The Review of Financial Studies

Geographic Lead-Lag Effects

Christopher A. Parsons
University of Washington

Riccardo Sabbatucci
Stockholm School of Economics

Sheridan Titman
University of Texas at Austin

We document lead-lag effects on returns between coheadquartered firms in different sectors. Geographic lead-lags yield risk-adjusted returns of 5%–6% annually, half that observed for industry lead-lag effects. Whereas industry lead-lag effects are strongest among small, thinly traded stocks with low analyst coverage, geographic lead-lags are unrelated to these proxies for investor scrutiny. We propose an explanation linked to the structure of the investment analyst business, which is organized by sector, not by geographic region. Our findings suggest that in lead-lag relationships, analysts common to both leading and lagging firms are important, regardless of the number of analysts covering each individually. (*JEL* G11, G12, G14, G17)

Received January 18, 2017; editorial decision May 27, 2019 by Editor Andrew Karolyi.

Stock prices of firms with common characteristics tend to move together. However, empirical studies document significant lead-lag relationships, indicating that these common movements are not always perfectly synchronized, with some stocks reacting to common information before others. One of the more robust cross-sectional phenomena is that such *lead-lag effects* are weakest among stocks more heavily scrutinized by investors. In particular, lead-lag profits tend to be modest when the "lagging" firm is heavily covered by analysts.[1]

In this paper, we revisit the relation between analyst coverage and lead-lag effects on stock returns, but crucially, distinguish between analyst coverage

We appreciate helpful comments from Jonathan Lewellen (FRA 2016), Diego Garcia (Miami Behavioral Finance Conference 2016), Xing Huang (FIRS 2017), Christian Schlag (CEPR Gerzensee 2017), Zhenyu Wang (EFA 2017), Binying Liu (HKUST Finance Symposium 2018), and Allaudeen Hameed (SFS Asia Pacific 2018) and seminar participants at the Stockholm School of Economics. Send correspondence to Christopher A. Parsons, University of Washington, Foster School of Business, PO Box 587, Seattle, Washington 98195; telephone: (206) 685-7893. E-mail: cap1975@uw.edu.

[1] Other proxies for investor scrutiny, such as firm size and trading volume, give similar results (Lo and MacKinlay 1990; Brennan, Jegadeesh, and Swaminathan 1993; McQueen, Pinegar, and Thorley 1996; Chordia and Swaminathan 2000).

The Review of Financial Studies 33 (2020) 4721–4770

doi:10.1093/rfs/hhz145 Advance Access publication January 8, 2020

measured at the level of the *individual firm*, and between *pairs of firms*, that is, the extent to which different firms are covered by one or more common analysts.

Making this distinction requires a lead-lag strategy that fulfills two criteria. First, as with any lead-lag strategy, we must identify a sorting characteristic that groups firms by their sensitivity to common fundamental shocks. Second, however, this sorting criteria should *not* generate substantial overlaps in analyst coverage between the leading and (potentially) lagging firms. That is, we seek groups of firms that, even though their individual members are exposed to common fundamental variation, are generally not covered by the same sets of analysts.

As shown in the lower-left corner of the 2×2 box shown in Figure 1, firms headquartered in the same city, but operating in different industries, such as Seattle-based Costco (retail) and Amazon (technology), satisfy both conditions. With respect to the first, a growing body of research has shown that coheadquartered firms are subject to common fundamental shocks, which generates comovement in their stock returns (Pirinsky and Wang 2006). With respect to the second, because equity analysts tend to specialize by industry classification rather than headquarters' location, it is unusual for the same set of analysts to cover geographic peers operating in different sectors, even for companies with a very large analyst following. For example, in 2013, of the 12 analysts that covered Costco and the 17 that covered Amazon, none covered both simultaneously. We hypothesize that such little overlap creates the potential for lead-lag effects that involve geographic information (here about Seattle), even among highly scrutinized firms.

A natural comparison to geographic lead-lags is industry lead-lags, which involve firms in the same industry, but headquartered in different cities (Moskowitz and Grinblatt 1999; Hou 2007). As the upper-left corner of Figure 1 shows, industry peers, such as Amazon and Google (SF Bay Area), not only share common fundamental shocks but also share substantial overlap in their respective analyst followings. Indeed, whereas Amazon and Costco had zero analysts in common, Amazon and Google shared nine. In such cases involving scrutiny by many of the same analysts, we would expect a nearly simultaneous reaction in stock prices to industry information, and consequently, minimal lead-lag effects.

To formalize this intuition, we start by developing a stylized model where one firm announces its earnings early (at date 1), and two other firms announce earnings later (date 2). Earnings are generated by an industry factor, a location factor, and a firm-specific factor. As such, early earnings announcements provide information about the realizations of the industry and location factors that can help predict the earnings of the late announcers. To keep our analysis simple, we assume the firm that announces early shares an industry factor with one of the late announcers, and a location factor with the other late announcer.

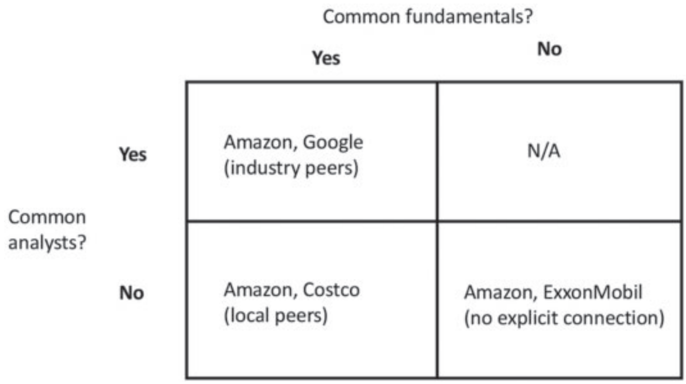

Figure 1
Common analysts versus common fundamentals
This figure shows four possible combinations of analyst followings and firms' fundamentals that pairs of firms can share. On the left axis we have analyst overlap (e.g., whether or not any pair of firms is covered by common analysts), whereas on the horizontal axis we have common fundamentals (e.g., whether any pair of firms shares fundamentals, often proxied by industry affiliation). Most of the investors' focus is on the top-left box (e.g., industry peers), whereas we focus on the bottom-left box (e.g., local peers), highlighting common dynamics in both fundamentals and stock returns.

If investors are fully informed, both late announcers' stock prices will immediately incorporate the information about the relevant factor implied by the date 1 earnings release of the early announcer. In this case there is no lead-lag effect because prices fully respond to realizations of both the industry factor and the geography factor. In contrast, if the late announcers' stock prices react only when they disclose earnings, they will fail to react to information about the industry and the geography factor implied by the early earnings announcement, giving rise to lead-lag patterns and momentum among industry or geographically sorted portfolios.

An intermediate case, where only some firms underreact, is most interesting. Here, firms differ in the extent to which they are scrutinized by a common set of investors. More overlap hastens the incorporation of common information into prices, thereby weakening lead-lag effects, and reducing profits from a momentum strategy that exploits them.

After presenting the model, Section 2 starts by verifying its key assumptions relating to analyst coverage. In particular, we provide direct evidence that analysts disproportionately focus on single industry segments. A direct consequence (which we also verify) is that firms in the same industry, even in different HQ locations, tend to share a considerable number of common analysts. In stark contrast, among firms headquartered in the same cities, but in different operating sectors, analyst overlap is minimal, with the median firm having *zero* analysts in common with its geographic, nonindustry peer firms (as with Costco and Amazon).

The remainder of the paper tests the model's predictions with respect to lead-lag effects on stock returns. Beginning in Section 3, we report the results of regressions that predict individual stock returns using the lagged returns of two portfolios: (1) a portfolio comprised of the stock's industry (nonlocal) peers, and (2) a portfolio comprised of the stock's local (nonindustry) neighbors. Both regressors are significant. We find that a 1% change in the prior month's returns of the industry portfolio forecasts a 24-basis-point (bp) return the following month, consistent with Moskowitz and Grinblatt (1999)'s original documentation of industry momentum. The lagged returns of a firm's geographic peers also predict a stock's return, with magnitudes roughly one-half to one-fourth the magnitude generated in our industry-sorted regressions.

Subsequent cross-sectional tests support the model's key predictions. First, consistent with prior research, we find that the lead-lag effect between industry peers strongly decreases with analyst coverage. That is, potentially lagging firms with a significant analyst coverage tend to have stock price reactions that are more synchronized with the returns of an industry portfolio, and consequently, are associated with little return predictability. More specifically, firms with zero analyst coverage display the largest lead-lags (28 bps in response to a 100-bp change in 1-month lagged industry returns), followed by firms with 1–4 analysts (24 bps), 5–9 analysts (14 bps), and finally, by those with 10 or more analysts (10 bps). Alternative sorts on firm size and trading volume give virtually identical patterns, similar to most return anomalies, which also tend to be strongest among the least scrutinized firms.

Based on this finding, we cannot distinguish between the effects of analyst coverage at the individual firm level (i.e., how many analysts cover the lagging firm), and the effect of having overlapping analyst coverage (i.e., how many analysts simultaneously cover both the lagging firm and firms in the industry portfolio). The reason, as mentioned above, is that analysts tends to specialize by industrial sector, and therefore, a higher individual analyst following is almost certain to generate significant overlaps with industry peers, particularly when these peers are heavily covered themselves.[2] Consequently, industry lead-lags may be weaker among highly covered firms because either (1) a large number of analysts makes stock prices more informationally efficient *generally* or (2) a large number of analysts implies a large number of overlaps, and these overlaps are what make prices more informationally efficient with respect to *industry* information.

In contrast, our analysis of lead-lag effects between local geographic peers does allow us to make this distinction. Because analysts do not commonly specialize by geography, we can sort lagging firms by their individual analyst following, without concern that we have simultaneously sorted on analyst overlap between local peer firms in different sectors. The results of this

[2] See Table 3 for direct empirical evidence.

exercise reveal dramatically different results compared to industry-level lead-lag effects. Whereas the magnitude of industry lead-lag effects are highly sensitive to analyst coverage, and/or other proxies for general scrutiny by investors, geographic lead-lag effects appear to be completely unrelated to the number of analysts following a firm, its market capitalization, or its trading volume. To give a specific example, whereas industry lead-lag effects are 70% weaker among lagging firms with 10+ analysts compared those with none, the comparable lead-lag sensitivities are nearly identical (0.067 and 0.060) when they involve geographic peers.[3]

Finally, we estimate Fama-MacBeth lead-lag regressions that include, for each stock, lagged city- and industry-level returns, as well as these same returns interacted with either the number of analysts following the firm or the number of overlapping analysts covering the firms in the lagging portfolio. The results of these regressions confirm the prior results. The interaction term is highly significant for industry lead-lags, with each additional analyst overlap reducing the magnitude by about 1 bp ($p < .05$). However, because overlaps are virtually nonexistent among geographic peers, the analogous interaction between overlapping analyst coverage and the geography portfolio is insignificant.[4] Overall, the results of the cross-sectional tests suggest that when estimating lead-lag effects, how one measures analyst coverage is of first-order importance. Specifically, whether a stock responds in a timely manner to information shocks shared with peer firms (however peers are defined) appears to depend on the extent to which it shares analysts in *common*.

Our contributions build on two strands of the literature. Our focus on regional patterns in stock returns builds on Pirinsky and Wang (2006), who documents comovement (but not lead-lags) among firms headquartered in the same location, and on Korniotis and Kumar (2013), who examines the link between state-level economic variables and (future) stock returns of locally headquarters firms.[5] In addition, our paper suggests that common variation in cash flows also may be important for neighboring firms and that the market's awareness of these regional linkages may be incomplete.

Although our focus on geographically sorted lead-lags is novel, there is a large literature that explores nonsynchronous return patterns.

[3] Likewise, sorts on trading volume and/or firm size give similar nonresults. For example, geographic lead-lags are *strongest* among the quartile of firms with the highest trading volume.

[4] We also interact the number of individual analysts with each lagged portfolio and find a significant effect for industry lead-lags and nothing comparable for geographic lead-lags. Because individual analyst coverage and overlaps are so highly correlated between industry peers (the primary motivation for conducting the geographic lead-lag comparison at all), there is little power to distinguish between these in the industry case.

[5] Both papers emphasize that discount rates may be influenced by local factors, particularly when a firm's investors are geographically concentrated and undiversified. Hong, Kubik, and Stein (2008), Becker, Ivkovic, and Weisbenner (2011), John, Knyazeva, and Knyazeva (2011), Garcia and Norli (2012), Kumar, Page, and Spalt (2013), Tuzel and Zhang (2017), and Bernile, Kumar, and Sulaeman (2015) also examine the impact of location on asset prices and firm policies.

Atchison, Butler, and Simonds (1987) was among the earliest to consider how these patterns generate serial correlation in portfolio returns. Lo and MacKinlay (1990) were the first to link lead-lags to the profitability of contrarian strategies, and to show that size is a determinant of lead-lag effects across securities, with large firms leading small firms. Brennan, Jegadeesh, and Swaminathan (1993), Badrinath, Kale, and Noe (1995), and Chordia and Swaminathan (2000) linked lead-lag return patterns to analyst coverage, institutional ownership and trading volume, respectively.[6] Relative to these earlier papers, our contribution is to more explicitly understand the channel linking the level of scrutiny to observed lead-lag relationships.

We are also not the first to suggest that lead-lag effects are generated by slow information diffusion. Hong, Lim, and Stein (2000), for example, finds that momentum—particularly when firms with negative returns are involved—sharply weakens with firm size and analyst coverage. This suggests that delayed awareness of, or reaction to, information is responsible for the sluggish price reaction observed in momentum. Other prominent examples include Cohen and Frazzini (2008), who examines the lead-lag relation between the stock returns of firms in a supply chain, and Cohen and Lou (2012), who documents underreaction between focused firms and conglomerates. We, however, are the first to explicitly tie the nature of the lead-lag relation to the *organization* of the analyst community, examine how the lead-lag relation depends on investor scrutiny in alternative settings, and document significant momentum within geographically sorted portfolios.

Finally, our paper is also related to the literature on limited attention, which provides a behavioral explanation for why stock prices may react sluggishly to public information (Hirshleifer, Lim, and Teoh 2011). Early work on this topic (Hirshleifer and Teoh 2003) emphasizes how the information's presentation and/or timing chosen by firms can affect investors' abilities to process disclosures. Subsequent studies consider events outside the firm using, for example, the day of the week (DellaVigna and Pollet 2009) or number of competing news releases (Hirshleifer, Lim, and Teoh 2009) to measure "information overload" by investors, during which underreaction is more severe. One contribution of our study is to provide an institutional rationale for limited attention, that is, drawing on the structure of the investment

6 Numerous prior studies have examined lead-lag relationships in stock returns. Jegadeesh and Titman (1995) find that delayed reactions to common factors give rise to a size-related lead-lag effect in stock returns, whereas Mech (1993) and McQueen, Pinegar, and Thorley (1996) show that lead-lag effects also can be the result of nonsynchronous trading or time-varying expected returns. Hou (2007) find that the lead-lag relationship between large and small firms found in the literature is predominantly an intra-industry phenomenon. Within the same industry, big firms lead small firms, and this effect is more important than the effect across industries. Hameed and Mian (2015) find intra-industry reversals in monthly returns that are consistently present over time, and prevalent across subgroups of stocks, including large and liquid stocks.

analyst business to intuit why analysts and investors organize their information gathering efforts as they do.

1. Data and Descriptive Statistics

1.1 Firm location

Our analysis pertains to stocks headquartered in, or immediately proximate to, the twenty largest urban centers in the United States. To construct our sample, we begin with the universe of domestic common stocks (codes 10 and 11) traded on the NYSE, NASDAQ, and AMEX over the period 1970–2013. Then, we assign to each firm a location variable, based on the ZIP code (ZIP) corresponding to its headquarters' location in the Compustat database. Because Compustat lists only the zip code of the firm's *current* headquarters, we will misclassify firms that have relocated, such as Boeing, which moved its HQ from Seattle to Chicago in 2001. Though this introduces measurement error into our analysis, this works against us; that is, the effects we estimate will be closer to zero than they would be absent headquarters misclassification.[7]

Headquarters' locations are grouped by economic areas (EA), as defined by the Bureau of Economic Analysis. EAs are intended to capture local nodes of economic activity, and typically involve a main metropolitan area, along with smaller surrounding regions from which workers may commute. Examples of EAs include San Jose-San Francisco-Oakland (CA), Atlanta-Sandy Springs-Gainesville (GA-AL), and Houston-Baytown-Huntsville (TX).

1.2 Industry classification

In addition to categorizing firms by headquarters' location, we also group them by industry affiliation. Every month, we link each firm to a single Fama-French 12 industry, which groups firms by SIC designations. The industries are nondurables (1), durables (2), manufacturing (3), energy (4), chemicals (5), business equipment (6), telecommunications (7), utilities (8), shops (9), health care (10), finance (11), and other (12). We intentionally select such relatively broad groupings to reduce the extent of overlap between firms classified in different industries.

1.3 Other data

Our focus is on the relation between a firm's stock returns and the returns of other firms headquartered in the same city. To explain this relation, we use a comprehensive set of firm-specific controls suggested in the literature. Following Harvey, Liu, and Zhu (2016) and Novy-Marx (2013), we use these main control variables: the firm's own 1-month lagged return, individual stocks'

[7] In Section 4.1, we perform some robustness checks by showing that misclassifying a small percentage of locations does not affect our results.

12-month momentum, firm size, book-to-market ratio, trading volume, gross profitability, asset growth and institutional ownership.

1.4 Summary statistics

Table 1 presents summary statistics for our sample, broken down by decade. Panel A shows the results by city (EA) and panel B by industry. Progressing from the left to right, we see a steady increase in the number of publicly traded firms, with an average (per city) of 73 in the 1970s to 178 in the 2000s. However, this growth is unequally distributed among both cities and industries.

As shown in panel B, "old economy" industries have dwindled since the 1970s, with declines in the number of publicly traded firms observed for nondurables, durables, manufacturing, and utilities (chemicals is virtually flat). In contrast, rapid growth is observed in business equipment (341% increase in public companies from the 1970s to 2000s), telecommunications (+273%), health care (+585%), and finance (+629%). Many of these same patterns are reflected in panel A, which indicates stagnation for traditional manufacturing hubs like Detroit, St. Louis, Cleveland, and Indianapolis, and a burgeoning among technology centers like Boston, Denver, Seattle, and San Francisco.

The last four columns present average monthly returns by decade for cities (panel A) and industries (panel B). Across industries, we observe substantial heterogeneity with, for example, the energy sector having among the highest average return of any industry in the 1970s (2.64%) and again after 2000 (1.47%), the intermediate decades being dominated by telecommunications (2.14% in the 1980s) and business equipment (2.59% in the 1990s). To some extent, these industrial patterns are reflected geographically; for example, Houston-based firms performed very well in the 1970s and 2000s. However, the data seem to indicate regional differences in stock returns beyond industrial clustering. For example, in the 1990s, monthly stock returns of Washington, DC-based firms averaged almost one-half percent higher than those headquartered in Chicago (1.51% vs. 1.09%), despite neither area being heavily concentrated in a single industry. Similar geographical heterogeneity is observed in other decades, for example, Minneapolis (1.30%) versus Miami (0.79%) in the 2000s, Los Angeles (1.51%) versus Atlanta (1.06%) in the 1970s, and St. Louis (1.60%) versus Boston (0.99%) in the 1980s. Such regional differences are the basis of our analysis.

2. Theoretical Motivation and Institutional Background

In this section, we develop a simple model (Section 2.1) that generates lead-lag effects between both industry and area peers. The key assumption is a type of *limited attention*, resulting from stock analysts being organized primarily along industry lines. As the model shows, although both industry and geographic lead-lag effects can emerge, mispricing between industry peers is limited to firms

Table 1
Descriptive statistics

A. Cities

	Average # of firms				Average return (volatility)			
	1970-1979	1980-1989	1990-1999	2000-2013	1970-1979	1980-1989	1990-1999	2000-2013
Atlanta	44	90	163	140	1.06% (13.76%)	1.39% (15.78%)	1.41% (18.28%)	0.89% (18.67%)
Boston	100	210	355	303	1.44% (14.87%)	0.99% (15.10%)	1.75% (18.46%)	1.01% (18.96%)
Chicago	122	173	298	296	1.12% (12.64%)	1.49% (13.50%)	1.09% (14.73%)	0.85% (13.42%)
Cleveland	58	72	93	68	1.10% (13.51%)	1.32% (13.71%)	1.15% (16.85%)	1.31% (15.39%)
Dallas	89	183	243	174	1.60% (14.75%)	0.77% (17.13%)	1.16% (20.10%)	1.14% (18.65%)
Denver	32	122	141	108	2.12% (17.29%)	−0.10% (22.48%)	1.40% (22.00%)	0.96% (19.81%)
Detroit	58	73	92	70	1.22% (13.49%)	1.26% (14.10%)	1.28% (17.15%)	1.07% (19.15%)
Houston	77	144	206	193	1.89% (13.67%)	0.60% (18.98%)	1.04% (19.28%)	1.55% (19.30%)
Indianapolis	17	29	51	40	1.10% (11.76%)	1.12% (13.21%)	1.30% (14.36%)	1.08% (15.62%)
Los Angeles	127	287	403	291	1.51% (16.90%)	1.07% (18.58%)	1.23% (23.89%)	0.93% (21.22%)
Miami	46	113	170	102	1.22% (17.10%)	0.83% (20.02%)	1.15% (24.93%)	0.79% (22.30%)
Minneapolis	45	108	181	114	1.49% (13.91%)	1.27% (16.43%)	1.42% (18.14%)	1.30% (18.07%)
New York	372	674	856	666	1.26% (15.13%)	1.14% (18.07%)	1.27% (20.39%)	0.92% (18.37%)
Orlando	13	33	39	26	1.54% (18.77%)	0.81% (16.60%)	1.80% (24.94%)	0.86% (22.19%)
Philadelphia	74	127	227	240	1.31% (14.07%)	1.38% (15.46%)	1.42% (18.02%)	0.89% (15.38%)
Phoenix	24	48	71	55	1.51% (16.37%)	0.52% (19.90%)	1.48% (21.10%)	1.16% (20.15%)
San Francisco	54	172	338	356	1.61% (14.93%)	0.75% (17.07%)	2.31% (22.94%)	0.86% (18.67%)
Seattle	14	37	63	69	1.90% (15.36%)	1.15% (15.69%)	1.81% (19.98%)	0.94% (22.34%)
St. Louis	31	42	61	49	1.04% (12.27%)	1.60% (15.15%)	1.22% (14.20%)	1.45% (18.71%)
Washington, DC	55	132	214	191	1.27% (15.45%)	1.04% (16.78%)	1.51% (19.32%)	1.17% (20.64%)

B. Industries

	Average # of firms				Average return (volatility)			
	1970-1979	1980-1989	1990-1999	2000-2013	1970-1979	1980-1989	1990-1999	2000-2013
Consumer nondurables	166	178	211	135	1.07% (14.07%)	1.62% (15.03%)	0.65% (19.18%)	1.22% (16.45%)
Consumer durables	66	79	87	55	1.16% (13.86%)	1.20% (16.70%)	0.97% (19.64%)	0.88% (21.03%)
Manufacturing	278	347	357	236	1.43% (14.52%)	1.21% (16.45%)	1.09% (18.29%)	1.36% (17.70%)
Energy	64	172	147	123	2.64% (15.22%)	-0.11% (21.83%)	0.89% (18.68%)	1.47% (17.21%)
Chemicals	63	80	92	68	1.13% (12.89%)	1.35% (14.32%)	0.98% (16.52%)	1.18% (17.70%)
Business equipment	152	469	736	671	2.01% (18.17%)	0.69% (19.54%)	2.59% (25.35%)	0.80% (24.53%)
Telecoms	26	57	114	97	1.39% (14.00%)	2.14% (16.88%)	2.24% (23.24%)	0.28% (25.38%)
Utilities	61	76	74	55	0.92% (7.45%)	1.62% (8.10%)	1.13% (8.26%)	1.14% (9.64%)
Wholesale and retail	187	320	414	274	1.10% (14.77%)	1.04% (17.52%)	0.89% (21.23%)	1.17% (18.78%)
Health care	55	182	412	377	1.41% (15.64%)	1.13% (20.05%)	1.51% (22.49%)	1.51% (24.92%)
Finance	148	492	1,085	1,079	1.10% (13.80%)	1.02% (13.44%)	1.23% (13.54%)	0.87% (11.32%)
Others	187	414	536	382	1.37% (16.12%)	1.07% (18.65%)	1.10% (22.92%)	0.85% (20.23%)

Panel A: Average number of firms, cross-sectional mean, and volatility of monthly stock returns for the twenty largest U.S. cities, by decade. Panel B: Average number of firms, cross-sectional mean, and volatility of monthly stock returns for the twelve Fama and French (1992) industries, by decade. Monthly data, 1970-2013.

with scant analyst coverage (and, consequently, minimal overlap with other industry peers). This is not so for geographic lead-lags, which are shown to be fairly insensitive to traditional proxies for investor scrutiny.

Before proceeding to our main tests involving lead-lags in stock returns, we provide empirical support for two of the model's key assumptions in subsection 2.2. The first is the existence of geographic shocks to firm fundamentals, which we test using a variety of performance measures. The second is that equity analysts are highly specialized along industry lines. As we show, this implies that industry peers tend to share common analysts, whereas such overlap is far less frequent between geographic peers in different sectors.

2.1 A model of industry and geographic momentum

We begin with a stylized model that generates cross-serial correlation (lead-lags) at both the regional and industry level. The model has three dates, $t=0,1,2$, and involves three firms $i \in \{1,2,3\}$. The interest rate is zero, and all investors are risk neutral. Each firm i realizes a liquidating dividend π_i at $t=2$. The realization of the liquidating dividend depends on three factors: (1) an industry factor, I, (2) a local factor, L, and (3) a firm-specific factor, ϵ.

There are two industries A and B, and two locations, X and Y. Firms 1 and 2 are in the same industry, and thus share industry shocks (I_A), but realize different values of the local shock, denoted L_X and L_Y, respectively. Firms 2 and 3, on the other hand, are both headquartered in location Y, but because they operate in different industries, are exposed, respectively, to I_A and I_B. Combining these assumptions, the realization of firm i's liquidating dividend at $t=2$ is

$$\begin{aligned}\pi_1 &= I_A + L_X + \epsilon_1\\ \pi_2 &= I_A + L_Y + \epsilon_2\\ \pi_3 &= I_B + L_Y + \epsilon_3.\end{aligned} \tag{1}$$

Industry, area, and firm-specific shocks are all normally distributed, that is, $(I_A, I_B) \sim N(0, \sigma_I^2 = \frac{1}{\tau_I})$, $(L_X, L_Y) \sim N(0, \sigma_L^2 = \frac{1}{\tau_L})$, and $(\epsilon_1, \epsilon_2, \epsilon_3) \sim N(0, \sigma_\epsilon^2 = \frac{1}{\tau_\epsilon})$. The covariance between all signals, both within and across groups, is zero.

Figure 2 illustrates the relevant timing. Initially, at $t=0$, the expected liquidating dividends for each firm, and thus prices, are zero, that is, $P^1_{t=0} = P^2_{t=0} = P^3_{t=0} = 0$. At $t=1$, firms 1 and 3 both announce earnings, information which can be used to update the stock price of firm 2. The model ends at $t=2$, when the realization of π_2 is observed.

2.1.1 Analysts' reports.

The model focuses on the stock price of firm 2, which shares an industry linkage (and only an industry linkage) with firm 1, and (only) a location linkage with firm 3. Analysts play an important role in the way

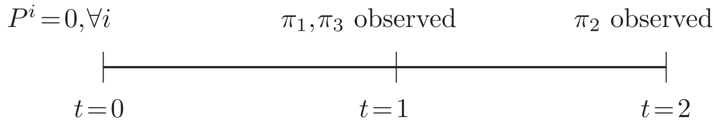

Figure 2
Time line

stock prices are determined. Specifically, there exist a set of analysts indexed by $n \in \{1,2,3,....N\}$ that cover stock 2, each of which may or may not also cover its industry peer (firm 1) or geographic neighbor (firm 3). Investors read analysts' reports and set the price of firm 2 as the expectation of π_2, conditional on the information produced by analysts that cover firm 2. Denoting the report produced by analyst n as r_n,

$$P^2_{t=1} = E[\pi_2 | (r_1, r_2, r_3, ...r_N)]. \tag{2}$$

The model takes a stylized view of analysts' reports. In reality, analysts and investors collect and analyze information from a wide variety of sources, many of which are specific to firms being covered (e.g., talking with management, surveying customers). However, because we are interested in cross-serial correlation *between* firms, we focus on information about other companies that analysts may view as relevant. In particular, analyst n may choose to report π_1, the profit of firm 2's industry peer, and/or π_3, the profit of firm 2's geographic neighbor. There are thus four possible reports each analyst can produce: $\{\pi_1, \pi_3\}, \{\pi_1\}, \{\pi_3\}, \{\}$.

The first would correspond to an analyst that followed both firms 1 and 3, in addition to firm 2 (the subject of his report). The second and third, respectively, correspond to analysts that cover only firm 2's industry peer (firm 1) and geographic neighbor (firm 3). In the last case, the analyst covers neither firm 1 nor 3, and therefore reports neither's profits in his report.

Because investors of firm 2 read all available reports, they form expectations using the union of all information produced by the analyst community. Thus, for forming expectations, it would make no difference whether all the information came from one analyst (e.g., $r_1 = \{\pi_1, \pi_3\}, r_2 = \{\}$), or whether the information is spread across analysts (e.g., $r_1 = \{\pi_1\}, r_2 = \{\pi_3\}$). The same intuition applies for more than two analysts, and for different values for the union of all reports. For example, the price formed with the set of reports $(r_1 = \{\pi_1\}, r_2 = \{\pi_1\}, r_3 = \{\pi_1\}, r_4 = \{\pi_1\})$ would be the same as that formed as with reports $(r_1 = \{\pi_1\}, r_2 = \{\}, r_3 = \{\}, r_4 = \{\})$.

2.1.2 Prices and returns. Using the factor structure in Equation (1) as given, the stock price of firm 2 at $t = 1$ can take on four possible values:

$$P^2_{t=1} = \begin{cases} 0 & \text{if neither } \pi_1 \text{ nor } \pi_3 \text{ reported,} \\ \pi_1\left(\frac{\sigma_I^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\right) & \text{if only } \pi_1 \text{ reported,} \\ \pi_3\left(\frac{\sigma_L^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\right) & \text{if only } \pi_3 \text{ reported,} \\ \pi_1\left(\frac{\sigma_I^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\right)+\pi_3\left(\frac{\sigma_L^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\right) & \text{if both } \pi_1 \text{ and } \pi_3 \text{ reported.} \end{cases}$$

In the first case when neither π_1 nor π_3 is reported by the analyst community, no updating occurs, and $P^2_{t=1}=0$. In the second case, only the industry signal is reported; here, $P^2_{t=1}$ is efficient with respect to industry information (π_1), but inefficient with respect to the geographical shock reflected by π_3. The third case is the converse, with $P^2_{t=1}$ capturing the impact of geographic, but not industry, information. The final case corresponds to the fully efficient case, where both industry and geographic shocks are appropriately incorporated into the stock price of firm 2.

We wish to characterize the conditional expected return of firm 2 from $t=1$ to $t=2$, using either the $t=0$ to $t=1$ return of firm 1 as the conditioning variable, $E[P^2_{t=2}-P^2_{t=1}|P^1_{t=1}]$, or the return of firm 3 over the same horizon, $E[P^2_{t=2}-P^2_{t=1}|P^3_{t=1}]$. The first corresponds to cross-serial correlation between industry peers (industry momentum), and the second to cross-serial correlation between local neighbors that are in different industries (geographic momentum).

Calculating these quantities requires the probabilities for the prices given above. However, the fact that π_1 and π_3 are statistically independent allows us to take a notational shortcut. Rather than having to specify prices for each price realization (four probabilities), all that is needed is the probability of π_1 being reported, regardless of whether π_3 is reported, and vice versa. Denote these, respectively, as $p_1(N)$ and $p_3(N)$. We will later be explicit about how $p_1(N)$ and $p_3(N)$ are expected to vary with the number of analysts N, as well as potentially with firm size, but for now we treat them as constant.

Industry momentum occurs when $cov(P^1_{t=1}-P^1_{t=0}, P^2_{t=2}-P^2_{t=1})= cov(\pi_1,\pi_2-P^2_{t=1})>0$. Expanding this using the factor structure given in Equation (1), we have

$$\begin{aligned} cov(\pi_1-0,\pi_2-P^2_{t=1}) &= cov(\pi_1,\pi_2)-cov(\pi_1,P^2_{t=1}) \\ &= \sigma_I^2 - cov\Big(\pi_1, \frac{\sigma_I^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\, p_1(N)\pi_1 \\ &\quad + \frac{\sigma_L^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2}\, p_3(N)\pi_3\Big) \\ &= \sigma_I^2(1-p_1(N)). \end{aligned}$$

Regional momentum takes a similar form:

$$
\begin{aligned}
cov(\pi_3-0,\pi_2-P^2_{t=1}) &= cov(\pi_3,\pi_2)-cov(\pi_3,P^2_{t=1})\\
&= \sigma_L^2 - cov\Big(\pi_3, \frac{\sigma_I^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2} p_1(N)\pi_1\\
&\quad + \frac{\sigma_L^2}{\sigma_I^2+\sigma_L^2+\sigma_\epsilon^2} p_3(N)\pi_3\Big)\\
&= \sigma_L^2(1-p_3(N)).
\end{aligned}
$$

Proposition 1. The magnitude of industry and regional momentum (a) decreases with the probability that the relevant signal is observed, p_1, and (b) increases with the variance of the shock, σ.

For any mispricing (in expectation) to occur at $t=1$, there must be some probability that investors of firm 2 ignore relevant information conveyed in the profits of its industry ($p_1 < 1$) and/or geographic peers ($p_3 < 1$). High values for these probabilities—that is, when investors are more attentive—imply a more efficient stock price for firm 2 at $t=1$, and accordingly, less return predictability between $t=1$ and $t=2$. Moreover, shocks arising from a more volatile distribution are associated, in expectation, with stronger predictability. Intuitively, for a given probability that a signal is ignored $(1-p)$, shocks with higher volatility create a larger wedge between prices and fundamental value.

These observations will be useful when we compare the magnitudes of industry and geographic momentum in our empirical tests. We generally expect industry shocks to have more influence on cash flows than geographic shocks ($\sigma_I > \sigma_L$), but the probability that regional shocks are reported by analysts is probably less ($p_3 < p_1$). Consequently, it is an empirical question which effect dominates.

2.1.3 Sensitivity to analyst coverage. To this point, we have taken $p_1(N)$ and $p_3(N)$ as given, so as to simplify the return predictability expressions. We now attempt to be more explicit about their relationship with the number of analysts (N) covering firm 2.

Recall that a report may take on four possible values: $\{\pi_1,\pi_3\},\{\pi_1\},\{\pi_3\},\{\}$. Denote the probability of each, respectively, as x, y, z, and $1-x-y-z$. Let us assume that reports are written independently. Then, with N reports, the aggregate probability that π_1 is reported by at least one analyst, $p_1(N)$, is equal to $1-(1-x-y)^N$. Likewise, the analogous expression for π_3 is $1-(1-x-z)^N = p_3(N)$.

One of our key assumptions is that analysts are unlikely to cover firms operating in fundamentally different sectors, consistent with the patterns

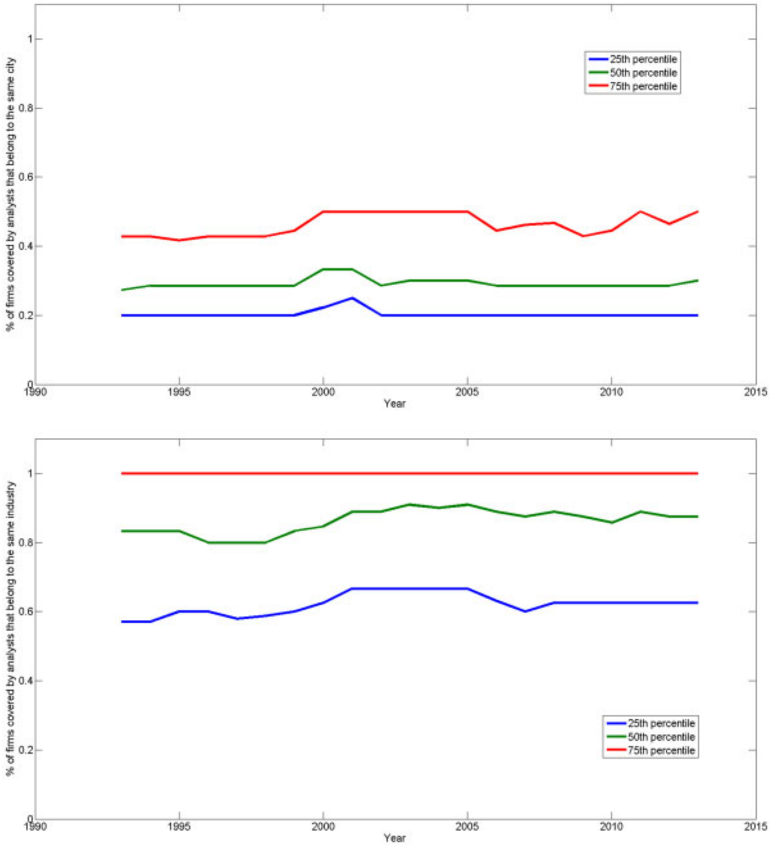

Figure 3
Distribution of analyst coverage by cities and industries
These graphs show the time-series distribution of city (top panel) and industry (bottom panel) concentration of analyst coverage. For each analyst in every year, we identify the modal (i.e., most commonly represented) industry and city. Then, for each analyst, we identify the fraction of covered firms in these modal industries and cities and sort the analysts according to these fractions. For example, in the top panel, less than 28% of the firms covered by the median analyst in 1995 are headquartered in the same city. As another example, in the bottom panel, every year, at least a quarter of the analysts cover only firms that belong to the same industry. Sample: 1993-2013.

observed in Figure 3. Applied to the probabilities above, this implies that $x \approx z \approx 0$, which in turn implies that $p_1(N) \approx 1-(1-y)^N$ and that $p_3(N) \approx 0$.

Two empirical implications follow. First, *industry momentum should decline with analyst coverage.* The intuition is that because analysts tend to specialize by industry, a larger number of analysts increases the probability that π_1 is reported by at least one of them. Consequently, the chance that investors of firm 2 will become aware of firm 1's earnings—allowing them to incorporate this information into prices—increases with N.

The expression allows us to be even more specific. Noting that $\frac{\partial p_1(N)}{\partial N} = -log(1-x-y)^N(1-x-y)^N \approx -log(1-y)^N(1-y)^N$, we can see that the relation between p_1 and N depends crucially on y. When the per-analyst probability of overlap (y) is high, even a small number of analysts will virtually ensure that π_1 is reported, that is, $p_1 \approx 1$. On the other hand, for moderate or small probabilities of overlap, p_1 continues to increase even for relatively large N. For example, if $y = .15$, then $p_1(10) = 56\%$, but increases to 96% if 20 analysts are involved.

The second implication is that *geographical momentum should be relatively insensitive to analyst following.* If the probability that a given analyst covers both 2 and 3 is sufficiently small, then not only is p_3 similarly small but also relatively insensitive to changes in N. As the mirror image to p_1, $\frac{\partial p_3(N)}{\partial N} = -log(1-x-z)^N(1-x-z)^N \approx 0$. For example, if $x+z = .01$, then with five analysts (beyond the 90th percentile in the data), p_3 is still less than 5%, and for ten analysts (98th percentile), the probability that π_3 is reported is less than 10%. The lack of sensitivity to N implies that geographical lead-lags may remain significant, even for firms covered by a large number of analysts.

2.2 Characterizing the model's key assumptions

As described in Proposition 1, lead-lag effects between a pair of firms depend upon two parameters. The first is the magnitude of the common shock (σ) that simultaneously influences both of their fundamental values. Accordingly, subsection 2.2.1 characterizes the relative sizes of both industry- and area-level shocks, across a variety of performance measures. The key takeaway is that even after accounting for the effect of industry fluctuations, geographic fluctuations have a substantial effect on firm fundamentals. The second parameter is the probability that industry and city shocks are immediately incorporated into a stock's price (p), which our model assumes is related to the extent to which the analysts following the stock also follows other firms in either the industry or the region. In subsection 2.2.2, we examine data on analyst-level specialization by industry as well as by geography, and estimate the extent to which industry and/or geographic peers share analysts in common.

2.2.1 Geographic effects on firms' performance. As indicated in Equation (1), one of the model's key assumptions is that in addition to firm-specific and industry components, there exists location-specific variation in firm fundamentals, that is, that $\sigma_L > 0$, which influence stock returns. In this subsection we extend prior research by Pirinsky and Wang (2006) and show that there is indeed significant contemporaneous geographic and industry comovement. We also document that various measures of firm fundamentals exhibit significant geographic comovement, indicating that geographic return comovement is not driven entirely by the common investor base of local stocks.

Table 2
Area comovement in fundamentals

	ΔEPS	ΔSales	ΔEmployees	ΔNewCapital_EV	Returns monthly	Returns annual
city	.151***	.164***	.178***	.210***	.228***	.298***
	(4.70)	(5.98)	(5.92)	(6.61)	(16.59)	(6.91)
industry	.641***	.634***	.633***	.671***	.927***	.949***
	(20.68)	(26.48)	(24.16)	(27.33)	(88.04)	(18.70)
R^2	9.71%	26.13%	19.54%	26.66%	13.75%	21.36%
Observations	125,196	149,153	133,220	86,598	1,626,775	124,956
Time FE	Y	Y	Y	Y	Y	Y
# time clusters	54	66	66	47	528	44
Firm FE	Y	Y	Y	Y	Y	Y
# firm clusters	12,103	13,232	12,401	9,433	13,033	12,193

The table reports results of panel regressions of individual firms' fundamental X ($X_{i,c,j,t}$) onto their contemporaneous city portfolio for the same variable X (e.g., equally weighted portfolio of fundamental X for firms located in city c, outside industry j) and contemporaneous industry portfolio (e.g., equally-weighted portfolio of fundamental X for firms in the same industry j, outside the city). Δ EPS is the change in EPS standardized by lagged stock price (as in Kothari, Lewellen, and Warner 2006); ΔSales is the revenues growth; ΔEmployees is the growth in the number of employees; and ΔNewCapital_EV is the sum of net equity issuance plus net debt issuance standardized by lagged enterprise value (as defined in Baker and Wurgler (2002)). When computing city and industry portfolios of the fundamentals X, in order to limit the impact of outlier firms and industries, we require a minimum number of 6 observations for each industry in each city every year, and at least 6 industries (out of 12) are required to exist in every city. Accounting variables are winsorized at the 1% and 99% level. Regressions include time and firm fixed effects. Annual data (returns also at monthly frequency). $*p<.1$; $**p<.05$; $***p<.01$.

The fundamental variables we consider include changes in earnings-per-share, sales, number of employees, and external capital.[8]

To measure the importance of geographic and industry factors on firm fundamentals, we estimate the following panel regression and report the results in Table 2:

$$X_{i,c,j,t} = \alpha + \beta_1 X_{c,\notin j,t} + \beta_2 X_{\notin c,j,t} + \epsilon_{i,c,j,t}. \tag{3}$$

The dependent variable, $X_{i,c,j,t}$ is a fundamental performance measure for firm i, industry j, city c, and at time t. The explanatory variables are $X_{c,\notin j,t}$, which is the contemporaneous (t), equally weighted portfolio of fundamental X for firms located in the same city c, in different industries $\notin j$, and $X_{\notin c,j,t}$, the contemporaneous, equally weighted portfolio of fundamental X for firms in the same industry j, but outside of the city.[9] The unit of observation is at the firm-year level, with the exception of the fifth column (monthly). Neither portfolio includes the firm whose performance is being estimated (e.g., firm i).

[8] We are not the first to look at how firm fundamentals vary by regions. Recent examples include executive compensation (e.g., Kedia and Rajgopal 2009; Bouwman 2014; Francis et al. 2016), corporate fraud (e.g., Parsons, Sulaeman, and Titman 2016, 2018), capital expenditures (e.g., Dougal, Parsons, and Titman 2015), and CEO choice (e.g., Yonker 2017).

[9] Variables are winsorized at the 1% and 99% levels. ΔEPS, which is defined like in Kothari, Lewellen, and Warner (2006), takes into account negative earnings. When computing the city and industry portfolios of the fundamental X, to limit the impact of outliers, we require a minimum number of 6 observations for each industry in each city every year, and at least 6 (of 12) industries are required to exist in every city.

We are interested in the coefficients β_1 and β_2, which measure the average time series sensitivity between a firm's performance, and that of an equally weighted portfolio of either nonlocal industry (β_2) or a portfolio of local, nonindustry (β_1) peers. Comparing the magnitudes of β_1 and β_2 across the columns, a consistent picture emerges. Other than for stock returns (shown in the last two columns), industry sensitivities hover around 0.6, with local sensitivities in the neighborhood of one-fourth the size. Compared to the industry factor, local comovement is smallest for EPS growth (23% as large), and largest for changes in external capital raising (31%). However, for all variables, β_1 is highly significant.

Comovement in stock returns, originally documented by Pirinsky and Wang (2006), is of particular significance for our analysis. Accordingly, the last two columns of Table 2 compare contemporanous comovement with city and industry-sorted portfolios. At the yearly level, both local- and industry-level sensitivities are somewhat higher compared to prior columns, although both remain highly significant, and the ratio (31%) is similar to the metrics shown in prior columns. The second-to-last column presents the same specification when estimated at a higher frequency. The monthly sensitivity to the area portfolio is 0.23 ($t=16.59$), and 0.927 ($t=88.04$) for the industry portfolio.

Although the magnitudes are somewhat smaller, the last two columns of Table 2 are largely consistent with the local comovement in stock returns originally reported by Pirinsky and Wang (2006) (PW). In contrast, our findings on the comovement of firm fundamentals is substantially different.[10] In particular, while PW find that the changes in earnings of firms tend to be negatively correlated with the contemporaneous earnings changes of local firms, our analysis indicates that the contemporaneous correlation is positive.

2.2.2 Analyst specialization by industry. The second parameter in Proposition 1 is p, the probability that shocks to one firm (either industry peer firm 1 or geographic peer firm 3) are recognized and incorporated into the stock price of peer firm 2. In this short section, we provide empirical evidence suggesting that the probability of industry shocks being simultaneously recognized between nonlocal industry peers (p_1) is likely to be much higher than the corresponding probability between nonindustry, local peers (p_3).

The primary reason is that sell-side equity analysts tend to specialize by industry,[11] which is not particularly surprising given the evidence that industry

[10] While this partly reflects differences in samples (i.e., ours ends in 2013, rather than in 2002), there are also important methodological differences between our respective studies. For example, PW average the coefficients from time-series regressions estimated for each individual firm, whereas we estimate a unified panel regression with firm and year fixed effects. Our replication of PW indicates that their approach may be heavily influenced by outliers, particularly in the analysis of earnings growth, which prior research (e.g., Lakonishok, Shleifer, and Vishny 1994) indicates is highly volatile.

[11] Academic research confirms the importance of industry affiliations in the day-to-day operations, evaluation, and career paths of analysts. Kadan et al. (2012) document, for example, that industry expertise is a key dimension

Table 3
Analysts' portfolio overlap

	Full sample	*Firms with 1-3 analysts*	*Firms with 4-9 analysts*	*Firms with 10+ analysts*
Avg # of analysts	4.81	1.83	5.84	13.81
Overlaps between geographic peers				
% firm-years with zero overlap	57.93%	68.87%	49.37%	38.28%
# of peers connected via at least one overlap	2.08	0.84	2.61	5.55
Overlaps between industry peers				
% firm-years with zero overlap	7.39%	12.67%	2.32%	0.49%
# of peers connected via at least one overlap	15.73	5.81	19.05	45.95

This table reports statistics on analysts' overlap at the area and industry level, conditioning on the total number of analysts following each firm (e.g., 1-3 in Column 2, 4-9 in Column 3, and greater than 9 in Column 4). The first row reports the average number of analysts in each category. The next two rows reports statistics on analysts' overlaps between geographic peers. The second row reports the percentage of firms with zero analysts' overlaps with firms in the same city (but outside their industry). The third row reports the total "connections" a firm has with other firms in the same area (but outside the same industry). In other words, suppose that firm X is followed by three analysts. We form the union of all other firms that those three analysts cover in the same city of firm X but outside its industry. This is a measure of potential firm "connections" through analyst coverage. Rows 4 and 5 report the same statistics for industry overlaps. Yearly data, 1993-2013.

affiliation is a strong predictor of both investment rates and profitability.[12] We illustrate this in Figure 3, which plots the percentage of firms (from 1993 to 2013) that an analyst covers that are in the her/his primary industry sector. For example, in 1995, the graph indicates that for the median analyst, about 83% of the stocks covered were in the same industry. The interquartile range indicates that 75% of the analysts have more than two-thirds of the firms they cover in a single sector, and about 25% devote all their time to just one industry.

A direct implication of this evidence is that firms within the same industry tend to be covered by a common set of analysts. To explore this further, we measure the extent to which the analysts that cover a given firm also cover other firms: (1) in the same industry, but in different cities, and (2) in the same city but in different industries. We refer to these, respectively, as industry and geographic analyst overlaps. Table 3, which measures these tendencies starting in 1993,[13] indicates that firms in the same industry are far more likely to be covered by a common set of analysts than firms located in the same city. For this analysis, we exclude analysts that cover only a single firm, and firm-years without any analyst coverage, since overlap is impossible in both cases.

As an illustration of our methodology, consider firm A, which is covered by analysts 1, 2, and 3. We calculate how many of these three analysts also

---

that defines an analyst's skill. Being recognized by institutional clients as an "all-star" depends on a ranking amongst peer analysts who cover firms in a given sector (Stickel 1992; Clement 1999).

12 Schmalensee's (1985) seminal study used cross-sectional data from the year 1975 to decompose the rates of return on assets into industry-, firm-, and market-specific factors. Industry-specific factors were identified as the most important in generating differences in performance between firms. Though the findings and interpretation have been challenged—most prominently by Rumelt (1991)—subsequent work (see, e.g., McGahan and Porter 1997) continues to identify industry affiliation as a key source of variation between business units.

13 The IBES Recommendations History file starts in 1993.

cover at least one firm in firm A's city, and how many cover at least one firm in its industry. For example, if firms B, C, and D are in firm A's industry, and the union of their analyst following consists of analysts 2, 3, 6, 8, 11, 34, and 38, then the number of "overlapping" analysts between firm A and its industry peers would be two (analysts 2 and 3). The identical calculation is performed involving a firm's coheadquartered firms operating in different sectors.

The first column of Table 3 indicates that on average in 57.93% of the firm-year observations, there are zero overlapping analysts between a firm and its geographic (nonindustry) peer firms. In other words, for the typical firm with about five analysts, there is not a single case in which *any* of the five analysts also cover one (or more) of the firm's geographic peers. In contrast, the analogous case is true only 7.39% of the time for firm's industry (nonlocal) peers.

Moving across the columns, we can see how the incidence of zero analyst overlap varies with the number of analysts following a given firm. For firms with three or fewer (Column 2) analysts, over two-thirds of the observations feature zero overlap, whereas the corresponding percentage is only 12.67% among industry peers. The ratios of these percentages increase markedly with analyst following. For firms in the middle of the distribution (4–9 analysts), the likelihood of having no overlap is over 20 times higher (49.37% vs. 2.32%) for a firm's geographic versus industry peers. This ratio increases to around 80 in the last column, which pertains to firms with ten or more analysts.

A complementary way to characterize differences in city- and industry-level overlap in analyst coverage is to form a "network" of firms connected by one or more common analysts. For example, if firm A is covered by analysts 1 and 2, firm B by analysts 2 and 3, and firm C by analysts 3 and 4, then we would denote firms A and B as being connected to one another (via analyst 2), as well as firms B and C (via analyst 3).[14] We conduct this exercise for both a firm's geographic (row 3) and industry (row 5) peers and report the average number of connected firms in the table.

For example, in the first column we see that on average, firms are connected (via analyst overlaps) to only two other geographic peers (in different industries), but they are connected to over fifteen industry peers (that are in different cities). As expected, both figures increase as one moves rightward in the table, but grow much faster among a firm's industry peers. For firms with 10 or more analysts, remarkably, firms share about as many overlaps with geographic peers (5.55) as industry overlaps among the cohort with least analyst coverage (5.81, per column 2). In contrast, the typical firm with at least ten analysts is connected to almost fifty firms, and many of these are through multiple analyst overlaps (which here are counted only once).

[14] Higher-level (here second-order) connections are ignored, so firms A and C would not be connected according to this definition.

3. Lead-Lag Effects: Industry versus Regional Groups

This section describes our main empirical tests of lead-lag effects between industry and geographic peers. Subsection 3.1 uses Fama and MacBeth (1973) regressions to establish the presence of lead-lag effects, both between industry peers as well as between regional neighbors operating in different sectors. We then show how these lead-lags can be used to create profitable trading strategies in subsection 3.2. In the final subsection (3.3), we compare the cross-sectional patterns between industry and geographic momentum. Consistent with the model's predictions, we observe industry lead-lag effects mostly among small, thinly traded companies; on the other hand, regional lead-lags appear as strong for large and/or heavily covered firms as for small, less covered ones.

3.1 Fama-MacBeth regressions

3.1.1 Observations defined at the firm-month level. Our benchmark specification predicts firm-level monthly stock returns using two predictors: (1) the lagged returns of a portfolio consisting of nonlocal industry peers and (2) the lagged returns of a portfolio consisting of local nonindustry peers. The former portfolio is intended to capture lead-lag effects within industries (Moskowitz and Grinblatt 1999) and the latter to capture cross-industry lead-lag effects within cities.

We estimate the following stock-level predictive regression at the monthly level using the Fama and MacBeth (1973) methodology:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1}, \tag{4}$$

where $r_{i,c,j,t+1}$ is the month $t+1$ excess return of firm i, headquartered in city c, and operating in industry j. There are two predictor variables, both measured at time t. The first is $r_{c,\notin j,t}$, the equally weighted, lagged return of firms headquartered in city c, but operating outside firm i's industry ($\notin j$). Coefficient β_1 thus estimates the lead-lag effect within cities, but across industrial sectors. The second predictor is $r_{\notin c,j,t}$, capturing the lagged returns of firm i's industry peers (j) located outside its city ($\notin c$). Thus, β_2 measures lead-lag effects between industry (but not local) peers. X_t are a set of firm-specific controls: firm's 1-month lagged return, individual stocks' 12-month momentum, firm size, book-to-market ratio, trading volume, gross profitability, asset growth and institutional ownership.

Panel A of Table 4 reports estimates of Equation (4), with successive panels corresponding to increasing horizons of the forecasting variables. In panel A, both the industry and area portfolios are measured over the preceding month. For example, if the dependent variable is the July 2007 return of Coca-Cola (NYSE:KO), the city portfolio would include the June 2007 returns of such Atlanta peers as Home Depot, and the industry portfolio would include the June 2007 return of nonlocal bottlers, such as Pepsi-Cola, headquartered in New York City.

Table 4
Predictability of stock returns by area and industry portfolios with controls (Fama-MacBeth)

	full sample	*1970-1990*	*1991-2013*
A. Individual stock returns			
$r_{city,t-1}$	.061***	.066***	.057***
	(5.11)	(3.81)	(3.45)
$r_{industry,t-1}$	.243***	.249***	.239***
	(11.71)	(8.26)	(8.34)
Avg R^2	6.48%	6.55%	6.42%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
B. City-industry portfolio returns			
$r_{city,t-1}$	.083***	.078***	.087***
	(5.51)	(3.22)	(4.68)
$r_{industry,t-1}$	.204***	.221***	.189***
	(9.08)	(6.08)	(6.90)
Avg R^2	19.04%	18.54%	19.47%
Observations	99,971	44,342	55,629
# time clusters	516	240	276

Panel A reports the estimates of the following Fama-MacBeth (1973) predictive regression:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1},$$

where $r_{i,c,j,t+1}$ is the individual firm monthly stock return, $r_{c,\notin j,t}$ is the lagged city portfolio (e.g., equally weighted return of firms located in city c, outside industry j), $r_{\notin c,j,t}$ is the lagged industry portfolio (e.g., equally weighted lagged return of firms in the same industry j, but outside the city c) and X_t are the control variables defined in the data section. Panel B reports the coefficients of the following Fama-MacBeth predictive regression:

$$r_{j,c,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 \bar{X}_t + \epsilon_{i,c,j,t+1},$$

where $r_{j,c,t+1}$ is the return of industry j, in city c, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), $r_{\notin c,j,t}$ is the equally-weighted lagged return of firms in the same industry j, but outside the city c, and $\bar{X}_t$ are the portfolio averages of the control variables. Column 1: 1970-2013 (full sample). Column 2: 1970-1990. Column 3: 1991-2013. Newey-West standard errors (3 lags). Monthly data. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Starting with the first column of panel A, we see that both β_1 and β_2 are significant, with lead-lags within cities being around one-fourth as strong as those within industry groups. A one percent increase in a firm's lagged industry portfolio is associated with a positive return of 24 bps the following month (t=11.71), compared to 6 bps (t=5.11) for the same change in a firm's lagged city portfolio over the full sample. For a visualization of the lead-lag relationships shown in the table, we estimate panel regressions that predict the firm's current (month t) return using the prior 24 lags of both the city and industry portfolio returns. We then cumulate the coefficients for each horizon, (e.g., the coefficient on month $t-1$ returns, the sum of the coefficients on month $t-1$ and $t-2$) and plot these cumulated coefficients in Figure 4.[15]

15 These plots were originally presented by Jonathan Lewellen (our discussant) at the 2016 FRA conference.

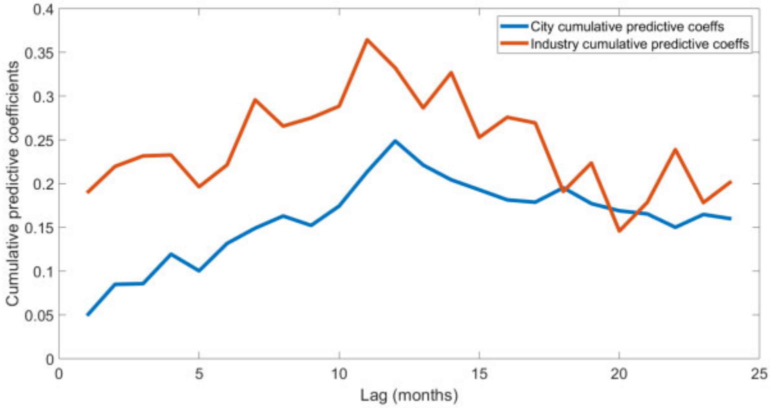

Figure 4
Speed of information diffusion
This graph plots the cumulative coefficients from a 1-month predictive regression of firm-level excess returns on 24 separate monthly return lags, for both an industry and geographic factor. For example, the value of the blue line (the geographic factor) at one lag is the coefficient on month $t-1$ returns when predicting month t returns, whereas the plotted value at two lags represents the sum of the $t-1$ and $t-2$ coefficients. Monthly data, 1970-2013.

The industry and geographic profiles provide an interesting comparison. In both cases, the 1-month lagged return is an important predictor, but the additional lags of the geographic factor are comparatively much more important. For example, the 1-month lag comprises approximately 50% of the entire 12-month lagged cumulative effect for the industry factor, but only about 20% for the city factor. In other words, realizations of the geographic factors are incorporated into stock prices much more slowly. There also seems to be some evidence that markets tend to somewhat overreact to industry factors, as indicated by a clear reversal after 1 year. In contrast, innovations in the geographic factors are fully captured by stock prices within 12 months, but show little to no sign of reversal.

Columns 2 and 3 show the results separately for the first (1970-1990) and second half (1991–2013), respectively, of our sample period. Regardless of the predictive horizon, both the area- and industry-level predictors remain significant in both subsamples. We note, however, that lead-lags between geographic peers appear to have weakened somewhat over the last two decades, whereas industry momentum has maintained relatively constant. As we will see shortly, industry momentum is driven primarily by smaller firms, whereas geographic momentum is relatively constant for firms across the size distribution. Thus, the results in Columns 2 and 3 could possibly reflect high, persistent limits to arbitrage for small firms (thus allowing industry-level mispricing to persist for decades), but gradually more efficient pricing over time for large, liquid firms.

3.1.2 Observations defined at the industry-city-month level. An alternative way of testing for lead-lag relationships is to combine firms within the same industry and city into a single portfolio, rather than consider each firm separately. Doing so allows us to construct observations at the city-industry-month level, and then run a regression similar to Equation (4), except that now the dependent variable is a portfolio return. Note that this aggregation reduces the number of observations to a little under 100,000, corresponding roughly to the product of the number of industries (12), the number of cities (20), and the number of months in our sample (527). We lose about 15,000 possible city-industry-month observations to cases when a potential city-industry-month group contains zero firms.

Panel B of Table 4 shows the results. Compared to the firm-level results, city-industry portfolio regressions give similar estimates. Industry-level lead-lags are about 15% smaller across the board, while area-level lead-lags are approximately 50% larger. Consequently, when we compare the respective magnitudes, the coefficients on the geographic portfolios vary between being about one-third to one-half the size of the industry coefficients.[16]

It is also worth noting that despite reducing the number of observations by over an order of magnitude, the statistical significance is similar between panels A and B of Table 4. This suggests that the reduction in statistical power—all else equal, our estimated t-statistics should decrease by about $\sqrt{\frac{1{,}458{,}783}{99{,}971}} \approx 3.8$—is compensated for by portfolio returns measured with less noise. In any event, the results here confirm the firm-level analysis, and provide strong evidence that for at least some firms, area-level information is incorporated into stock prices with a delay.

3.2 Trading profits

The above results lend themselves to a trading strategy that exploits cross-serially correlated returns between geographic neighbors. Every month, we rank each firm i not by its own lagged return (as we would in a simple momentum strategy), but by $r_{c,\notin j,t}$, the average lagged return of firms headquartered in the same region, but operating in different sectors. We use a 1-month horizon both for the sorting criterion (i.e., area-level stock returns are measured over a month) as well as the holding period (i.e., portfolios are reformed at the end of every month). Based on these rankings, we form value-weighted portfolios.

Figure 5 plots the value of a hypothetical dollar invested in each of three portfolios. The first, shown in blue, shows the evolution of a dollar invested in the market portfolio. Dividends are assumed to be reinvested. Against this benchmark, we also plot the 20% of firms with the highest lagged 1-month area returns (green), as well as the 20% of firms with the lowest lagged 1-month

[16] The magnitudes and significance are similar if we augment the specification to include the lagged portfolio returns of the same-city-same-industry portfolio. See Appendix Table A1.

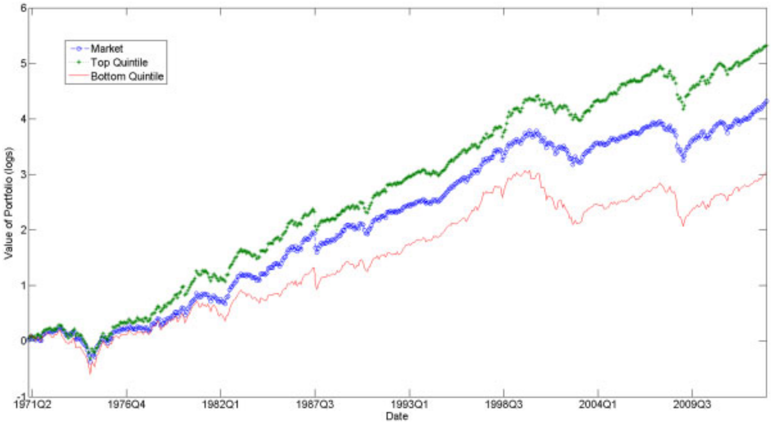

Figure 5
Cumulative performance of the trading strategy
This graph shows the time-series evolution of a $1 investment in each of three trading strategies. The blue line is a market (S&P 500) strategy, where dividends are reinvested in the market. The green (red) line represents a long-only strategy that value weights the top (bottom) 20% of firms, when ranked by area-level stock returns the prior month. Monthly rebalancing. Numbers are in logs. Sample period: February 1971 - December 2013.

area returns (red). Note that the *y*-axis is displayed in natural logarithms. While the market portfolio grows by a (log) factor of over 4 during the four decades in our sample, bringing $1 invested in the market to around $70, $1 invested in the lowest quintile barely exceeds $20. On the other hand, a $1 investment in the highest quintile performs almost an order of magnitude better, growing to approximately $185 by 2013.

Table 5 makes these comparisons more formally. Starting with the first row, we see that the average monthly return for the quintile of firms surrounded by the poorest lagged returns is 74 bps. Regressing the average returns of this portfolio against the market yields a statistically significant intercept of -26 bps ($t=-3.23$), nearly identical to that obtained from a regression that also includes the Fama and French (1993) size and value factors (-24 bps, $t=-3.01$). Using the Fama and French (2015) 5-factor model, the results are similar (-20 bps, $t=-2.33$).The resultant Sharpe ratio is about 0.2, less than half what one would obtain by simply holding a market portfolio.

Proceeding down the table, we see that average returns increases steadily. The middle three groups appear fairly representative of the market as a whole, with similar average returns (capital asset pricing model, or CAPM, alphas are small and insignificant for each group) and Sharpe ratios. However, outperformance is observed for the highest quintile, with raw monthly returns of 116 bps, and statistically significant alphas relative to both the CAPM (t=3.20) and Fama-French five factor model (t=2.71). The Sharpe ratio for this portfolio is 0.53. (See also the nearly identical results in Appendix Table A2, which displays the results when portfolios are sorted into ten, rather than five, groups.)

Table 5
Area momentum trading strategy

Momentum trading strategy, quintiles

	Returns										Portfolio characteristics			
	Mean (%)	*CAPM α*	*t-stat*	*FF-3 α*	*t-stat*	*FF-3+MOM α*	*t-stat*	*FF-5 α*	*t-stat*	*Sharpe Ratio*	*Volatility (%)*	*Mkt share (%)*	*Size*	*B/M*
Lowest city return	0.735	−0.258	−3.228	−0.244	−3.011	−0.223	−2.713	−0.199	−2.333	0.207	5.292	0.182	15.312	0.597
	0.876	−0.077	−1.112	−0.038	−0.520	0.002	0.021	−0.016	−0.191	0.318	4.986	0.212	15.682	0.567
	1.027	0.109	1.432	0.127	1.583	0.178	2.309	0.132	1.553	0.452	4.661	0.219	15.778	0.564
	0.949	−0.009	−0.102	−0.002	−0.022	−0.024	−0.288	0.013	0.122	0.366	5.030	0.206	15.625	0.571
Highest city return	1.158	0.212	3.196	0.211	3.029	0.195	2.629	0.217	2.714	0.526	4.863	0.181	15.264	0.590
5-1 spread	0.423 [3.65]	0.471	[4.16]	0.455	[3.99]	0.419	[3.40]	0.417	[3.319]		2.629			

This table reports the performance of a trading strategy that exploits return continuation at the geographic level. Every month, we rank each firm i by the equally weighted lagged return of firms headquartered in the same city, outside its industry. We then construct quintile value weighted portfolios of the sorted firms, and hold them for one month. Portfolios are rebalanced every month. Displayed are mean returns, CAPM α, FF-3 α, FF-3 + Momentum α, and FF-5 α of each quintile portfolio. *Volatility* is the monthly standard deviation of the portfolio returns; *Mkt share* is the proportional market share of the individual portfolios; *Size* is the natural logarithm of the market value of the portfolios (in thousands); *B/M* is the book-to-market ratio of the portfolios. Newey-West (5 lags) standard errors. Monthly data, 1971-2013. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Foreshadowing results in the following section, the most remarkable aspect of Table 5 is the apparent orthogonality to traditional risk factors. To see this, note that the difference in raw returns between the first and fifth quintiles (42 bps, $t=3.65$) is nearly identical to the intercept estimated from either a regression against the market (47 bps, $t=4.16$) or against the FF-5 factors (42 bps, $t=3.32$).[17]

Further evidence against a risk-based explanation can be inferred from the average portfolio characteristics within each quintile, shown in the far right-hand side of the table. Here, too, we observe no trends relevant for the pattern in average returns. Firm-specific volatility is highest among the quintile with the lowest returns, followed by the second-highest quintile, then the second-lowest, the highest, and then the median. Size is humped shaped, with average market capitalization being highest for the middle group; indeed, we find almost identical results for a trading strategy that focuses on the largest 20% of firms in each period (see Appendix Table A3). Book-to-market ratios display the opposite patterns, dipping in the center. These results indicate that adjusting for characteristics rather than factor loadings as in Daniel and Titman (1997), tells the same story: a geographic momentum strategy is profitable, but appears unrelated to standard risk factors.

3.3 Richness of the information environment

In the last section, we presented evidence of significant lead-lag effects for both industry and geographic sorted portfolios. While underreaction to industry news has been recognized since Moskowitz and Grinblatt (1999), we find significant lead-lags within regions *between* sectors, suggesting an additional source of common information not (completely at least) appreciated by investors. In this section, we attempt to be more precise about the specific situations where geographic return predictability should be stronger or weaker.

The key concept of the model in Section 2 is that when two firms are simultaneously covered by many of the same analysts, their stock prices are more likely to react synchronously to common information shocks, and consequently, less likely to exhibit lead-lag relationships. Importantly, what the model emphasizes it is not analyst coverage per se that matter; rather, the lead-lag relationship is determined by the extent to which analysts overlap between two (or more) firms. Indeed, this concept of analyst overlap is the main contribution of the paper, both theoretically and empirically.

Of course, given that most asset pricing anomalies, including lead-lags, are recognized as being weaker among firms heavily scrutinized by analysts, a natural question is whether the mitigating effects on lead-lags of analyst overlap, and those resulting from higher (or lower) individual analyst following, can be distinguished empirically. In other words, given that firms with a large analyst

[17] Likewise, adjusting for momentum (Carhart 1997) makes no difference.

following are also expected to share substantial overlap with other peer firms, is it possible to tell which measure of analyst coverage is most relevant when thinking about lead-lag effects on stock returns?

The answer to the above question is likely to be no, if we examine lead-lag effects amongst industry peers. Analysts tend to specialize by industry, which means that a firm covered by lots of analysts will almost certainly also share substantial overlap in coverage with its industry peers.[18] However, if our focus is on lead-lags between geographic peers, this is not the case, because analysts do *not* tend to focus on geographic segments. As a result, a stock with substantial analyst coverage may still have very little analyst overlap with its geographic peers. This is true even for large geographic peers (e.g., Costco and Amazon, as mentioned in the introduction) that are in different industries. Accordingly, if we sort lagging firms by number of analysts, and then estimate geographic lead-lags effects, we are less concerned about this sort picking up meaningful variation in analyst overlaps.

Panel A in Table 6 reports our geographic and industry lead-lag regressions estimated on subsamples of our data stratified by the number of analysts covering the lagging firm. Successive columns feature progressively higher numbers of analysts, starting with 0 (Column 1), and then progressing to 1–4 (Column 2), then to 5–9 (Column 3), and, finally, to 10 or more analysts in Column 4. All estimations are from Fama-MacBeth regressions at the 1-month horizon and feature the same set of controls used in Table 4 (though not tabulated).

Starting first with the coefficient on the lagged industry portfolio, a strong, declining relation with analyst coverage is observed. Stock returns of firms with zero identifiable analysts are most sensitive to lagged industry returns, with estimated coefficients of 0.28 ($t=8.79$). The magnitude drops by almost half to 0.14 ($t=4.64$) for firms with between five and nine analysts, and by one-third again for firms with ten or more analysts (0.098). The last column tests for equality between the coefficient in the first quartile (zero analysts) and that in the fourth (10 or more analysts), rejecting this at the 1% level.

A different result obtains for geographic lead-lags. Firms with zero analyst coverage actually have the second largest magnitude (0.067, $t=3.63$), though this is not significantly different from the estimates in the other columns. The coefficient on the lagged area portfolio is fairly stable across columns, with sensitivities of 0.060 ($t=3.27$), 0.090 ($t=4.12$), and 0.060 ($t=2.76$) for firms with progressively more analyst coverage. In contrast to the industry-level comparison, the final column indicates a p-value of 0.877, suggesting no statistically significant difference in area lead-lags for firms with low (Column 1) and high (Column 4) analyst coverage.

[18] Per Table 3, roughly 3.3 new industry overlaps are formed with each new analyst.

Table 6
Predictive regressions with cross-sectional cuts (Fama-MacBeth)

A. Number of analysts

	(0)	*(1-4)*	*(5-9)*	*(10+)*	Δ High/Low
$r_{city,t-1}$	0.067***	0.060**	0.090***	0.060***	
	(3.63)	(3.27)	(4.12)	(2.76)	[0.877]
$r_{industry,t-1}$	0.283***	0.245***	0.140***	0.098**	
	(8.79)	(8.83)	(4.64)	(3.54)	[0.00***]
Avg R^2	6.42%	7.33%	10.72%	13.98%	
Observations	503,536	317,030	167,289	170,333	
# time clusters	336	336	336	336	

B. Firm size

	(1)	*(2)*	*(3)*	*(4)*	Δ High/Low
$r_{city,t-1}$	0.115***	0.063***	0.039**	0.058***	
	(4.89)	(3.76)	(2.72)	(4.39)	[0.031**]
$r_{industry,t-1}$	0.348***	0.248***	0.225***	0.141***	
	(10.52)	(9.70)	(10.07)	(7.11)	[0.00***]
Avg R^2	7.15%	8.54%	9.48%	11.85%	
Observations	356,571	357,315	362,549	382,348	
# time clusters	516	516	516	516	

C. Trading volume

	(1)	*(2)*	*(3)*	*(4)*	Δ High/Low
$r_{city,t-1}$	0.070***	0.063***	0.048**	0.086***	
	(3.85)	(3.35)	(2.56)	(4.71)	[0.431]
$r_{industry,t-1}$	0.261***	0.242***	0.235***	0.172***	
	(10.78)	(9.40)	(9.42)	(6.94)	[0.006***]
Avg R^2	7.85%	8.62%	9.24%	11.98%	
Observations	349,924	342,399	341,868	356,690	
# time clusters	516	516	516	516	

This table reports the coefficients of the following Fama-MacBeth predictive regression:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1},$$

with cuts on analyst coverage, size and trading volume. $r_{i,c,j,t+1}$ is the stock return of firm i, in city c, industry j, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), and $r_{\notin C,j,t}$ is the equally weighted lagged return of firms in the same industry j, but outside the city (industry portfolio). Regressions include firm-specific controls X_t defined in Section 1. Quartiles are estimated within every month. Quartile 1 is the smallest quartile. Monthly data, 1970-2013. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Panels B and C reports similar regressions where the samples are stratified on market capitalization and trading volume rather than analyst coverage. Size and volume are highly correlated with analyst coverage, and by using these sorting criteria we gain almost 500,000 observations, due to size and trading volume being more uniformly populated, particularly in the early part of the sample.[19]

19 In some sense, size and volume may be better measures of total analyst coverage, since buy-side analysts are not included in I/B/E/S (Cheng, Liu, and Qian 2006; Groysberg et al. 2013), and the I/B/E/S database is subject to alterations of recommendations, additions and deletions of records, and removal of analysts' names (Ljungqvist, Malloy, and Marston 2009).

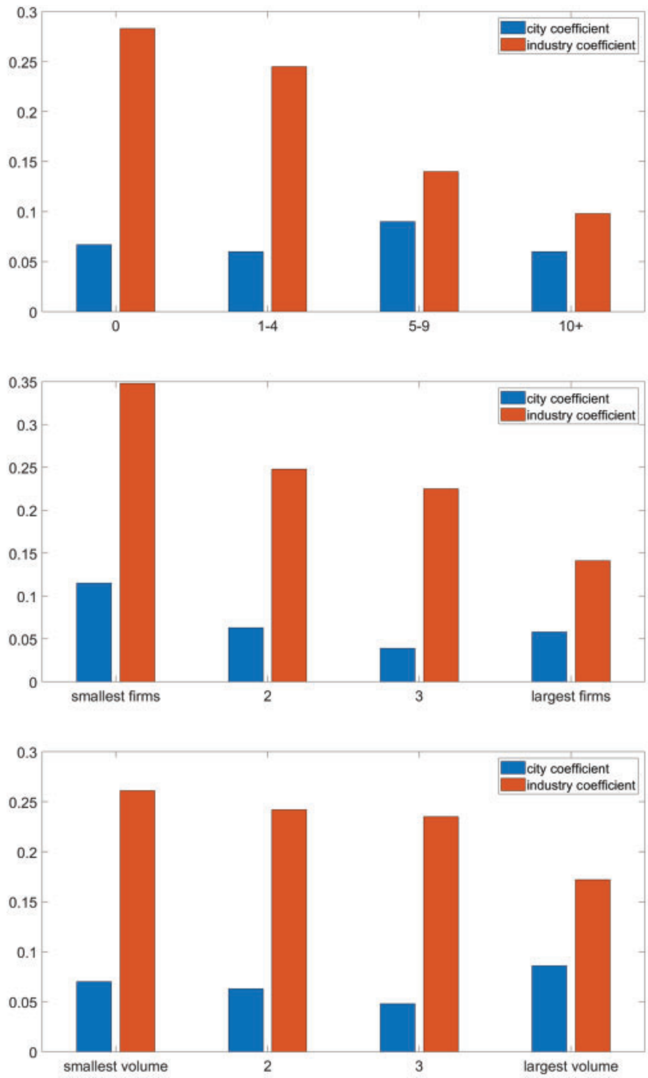

Figure 6
Plot of cross-sectional lead-lag coefficients
This graph corresponds to Table 6 and plots the lead-lag coefficients for industry (red) and area (blue) portfolios, which are estimated from 1-month Fama-MacBeth predictive regressions. Cross-sectional cuts are obtained by splitting the sample into four groups based on number of analysts following a firm (top figure) and quartiles based on firm size (middle figure) and trading volume (bottom figure).

Overall, the patterns in panel A are generally confirmed by those in panels B and C. In the case of industry lead-lags, both size and trading volume substantially affect the estimated coefficient, cutting the magnitude by almost one-third (trading volume) to two-thirds (firm size), when comparing the 1st and

4th quartiles. Both differences are statistically significant. In contrast, and as we saw in panel A, geographic lead-lags do not appear to meaningfully weaken in samples of larger and/or more heavily traded firms.[20] The one exception is some evidence ($p=0.03$) that the smallest quartile of firms (Column 1) gives somewhat higher lead-lag profits than the largest quartile (Column 4); however, there is no discernable relationship between the remaining Columns (2, 3, and 4). Moreover, with trading volume, geographic lead-lags are *largest* among the most heavily traded quartile; no column gives a statistically significant difference relative to any other column. Figure 6 visually summarizes the results provided in Table 6. For all three cross-sectional cuts, the magnitude of the industry coefficient (red bars) declines, with the most pronounced decline coinciding with the highest quartiles. In contrast, the blue bars, which represent the geographic coefficients, display no clear relation to the sorting variables. Taken together, the results in this section provide broad support for the model's predictions. While the lead-lag effect at the industry level is generally larger than that at the regional level, it is mainly restricted to small firms, and to those with low analyst coverage and trading volume. Lead-lags at the regional level, though smaller on average, seem to apply equally well to firms of differing sizes, trading volumes, and analyst coverage.

The overall takeaway from Table 6—the sharp attenuation in industry lead-lags with analyst coverage, and the corresponding lack of attenuation for geographic lead-lag effects—suggests that analyst overlap (or its absence) plays an important role in the relative stock price efficiency between companies subject to common shocks. This observation is also illustrated in the Fama-MacBeth regressions reported in Table 7. The right-hand side of these regressions introduces new variables that interact the lagged returns of the relevant industry and city portfolios with the corresponding number, respectively, of firms in the portfolio connected to firm i through common analysts (measured identically as in the analysis in Table 3).

For industry overlaps, the interaction coefficient is negative and significant, indicating that more industry-level overlaps mitigates the industry-level lead-lag effect. In contrast, and consistent with overlaps being uncommon between geographic peers, no comparable result is observed for lead-lags involving a firm's geographic neighbors. For a more direct comparison with Table 6, the second column replaces these overlap measures with the raw number of analysts following firm i. Here too, we see that for firms with a higher analyst following, industry lead-lags tend to weaken, whereas geographic lead-lags do not. Estimating a model with all four interaction terms (Column 3) thus provides a unifying picture of the results:

[20] Lee and Swaminathan (2000) also study the cross-sectional relation between trading volume and price momentum strategies. They find that, conditional on past returns, stocks with low trading volume generally exhibit higher expected returns than high volume stocks. We find similar results for industry lead-lags, but not for geographic lead-lags, which, after controlling for the firm's own past returns, appears unrelated to trading volume.

Table 7
Portfolio overlap regressions

Regressions with overlaps				
	(1)	*(2)*	*(3)*	*(4)*
$r_{city,t-1}$	0.070***	0.056***	0.056***	0.056***
	(5.86)	(4.41)	(4.38)	(4.44)
$r_{industry,t-1}$	0.245***	0.264***	0.265***	0.265***
	(11.65)	(12.21)	(12.24)	(12.33)
$city_{i,t-1} * city_{overlap,i,t-1}$	−.000		−.004	−.011
	(−0.03)		(−0.32)	(−0.50)
$industry_{i,t-1} * industry_{overlap,i,t-1}$	−.011***		−.008**	.002
	(−3.01)		(−2.25)	(0.16)
$city_{i,t-1} * city_{numofanalysts,i,t-1}$		.006	.006	.006
		(1.43)	(1.46)	(1.44)
$industry_{i,t-1} * industry_{numofanalysts,i,t-1}$		−.017*	−.017*	−.017*
		(−1.83)	(−1.79)	(−1.79)
$city_{i,t-1} * city_{overlap,i,t-1}$ $* city_{numofanalysts,i,t-1}$				.001
				(0.62)
$industry_{i,t-1} ** industry_{overlap,i,t-1}$ $* industry_{numofanalysts,i,t-1}$				−.001
				(−0.90)
Avg R^2	6.41%	6.48%	6.51%	6.53%
Observations	1,458,783	1,458,783	1,458,783	1,458,783
# time clusters	516	516	516	516

This table reports results of Fama-Macbeth regressions explicitly controlling for analysts overlap at the area and industry levels. Column 1 shows the coefficients of the usual benchmark predictability regression in addition to the interactions between the city and industry portfolios and their respective analysts' overlaps. Column 2 includes the interaction of the area and industry portfolios with the raw number of analysts following the firms. Column 3 includes all the four interaction terms. Column 4 includes the interactions between the analysts' overlaps and the raw number of analysts, at both the city and industry level. Newey-West standard errors (3 lags).

1. Industry lead-lags are negatively related to overlaps and/or raw analyst coverage;
2. A firm's raw analyst coverage and number of industry overlaps captures essentially the same information, so cannot be reliably distinguished in the data. This also motivates cross-sectional sorts on raw analyst following, or other proxies for general scrutiny by investors;
3. Analyst overlaps between geographic, nonindustry peers are sufficiently rare that regardless of how it is measured, the magnitude of lead-lag effects between geographic peers is approximately constant. Accordingly, with very little cross-sectional variation in analyst overlap between geographic peers, interacting overlaps with lagged returns in a lead-lag regression is expected to produce (and does produce) a null result.

4. Robustness and Extensions

In this section, we present the results of a number of robustness checks and specification alternatives to our main results. The first two subsections address the possibility that our measure of a firm's headquarters may be an imperfect proxy for its location, and therefore, its sensitivity to local factors. Subsection

4.1 quantifies the potential impact of mismeasured headquarters' locations, which may arise when firms relocate, and subsection 4.2 expands beyond headquarters to consider, for example, the location of a companies' operations, manufacturing. Finally, we present our main predictability results under several alternative specifications in subsection 4.3.

4.1 Misclassified headquarters' locations

Our measure of firm location is its headquarters, as inferred by the ADDZIP variable in Compustat, which reports the zip code of the firm's *most recent* headquarters. Consequently, and unfortunately, in most cases, we do not observe when a firm changes headquarters, resulting in a type of look-ahead bias. For example, General Dynamics moved from St. Louis to the Washington, DC, area in 1992, but the ADDZIP variable takes a value of 22042, corresponding to Falls Church, Virginia (near Washington, DC), both for years prior to its move (pre-1992), as well as afterward (1992 and beyond).

Comprehensive data on changes in firms' headquarters are conspicuously absent in the finance literature, but studies indicate that they are fairly uncommon. Pirinsky and Wang (2006), for example, use news data to track headquarters changes from 1992 to 1997. Excluding firms that moved as a result of mergers or other major restructuring, as well as those moving within the same MSA, the authors estimate that between 2% and 3% of firms moved during this 5-year period, or about 0.5% per year. If we assume that all firms have this rate of relocation, then over 40 years, we also expect about $0.995^{43} \approx 80.6\%$ of firms to be correctly classified in 2013, the last year of our sample. However, because the location reported in 2013 would be correct, on average, half the time (as for General Dynamics post-1992), we should expect error rates by firm-year in the range of perhaps $0.5*(100\%-80.6\%)=9.7\%$. Even this, however, is probably conservative. Because our panel is disproportionately represented by large firms with long histories, and because large firms are less likely to move than small ones (as Pirinsky and Wang 2006 also show), the percentage of misclassification is likely even lower. Most importantly, bad location data biases against our findings.

Because we have some uncertainty about the true misclassification rate, Table A4 presents our 1-month predictive regressions under various scenarios. In the first panel, 1% of the headquarters' locations are scrambled randomly, followed by successively higher percentages in each panel. For misclassification rates of 1% and 5%, the impact on the area coefficient is trivial, and are only slightly affected by misclassifications of 10%. For 20%, the magnitude is cut by one-third, although it remains statistically significant for the full sample. With half the locations assigned incorrectly (panel E), the result vanishes entirely.

Given the Pirinsky and Wang (2006) estimates, along with our intuition about the composition of firms throughout the sample, our best guess is an error rate in the 5%–10% rate over all firm-years in the panel. If so, this suggests that the reported estimates in our prior tables are not meaningfully affected by misclassifications. On the other hand, if we are wrong by a factor of (10%–20%), then our reported results should be grossed up by about 30% to account for measurement error.

4.2 Location beyond firms' headquarters

Throughout the paper, we have identified a firm's location using its headquarters. We do this for three reasons. First, from both prior research and our earlier results (Table 2), we know that firm fundamentals exhibit industry-adjusted comovement within cities. Second, firms' headquarters are not only simple to measure, but are observable for every firm in the sample (subject to the caveat discussed above about stale locations). Finally, although a detailed examination of the types of local shocks most responsible for lead-lag effects is not a primary objective of the paper, the recent urban economics literature stresses the importance of spillovers through direct and indirect knowledge sharing as an important location-based comparative advantage for firms.[21] Given that top managers, software engineers, and others in idea-based activities—those most affected by changes in a city's vibrancy—tend to reside at the firm's headquarters, we believe that this is the most relevant location for our empirical tests.

These reasons notwithstanding, a single measure of location likely ignores differences in the extent to which a firm's facilities, customer base, or labor force are concentrated in a particular geographic region. For example, at one end of the spectrum are retail firms with a national (or even global) presence, for example, Wal-Mart, Home Depot, Whole Foods, and Costco which have highly dispersed stores, customers, and workers. At the other extreme are companies with most or all their operations conducted at a single location. DTE Energy, a Michigan-based utility company, which mentions only Michigan and Indiana in its annual reports, and AutoDesk (mentioning only California) are at the other extreme.

Accordingly, the question we explore in this section is whether regional predictability is stronger for more regionally concentrated firms (e.g., AutoDesk) compared to those with a more disperse presence (e.g., Whole Foods). To obtain a more general measure of a firm's geographical presence, Garcia and Norli (2012) utilize a text-based parsing algorithm that counts the number of unique state names mentioned in the annual reports of publicly traded firms from 1994 to 2008.[22] As the authors describe, state names are often listed when describing and/or discussing the locations of stores, manufacturing facilities, or other operations. We follow their approach, after downloading the relevant data set from Diego Garcia's Web site. For each firm we calculate the

21 See Moretti (2012) for a synthesis of recent contributions.

22 Other papers adopting a similar approach to measuring firm location include Addoum, Kumar, and Law (2015), Bernile et al. (2015), and Bernile, Kumar, and Sulaeman (2015).

Table 8
Geographic concentration

	Full sample		Small firms		Large firms	
	High	*Low*	*High*	*Low*	*High*	*Low*
$r_{city,t-1}$	0.100***	0.057***	0.132***	0.079***	0.059***	0.050**
	(5.56)	(3.39)	(4.55)	(2.61)	(3.30)	(2.81)
$r_{industry,t-1}$	0.232***	0.208***	0.250***	0.269***	0.169***	0.165***
	(9.21)	(9.52)	(6.76)	(6.60)	(6.74)	(7.50)
Avg R^2	8.39%	9.06%	9.64%	12.62%	13.06%	12.00%
Observations	483,180	490,465	279,334	172,589	203,846	317,876
# time clusters	516	516	516	516	516	516
Avg. # of states	3	9	3	8	4	10

This table reports the coefficients of the following Fama-MacBeth predictive regression:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin C,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1},$$

conditioning on geographic concentration. $r_{i,c,j,t+1}$ is the stock return of firm i, in city c, industry j, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), and $r_{\notin c,j,t}$ is the equally-weighted lagged return of firms in the same industry j, but outside the city (industry portfolio), and X_t is a set of controls defined in the data section. Geographic concentration is defined as in Garcia and Norli (2012), based on the number of states mentioned in the 10K. The first column of every block ("High") indicates the most geographic concentrated firms. The second column of every block ("Low") indicates the least geographic concentrated firms. Columns 3-4 (5-6) report the geographic concentration results for small (large) firms. Newey-West standard errors (3 lags). Monthly data, 1970-2013. *$p<.1$; **$p<.05$; ***$p<.01$.

time-series average of state names over the available time period (1994–2008), and apply this measure to all years (including before 1994 or after 2008) in which data are available.[23]

Table 8 presents the results of our 1-month Fama-MacBeth predictive regressions, when sorted by the above/below median level of geographic concentration. The first two columns correspond to the entire sample, with Columns 3 and 4 (5 and 6) to small (large) firms.[24] Firms below the median list nine states on average, compared to three states for firms above it. Note that because we use the same cutoff (5.46 states) for each of the subsamples, the corresponding subsample averages are similar, but need not be identical to the aggregate sample.

In all cases, the point estimates for the more regionally concentrated firms are somewhat larger compared to their less concentrated counterparts. Small firms are associated with the biggest differential, with highly concentrated firms being 70% more sensitive to lagged area returns (0.132, $t=4.55$) than firms that mention more states in their annual reports (0.079, $t=2.61$). Although

23 Because Garcia and Norli (2012) data are available only for 15 years of our 43-year panel, an extrapolative approach is required in order to apply the concentration measure to our entire sample period. Taking the time-series average of state names for each state, unfortunately, ignores dynamics. However, it is unusual for firms to become dramatically more or less concentrated over time, leading us to believe that the ranking obtained from 1994 to 2008 provides a good proxy for its ranking across all years. For example, the median time-series standard deviation of state counts for firms in Garcia and Norli (2012) sample is 1.34, suggesting little aggregate time variation of geographical concentration.

24 Note that the sample size is reduced by about 500,000 firm-month observations, corresponding to firms not in the Garcia-Norli database.

this difference is not statistically significant at conventional levels, we have experimented with other specifications and find stronger results. For example, a Fama-MacBeth regression (1-month horizon) that interacts the number of states mentioned with the lagged city portfolio returns yields a p-value less than 3%. Given these suggestive results using a fairly coarse measure of regional concentration, we hypothesize that more refined measures, such as establishment data from the U.S. Census, might give even stronger results.

4.3 Alternative specifications and robustness

4.3.1 Panel regressions with time fixed effects. Our main empirical tests use Fama-MacBeth cross-sectional regressions. Panel A of Table A.5 shows an alternative, in which we reestimate Equation (4), but with date fixed effects, and residuals double-clustered by firm and date (Petersen 2009). When comparing these area-level lead-lags to the Fama-MacBeth estimates shown in Table 4, we observe slightly larger point estimates with similar statistical significance. As an example, the full sample area coefficient (t-statistic) for our 1-month predictive regression is 0.081 (4.87) in panel regressions, and 0.061 (5.11) with Fama-MacBeth. Results are also slightly stronger at the 3-, 6-, and 12-month predictive horizons (untabulated). Note, also, that geographic lead-lags remain significant at all horizons.

Industry momentum also weakens from a significance perspective. In the double-clustered panel estimation for example, industry lead-lags are profitable only at relatively short horizons (within 3 months). We have experimented with various specifications, in an attempt to better understand which of the three factors mentioned at the beginning of this section are most responsible for the weakened results. It turns out that firm-clustering is relatively insignificant; the standard errors reported in panel A of Table A.5 are almost identical if this cluster is removed. Rather, the additional clustering by time is most responsible for the differences. This exercise thus indicates that accounting for remaining cross-sectional correlation within time may be relevant, and likewise suggests that our panel-generated estimates are considerably more conservative than Fama-MacBeth's methodology.

4.3.2 Delayed portfolio formation. In this section we examine the time it takes the information in one firm's stock price to be incorporated in the stock prices of its industry and location peers. Specifically, we examine whether there is still predictability when we skip a month between when the past returns are measured, and when the strategy is implemented. If information is transmitted relatively quickly, we expect that predictability should be largely eliminated.

Panel B of Table A.5 reports Fama-MacBeth regressions that are identical to the predictive regressions reported in panel A of Table 4, save for the 1-month skip. As the table reveals, area- and industry-level lead-lags are weaker, but they remain statistically significant. The impact of delayed portfolio formation is most severe when the predictor variables are measured over short horizons.

For example, the coefficient on the area-level 1-month predictor drops from 0.061 to 0.040 (t=3.71), and that on the industry-level predictor drops from 0.243 to 0.136 (t=6.03). Together, these results suggest that prices remain inefficient for at least a month after portfolio formation, suggesting a fairly long delay in processing industry or area-specific information.

4.3.3 Value-weighted portfolios. In the main results of Table 4 we construct the local and industry portfolios by equally weighting firms within each group, similar to Pirinsky and Wang (2006). As a robustness check, in panel C of Table A.5 we reestimate our 1-month Fama-MacBeth predictive regression using value-weighted local and industry portfolios. Whereas both industry and geographic momentum remain statistically significant, the magnitudes are 40%–50% smaller, depending on the horizon. In retrospect, this result is intuitive. If the goal is to measure local economic fundamentals using portfolio returns, an equally weighted basket is more likely to be informative, compared to one that puts disproportionate weight on a few large firms (e.g., Dallas's ExxonMobil, Seattle's Amazon), especially given that they are less likely to be regionally concentrated.

4.3.4 Alternative sources of lead-lags. A key assumption of the model, and the foundation for the empirical tests, is that firms headquartered in the same city are exposed to common fundamental shocks. Here, we consider the possibility that geographically sorted portfolios may also potentially sort firms in a manner that captures lead-lag effects originating from nongeographic sources.[25] For example, prior research has identified that the returns of large firms tends to lead the returns of small firms (Lo and MacKinlay 1990), and that the returns of firms with high analyst coverage tends to lead the returns of firms with low analyst coverage (Brennan, Jegadeesh, and Swaminathan 1993). Accordingly, we augment our Fama-McBeth regressions to explicitly account for such lead-lag relationships.

Specifically, during each month, we sort every firm into quintiles based on firm size, analyst coverage, trading volume, and institutional ownership. Then for each of these characteristics, in our Fama-McBeth regressions, we include the portfolio returns of both the quintile corresponding to the firm on the left-hand-side, as well as a portfolio of firms ranked "higher." For example, consider a firm on the left-hand-side that ranks in the 2nd quintile in terms of the firm size. In this case, the right-hand-side would include the portfolio returns of the 2nd quintile based on firm size, as well as the portfolio returns of 3rd, 4th, and 5th quintiles (all in one portfolio).[26] We repeat this procedure for each of the four aforementioned characteristics, for a total of eight control portfolios.

---

[25] We acknowledge a referee, who suggested the analysis in this section.

[26] The results are unchanged if we instead include the returns of each of these "higher-ranked" portfolios (here the 3rd, 4th, and 5th quintiles) separately.

Appendix A.6 shows the results of this analysis. The key takeaway is that the magnitude and statistical significance of the geographic lead-lag is, if anything, stronger than those in our benchmark estimates. Without these control portfolios, the coefficient on the city lead-lag is 0.061 ($p < .01$), as per panel A of Table 4, whereas with these control portfolio returns, the coefficient is 0.079 ($p < .01$).

The next two appendix tables present analysis with a similar intent. First, instead of including control portfolios intended to capture other potential sources of lead-lag effects, we alter the composition of the same-city portfolio itself. Appendix Table A.7 excludes all firms from the right-hand side, which are either (a) larger than the firm on the left-hand side or (b) have more analyst coverage, both calculated dynamically as the firm on the left-hand side potentially changes through time. Although these filters drastically reduce the number of estimable observations,[27] we still observe a statistically significant lead-lag effect between neighboring firms, even when *every* forecasting firm is both smaller in size, and with fewer analysts than the firm whose returns are being forecasted.

The following Appendix Table A.8 repeats this analysis for city-industry-month portfolios (in an analog to the analysis in panel B of Table 4), but because there are multiple firms on the right- and left-hand sides, we include only observations where the *average* size and analyst coverage is lower on the right-hand side. Similar estimates and significance are observed, relative to the full sample.

Finally, prior research has identified lead-lags between customers and suppliers, relationships that may transcend traditional industrial classifications (Cohen and Frazzini 2008). To the extent that customers and suppliers are colocated, part of the lead-lag relation between geographic peers may reflect such linkages. Accordingly, Appendix Table A.9 presents results that control for the lagged returns of a firm's customers (panel A) or suppliers (panel B), in addition to the lagged industry and city portfolio returns. Despite data limitations severely limiting the sample size (resulting in a loss of over 98% of our firm-month observations), geographic lead-lags remain statistically significant.

5. Conclusions

Analyzing lead-lag effects between the returns of related securities provides a useful way to gauge the efficiency of financial markets. In addition to examining links between various securities issued by single firms (e.g., lead-lags between the returns of firms' stocks and bonds) prior research has identified a number of ways to identify links between firms. These include relationships

[27] Three reasons contribute to the reduction in sample size. First, all years prior to 1993 are eliminated, since analyst coverage is not observable. Second, after 1993, all firms with zero analyst coverage are, by construction, dropped, since it is impossible to form a populated portfolio consisting of firms with fewer analysts. Third, and finally, by dropping firms in the leading portfolio that are larger and/or have more following analysts, we lose additional observations because of the inability to form lead portfolios with at least ten firms, the same threshold required throughout the paper.

between companies in the same industry, between firms and their customers and suppliers, and between conglomerates and focused firms in the same industry segments. Such classifications play an important role in the trading strategies of quantitative hedge funds, which exploit lead-lag effects between related stocks, bonds, options, and other derivatives. The underlying rationale is that although similar securities are exposed to common fundamental shocks, there may still exist variation in the rate at which this information is reflected in prices.

This paper contributes to this literature by identifying geography—using firms' headquarters—as a common source of fundamental value. We find that regionally sorted portfolios generate trading profits that are a quarter to half as large as those using industry sorts. Most importantly however, because of the way that analysts are organized, geography is fundamentally different from the other sources of common variation, which, in turn, implies that the corresponding lead-lag patterns between local peers also behave differently. In particular, whereas lead-lags between nonlocal industry peers drop off sharply when the lagging firm is heavily scrutinized by analysts, lead-lags between nonindustry local peers are comparatively invariant to cross-sectional sorts on analyst coverage, size and/or trading volume. In this way, geographic lead-lags are relatively unique among pricing anomalies, which tend to concentrate among stocks with significant limits to arbitrage.

To help explain this finding, we develop a simple model featuring a refinement of limited attention, one based on *overlap* in analyst coverage between two firms or portfolios. Our intuition is that when an investor/analyst simultaneously monitors two stocks, he/she is more likely to recognize common relevant sources of information, and through their trading, reduce lead-lag effects. Here, the fact that analysts specialize by industry plays a key role in shaping which, and how many, firms are connected via common analysts. Intuitively, because analysts tend to cover firms in the same sector, firms with substantial analyst coverage are likely to be connected to a large number of industry peers via common analysts.

However, the relation between analyst coverage and overlap with nonindustry geographic peers does not occur, since the analysts following these firms are almost entirely nonoverlapping.[28] Consequently, while the number of analysts covering a firm is roughly proportional to the number of analysts following its industry peers, analyst coverage fails to capture much variation in the number that follows its geographic peers in different sectors (which is zero for most firms). For lead-lag effects, our model thus indicates that *shared*, rather than *individual*, analyst coverage may be a more useful concept for predicting relative mispricing.

[28] For example, in 2013, Google and Amazon were both heavily covered, with 21 and 17 unique analysts, respectively. Nine analysts covered both firms. On the other hand, although Costco also had a substantial analyst following in 2013 (12), none of these analysts covered its Seattle-based neighbor Amazon.

This raises the natural question of how to optimally define an industry. Traditional categorizations include ad hoc groupings of SIC codes (e.g., Fama-French), with more contemporary methods using textual analytics (Hoberg and Phillips 1995) and analyst coverage decisions themselves to segregate firms into sectors (Kaustia and Rantala 2019). Using the latter approach, Ali and Hirshleifer (2019) find that lead-lag effects between firms with shared analysts are significant, and explain and/or weaken other sources of lead-lag profits identified in the literature, including geographic lead-lags.[29] Our conjecture is that part of the advantage of the Ali and Hirshleifer (2019) shared-analyst industry groupings is that it captures some of the location-based subtleties of industry affiliation that contribute to comovement, as well as to lead-lag effects. Indeed, Engelberg, Ozoguz, and Wang (2018) finds that within standard industry groupings, analysts tend to specialize in the coverage of stocks that are located relatively close together (i.e., in industry clusters).

Broadly interpreted, industry affiliation appears to create a type of "informational segmentation," which is in some ways similar to the segmentation of markets that arises because of home bias, as studied in the international finance literature.[30] Whereas both our paper and the international literature focus on regional diffusion of information, the relevant geographic unit—cities versus countries—is an important difference. Specifically, whereas the evidence in this paper indicates that analysts do not display regional specialization (beyond industry clustering), this is not true internationally, where analysts do tend to cover firms within a given country. Hence, were we to perform a similar analysis across countries, we might find that *industry* information, revealed by market prices in (say) Europe, may not be immediately incorporated in share prices in the United States, even for heavily covered stocks. We view this as an interesting avenue for future study.

Finally, it should be noted that our analysis, which suggests that the organization of the analyst community affects the comovement of securities, takes that organization as given. Of course, this structure is endogenous, relying both on various synergies associated with analyzing a closely related group of firms, as well as constraints on information processing (Peng and Xiong 2006). While we cannot definitively conclude that analysts should be regionally focused, our findings indicate that location-based return information, which is virtually free, can be used to supplement the industry information of stock market analysts. Moreover, given the trend toward urbanization, and the importance of spillovers and other city-level dynamics (Moretti 2012), the relevance of geographic-specific information is likely to increase.

---

29 Notably, though most of the reduction comes from differences between benchmark geographic lead-lags *without* the shared analyst portfolio Ali and Hirshleifer 2019 estimate geographic lead-lags one-third the size of ours (2.2 bps vs. 6 bps) we believe that the shared-coverage approach to categorizing firms is promising.

30 See, for example, Chan, Covrig, and Ng (2005), Coeurdacier and Gourinchas (2016), and Karolyi and Wu (2018) and the references therein.

Appendix

Table A1
Predictability of city-industry portfolios (Fama-MacBeth) with more than five firms in each period

	1-month predictors		
	Full sample	*1970-1990*	*1991-2013*
r_{t-1}	−.019	−.040***	.000
	(−2.31)	(−3.25)	(0.07)
$r_{city,t-1}$	.092***	.121***	.065***
	(6.51)	(5.37)	(3.93)
$r_{industry,t-1}$	.285***	.314***	.259***
	(12.13)	(9.21)	(8.14)
Avg R^2	9.78%	9.81%	9.75%
Observations	60,906	24,976	35,930
# time clusters	527	251	276

The table reports the coefficients of the following Fama-MacBeth predictive regression:

$$r_{j,c,t+1} = \alpha + \beta_1 r_{j,c,t} + \beta_2 r_{c,\notin j,t} + \beta_3 r_{\notin c,j,t} + \epsilon_{i,c,j,t+1},$$

where $r_{j,c,t+1}$ is the return of industry j, in city c, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), and $r_{\notin c,j,t}$ is the equally weighted lagged return of firms in the same industry j, but outside the city (industry portfolio) using 1-month predictors. Column 1: 1970-2013 (full sample). Column 2: 1970-1990. Column 3: 1991-2013. Newey-West standard errors (3 lags). Monthly data. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A2
Area momentum trading strategy (deciles)

	Returns										Portfolio characteristics			
	Mean (%)	*CAPM α*	*t-stat*	*FF-3 α*	*t-stat*	*FF-3+MOM α*	*t-stat*	*FF-5 α*	*t-stat*	*Sharpe Ratio*	*Volatility (%)*	*Mkt share (%)*	*Size*	*B/M*
Lowest city return	0.722	−0.253	−2.554	−0.276	−2.809	−0.299	−3.189	−0.033	−3.216	0.199	5.275	0.088	15.109	0.613
	0.785	−0.211	−1.824	−0.191	−1.733	−0.135	−1.216	−0.096	−0.845	0.230	5.510	0.094	15.281	0.597
	0.857	−0.095	−0.929	−0.114	−1.007	−0.054	−0.457	−0.106	−0.779	0.296	5.129	0.103	15.452	0.582
	0.936	−0.027	−0.259	0.044	0.452	0.061	0.624	0.054	0.559	0.335	5.348	0.109	15.616	0.568
	0.980	0.050	0.494	0.070	0.638	0.073	0.721	0.127	1.073	0.388	5.010	0.110	15.636	0.565
	1.098	0.169	1.564	0.184	1.629	0.271	2.436	0.130	1.087	0.469	5.015	0.108	15.586	0.575
	1.008	0.072	0.775	0.042	0.445	0.031	0.324	0.042	0.368	0.408	5.003	0.105	15.543	0.573
	0.931	−0.042	−0.397	−0.040	−0.369	−0.046	−0.454	−0.056	−0.476	0.331	5.357	0.101	15.445	0.579
	1.105	0.139	1.404	0.085	0.764	0.119	1.014	0.119	0.922	0.451	5.265	0.095	15.273	0.592
Highest city return	1.132	0.191	2.022	0.192	2.137	0.136	1.413	0.166	1.706	0.487	5.072	0.087	15.042	0.602
10-1 spread	0.410 [2.88]	0.444	[3.167]	0.468	[3.309]	0.434	[3.026]	0.491	[3.357]		3.198			

This table reports the performance of a trading strategy that exploits return continuation at the geographic level. Every month, we rank each firm i by the equally weighted lagged return of firms headquartered in the same city, outside its industry. We then construct decile value-weighted portfolios of the sorted firms, and hold them for one month. Portfolios are rebalanced every month. Displayed are mean returns, CAPM α, FF-3 α, FF-3 + Momentum α, and FF-5 α of each decile portfolio. *Volatility* is the monthly standard deviation of the portfolio returns; *Mkt share* is the proportional market share of the individual portfolios; *Size* is the natural logarithm of the market value of the portfolios (in thousands); *B/M* is the book-to-market ratio of the portfolios. Newey-West (5 lags) standard errors. Monthly data, 1971-2013. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A3
Area momentum trading strategy with the 20% largest firms

	Returns										Portfolio characteristics			
	Mean (%)	*CAPM α*	*t-stat*	*FF-3 α*	*t-stat*	*FF-3+MOM α*	*t-stat*	*FF-5 α*	*t-stat*	*Sharpe Ratio*	*Volatility (%)*	*Mkt share (%)*	*Size*	*B/M*
Lowest city return	0.779	−0.198	−2.212	−0.191	−2.209	−0.222	−2.535	−0.186	−2.149	0.238	5.225	0.169	15.679	0.591
	0.829	−0.113	−1.311	−0.032	−0.392	0.008	0.095	0.035	0.370	0.289	4.928	0.213	16.054	0.564
	0.972	0.055	0.666	0.102	1.217	0.154	2.041	0.123	1.375	0.408	4.700	0.220	16.131	0.554
	0.903	−0.047	−0.524	−0.001	−0.011	−0.001	−0.013	0.011	0.105	0.336	4.982	0.210	16.043	0.565
Highest city return	1.208	0.271	3.186	0.302	3.192	0.226	2.533	0.317	3.098	0.557	4.909	0.187	15.770	0.583
5-1 spread	0.429 [3.07]	0.468	[3.36]	0.493	[3.44]	0.449	[3.24]	0.503	[3.365]		2.963			

This table reports the performance of a trading strategy that exploits return continuation at the geographic level only using the top 20% firms by market capitalization. Every month, we rank each firm i by its market capitalization and keep the top 20%. We then re-rank those firms by the equally weighted lagged return of firms headquartered in the same city, outside its industry. We then construct quintile value-weighted portfolios of the sorted firms, and hold them for one month. Portfolios are rebalanced every month. Displayed are mean returns, CAPM α, FF-3 α, FF-3 + Momentum α, and FF-5 α of each quintile portfolio. *Volatility* is the monthly standard deviation of the portfolio returns; *Mkt share* is the proportional market share of the individual portfolios; *Size* is the natural logarithm of the market value of the portfolios (in thousands); *B/M* is the book-to-market ratio of the portfolios. Newey-West (5 lags) standard errors. Monthly data, 1971-2013. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

Table A4
Misclassified locations

A. 1% misclassified HQ locations			
	Full sample	*1970-1990*	*1991-2013*
$r_{city,t-1}$	0.067***	0.071***	0.064***
	(5.53)	(4.02)	(3.83)
$r_{industry,t-1}$	0.242***	0.247***	0.238***
	(11.52)	(8.10)	(8.22)
Avg R^2	6.37%	6.40%	6.34%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
B. 5% misclassified HQ locations			
$r_{city,t-1}$	0.064***	0.067***	0.061***
	(5.29)	(3.87)	(3.65)
$r_{industry,t-1}$	0.242***	0.247***	0.238***
	(11.50)	(8.06)	(8.23)
Avg R^2	6.36%	6.39%	6.33%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
C. 10% misclassified HQ locations			
$r_{city,t-1}$	0.056***	0.058***	0.055**
	(4.78)	(3.35)	(3.42)
$r_{industry,t-1}$	0.242***	0.246***	0.238***
	(11.49)	(8.03)	(8.25)
Avg R^2	6.36%	6.39%	6.33%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276

(Continued.)

Table A.4
(Continued.)

D. 20% misclassified HQ locations			
	Full sample	*1970-1990*	*1991-2013*
$r_{city,t-1}$	0.038***	0.029*	0.045***
	(3.45)	(1.86)	(2.98)
$r_{industry,t-1}$	0.242***	0.246***	0.238***
	(11.54)	(7.99)	(8.34)
Avg R^2	6.34%	6.36%	6.33%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
E. 50% misclassified HQ locations			
$r_{city,t-1}$	0.015	0.019	0.011
	(1.68)	(1.37)	(1.00)
$r_{industry,t-1}$	0.240***	0.247***	0.234***
	(11.52)	(8.04)	(8.26)
Avg R^2	6.33%	6.35%	6.30%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276

This table reports the coefficients of the following Fama-MacBeth predictive regression:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1},$$

where $r_{i,c,j,t+1}$ is the stock return of firm i, in city c, industry j, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), and $r_{\notin c,j,t}$ is the equally weighted lagged return of firms in the same industry j, but outside the city (industry portfolio). Regressions include firm-specific controls X_t defined in Section 1. In every panel, the predictors are the 1-month lagged "random" city and industry portfolio returns. 1% of the locations are randomized in panel A, 5% in panel B, 10% in panel C, 20% in panel D, 50% in panel E. Column 1: 1970-2013 (full sample). Column 2: 1970-1990. Column 3: 1991-2013. Newey-West standard errors (3 lags). Monthly data. $*p<.1$; $**p<.05$; $***p<.01$.

Table A.5
Predictability of individual stock returns by area and industry portfolios (robustness checks)

	Full sample	*1970-1990*	*1991-2013*
A. Pooled OLS			
$r_{city,t-1}$	.081***	.082***	.076***
	(4.87)	(4.53)	(3.25)
$r_{industry,t-1}$	.240***	.279***	.226**
	(3.26)	(8.16)	(2.57)
Adj R^2	10.87%	15.34%	9.36%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
# firm clusters	12,480	5,571	10,132
B. Skipping-a-month			
$r_{city,t-1}$	.040***	.057***	.025*
	(3.71)	(3.73)	(1.68)
$r_{industry,t-1}$	.136***	.146***	.127***
	(6.03)	(4.74)	(3.89)
Avg R^2	6.30%	6.30%	6.29%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276
C. Value weighted			
$r_{city,t-1}$	.031***	.036***	.026**
	(3.70)	(3.15)	(2.19)
$r_{industry,t-1}$	.120***	.140***	.103***
	(8.35)	(6.43)	(5.45)
Avg R^2	6.21%	6.24%	6.18%
Observations	1,458,783	481,729	977,054
# time clusters	516	240	276

Panel A reports the coefficients of the following panel predictive regression with fixed effects:

$$r_{i,c,j,t+1} = \alpha + \beta_1 r_{c,\notin j,t} + \beta_2 r_{\notin c,j,t} + \beta_3 X_t + \epsilon_{i,c,j,t+1},$$

where $r_{i,c,j,t+1}$ is the stock return of firm i, in city c, industry j, $r_{c,\notin j,t}$ is the equally weighted lagged return of firms located in city c, outside industry j (city portfolio), and $r_{\notin c,j,t}$ is the equally weighted lagged return of firms in the same industry j, but outside the city (industry portfolio), and X_t is a set of controls defined in the data section. Panel B lags the city and industry predictors by an additional month (e.g., skip a month). Panel C value-weights the city and industry returns. Column 1: 1970-2013 (full sample). Column 2: 1970-1990. Column 3: 1991-2013. Monthly data. $*p<.1$; $**p<.05$; $***p<.01$.

Table A.6
Predictability of individual stock returns using portfolio returns based on firm size, trading volume, analyst coverage and institutional ownership (Fama-MacBeth)

1-month predictors			
	Full sample	*1970-1990*	*1991-2013*
$r_{city,t-1}$	0.079***	0.091***	0.074***
	(4.56)	(2.88)	(3.55)
$r_{industry,t-1}$	0.275***	0.219***	0.301***
	(11.02)	(5.12)	(9.96)
$portf\_firmsize\_same_{t-1}$	0.164**	0.125	0.182**
	(2.09)	(0.83)	(1.99)
$portf\_firmsize\_larger_{t-1}$	0.228	−0.103	0.378
	(0.66)	(−0.15)	(0.95)
$portf\_tradingvolume\_same_{t-1}$	0.070	−0.330*	0.251*
	(0.63)	(−1.78)	(1.93)
$portf\_tradingvolume\_larger_{t-1}$	−0.073	−0.059	−0.079
	(−0.21)	(−0.08)	(−0.22)
$portf\_analystcoverage\_same_{t-1}$	−0.190	−0.123	−0.220
	(−1.00)	(−0.26)	(−1.28)
$portf\_analystcoverage\_larger_{t-1}$	0.359	2.392	−0.561
	(0.45)	(1.07)	(−1.08)
$portf\_institutionalownership\_same_{t-1}$	0.038	−0.027	0.067
	(0.64)	(−0.26)	(0.94)
$portf\_institutionalownership\_larger_{t-1}$	0.090	0.428	−0.064
	(0.41)	(1.21)	(−0.23)
Avg R^2	6.45%	5.66%	6.81%
Observations	795,463	186,935	608,528
# time clusters	401	125	276

The table reports results of Fama-MacBeth (1973) regressions of individual monthly stock returns ($r_{i,c,j,t}$) onto their lagged city and industry portfolios. Regressions include the portfolio returns of firms with the same and larger size, trading volume, analyst coverage and institutional ownership, and the usual firm-specific control variables defined in Section 1. Column 1: 1970-2013 (full sample). Column 2: 1970-1990. Column 3: 1991-2013. Newey-West standard errors (3 lags). Monthly data. $*p<.1$; $**p<.05$; $***p<.01$.

Table A.7
City portfolio incorporating the filters (larger, more analysts, same industry eliminated)

1-month predictors	
	Full sample
$r_{city,t-1}$	.031**
	(2.47)
$r_{industry,t-1}$	.003
	(0.12)
Avg R^2	14.05%
Observations	107,293
# time clusters	241

The table reports results of Fama-MacBeth (1973) regressions of individual monthly stock returns ($r_{i,c,j,t}$) onto their lagged city and industry portfolios. The city portfolio excludes firms with larger size and more analyst coverage with respect to the firm i on the LHS. The usual controls defined in the data section are included. Newey-West standard errors (3 lags). Monthly data. $*p<.1$; $**p<.05$; $***p<.01$.

Table A.8
Average LHS firms being larger and with more analysts than average firms in the city portfolio

1-month predictors	
	Full sample
$r_{city,t-1}$	.059**
	(2.14)
$r_{industry,t-1}$	.183***
	(5.62)
Avg R^2	10.95%
Observations	23,485
# time clusters	404

The table reports results of Fama-MacBeth (1973) regressions of city-industry portfolio returns ($r_{c,j,t}$) onto their lagged city and industry portfolios, when average LHS firms are larger and with more analysts than the average firm in the city portfolio. Monthly data. $*p < .1$; $**p < .05$; $***p < .01$.

Table A.9
Predictability of individual stock returns by area, industry and customers/suppliers portfolios

A. One-month predictors: Customers' portfolio	
	Full sample
$r_{city,t-1}$	0.146***
	(3.80)
$r_{customers,t-1}$	0.038**
	(2.59)
$r_{industry,t-1}$	0.099***
	(2.66)
Avg R^2	1.77%
Observations	22,404
B. One-month predictors: Suppliers' portfolio	
$r_{city,t-1}$	0.064**
	(2.55)
$r_{suppliers,t-1}$	0.004
	(0.68)
$r_{industry,t-1}$	0.031
	(1.22)
Avg R^2	0.64%
Observations	25,222

The table reports results of regressions of individual monthly stock returns ($r_{i,c,j,t}$) onto their lagged city portfolio, industry portfolio and a portfolio of customers or suppliers. Regressions include controls variables defined in the data section. Monthly data, 1980-2005. $*p < .1$; $**p < .05$; $***p < .01$.

References

Addoum, J. M., A. Kumar, and K. Law. 2015. Slow diffusion of state-level information and return predictability. Working Paper.

Ali, U., and D. A. Hirshleifer. 2019. Shared analyst coverage: unifying momentum spillover effects. Working Paper.

Atchison, M. D., K. C. Butler, and R. R. Simonds. 1987. Nonsynchronous security trading and market index autocorrelation. *Journal of Finance* 42:111–8.

Badrinath, S., J. R. Kale, and T. H. Noe. 1995. Of shepherds, sheep, and the cross-autocorrelations in equity returns. *Review of Financial Studies* 8:401–30.

Baker, M., and J. Wurgler. 2002. Market timing and capital structure. *Journal of Finance* 57:1–32.

Becker, B., Z. Ivkovic, and S. Weisbenner. 2011. Local dividend clienteles. *Journal of Finance* 66:655–83.

Bernile, G., S. Delikouras, G. M. Korniotis, and A. Kumar. 2015. Geography of firms and propagation of local economic shocks. Working Paper.

Bernile, G., A. Kumar, and J. Sulaeman. 2015. Home away from home: Geography of information and local investors. *Review of Financial Studies* 28:2009–49.

Bouwman, C. H. 2014. The geography of executive compensation. Working Paper.

Brennan, M. J., N. Jegadeesh, and B. Swaminathan. 1993. Investment analysis and the adjustment of stock prices to common information. *Review of Financial Studies* 6:799–824.

Carhart, M. M. 1997. On persistence in mutual fund performance. *Journal of Finance* 52:57–82.

Chan, K., V. Covrig, and L. Ng. 2005. What determines the domestic bias and foreign bias? Evidence from mutual fund equity allocations worldwide. *Journal of Finance* 60:1495–534.

Cheng, Y., M. H. Liu, and J. Qian. 2006. Buy-side analysts, sell-side analysts, and investment decisions of money managers. *Journal of Financial and Quantitative Analysis* 41:51–83.

Chordia, T., and B. Swaminathan. 2000. Trading volume and cross-autocorrelations in stock returns. *Journal of Finance* 55:913–35.

Clement, M. B. 1999. Analyst forecast accuracy: do ability, resources, and portfolio complexity matter? *Journal of Accounting and Economics* 27:285–303.

Coeurdacier, N., and P.-O. Gourinchas. 2016. When bonds matter: Home bias in goods and assets. *Journal of Monetary Economics* 82:119–37.

Cohen, L., and A. Frazzini. 2008. Economic links and predictable returns. *Journal of Finance* 63:1977–2011.

Cohen, L., and D. Lou. 2012. Complicated firms. *Journal of Financial Economics* 104:383–400.

Daniel, K., and S. Titman. 1997. Evidence on the characteristics of cross sectional variation in stock returns. *Journal of Finance* 52:1–33.

DellaVigna, S., and J. M. Pollet. 2009. Investor inattention and Friday earnings announcements. *Journal of Finance* 64:709–49.

Dougal, C., C. A. Parsons, and S. Titman. 2015. Urban vibrancy and corporate growth. *Journal of Finance* 70:163–210.

Engelberg, J., A. Ozoguz, and S. Wang. 2018. Know thy neighbor: Industry clusters, information spillovers, and market efficiency. *Journal of Financial and Quantitative Analysis* 53:1937–61.

Fama, E., and K. French. 1992. The cross-section of expected stock returns. *Journal of Finance* 47:427–65.

———. 1993. Common risk factors in the returns on stocks and bonds. *Journal of Financial Economics* 33:3–56.

———. 2015. A five-factor asset pricing model. *Journal of Financial Economics* 116:1–22.

Fama, E. F., and J. D. MacBeth. 1973. Risk, return, and equilibrium: Empirical tests. *Journal of Political Economy* 81:607–36.

Francis, B. B., I. Hasan, K. John, and M. Waisman. 2016. Urban agglomeration and CEO compensation. *Journal of Financial and Quantitative Analysis* 51:1925–53.

Garcia, D., and O. Norli. 2012. Geographic dispersion and stock returns. *Journal of Financial Economics* 106:547–65.

Groysberg, B., P. Healy, G. Serafeim, and D. Shanthikumar. 2013. The stock selection and performance of buy-side analysts. *Management Science* 59:1062–75.

Hameed, A., and G. M. Mian. 2015. Industries and stock return reversals. *Journal of Financial and Quantitative Analysis* 50:89–117.

Harvey, C. R., Y. Liu, and H. Zhu. 2016. ... and the cross-section of expected returns. *Review of Financial Studies* 29:5–68.

Hirshleifer, D., and S. H. Teoh. 2003. Limited attention, information disclosure, and financial reporting. *Journal of Accounting and Economics* 36:337–86.

Hirshleifer, D., S. S. Lim, and S. H. Teoh. 2009. Driven to distraction: Extraneous events and underreaction to earnings news. *Journal of Finance* 64:2289–325.

Hirshleifer, D., S. S. Lim, and S. H. Teoh. 2011. Limited investor attention and stock market misreactions to accounting information. *Review of Asset Pricing Studies* 1:35–73.

Hoberg, G., and G. Phillips. 2016. Text-Based network industries and endogenous product differentiation. *Journal of Political Economy* 124:1423–65.

Hong, H., J. D. Kubik, and J. C. Stein. 2008. The only game in town: Stock-price consequences of local bias. *Journal of Financial Economics* 90:20–37.

Hong, H., T. Lim, and J. C. Stein. 2000. Bad news travels slowly: Size, analyst coverage, and the profitability of momentum strategies. *Journal of Finance* 55:265–95.

Hou, K. 2007. Industry information diffusion and the lead-lag effect in stock returns. *Review of Financial Studies* 20:1113–38.

Kaustia, M., and V. Rantala. 2019. Common analysts - method for defining peer firms. Working Paper

Jegadeesh, N., and S. Titman. 1995. Overreaction, delayed reaction, and contrarian profits. *Review of Financial Studies* 8:973–93.

John, K., A. Knyazeva, and D. Knyazeva. 2011. Does geography matter? Firm location and corporate payout policy. *Journal of Financial Economics* 101:533–51.

Kadan, O., L. Madureira, R. Wang, and T. Zach. 2012. Analysts' industry expertise. *Journal of Accounting and Economics* 54:95–120.

Karolyi, G. A., and Y. Wu. 2018. A new partial-segmentation approach to modeling international stock returns. *Journal of Financial and Quantitative Analysis* 53:507–46.

Kedia, S., and S. Rajgopal. 2009. Neighborhood matters: The impact of location on broad based stock option plans. *Journal of Financial Economics* 92:109–27.

Korniotis, G. M., and A. Kumar. 2013. State-level business cycles and local return predictability. *Journal of Finance* 68:1037–96.

Kothari, S., J. Lewellen, and J. B. Warner. 2006. Stock returns, aggregate earnings surprises, and behavioral finance. *Journal of Financial Economics* 79:537–68.

Kumar, A., J. K. Page, and O. G. Spalt. 2013. Investor sentiment and return comovements: Evidence from stock splits and headquarters changes. *Review of Finance* 17:921–53.

Lakonishok, J., A. Shleifer, and R. W. Vishny. 1994. Contrarian investment, extrapolation, and risk. *Journal of Finance* 49:1541–78.

Lee, C. M., and B. Swaminathan. 2000. Price momentum and trading volume. *Journal of Finance* 55:2017–69.

Ljungqvist, A., C. Malloy, and F. Marston. 2009. Rewriting history. *Journal of Finance* 64:1935–60.

Lo, A. W., and A. C. MacKinlay. 1990. When are contrarian profits due to stock market overreaction? *Review of Financial Studies* 3:175–205.

McGahan, A. M., and M. E. Porter. 1997. How much does industry matter, really? *Strategic Management Journal* 18:15–30.

McQueen, G., M. Pinegar, and S. Thorley. 1996. Delayed reaction to good news and the cross-autocorrelation of portfolio returns. *Journal of Finance* 51:889–919.

Mech, T. S. 1993. Portfolio return autocorrelation. *Journal of Financial Economics* 34:307–44.

Moretti, E. 2012. *The new geography of jobs*. New York: Houghton Mifflin Harcourt.

Moskowitz, T. J., and M. Grinblatt. 1999. Do industries explain momentum? *Journal of Finance* 54:1249–90.

Novy-Marx, R. 2013. The other side of value: The gross profitability premium. *Journal of Financial Economics* 108:1–28.

Parsons, C. A., J. Sulaeman, and S. Titman. 2016. Swimming upstream: Struggling firms in corrupt cities. Working Paper.

———-. 2018. The geography of financial misconduct. *Journal of Finance* 73:2087–137.

Peng, L., and W. Xiong. 2006. Investor attention, overconfidence, and category learning. *Journal of Financial Economics* 80:563–602.

Petersen, M. A. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies* 22:435–80.

Pirinsky, C., and Q. Wang. 2006. Does corporate headquarters location matter for stock returns? *Journal of Finance* 61:1991–2015.

Rumelt, R. P. 1991. How much does industry matter? *Strategic Management Journal* 12:167–85.

Schmalensee, R. 1985. Do markets differ much? *American Economic Review* 75:341–51.

Stickel, S. E. 1992. Reputation and performance among security analysts. *Journal of Finance* 47(5):1811–1836.

Tuzel, S., and M. B. Zhang. 2017. Local risk, local factors and asset prices. *Journal of Finance* 72:325–70.

Yonker, S. E. 2017. Geography and the market for CEOs. *Management Science* 63:609–30.