the Series 86 or Series 87 exam to register as a research analyst. Passing these exams allow the analyst to prepare written or electronic communications that analyze equity securities, companies, and industry sectors.

where A_{ij} is the number of financial advisors in CBSA *j* who work for a brokerage that participated as an underwriter in IPO *i*, and N_{ij} is the number of financial advisors employed in CBSA *j* in the year of the IPO. Conceptually, $Underwriter\ local\ advisors_{ij}$ represents the likelihood that a financial advisor, drawn at random from the local geographic area *j* at the time of IPO *i*, works for a brokerage that served as an underwriter in the IPO. According to Hypothesis H1, there should be a positive relation between *Underwriter local advisors* and the level of abnormal retail investor buying activity during the IPO quiet period.

Additional distributional statistics of these variables are reported in Panel D of Table 1.

4. Methodology and results

4.1. Methodology

According to Hypothesis H1, there should be a positive association between retail investor buying activity in an IPO during its quiet period and the percentage of the area's financial advisors who work for one of the IPO underwriters. To test this, we estimate regressions of the form:

$$Local\ quiet\text{-}period\text{-}buying\ activity_{ij} = \beta_1\, Underwriter\ local\ advisors_{ij} + \beta_2\, Distance_{ij} + \lambda_i + \gamma_j + \varepsilon_{ij}. \quad (2)$$

Our three measures of retail investors' buying activity during the quiet period are the first three variables described in Table 1, Panel D, namely, the number of buy transactions, the total dollar value of the purchases, and the number of distinct households who purchased the IPO stock in the given CBSA during the IPO stock's quiet period. Our independent variable of interest, *Underwriter local advisors*, is defined in (1) and summarized in Table 1, Panel D.

When analyzing buying activity in a CBSA for an IPO firm, it is important to control for the distance between the CBSA and the IPO because investors disproportionately own stocks that are

headquartered close to them, a phenomenon known as the local bias (Ivkovic and Weisbenner, 2005). Thus, we include a control variable equaling the distance (in miles) between the CBSA and the IPO firm's headquarters.

It is also important to control for characteristics of the IPO and the CBSA. Obviously, there is more trading activity in New York City than Chico, CA, or Medford, OR. Rather than including variables such as the size of the CBSA, the number of households in that area have who accounts with the large discount brokerage, etc., we choose to include CBSA fixed effects in all our regressions to account for such heterogeneity (both observable and unobservable) across CBSAs. Similarly, there are many characteristics of an IPO that should affect the level of trading in any given CBSA: its size, the public attention it receives, etc. Rather than including variables such as these, we include IPO fixed effects in all regressions to account for the heterogeneity in IPOs. Note that these IPO fixed effects also control for the collection of underwriters who participate in the IPOs; e.g., if IPOs underwritten by Goldman Sachs generally receive a lot of attention and retail trading volume, such an effect would be captured by the IPO fixed effects.

4.2. Baseline results

To ease the interpretation of economic magnitudes, when running regressions, we normalize the independent variables of interest to have 0 mean and unit standard deviation.[9] In column 1 of Table 2, we see that a one-standard deviation increase in an area's *Underwriter local advisors* for a given IPO is associated with a 0.0010 increase in the number of buy transactions for the IPO stock within that local area during the IPO quiet period, which is highly statistically significant (t = 4.71). The coefficient is similar in magnitude to the coefficient of *Distance CBSA to HQ (hundreds of miles)*, indicating that a one-standard deviation increase in *Underwriter local advisors* is comparable to a 100-mile

[9] As we discuss in its caption, we do not normalize any of the independent variables in Table 7.

reduction in the distance between the CBSA and IPO firm's headquarters. While the economic magnitude might seem small, it is worth recalling that the sample consists of many small IPOs and small CBSAs. In addition, our data on retail investor trading comes from just a single discount brokerage and does not capture any of the retail trading placed through the large financial institutions who employ the financial advisors in the CBSA. Presumably, if we could observe such trading, the effects would be much larger. Thus, comparing the effects to the mean of the dependent variable likely provides a more accurate depiction of the economic magnitudes. Consistent with the idea that our sample contains many CBSAs and IPOs (many of which are small), the average number of quiet-period-buy transactions is just 0.0054 across our entire sample of 655,175 IPO-CBSA pairs.[10] Thus, the 0.0010 increase in buy transactions represents an 18.5% increase relative to its mean.

[Insert Table 2 here]

In column 2, we consider the total value of the IPO's stock purchased by all retail investors in the CBSA during the quiet period. We find that a one-standard deviation increase in the proportion of the area's advisors that are employed by one of the IPO's underwriters is associated with a $5.35 increase, which represents a 10.8% increase relative to its mean ($49.44).

In column 3, we consider the number of distinct households in the CBSA who purchase the IPO stock during the quiet period. The estimated effect (0.0009 households) represents a 19% increase relative to its mean (0.0047 households).

Often, an IPO will have multiple underwriters. The lead underwriter who organizes the process is known as the "bookrunner." Thus far, we have not made any distinction between the bookrunner and the non-lead underwriters. To examine whether our baseline effects differ between

[10] We will later restrict attention to large CBSAs and large IPOs in Table 3

bookrunners and non-lead underwriters, we define the variable *Bookrunner local advisors* to equal (prior to normalization) the proportion of the CBSA's local advisor workforce who works for the IPO's bookrunner. Because the bookrunner's advisor are also counted towards the *Underwriter local advisor* measure, the variable *Bookrunner local advisors* can be interpreted as the incremental impact of having the bookrunner's advisors in an area relative to having one of the non-bookrunner's advisors in the area, much like an interaction term. In columns 4-6, we see that the coefficient of *Bookrunner local advisors* is small and insignificant, suggesting that there is not a significant difference in the effect of a bookrunner's local advisor workforce compared to the non-lead underwriters. Thus, for the remainder of the paper, we will ignore the distinction between the bookrunner and non-lead underwriters.

We next consider how the strength of the relation varies across CBSA and IPO characteristics. In Panel A of Table 3, we consider large versus small CBSAs, where large (small) CBSAs are defined as those within (outside) the top 25 based on their 1990 population. Among the largest CBSAs, we find an economically and statistically significant relation between *Underwriter local advisors* and all three measures of retail investor buying activity during the quiet period. When we restrict attention to the non-largest CBSAs, we find an economically and statistically significant relation between *Underwriter local advisors* and our two measures of trade frequency (the number of distinct buy transactions and the number of distinct households buying the stock), but an insignificant relation between it and the total dollars of the IPO stock that are purchased during the quiet period. These results are reported in Panel A of Table 3.

[Insert Table 3 here]

In Panel B, we consider large versus small IPOs where an IPO is considered large (small) if its proceeds are greater (less) than the median proceeds for the given year. We find that the effects of *Underwriter local advisors* on local buying activity is stronger economically and statistically for small IPOs.

In Panel C, we run the regressions separately for hot and cold IPOs, where an IPO is classified as hot (cold) if its level of underpricing is greater (less) than 7.5%, which is the median level of underpricing in our sample period.[11] We find that the effects are stronger among hot IPOs than cold ones.

Overall, the results from Table 3 suggest that the relation between an area's *Underwriter local advisors* and its level of retail buying activity in the IPO stock is robust, although the effect seems strongest among small IPOs that are significantly underpriced.

Having confirmed our first prediction of a positive association between retail investor buying activity in an IPO during its quiet period and the percentage of the area's financial advisors who work for one of the IPO underwriters, we now examine whether this effect is related to the underwriter's optimism about the IPO. For the underwriters who publish equity research following the quiet period, we can observe their level of optimism for the IPO stock by observing their long-term earnings growth forecasts. FINRA rule 2241.b.1 requires each member institution to establish policies and procedures to manage the potential conflicts of interest associated with the interaction between research analysts and sales and trading personnel, so these sorts of projections should arguably be kept from the underwriter's financial advisors during the quiet period. Moreover, even if information regarding the projections makes its way to the financial advisors, according to the quiet period restrictions, the information should not be shared with the advisors' investor clients until after the quiet period (SEC release 33-5180). Despite these restrictions, we hypothesize that the relation between an area's buying activity in an IPO quiet period and the proportion of the area's advisor workforce that is employed

[11] Underpricing is defined as the closing price on the first day of trading divided by the IPO offer price, minus 1.

by the underwriter will be strongest when the underwriters' initial long-run earnings growth forecasts (which are not published until after the quiet period) are relatively optimistic.

We define the variable *Underwriter LA with EPS forecasts* to equal the proportion of an area's advisor workforce who works for one of the IPO underwriters that issue an earnings forecast in the 90 days following the end of the quiet period. Some of the forecasts in IBES contain projections for the long-run earnings growth. We classify such a forecast as optimistic (pessimistic) if it is above (below) the average forecast for all IPOs in the given industry and year. The variable *Underwriter LA with optimistic LTG forecasts* (*Underwriter LA with pessimistic LTG forecasts*) is defined as the proportion of a local area's financial advisor workforce that is employed by one of IPO's underwriters that issues an optimistic (pessimistic) long-term earnings growth forecast for the IPO firm. The variable *Underwriter LA with LTG EPS forecasts* is the proportion of the area's advisor workforce that is employed by one of the IPO's underwriters who issue a long-term earnings growth forecast that can be either optimistic or pessimistic; prior to normalization, this variable equals the sum of *Underwriter LA with optimistic LTG forecasts* and *Underwriter LA with pessimistic LTG forecasts*.

In column 1 of Table 4, we regress the number of buy transactions in a CBSA during the IPO's quiet period onto *Underwriter local advisors*, *Underwriter LA with EPS forecasts*, *Underwriter LA with optimistic LTG forecasts*, *Underwriter LA with pessimistic LTG forecasts*, and controls. We find that the coefficient of *Underwriter local advisors*, which captures the effect of quiet-period exposure to financial advisors who work for an underwriter who does not publish equity research in the 90 days following the quiet period, is insignificant. Presumably, these 57.4% of underwriters who choose not to cover the IPO stock are relatively pessimistic about the stock's future prospects, so the fact that there is no effect here is consistent with underwriters' opinions reaching investors in the quiet period through the underwriter's financial advisor workforce. The coefficient of *Underwriter LA with EPS forecasts* is positive and significant, which is consistent with information flow during the quiet period and the idea

that the 42.6% of underwriters who choose to cover a firm immediately after the quiet period are relatively optimistic about the IPO. Finally, within this group of relatively optimistic institutions that initiate coverage, we can further sort them on their relative optimism by comparing the magnitude of their forecasts for the IPO firm's long-run earnings growth. The coefficient of *Underwriter LA with optimistic LTG forecasts* is positive and significant, while the coefficient of *Underwriter LA with pessimistic LTG forecasts* is insignificant and close to 0, consistent with H2.

[Insert Table 4 here]

In column 2, we replace the variable *Underwriter LA with pessimistic LTG forecasts* with *Underwriter LA with LTG EPS forecasts*. In this specification, the coefficient of *Underwriter LA with optimistic LTG forecasts* captures the incremental effect of exposure to an underwriter whose analysts will soon issue optimistic long-term growth forecasts compared to exposure to underwriters whose analysts will soon issue relatively pessimistic long-term growth forecasts. The coefficient of is positive (0.0008) and significant (t = 2.45), revealing that the difference between the optimistic and pessimistic coefficients in column 1 significant, consistent with our prediction.

In columns 3-4 we use the total dollar value of IPO stock purchased as our measure of a CBSA's buying activity during the quiet period, and in columns 5-6, we use the number of distinct households who purchase the IPO stock. The results in these regressions are qualitatively similar to our findings in columns 1-2: the coefficient of *Underwriter local advisors*, which captures the effect of exposure to advisors who work for underwriters who do not initiate coverage immediately after the quiet (and are therefore presumably relatively pessimistic), is close to 0, while the coefficient of *Underwriter LA with EPS forecasts*, which captures the effect of exposure to advisors who work for underwriters who initiate coverage (and are therefore likely relatively optimistic) is significantly

positive. Further distinguishing underwriters based on their level of optimism, we find that the coefficient of *Underwriter LA with optimistic LTG forecasts* is positive and significantly greater than the coefficient of *Underwriter LA with pessimistic LTG forecasts*, which further supports the idea that an underwriter's *opinions* about an IPO stock (not just neutral facts about the IPO firm) reach retail investors through the underwriter's advisor workforce.

Taken together, these findings strongly suggest that the relation between an area's buying activity in an IPO quiet period and the proportion of the area's advisor workforce that is employed by the underwriter is strongest when the underwriter is optimistic about the IPO, consistent with H2.

4.3 Effects of underwriter local advisors on longer-run holdings

In the prior section, we confirmed our prediction that the variable *Underwriter local advisors* is associated with elevated levels of retail buying in IPO stocks during the quiet period. We now consider its effect on ownership in IPO stocks in the longer run. While it is natural to expect the effects to persist, it is also possible that an underwriter's advisor workforce merely speeds up the information flow to the local area and that in the months following the quiet period, there would be no discernable effects.

To address this, we consider two measures of ownership -- the number of households in an area owning the stock and the total dollar value of the area's stock holdings – and four different horizons (3, 6, 9, and 12 months after the IPO date). As before, our independent variable of interest is *Underwriter local advisors*, we control for the distance between the CBSA and the IPO firm's headquarters, and we use fixed effects to control for heterogeneity in IPOs and CBSAs. In column 1, we find that a one-standard deviation increase in an area's *Underwriter local advisors* is associated with an increase in 0.0010 households who own the IPO stock 3 months after the IPO, representing a 19.6%

increase relative to the mean of the dependent variable. This effect is also highly statistically significant ($t = 5.73$).

[Insert Table 5 here]

In columns 2-4, we see that the coefficient of *Underwriter local advisors* remains economically and statistically significant 3, 6, and 12 months after the IPO. Moreover, the coefficient grows at a faster rate than the growth of the mean of the dependent variable. Specifically, as we go from 3 to 12 months after the IPO, the coefficient of *Underwriter local advisors* grows by 160% (= 0.0026/0.0010 − 1), while the mean of the dependent variable only rises by 114% (= 0.0109/0.0051 −1). These findings are consistent with the idea that stock-related information gradually diffuses across a local population through social interactions and that an area's financial advisor workforce can increase this effect.

In columns 5-8, we find similar pattens with the total value of a CBSA's ownership of an IPO stock. Overall, our results suggest that exposure to an IPO underwriter's financial advisors has longer-run effects on retail ownership in the IPO stock that persist long past the quiet period.

4.4 Interaction with the local bias

Prior researchers have documented that investors exhibit a "local bias" in that they tend to invest in stocks that are headquartered close to them. One might expect the geographic dispersion of an underwriter's financial advisor workforce might mitigate the local bias—by having access to a financial advisor who works for one of the IPO underwriters, one might learn about IPO stocks that are headquartered far away. For example, in the introduction we discussed an investor in Medford, Oregon, who bought shares of HGS, an IPO firm headquartered thousands of miles away in Rockville,

Maryland. By being exposed to advisors who work for HGS's underwriter (and/or those advisors' investor clients), perhaps the investor became familiar with HGS much like she became familiar with local stocks headquartered near Medford. Conversely, it is plausible that the effect of *Underwriter local advisors* exacerbates the local bias, e.g., perhaps advisors are more likely to learn about and disseminate their firm's opinions about local IPOs.

To examine this, we take our baseline regressions reported in columns 2-3 of Table 2 and include the interaction *Underwriter LA* × *Distance*, which is simply the product of the other two variables.[12] We find that this interaction is significantly negative, consistent with the idea that *Underwriter local advisors* exacerbate the local bias and inconsistent with the notion that these advisors mitigate the local bias.

[Insert Table 6 here]

In columns 3-5 and 6-8 we consider the number of distinct households who own the IPO stock and the total value of IPO owned by retail investors in the CBSA, respectively. The horizons we consider are 3, 6, and 12 months after the IPO. In all these regressions, we find that the interaction term is negative and significant, which again is consistent with the idea that *Underwriter local advisors* exacerbate the local bias.

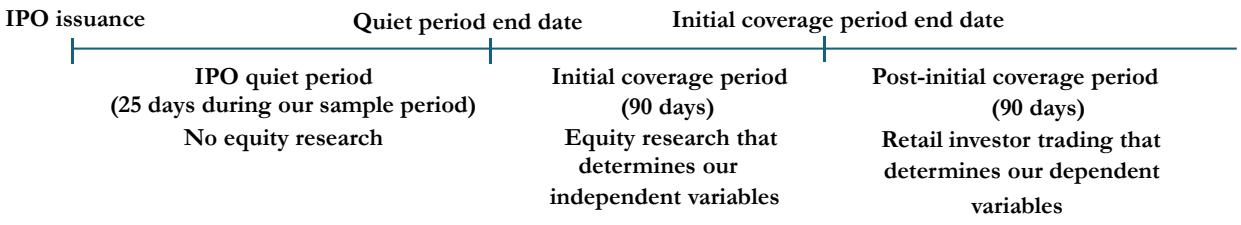
4.5 The effect of analyst equity research and broker geography on retail investor behavior

After the quiet period, analysts are free to publish equity research containing buy or sell recommendations, projections of the firm's future financial performance, and price targets indicating

[12] The result for the regression in column 1 of Table 2 is very similar and is omitted due to formatting constraints. It is available upon request.

the analyst's assessment of the stock's fundamental value. In this section, we examine if and how these recommendations affect the geography of retail investor ownership in stocks.

We classify the 90 days after the end of the quiet period as the "initial coverage period" and the 90 days following that as the "post-initial coverage period." We will examine a CBSA's retail investor trading activity in the post-initial coverage period as a function of the retail trading in the CBSA during the IPO quiet period and the CBSA's exposure to analyst equity research as proxied by the number of financial advisors in the CBSA who work for a brokerage that issues equity research for the IPO firm in the initial coverage period. The figure below illustrates the timeline.



Given the timeline above, we need roughly 7 months after the IPO issue date to examine the trading activity in the post-initial coverage period. Because the discount brokerage data ends in November 1996, we restrict attention to IPOs that occur before April 30, 1996, for this analysis.

We define *Local underwriter coverage (EPS)* as the proportion of the CBSA's advisors who work for one of the IPO's underwriters who issue an earnings forecast during the initial coverage period, and *Local non-underwriter coverage (EPS)* is defined analogously for the advisors working for non-underwriters of the IPO. Local coverage (EPS) is the proportion of the CBSA's advisors who work for a brokerage that issues a forecast for the IPO firm during the initial coverage period and it equals the sum of *Local underwriter coverage (EPS)* and *Local non-underwriter coverage (EPS)*. In each regression, we control for the value of the dependent variable during the IPO quiet period as well as the distance

between the CBSA and the IPO firm's headquarters. We continue controlling for IPO and CBSA heterogeneity by including IPO and CBSA fixed effects.

We begin by considering the number of buy transactions in a CBSA for an IPO stock during the post-initial coverage period. Regarding our control variables, the level of buy transactions during the quiet period (*Num buy transactions (quiet)*) is positive and highly significant, and the distance between the CBSA and the IPO firm's headquarters is negative and highly significant, as expected. Regarding our independent variables of interest, the effect of local advisors working for an underwriter that covers the IPO firm (*Local underwriter coverage (EPS)*) is positive but insignificant.[13] However, the effect of exposure to advisors working for a non-underwriter who covers the firm is positive and significant. These results are reported in column 1 of Table 7, Panel A.

[Insert Table 7 here]

In column 2, we replace the variable *Local underwriter coverage (EPS)* with *Local coverage (EPS)* so that the coefficient of *Local non-underwriter coverage (EPS)* captures the difference between the effect of exposure to advisors who work for a non-underwriter who works for a brokerage covering the firm and advisors who work for an underwriter covering the firm. We see that this incremental effect (0.0165, which can also be inferred from the difference in the coefficients in column 1) is statistically significant at the 10% level ($t = 1.81$). While this result might seem puzzling, it is consistent with the idea that the underwriter's assessment of the IPO firm reaches a local area during the quiet period, whereas the non-underwriters' assessments do not reach the CBSA until analyst coverage is initiated in the initial coverage period.

[13] Note that in contrast to the previous tables, the independent variables of interest are not normalized to have 0 mean and unit standard deviation, so the magnitudes of the coefficients are not comparable to those in the other tables. We do not normalize these variables to ease the interpretation of the variables in the even-numbered regressions in this table.

In columns 3-6, we repeat these regressions using the alternative measures of local retail investor buying activity, and the results are qualitatively similar in that exposure to advisors who work for a non-underwriter that covers the firm has a stronger effect on local retail buying than exposure to an underwrite that covers the firm.

In Panel B, we consider coverage based on the IBES recommendation data which does not begin until January 1994. Thus, the sample in these regressions is considerably smaller than the sample in Panel A. Nevertheless, the results are qualitatively similar to those found in Panel A.

Overall, the results reported in Table 7 reveal that the geography of a brokerage's financial advisor workforce affects the geography of retail investor holdings through the brokerage's equity research.

5. Conclusion

When firms go public, there is a quiet period that lasts 40 days after the IPO. During this time, the IPO firm and its underwriters are prohibited from providing predictions, projections, forecasts, or estimates for the firm's future performance and from sharing their opinions about the value of the firm's stock. Although these restrictions have existed for many decades, to the best of our knowledge, we are the first to examine whether financial institutions abide by them. We do so by comparing the geography of retail investor activity during the IPO quiet period and the geography of financial institutions' advisor workforce. Specifically, we document that a one-standard deviation increase in the percentage of an area's financial advisors who are employed by an underwriter is associated with a 18.5% increase in the area's buying activity during the quiet period. Further, we show that this relation is increasing in the optimism of the underwriter's equity research, which is not published until after the quiet period has ended. We thus conclude that some of this optimism makes its way to the

local retail investors through the underwriter's financial advisor workforce, in violation of the quiet period restrictions.

An obvious concern with this type of empirical analysis is endogeneity – perhaps there is an important omitted variable that has a causal effect on the level of retail buying activity during the IPO. We control for the well-documented local bias by including the distance between each area and the IPO firm's headquarters in all our regressions, and we control for unobservable characteristics of each IPO and each local area by including both IPO and CBSA fixed effects in all our regressions. With this stringent set of controls, it seems unlikely that there is an omitted variable that causally affects the level of quiet-period-buying activity in a given locale, which allows us to interpret the coefficient of interest as a causal effect.

In the time since our 1991-1996 sample period, some of the specifics about IPO quiet-period regulations have changed. The regulations were strengthened in the early 2000's when the quiet period was extended from 25 days to 40 days past the IPO date, and it was loosened for "emerging growth companies" (EGCs) by the 2012 JOBS Act, which explicitly allows investment banks to publish and distribute research reports about such firms throughout the registration process. Nevertheless, quiet period restrictions remain in effect for non-EGC IPOs, and the question of whether there should be an IPO quiet period is beyond the scope of this paper. Our paper suggests that regardless of whether the quiet period is warranted or not, financial institutions appear to violate its restrictions because their analysts' opinions about an IPO firm seem make their way to retail investors during the quiet period before the equity research is published.

REFERENCES

Altinkilic, Oya, and Robert Hansen, 2009, On the information role of stock recommendation revisions, *Journal of Accounting and Economics* 48(1), 17-36.

Bajo, Emanuele, Thomas Chemmanur, Karen Simonyan, and Hassan Tehranian, *Journal of Financial Economics* 122(2), 376-408.

Barber, Brad, and Terrance Odean, 2000, Trading Is Hazardous to Your Wealth: The Common Stock Investment Performance of Individual Investors, *Journal of Finance* 55(2), 773-806.

Bradley, Daniel, Sinan Gokkaya, and Xi Liu, 2020, Ties That Bind: The Value of Professional Connections to Sell-Side Analysts, *Management Science* 66(9), 3799-4358.

Cai, Ye, and MerihSevilir, 2012, Board connections and M&A transactions, *Journal of Financial Economics* 103(2), 327-349.

Cohen, Lauren, Andrea Frazzini, and Christopher Malloy, 2010, Sell-Side School Ties, *Journal of Finance* 65(4), 1409-1437.

Dhar, Ravi, and Ning Zhu, 2006, Up Close and Personal: Investor Sophistication and the Disposition Effect, *Management Science* 52(5), 726-740.

Di Maggio, Marco, Francesco Franzoni, Amir Kermani, and Carlo Sommavilla, 2019, The relevance of broker networks for information diffusion in the stock market, *Journal of Financial Economics* 134(2), 419-446.

Easley, David, and Maureen O'hara, 2004, Information and the Cost of Capital, *Journal of Finance* 59(4), 1553-1583.

Engelberg, Joseph, Pengjie Gao, and Christopher Parsons, 2013, The Price of a CEO's Rolodex, *Review of Financial Studies* 26(1), 79-114.

Engelberg, Joseph, and Christopher Parsons, 2011, The Causal Impact of Media in Financial Markets, *Journal of Finance* 66(1), 67-97.

Foerster, Stephen, Juhani Linnainmaa, Brian Melzer, and Alessandro Previtero, 2017, Retail Financial Advice: Does One Size Fit All? *Journal of Finance* 72(4), 1441-1482.

Fracassi, Cesare, 2016, Corporate Finance Policies and Social Networks, *Management Science* 63(8), 2420-2438.

Hoi, Chun Keung, Qiang Wu, and Hao Zhang, 2019, Does social capital mitigate agency problems? Evidence from Chief Executive Officer (CEO) compensation, *Journal of Financial Economics* 133(2), 498-519.

Hong, Harrison, Jeffrey Kubik, and Jeremy Stein, 2005, Thy Neighbor's Portfolio: Word-of-Mouth Effects in the Holdings and Trades of Money Manager, *Journal of Finance* 60(6), 2801-2824.

Ivkovic, Zoran, and Scott Weisbenner, 2005, Local Does as Local Is: Information Content of the Geography of Individual Investors' Common Stock Investments, *Journal of Finance* 60(1), 267-306.

Kelly, Bryan, and Alexander Ljungqvist, 2012, Testing Asymmetric-Information Asset Pricing Models, *Review of Financial Studies* 25(5), 1366-1413.

Linnainmaa, Juhani, Brian Melzer, and Alessandro Previtero, 2021, The Misguided Beliefs of Financial Advisors, *Journal of Finance* 76(2), 587-621.

Merton, Robert, 1987, A Simple Model of Capital Market Equilibrium with Incomplete Information, *Journal of Finance* 42(3), 483-510.

Pool, Veronika, Noah Stoffman, and Scott Yonker, 2015, The People in Your Neighborhood: Social Interactions and Mutual Fund Portfolios, *Journal of Finance* 70(6), 2679-2732.

Womack, Kent, 1996, Do Brokerage Analysts' Recommendations Have Investment Value? *Journal of Finance* 51(1), 137-167.

Table 1: Summary statistics

In Panel A, we report the mean, standard deviation, 10th, 50th, and 90th percentile for several variables defined on our sample of 1,795 IPOs from January, 1991, through October, 1996. Underpricing is defined as the closing price on the first day divided by the offer price, minus 1. In Panel B, we report statistics for the financial advisor workforce data. There are 148 institutions in this sample, and for each of these institutions, we compute the average annual number of advisors they employ, the number of CBSAs in which they have advisors, and the number of IPOs they underwrite. We then report the distribution of these annual averages across the sample of 148 institutions. In Panel C, we report the distribution of the population and the number of households in the discount brokerage dataset for the 365 CBSAs in our sample. In Panel D, we report

	Mean	Std	p10	p50	p90
Panel A: IPO data (N = 1,795)					
IPO proceeds (millions)	32.99	41.61	6.8	24	61.2
Num underwriters	2.51	3.09	1	2	4
Underpricing	13.93%	20.97%	-1.32%	7.69%	38.89%
Panel B: Advisor data (N =148)					
Num advisors / year	395.35	1212.24	2	42.5	740
Num CBSAs / year	17.88	40.38	1	3	43
Num IPOs / year	4.47	6.83	1	1	13
Panel C: CBSA data (N = 365)					
1990 population	564,451	1,338,275	88,979	189,415	1,207,070
Num households (retail)	128	427	6	21	250
Panel D: CBSA x IPOs (N = 655,175)					
Num buys (quiet)	0.0054	0.1413	0	0	0
Dollar value of buys (quiet period)	$49.44	$2,164.26	0	0	0
Num households buying (quiet period)	0.0047	0.1215	0	0	0
Underwriter local advisors	0.0344	0.1171	0	0	0.1

Table 2: Local underwriter workforce and retail investor stock-buying activity during the quiet period

This table reports estimates from panel regressions of the form:

$$Local\ quiet\text{-}period\text{-}buying\ activity_{ij} = \beta_1\, Underwriter\ local\ advisors_{ij} + \beta_2\, Distance_{ij} + \lambda_i + \gamma_j + \varepsilon_{ij}.$$

The sample consists of all (IPO, CBSA) pairs (*i*,*j*). We consider three different measures of quiet-period-buying activity by retail investors in CBSA *j*: the number of buy transactions for IPO stock *i* in CBSA *j* during the quiet period (columns 1 and 4), the total dollar value of the buy transactions for IPO stock *i* in CBSA *j* during the quiet period (columns 2 and 5), and the number of distinct households that purchase shares of IPO stock *i* in CBSA *j* during the quiet period (columns 3 and 6). *Underwriter local advisors* is defined as the proportion of financial advisors in CBSA *j* who work for a brokerage that participated as an underwriter in IPO *i*, and it is normalized to have 0 mean and unit standard deviation. *Distance CBSA to HQ (hundreds of miles)* is the distance, in hundreds of miles, between the CBSA and the firm's headquarters. In columns 4-6, we augment the regression equation to include the variable *Bookrunner local advisors*, defined as the proportion of financial advisors in CBSA *j* who work for the brokerage that served as the bookrunner in IPO *i*, and it is normalized to have 0 mean and unit standard deviation. Note that the IPO bookrunner is always an underwriter, so the coefficient of the bookrunner variable captures the incremental effect of the local bookrunner workforce relative to local non-bookrunner underwriters. The regressions include IPO and CBSA fixed effects, represented by the terms λ_i and γ_j, respectively. Standard errors are clustered by IPO, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Num buys	Value ($)	Households	Num buys	Value ($)	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Underwriter local advisors	0.0010***	5.3475***	0.0009***	0.0010***	3.6407	0.0009***
	(4.71)	(2.62)	(4.78)	(4.33)	(1.60)	(4.37)
Bookrunner local advisors				0.0001	2.4202	0.0001
				(0.55)	(1.14)	(0.62)
Distance CBSA to HQ (hundreds of miles)	-0.0009***	-9.1680***	-0.0008***	-0.0009***	-9.1678***	-0.0008***
	(-8.61)	(-4.84)	(-8.53)	(-8.61)	(-4.84)	(-8.53)
Observations	655,175	655,175	655,175	655,175	655,175	655,175
R-squared	0.04	0.02	0.04	0.04	0.02	0.04
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0054	49.4402	0.0047	0.0054	49.4402	0.0047

Table 3: Cross-sectional differences

This table reports the results of the regressions described in Table X on various subsamples. In Panel A, we separately consider large versus small CBSAs, where large (small) CBSAs are defined as those in the top (not top) 25 in terms of population in 2000. In Panel B, we separately consider large versus small IPOs, where large (small) IPOs are defined as those that are above (below) the median IPO proceeds in the given year. In Panel C, we separately consider "hot" versus "cold" IPOs, where hot (cold) IPOs are defined as those that were underpriced more (less) than the median, where our measure of underpricing is the first day closing price divided by the IPO price minus 1.

Panel A: Large versus small CBSAs

	Large (top 25) CBSAs			Small (non-top 25) CBSAs		
	Num buys	Value ($)	Households	Num buys	Value ($)	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Underwriter local advisors	0.0312***	252.6267***	0.0256***	0.0003***	-0.3162	0.0003***
	(3.29)	(2.81)	(3.10)	(2.63)	(-0.27)	(3.09)
Distance CBSA to HQ (hundreds of miles)	-0.0057***	-49.8241***	-0.0050***	-0.0004***	-5.1436***	-0.0004***
	(-6.94)	(-5.38)	(-6.78)	(-8.71)	(-3.87)	(-9.04)
Observations	44,875	44,875	44,875	610,300	610,300	610,300
R-squared	0.16	0.12	0.16	0.04	0.02	0.04
IPO sample	All	All	All	All	All	All
CBSA sample	Top 25	Top 25	Top 25	Non-top 25	Non-top 25	Non-top 25
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0481	395.5257	0.0422	0.0022	23.9927	0.0020

Panel B: Large versus small IPOs	Large IPOs			Small IPOs		
	Num buys	Value ($)	Households	Num buys	Value ($)	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Underwriter local advisors	0.0006**	2.6263	0.0006**	0.0011***	5.0329**	0.0010***
	(2.07)	(1.00)	(2.14)	(4.40)	(1.99)	(4.49)
Distance CBSA to HQ (hundreds of miles)	-0.0012***	-11.8160***	-0.0011***	-0.0006***	-6.5150*	-0.0005***
	(-6.79)	(-6.91)	(-6.55)	(-5.62)	(-1.93)	(-6.50)
Observations	329,595	329,595	329,595	325,580	325,580	325,580
R-squared	0.05	0.03	0.05	0.04	0.02	0.04
IPO sample	Big	Big	Big	Small	Small	Small
CBSA sample	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0077	68.4357	0.0068	0.0030	30.2104	0.0027

Panel C: Hot versus cold IPOs	Hot IPOs			Cold IPOs		
	Num buys	Value ($)	Households	Num buys	Value ($)	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Underwriter local advisors	0.0024***	14.4260***	0.0021***	0.0001	-0.2606	0.0001*
	(4.86)	(3.17)	(4.79)	(1.27)	(-0.48)	(1.81)
Distance CBSA to HQ (hundreds of miles)	-0.0013***	-14.6021***	-0.0011***	-0.0004***	-2.7462***	-0.0004***
	(-7.04)	(-4.21)	(-6.96)	(-7.07)	(-5.07)	(-7.36)
Observations	326,310	326,310	326,310	324,485	324,485	324,485
R-squared	0.05	0.03	0.05	0.03	0.02	0.03
IPO sample	Hot	Hot	Hot	Cold	Cold	Cold
CBSA sample	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0087	85.5313	0.0076	0.0021	13.8135	0.0019

Table 4: Local optimistic versus pessimistic underwriter workforce

This table reports the results of regressions similar to the ones described in Table X except that it includes additional variables. *Underwriter LA with EPS forecasts* is defined as the percentage of the area's financial advisors that work for a brokerage that both (i) acts as an underwriter in the IPO and (ii) subsequently releases an earnings forecasts for the firm in the XX days after the end of the IPO quiet period. *Underwriter LA with LTG EPS forecasts* is defined analogously, except the advisor's employer must also provide a long-term earnings growth forecast for the IPO firm. *Underwriter LA with optimistic LTG forecasts* (*Underwriter LA with optimistic LTG forecasts*) is defined as the percentage of the area's financial advisors that work for a brokerage that both (i) acts as an underwriter in the IPO and (ii) subsequently releases earnings forecasts with relatively optimistic (pessimistic) long-term earnings growth for the IPO firm in the XX days after the end of the quiet period. All five of the local advisor workforce variables are normalized to have zero mean and unit standard deviation. Standard errors are clustered by IPO.

	Num buys	Num buys	Value ($)	Value ($)	Households	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Underwriter local advisors	0.0002	0.0002	-1.6066	-1.6068	0.0002	0.0002
	(0.93)	(0.93)	(-0.61)	(-0.61)	(0.98)	(0.98)
Underwriter LA with EPS forecasts	0.0007**	0.0006**	5.8584**	5.6313**	0.0006**	0.0006**
	(2.44)	(2.32)	(2.38)	(2.29)	(2.40)	(2.28)
Underwriter LA with LTG EPS forecasts		0.0001		-0.2150		0.0001
		(0.22)		(-0.08)		(0.31)
Underwriter LA with optimistic LTG forecasts	0.0008***	0.0008**	6.9280**	7.1334**	0.0007***	0.0006**
	(2.76)	(2.45)	(2.43)	(2.30)	(2.80)	(2.37)
Underwriter LA with pessimistic LTG forecasts	0.0000		-0.5295		0.0000	
	(0.09)		(-0.24)		(0.18)	
Distance CBSA to HQ (hundreds of miles)	-0.0009***	-0.0009***	-9.1456***	-9.1458***	-0.0008***	-0.0008***
	(-8.59)	(-8.59)	(-4.84)	(-4.84)	(-8.52)	(-8.52)
Observations	655,175	655,175	655,175	655,175	655,175	655,175
R-squared	0.04	0.04	0.02	0.02	0.04	0.04
IPO sample	All	All	All	All	All	All
CBSA sample	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0054	0.0054	49.4402	49.4402	0.0047	0.0047

Table 5: Local underwriter workforce and retail investor holdings

This table reports estimates from panel regressions of the form:

$$IPO\ holdings\ t\ months\ after\ the\ issue_{ij} = \beta_1\ Underwriter\ local\ advisors_{ij} + \beta_2\ Distance_{ij} + \lambda_i + \gamma_j + \varepsilon_{ij}.$$

The sample consists of all (IPO, CBSA) pairs (*i*,*j*). We consider two different measures of retail investor ownership in an IPO stock: the number of households in the CBSA that own the stock *t* months after the issue (columns 1-4) and the total value in USD of the CBSA's holdings *t* months after the issue (columns 5-8). *Underwriter local advisors* is defined as the proportion of financial advisors in CBSA *j* who work for a brokerage that participated as an underwriter in IPO *i*, and it is normalized to have 0 mean and unit standard deviation. *Distance CBSA to HQ (hundreds of miles)* is the distance, in hundreds of miles, between the CBSA and the firm's headquarters. The regressions include IPO and CBSA fixed effects, represented by the terms λ_i and γ_j , respectively. Standard errors are clustered by IPO, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Number of households				Value of holdings ($)			
	t = 3	*t* = 6	*t* = 9	*t* = 12	*t* = 3	*t* = 6	*t* = 9	*t* = 12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Underwriter local advisors	0.0010***	0.0019***	0.0020**	0.0026***	6.3985***	10.7297**	12.2435**	17.0597**
	(5.73)	(2.62)	(2.57)	(3.01)	(3.63)	(2.27)	(2.08)	(2.31)
Distance CBSA to HQ (hundreds of miles)	-0.0007***	-0.0011***	-0.0013***	-0.0015***	-6.6559***	-10.5683***	-14.0858***	-13.3065***
	(-8.72)	(-5.34)	(-5.53)	(-5.72)	(-8.13)	(-4.51)	(-4.68)	(-4.91)
Observations	633,640	609,185	575,240	542,025	633,640	609,185	575,240	542,025
R-squared	0.04	0.04	0.05	0.05	0.01	0.01	0.01	0.01
IPO sample	All	All	All	All	All	All	All	All
CBSA sample	All	All	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0.0051	0.0080	0.0096	0.0109	41.5635	63.3648	79.8911	84.0654

Table 6: Interaction effects

This table augments the regression equations described in Tables X and Y by including the additional variable *Underwriter LA × Distance*, which equals the product of *Underwriter local advisors* and *Distance CBSA to HQ* (which is measured in hundreds of miles). Standard errors are clustered by IPO, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Buying activity (quiet)		Num households owning the IPO stock			Value of IPO holdings ($)		
	Value ($)	Households	*t* = 3	*t* = 6	*t* = 9	*t* = 3	*t* = 6	*t* = 9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Underwriter local advisors	14.7603**	0.0023***	0.0024***	0.0044***	0.0054***	17.5142***	26.8031**	32.8967**
	(2.26)	(3.65)	(4.45)	(2.99)	(3.02)	(3.45)	(2.41)	(2.12)
Distance CBSA to HQ	-9.1501***	-0.0008***	-0.0007***	-0.0011***	-0.0015***	-6.6322***	-10.5361***	-13.2771***
	(-4.83)	(-8.54)	(-8.72)	(-5.35)	(-5.73)	(-8.11)	(-4.49)	(-4.91)
Underwriter LA × Distance	-0.8850*	-0.0001***	-0.0001***	-0.0002***	-0.0003***	-1.0442***	-1.5115**	-1.5001*
	(-1.86)	(-2.88)	(-3.48)	(-3.16)	(-2.86)	(-3.03)	(-2.34)	(-1.77)
Observations	655,175	655,175	633,640	609,185	542,025	633,640	609,185	542,025
R-squared	0.02	0.04	0.04	0.05	0.05	0.01	0.01	0.01
IPO sample	All	All	All	All	All	All	All	All
CBSA sample	All	All	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	49.4402	0.0047	0.0051	0.0080	0.0109	41.5635	63.3648	84.0654

Table 7: The effects of analyst coverage on subsequent retail investor buying activity

We consider analyst coverage in the 90 days after the quiet period (the "coverage period") and the local retail trading days in the 90 days after the coverage period (the "post-coverage period"). Because this timeline lasts roughly 7 months (25 days of quiet period, 3 months of coverage period, 3 months of post-coverage period trading) and the retail investor trading data ends in November 1996, we restrict attention to IPOs prior to April 30 1996. In columns 1-2, the dependent variable is the number of distinct stock purchase transactions in the CBSA for the IPO stock in the post-coverage period. In columns 3-4 and 5-6, the dependent variables are the total dollar value of those purchases and the total number of distinct households placing purchase orders. *Local underwriter coverage (EPS)* is the proportion of financial advisors in the CBSA who work for a brokerage who participated as an underwriter in the IPO and issued equity research with earnings forecasts during the coverage period. *Local non-underwriter coverage (EPS)* is defined analogously for non-underwriter brokerages. *Local coverage (EPS)* is the proportion of advisors who work for a brokerage issuing equity research with EPS forecasts during the coverage period, and it equals the sum of the other two independent variables of interest. None of these variables are normalized. *Num buy transactions (quiet)* is the number of buy transactions in the CBSA for the IPO stock during the quiet period, and *Dollar value of purchases (quiet)* and *Num households purchasing (quiet)* are defined analogously. Panel B is analogous for the IBES recommendation data.

Panel A: Earnings forecast coverage	Num buys	Num buys	Value ($)	Value ($)	Households	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Local underwriter coverage (EPS)	0.0023		79.4448*		0.0022	
	(1.00)		(1.69)		(1.08)	
Local non-underwriter coverage (EPS)	0.0188**	0.0165*	470.5185*	391.0737*	0.0122**	0.0100*
	(2.06)	(1.81)	(1.76)	(1.66)	(2.07)	(1.68)
Local coverage (EPS)		0.0023		79.4448*		0.0022
		(1.00)		(1.69)		(1.08)
Num buy transactions (quiet)	0.4403***	0.4403***				
	(4.89)	(4.89)				
Dollar value of purchases (quiet)			0.3208***	0.3208***		
			(6.19)	(6.19)		
Num households purchasing (quiet)					0.3926***	0.3926***
					(5.38)	(5.38)
Distance CBSA to HQ (hundreds of miles)	-0.0004***	-0.0004***	-5.9346***	-5.9346***	-0.0003***	-0.0003***
	(-6.43)	(-6.43)	(-2.95)	(-2.95)	(-6.58)	(-6.58)
Observations	593,855	593,855	593,855	593,855	593,855	593,855
R-squared	0.18	0.18	0.04	0.04	0.22	0.22
IPO sample	1991-Apr 96	1991-Apr 96	1991-Apr 96	1991-Apr 96	1991-Apr 96	1991-Apr 96
CBSA sample	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Stock recommendation coverage	Num buys	Num buys	Value ($)	Value ($)	Households	Households
	(1)	(2)	(3)	(4)	(5)	(6)
Local underwriter coverage (REC)	0.0007		52.3779		0.0012	
	(0.52)		(1.03)		(1.05)	
Local non-underwriter coverage (REC)	0.0477**	0.0469**	652.8303	600.4524	0.0322*	0.0310*
	(2.26)	(2.24)	(1.62)	(1.61)	(1.96)	(1.90)
Local coverage (REC)		0.0007		52.3779		0.0012
		(0.52)		(1.03)		(1.05)
Num buy transactions (quiet)	0.3361**	0.3361**				
	(2.28)	(2.28)				
Dollar value of purchases (quiet)			0.2756***	0.2756***		
			(3.92)	(3.92)		
Num households purchasing (quiet)					0.2542***	0.2542***
					(2.73)	(2.73)
Distance CBSA to HQ (hundreds of miles)	-0.0004***	-0.0004***	-6.4329*	-6.4329*	-0.0003***	-0.0003***
	(-5.10)	(-5.10)	(-1.76)	(-1.76)	(-5.96)	(-5.96)
Observations	273,020	273,020	273,020	273,020	273,020	273,020
R-squared	0.13	0.13	0.03	0.03	0.15	0.15
IPO sample	1994-Apr 96	1994-Apr 96	1994-Apr 96	1994-Apr 96	1994-Apr 96	1994-Apr 96
CBSA sample	All	All	All	All	All	All
IPO FE	Yes	Yes	Yes	Yes	Yes	Yes
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes