ON THE PERFORMANCE OF FINANCIAL ANALYSTS

Byoung-Hyoun Hwang, Jose Maria Liberti and Jason Sturgess[1]

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We study the degree to which performance of financial analysts is person-specific, i.e., independent of the brokerage employing the analyst in question, versus broker-resource related, which influences performance through research support, relationships with companies, and spillovers with the set of in-house colleagues the analyst can interact with. Using brokerage house mergers and "real" firm mergers as shocks to the analyst work environment, we provide evidence that most performance is tied to the analyst him-/herself, but also that spillovers can affect individual performance. The findings imply that human capital is portable and that human-capital theories of the firm might explain individual performance in knowledge-based industries, such as the financial industry.

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[1] Hwang is from the Krannert School of Management, Purdue University, 403 W State Street, West Lafayette, IN 47907. Liberti is from the Kellstadt Graduate School of Business, DePaul University, One East Jackson Boulevard, Chicago, IL 60604. Sturgess is from the McDonough School of Business, Georgetown University, Washington, DC 20057. Email: bhwang@purdue.edu, jliberti@depaul.edu and jds224@georgetown.edu. We thank Seoyoung Kim and seminar participants at Georgetown University for helpful comments.

1. Introduction

Financial analysts play an integral role in financial markets. They collect, process, and transmit information to market participants, who in turn use analysts' reports to guide their investment decisions. The evidence in the accounting and finance literature implies that analysts significantly alter market expectations (e.g., Stickel 1995, Womack 1996, Kothari 2001), and analysts deemed particularly successful in their endeavor quickly earn "superstar"-status via high profile awards, press coverage and lucrative compensation packages. Such accolades are predicated on the assumption that a large portion of the analyst's performance is person-specific and portable, i.e., independent of the brokerage employing the analyst in question.

While plausible, an alternative and equally reasonable proposition is that most of the analyst's performance does not reside with the person him-/herself, but rather with the brokerage organization that employs the analyst, which influences performance through research support, relationships with companies, and the set of in-house colleagues the analyst can interact with. The goal of this study is to systematically evaluate this idea and to assess the relative contribution of analyst-person versus brokerage-specific components of performance.

Our study has implications for other knowledge-based industries beyond the analyst literature. Knowledge-based industries, such as the financial, professional, academic, scientific and technical services industries, typically involve individual productivity that might result in individual accolade outside the firm. Whether higher performing firms in these industries successfully create their own high-productivity employees or simply attract more productive individuals is an important issue in the theory of the firm. Further, if higher performing firms create high-productivity employees, how do they achieve this? Does the individual performance increase because of better resources, corporate culture, or spillovers from more productive co-workers?

In this paper we attempt to address these theory of the firm issues by examining analyst productivity. Empirical assessments of these questions face the challenge that individual productivity rarely is observable. Therefore separating individual and firm productivity is somewhat difficult. Our

setting is unique in this regard as we can directly observe one of the most important performance outputs that financial analysts produce, namely earnings forecasts. We also have an objective benchmark, in the form of actual reported earnings, against which earnings forecasts can be compared. Thus we can both observe individual productivity and also measure individual performance.

We attempt to assess the relative contribution of analyst-person versus brokerage-specific components of performance by studying two natural experiments that provide shocks to the analyst work environment. Our first testing ground utilizes mergers of brokerage houses, which, generally, are accompanied with significant changes in the brokerage organization, but not necessarily with changes in the analyst-person-specific performance component. In particular, one may expect brokerage-house mergers to significantly affect resources available to the analyst, increase the number of company ties available to the analyst, and to widen the set of colleagues the analyst can interact with. This particularly applies to analysts (initially) working for a smaller brokerage that is subsequently acquired by a larger broker firm. If a meaningful part of performance resides with the brokerage organization, we, therefore, expect performance, as measured via earnings forecast errors, to improve post broker-firm merger where resources and networks improve, particularly for analysts initially working for a small brokerage.

Our second setting exploits mergers of "real" firms. We examine analysts' forecast accuracy for the acquirer firm prior to the merger and we compare it to their forecast accuracy for the newly merged firm. We exploit two sources of variation in this setting. Our first source of variation comes from evaluating (a) the forecasting performance of analysts working for a brokerage house that employs analysts covering both the acquirer firm and the target firm prior to the merger relative to (b) that of analysts working for a brokerage house that only employs analysts covering the acquirer firm. If analysts rely heavily on their broker firm's connections and brokerage-specific knowledge of companies, then analysts employed by a brokerage house that covers both the target and the acquirer should be at a significant advantage. In contrast, if performance mostly resides with the analyst and the person-specific intellectual and social capital that he/she has acquired over his/her career, then no meaningful differential change in forecasting performance may be observed.

In our second test exploiting variation in coverage of mergers of "real" firms, we once again examine analysts' forecast accuracy for the acquirer firm prior to the merger and we compare it to their forecast accuracy for the newly merged firm. However, we focus on just those brokers in "(a) the forecasting performance of analysts working for a brokerage house that employs analysts covering both the acquirer firm and the target firm prior to the merger", above. The source of variation comes from evaluating (i) the forecasting performance of analysts working that cover both the acquirer firm and the target firm prior to the merger relative to (ii) that of analysts that cover only the acquirer firm, but have a peer that covers the target firm prior to the merger. If performance mostly resides with the analyst and the person-specific intellectual and social capital that he/she has acquired over his/her career, then analysts covering both the acquirer and target prior to the merger should be at a significant advantage to those analysts covering the acquirer only, even where a peer also covers the target. In contrast, if performance mostly resides with the broker, then no meaningful differential change in forecasting performance may be observed.

Our study concludes that the analyst-person-specific component is more important than the brokerage-specific component. In particular, while we observe a marginal decrease in forecast error after a brokerage-house merger for all brokers, this decrease is explained fully by the change in competitive landscape documented by Hong and Kacperczyk (2010). The change in forecast error for analysts employed by the merging brokerages is economically inconsequential once we control for this aggregate change in forecast error due to the shift in competitive landscape that affects all brokers. This suggests, that on average, performance is unrelated to the organization, or brokerage. We do observe a somewhat noticeable decrease in forecast error for the subset of observations for which the new joint firm covers *substantially* more stocks, as well as for the subset of observations for which the number of in-house analysts that the analyst can interact with increases *significantly*. We also observe some performance improvement for analysts initially employed by a non-NYC-based broker firm that subsequently merges with a NYC-based brokerage. Combined, these three results provide weak evidence for resource-based theories of the firm being at play.

Our real firm merger setting points to a similar conclusion. We observe that forecast errors for the newly merged firm are higher than those for the acquirer, consistent with the newly merged firm being more uncertain and more difficult to forecast than the acquirer firm considered by itself, at least in the initial period after the merger. Forecast accuracy declines substantially less for analysts that cover both the acquirer- and the target firm prior to the merger. On average, the affiliation with a brokerage that covers both the acquirer and the target, however, has no performance-improving effects. In other words, analysts that previously covered the acquirer firm and work for a brokerage house that, previously, employed a (second) analyst covering the target firm face similar challenges as analysts that work for a brokerage house with no such brokerage-level ties to the target firm. Together, our results suggest performance is tied to the analyst him-/herself and, ultimately, that human capital in the financial industry is portable.

In extensions to the main results, we examine how networks might affect how the organization might affect individual performance. We identify those analysts in the "real" merger setting that cover the acquirer, but work at a brokerage that covers both the acquirer and target, for which the peer covering the target sits in the same locale. If knowledge-based theories of the firm explain how the organization contributes to individual performance then we should expect positive effects of spillovers and/or corporate culture to be more pronounced for analysts that previously covered the acquirer firm and work for a brokerage house that, previously, employed a (second) analyst covering the target firm sitting in the same locale. Forecast accuracy declines substantially less for these analysts, compared with both analysts that previously covered the acquirer firm and work for a brokerage house that, previously, employed a (second) analyst covering the target firm not sitting in the same locale and analysts that work for a brokerage house with no such brokerage-level ties to the target firm.

The market for analyst forecasts provides an ideal setting to study theory of the firm related questions. As mentioned previously, analyst forecasts provide a measure of individual performance not easily accessible in other markets. Further, we are able to exploit two natural experiments – broker mergers and "real" mergers. The appealing feature of our first setting is that broker-firm mergers can be

thought of as exogenous to the analyst-specific component. While analysts play an important role in financial markets, from a broker firm's perspective, research divisions only constitute a small portion of the firm's overall business and, as such, are unlikely to represent the motivating factor for investment firms to merge. Any shock to the broker-specific component introduced by a broker-firm merger can thus be thought of as independent of the analyst-person-specific component. The same qualification applies to our real-firm-merger setting as the decision of two companies to merge is unlikely to be determined by the forecast accuracy of analysts covering the respective acquirer- and target firm. Additionally, we have data on the entire population of sell-side analysts, alleviating concerns of selection bias.

Additionally, both the broker and "real" merger natural experiments offer the opportunity to compare the performance of an analyst post merger with the performance of the *same* analyst pre-merger. In the broker merge setting we compare the performance of an analyst covering a stock post broker merger with the performance of the *same* analyst covering the *same* stock pre-broker merger. We also control for aggregate changes in performance at the stock-level as well as aggregate changes in analyst performance. This absorbs any change in forecast error that is specific to a stock in any given time period, or macro-level effects that affect the performance of all analysts. In the "real" merger setting we compare the performance of an analyst covering an acquirer post merger with the performance of the *same* analyst covering the *same* acquirer pre-merger for a specific stock, while controlling for the aggregate effect of the "real" merger on forecast error, and also aggregate changes in analyst performance.

A few facets of our empirical design are noteworthy. For one, to gauge the relevance of analyst-person versus brokerage-specific effects, we could estimate a panel regression, which includes analyst- and brokerage fixed effects, and assess the incremental R-squared. One drawback of this approach is that the performance effects of analyst- and brokerage-specific characteristics are unlikely to be constant throughout our 15-year sample period. The fixed-effects method also makes it challenging to infer the exact mechanisms through which analyst- and brokerage effects manifest themselves. More crucially, the fixed-effects method draws its power from analyzing changes in performance as an analyst moves from one broker firm to another. Most job transfers cannot be thought of as independent of the analyst-person-

specific performance component and, instead, represent a promotion or demotion. Disentangling the brokerage-specific component from the analyst-person-specific component from job transfers is thus difficult, if not impossible, to do.

A second note concerns our use of earnings forecast error as a measure of analyst performance. Earnings forecasts represent only one of two primary quantifiable outputs that analysts produce. The second output is the analyst's overall recommendation on whether the stock should be bought, held or sold. We focus on earnings forecasts as they can be easily evaluated against the actual earnings announced; stock recommendations lack such a clear objective benchmark.

The paper is organized as follows: Section 2 describes the data. Section 3 presents our main findings, Section 4 conducts additional tests, and Section 5 concludes.

2. Data

Our first investigation exploits brokerage house mergers, which enables a relatively clean assessment of the degree to which analyst performance is person-specific versus broker-level-resource-dependent. We follow Hong and Kacperczyk (2010) and gather information on brokerage house mergers from the SDC Mergers and Acquisition database. We focus on mergers of financial institutions, where both the acquirer- and the target firm belong to SIC Code 6211 ("Investment Commodity Firms, Dealers, and Exchanges"). We match these institutions manually to the IBES database. We require the acquirer or the target to employ at least one analyst with forecasting data in IBES and we require the newly merged brokerage to continuously employ at least one analyst that, previously, was employed by the acquirer or the target. Our final sample consists of 87 brokerage house mergers and our sample period starts in 1994 and ends in 2005.

Our second setting examines real firm mergers. We identify target firms in the CRSP database via the delisting file and by whether a security is marked by a first-digit delisting code of 2 or 3. The delisting file provides us with the PERMNO of the disappearing target firm as well as the PERMNO of the firm, which overwrites the PERMNO of the disappearing firm. We match these PERMNOs to the IBES

database, and we require the newly merged firm to be covered by at least one analyst that also covers the acquirer firm prior to the real firm merger. Our final sample consists of 3,201 real firm mergers and the sample period spans from 1984 to 2011.

3. Is Analyst Performance Portable?

The goal of this study is to systematically evaluate to what extent analyst performance is person-specific versus broker-specific. Section 3.1 outlines our methodology. Sections 3.2 and 3.3 present our main results along with a more detailed description of our empirical design.

3.1 Methodology - Overview

To assess the magnitude of the performance effect residing within a broker firm, we require a source of variation in broker-level resources. Our first set of tests examines changes in performance around broker-firm mergers, and we condition on broker-firm mergers that are most likely to improve broker-level resources available to analysts.

Our second set of tests examines changes in performance around "real" firm mergers. In particular, we compare the analyst's forecast accuracy for the acquirer prior to the merger to the analyst's forecast accuracy for the newly merged firm. Our source of variation comes from comparing changes in forecasting performance for (a) analysts who work for a brokerage house that covers both the target and the acquirer prior to the merger and for (b) analysts who work for a brokerage house that covers only the acquiring firm.

3.2 First Setting: Brokerage House Mergers

We begin our analysis with the following difference-in-difference test. For each broker-firm merger m, we consider the analysts i affected by the merger and the stocks k that they are covering. For each (m,i,k), we compute the scaled forecast error (FE) for quarterly earnings announced in the two year-window around the effective date of the broker-firm merger. FE is defined as the absolute difference between the

announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end; we require EPS forecasts to be issued/updated at least once in the three months prior to the earnings announcement:

$$FE_{m,i,k,t} = \left|\frac{EPS_{k,t} - forecast_i(EPS_{k,t})}{P_{k,t}}\right|. \quad (1)$$

We also compute *FE* for the same set of stocks *k*, but for analysts *j* <u>not</u> affected by the broker-firm merger. For each earnings announcement *t* of stock *k* covered by analyst *i*, we compute the difference between *FE* of analyst *i* and the average *FE* of analysts *j*:

$$\Delta_{m,i,k,t} = FE_{m,i,k,t} - \overline{FE_{m,J,k,t}}. \quad (2)$$

We test how this difference, on average, changes from before to after the merger:

$$\Delta\Delta_{m,i,k} = \overline{\Delta_{m,i,k,after}} - \overline{\Delta_{m,i,k,before}}. \quad (3)$$

Table 2 reports the average $\Delta\Delta_{m,i,k}$ for all analyst-stock pairs (Column 1), as well as, separately, for analysts initially employed by the acquiring broker (Column 2) and analysts initially employed by targeted broker (Column 3). The results indicate that forecast errors, on average, decrease after the broker-firm merger; however, none of the differences are reliably different from zero. One interpretation of these patterns is that broker-firm mergers, on average, only marginally improve resources available to analysts and/or that most of the analyst performance is person-specific; a change in brokerage organization, thus, has little influence on forecast accuracy.

To further assess the mechanisms at hand, we estimate the following baseline regression equation:

$$\Delta\Delta_{m,i,k,t} = \alpha_{i,k} + \alpha_t + \beta_1 After_{m,i,k,t} + \varepsilon_{i,j,k,t}, \quad (4)$$

where *m* indexes the broker-firm merger, *i* indexes the analyst, *k* indexes the stock covered by analyst *i* and *t* denotes the quarterly earnings announcement. $\alpha_{i,k}$ and α_t are analyst-stock- and year-fixed effects, and *After* is an indicator that equals one if the forecast is made after the effective date of the broker-firm merger, and zero otherwise. Standard errors are clustered at the broker-merger level. (1) If broker-firm mergers change the brokerage organization and, in general, improve resources available to the analyst,

and (2) if a meaningful part of performance resides with the brokerage organization, then we may expect $\beta_1 < 0$.

As reported in Table 3, Column 1, the regression produces a weak negative slope on *After*, suggesting that broker-firm mergers only marginally improve resources available to the analyst and/or that most of the forecasting performance resides with the analyst him-/herself, rather than the surrounding brokerage organization. This result is in line with those produced by the difference-in-difference analysis reported in Table 2.

While the broker-specific component only appears to play a secondary role, for a small subset of observations, resources available to the analyst may improve so dramatically that the broker-specific component, despite its overall limited performance implications, becomes detectable. This line of thought leads us to our second regression specification:

$$\Delta\Delta_{m,i,k,t} = \alpha_{i,k} + \alpha_t + \beta_1 After_{m,i,k,t} + \beta_2 After_{m,i,k,t} * BrokerChar_{m,i} + \beta_3 BrokerChar_{m,i} + \varepsilon_{i,j,k,t}. \quad (5)$$

We experiment with the following broker-firm-characteristics (*BrokerChar*):

i. The percentage change from (a) the number of stocks covered by the acquirer/target prior to the merger to (b) the number of stocks covered by the new joint broker firm;
ii. the percentage change from (a) the number of analysts employed by the acquirer/target prior to the merger to (b) the number of analysts employed by the new joint broker firm;
iii. a binary variable that equals one for analysts initially employed by a non-NYC-based brokerage that subsequently merges with a NYC-based brokerage, and zero otherwise;

Resources available to analysts in the new joint broker firm are likely to exceed those available at either the acquirer or the target when the new joint firm covers significantly more stocks and when the number of in-house analysts that the analyst can interact with increases substantially subsequent to the merger. We therefore expect the coefficient estimate on β_2 to be negative for (i) and (ii). Similarly, broker firms based in NYC likely have greater resources available than non-NYC-based brokerages. We thus

expect performance improvement to be particularly strong for analysts initially employed by a non-NYC-based broker firm that subsequently merges with a NYC-based brokerage.

Our conjectures are mostly borne out by the data. The results presented in Table 3 suggest that performance improvements are stronger for analysts that are merged into a larger brokerage, both in terms of number of stocks covered and analysts employed. However, only the former effect is statistically significant. Performance is also improved when analysts initially employed by a non-NYC-based broker firm subsequently work for a NYC-based brokerage.

3.3 Second Setting: Real Firm Mergers

To introduce our second testing ground, consider the setting of three brokers (*Broker1-3*), an acquirer firm (*Acq*) and a target firm (*Tgt*), and four analysts, denoted by capital letters. The coverage prior to the merger is as follows:

Broker1: *Analyst 1* covers *Acq*	(*Analyst 1* ≡ Type 1)
Broker2: *Analyst 2* covers *Acq*, *Analyst 3* covers *Tgt*	(*Analyst 2* ≡ Type 2)
Broker3: *Analyst 4* covers *Acq* and *Tgt*	(*Analyst 4* ≡ Type 3)

Because the newly merged firm is larger and, at least initially, more uncertain than the acquirer firm considered by itself, we expect forecast accuracy for the newly merged firm to be lower than that for the acquirer. That is, we expect forecast accuracy to decline, on average. This decline should vary across analysts types, however. In particular, we expect forecast accuracy to decline less for analysts of Type 3, who cover both the acquirer and the target prior to the merger. Moreover, if a meaningful part of analyst performance resides with the brokerage organization, then analysts of Type 2, who can draw from brokerage-level knowledge about and connections to the target firm, should be at a significant advantage relative to analysts of Type 1. If, on the other hand, most of analyst performance is person-specific, then analysts of Type 2 will face similar challenges as analysts of Type 1.

We begin our analysis with the following univariate test. For each firm merger m, we consider analysts i covering the acquirer firm prior to the merger and the newly merged firm after the merger. We calculate, for each (m,i), the difference between the average FE for the newly merged firm (in the two years after the merger) and the average FE for the acquirer firm (in the two years prior to the merger):

$$\Delta_{m,i} = \overline{FE_{newly\ merged\ firm,i,t}} - \overline{FE_{acquirer,i,t}}. \qquad (6)$$

Table 4 reports the average $\Delta_{m,i}$ across all analysts, as well as the average $\Delta_{m,i}$ across Type-1-, Type-2- and Type-3 analysts. As a reminder, Type-2 analysts are analysts covering the acquirer firm and working for a brokerage that employs a (second) analyst covering the target firm prior to the merger. Type-3 analysts are analysts covering both the acquirer- and the target firm prior to the merger. Type-1 analysts are analysts that are neither of Type 2 nor of Type 3.

Table 4, Column 1, shows that scaled forecast errors, on average, increase by 0.067 (t-statistic = 17.62) for the newly merged firm relative to the acquirer firm (prior to the merger). This increase in forecast error is consistent with the notion that the newly merged firm, at least initially, is more uncertain than the acquirer firm considered by itself. Columns 2, 3 and 4 separate out changes in forecast errors by analyst types. We observe that forecast error increase substantially less for analysts of Type 3. However, no such differential decline in forecast accuracy is observed for analysts of Type 2. In other words, while forecast errors decline substantially less for analysts that cover both the acquirer- and the target firm prior to the merger, the affiliation with a *brokerage* that covers both the acquirer and the target, *per se*, has no performance-improving effects. These findings agree with the results from our first setting ("Broker-Firm-Merger Setting"), which point to limited broker-level-performance effects.

We arrive at similar conclusions when organizing our analysis around the following regression specification:

$$FE_{m,i,t} = \alpha_{i,m} + \alpha_t + \beta_1 After_{m,t} + \beta_2 \left(After_{m,t} \times Type_i\right) + \varepsilon_{m,i,t}, \qquad (7)$$

where $FE_{m,i,t}$ is the absolute forecast error of analyst i covering the acquirer firm prior to and the newly merged firm after the merger m; $\alpha_{i,m}$ and α_t are analyst-merger-firm and year-fixed effects, $After_{m,t}$ equals

one if the forecast used to compute scaled forecast error is made after the effective date of the merger, and zero otherwise. *Type* are indicator variables that equal one if analyst *i* is of Type 2 (Type 3), and zero otherwise. Standard errors are clustered are the analyst-level.

As reported in Table 5, the regression produces a strong positive slope on $After_{m,t}$. The slopes on the Type-2- and the Type-3-interaction term are unreliable and strongly negative, respectively. Together, these results agree with those from the univariate analysis that forecast errors for the newly merged firm are higher than those for the acquirer firm, but substantially less so for analysts that cover both the acquirer and the target prior to the merger. No performance difference is observed between analysts working for a brokerage that covers the target firm prior to the merger and analysts working for a brokerage with no target firm coverage.

4. Additional Analyses

We are mindful of the possibility that analysts of Type 2 or Type 3 may exhibit characteristics, which, in turn, affect performance around firm mergers. Table 6 attempts to control for this channel. We augment our baseline regression equation (7) with interaction terms between $After_{m,t}$ and various analyst characteristics, including:

i. The natural logarithm of (1 + the number of years the analyst has been covering the acquirer firm per the IBES database[2]), as a measure of the analyst's level of experience;
ii. an indicator denoting that the analyst is based in New York City, and zero otherwise;
iii. an indicator denoting that the analyst is female, and zero otherwise.

Table 6 reveals that our base findings are not overturned when controlling for analyst characteristics. The coefficient estimate on the Type-2-interaction term remains unreliable; the estimate on the Type-3-interaction term remains strongly negative. The estimates on the control variables imply a smaller forecast

[2] Firm-specific experience is measured at the time of the analyst's most recent earnings forecast for the last earnings announced prior to the effective date of the merger.

error increase for analysts with more experience and for analysts based in New York City. No differential rise in forecast errors is observed between male and female analysts. These results lend themselves to the intuitive interpretation that more experienced analysts and analysts based in New York City are better suited at handling the uncertainty involved in firm mergers.

As with the broker-firm-merger setting, while, in general, the broker-specific component only plays a limited role, there remains the possibility that it nevertheless becomes detectable for a small subset of observations. In particular, brokerage-level resources can be broken down into two components:

(a) research support and brokerage-level connections to the firm arising from business ties and/or the brand name attached to the brokerage, and

(b) in-house colleagues' knowledge about and connections to the firm that they are willing to share.

The latter channel is likely to be stronger if the analyst and the in-house analyst are based in the same locale. Our final investigation, thus, separates Type-2 analysts based on whether they are based in the same locale or not. The findings of this analysis are presented in Table 7. In contrast to previous results, the regression produces a negative slope on the interaction term between *After* and *Type 2* if Type-2 analysts are based in the same locale (coefficient estimate = -0.040; *t*-statistic = -2.46). The estimate on the interaction term slightly weakens when further conditioning on Type-2 analysts where both the analyst and his/her in-house colleague are based in New York City. As communication is facilitated and as social ties are more likely to develop within small group settings, the stronger results for the non-New York City-overlaps corroborate our overall interpretation of the results presented in this study: The performance implications of brokerage-level resources are limited and most analyst performance is tied to the analyst-person. In other words, human capital is portable. Whatever limited performance effect arises from the brokerage side is tied to in-house colleagues' knowledge about and connections to the firm that they are willing to share.

5. Conclusion

In this paper we employ two natural experiments that affect financial analysts' production and performance to study how human-capital and resource-based theories of the firm might explain performance in knowledge-based industries. We find that human-capital based theories such as knowledge sharing and spillovers under the umbrella of a single firm explain differences in individual performance more so than resource-based theories of the firm. However, we find that not only are the advantages of cooperation within a single firm somewhat dependent on the geographical organization of individuals within the firm, we also show that cooperation by two individuals is never a perfect substitute for a single individual, at least in our production setting. The results suggest that performance is tied to the individual him-/herself and ultimately, that human capital in knowledge-based industries, such as the financial industry, is portable.

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Table 1
Descriptive Statistics

This table reports descriptive statistics of the main variables used in this study. Panel A reports summary statistics for the variables from our brokerage-firm-merger setting. Panel B reports summary statistics for the variables from our real-firm-merger setting.

Variables	N	Mean	StDev	25th	50th	75th
Panel A: Broker Merger Variables						
Scaled Forecast Error	101,109	0.232	0.512	0.027	0.080	0.216
#Analysts Employed by Brokerage Firms Prior to the Merger	100,876	73.75	41.38	31.39	69.46	115.84
#Analysts Employed by Brokerage Firm After the Merger	101,109	80.80	40.43	45.59	78.73	121.12
#Stocks Covered by Brokerage Firm Prior to the Merger	100,876	665.14	338.37	340.35	700.00	940.21
#Stocks Covered by Brokerage Firm After the Merger	101,109	735.87	345.48	398.45	862.17	1,004.66
%Brokerage Firms Based in New York City Prior to the Merger	101,109	0.910	0.286	1.000	1.000	1.000
Panel B: Firm Merger Variables						
Scaled Forecast Error	251,929	0.258	0.621	0.025	0.072	0.211
#Analysts Covering the Acquirer Prior to the Merger	112,940	11.19	6.95	6.00	10.00	16.00
#Analysts Covering the Newly Merged Firm After the Merger	138,989	12.05	7.09	6.00	11.00	17.00
Firm-Specific Experience of Analysts	251,929	3.41	3.79	0.00	2.00	5.00
%Analysts Based in New York City	251,929	0.24	0.43	0.00	0.00	0.00
%Female Analysts	251,929	0.10	0.31	0.00	0.00	0.00

Table 2
Broker Firm Mergers: Univariate Analysis

This table reports changes in forecast errors for analysts around broker-firm mergers. For each broker-firm merger *m*, we consider the analysts *i* affected by the merger and the stocks *k* that they are covering. For each (*m*,*i*,*k*), we compute the scaled forecast error for quarterly earnings announced in the two-year window around the effective date of the broker-firm merger ($FE_{m,i,k,t}$). Scaled forecast error is defined as the absolute difference between the announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end; we require EPS forecasts to be issued/updated at least once in the three months prior to the earnings announcement. We also compute scaled forecast errors for the same set of stocks *k*, but for analysts *j* not affected by the broker-firm merger ($FE_{m,j,k,t}$). For each earnings announcement *t* of stock *k* covered by analyst *i*, we then compute the difference between the scaled forecast error of analyst *i* and the average scaled forecast error of analysts *j* ($\Delta_{m,i,k,t} = FE_{m,i,k,t} - \text{Avg}(FE_{m,j,k,t})$), and we compare how this difference, on average, changed from before to after the merger (diff-in-diff). This table reports the average diff-in-diff for all analyst-stock pairs (Column 1), as well as, separately, for analysts initially employed by the acquirer (Column 2) and analysts initially employed by target (Column 3). *T*-statistics are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1) All Analysts	(2) Acquirer Analysts	(3) Target Analysts
Change in Scaled Forecast Error[%]	-0.002	-0.002	-0.002
	(-0.94)	(-0.88)	(-0.35)
Number of Observations	12,446	10,276	2,170

Table 3
Broker Firm Mergers: Regression Analysis

This table reports determinants of forecast errors for analysts around broker firm mergers. For each broker-firm merger *m*, we consider the analysts *i* affected by the merger and the stocks *k* that they are covering. For each (*m*,*i*,*k*), we compute the scaled forecast error for quarterly earnings announced in the two-year window around the effective date of the broker-firm merger ($FE_{m,i,k,t}$). Scaled forecast error is defined as the absolute difference between the announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end; we require EPS forecasts to be issued/updated at least once in the three months prior to the earnings announcement. We also compute scaled forecast errors for the same set of stocks *k*, but for analysts *j* not affected by the broker-firm merger ($FE_{m,j,k,t}$). For each earnings announcement *t* of stock *k* covered by analyst *i*, we then compute the difference between the scaled forecast error of analyst *i* and the average scaled forecast error of analysts *j* ($\Delta_{m,i,k,t} = FE_{m,i,k,t} - \text{Avg}(FE_{m,j,k,t})$). $\Delta_{m,i,k,t}$ is the dependent variable of our regression equation. *After* equals one if the forecast used to compute scaled forecast error is made after the effective date of the merger, and zero otherwise. *Broker Characteristics* are: the percentage change from (a) the number of stocks covered by the acquirer/target prior to the merger to (b) the number of stocks covered by the new joint broker firm (Column 2); the percentage change from (a) the number of analysts employed by the acquirer/target prior to the merger to (b) the number of analysts employed by the new joint broker firm (Column 3); a binary variable that equals one when the analyst initially works for a non-New York City-based firm that is subsequently acquired by a New York City-based firm, and zero otherwise (Column 4). All coefficient estimates are multiplied by 100. *T*-statistics are based on standard errors clustered at the broker-merger level and are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1) Baseline	(2) Change in #Stocks Covered	(3) Change in #Analysts Employed	(4) Non-NYC Broker Acquired by NYC Broker
After	-0.003	0.000	-0.001	-0.003
	(-0.76)	(0.04)	(-0.40)	(-0.73)
*After * Broker Characteristics*		-0.019**	-0.008	-0.013
		(-2.06)	(-0.92)	(-1.45)
Number of Observations	101,109	100,876	100,876	101,109
Adjusted R-Squared	0.045	0.045	0.045	0.045

Table 4
Real Firm Mergers: Univariate Analysis

This table reports changes in forecast errors for analysts around firm mergers. For each firm merger *m*, we consider analysts *i* covering the acquirer firm prior to the merger and the newly merged firm after the merger. We calculate, for each (*m*,*i*), the difference between the average *FE* for the acquirer firm (in the two years prior to the merger) and the average *FE* for the newly merged firm (in the two years after the merger). *FE* is defined as the absolute difference between the announced quarterly earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end; we require EPS forecasts to be issued/updated at least once in the three months prior to the earnings announcement. Column (1) reports the mean difference across all analysts. Column (2) reports the mean difference for Type-1 analysts. Column (3) reports the mean difference for Type-2 analysts. Column (4) reports the mean difference for Type-3 analysts. Type-2 analysts are analysts covering the acquirer firm and working for a brokerage that employs an analyst covering the target firm prior to the merger. Type-3 analysts are analysts that cover both the acquirer- and the target firm prior to the merger. Type-1 analysts are analysts covering the acquirer firm and are neither Type-2 nor Type-3 analysts. *T*-statistics are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1) All Analysts	(2) Type-1 Analysts	(3) Type-2 Analysts	(4) Type-3 Analysts
Change in Scaled Forecast Error[%]	0.067*** (17.62)	0.073*** (15.92)	0.092*** (7.37)	0.036*** (4.48)
Difference between (3) and (2)			0.019 (1.34)	
Difference between (4) and (2)				-0.037*** (-3.86)
Number of Observations	20,109	14,441	1,599	4,069

Table 5
Real Firm Mergers: Regression Analysis

This table reports determinants of forecast errors for analysts around firm mergers. For each firm merger *m*, we consider the analysts *i* covering the acquirer firm prior to the merger and the newly merged firm after the merger. The dependent variable is the scaled forecast error (*FE*), for each (*m*,*i*), for quarterly earnings announced in the two year-window around the effective date of the firm merger. *FE* is defined as the absolute difference between the announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end. *After* equals one if the forecast used to compute scaled forecast error is made after the effective date of the merger, and zero otherwise. *Type 2* equals one if analyst *i* is of Type 2. *Type* 3 equals one if analyst *i* is of Type 3. Type-2 analysts are analysts working for a brokerage that employs an analyst covering the target firm prior to the merger. Type-3 analysts are analysts that cover both the acquirer- and the target firm prior to the merger. Panel B repeats our analysts for the subset of mergers that have coverage by all three analyst types. All coefficient estimates are multiplied by 100. *T*-statistics are based on standard errors clustered at the analyst-level and are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1)	(2)	(3)
	Panel A: All Firm Mergers		
After	0.053***	0.067***	0.070***
	(10.06)	(10.33)	(11.01)
*After * Type 2*		0.025	
		(1.41)	
*After * Type 3*		-0.072***	-0.075***
		(-6.90)	(-7.13)
Number of Observations	251,929	251,929	251,929
Adjusted R-Squared	0.411	0.412	0.412
	Panel B: Only Firm Mergers that have Type-1, Type-2 and Type-3 Coverage		
After	0.037***	0.044***	0.047***
	(6.88)	(6.78)	(7.73)
*After * Type 2*		0.015	
		(1.07)	
*After * Type 3*		-0.028***	-0.031***
		(-2.76)	(-3.15)
Number of Observations	109,162	109,162	109,162
Adjusted R-Squared	0.365	0.365	0.365

Table 6
Real Firm Mergers: Analyst Type and Analyst Characteristics

This table reports determinants of forecast errors for analysts around firm mergers. For each firm merger *m*, we consider the analysts *i* covering the acquirer firm prior to the merger and the newly merged firm after the merger. The dependent variable is the scaled forecast error (*FE*), for each (*m*,*i*), for quarterly earnings announced in the two year-window around the effective date of the firm merger. *FE* is defined as the absolute difference between the announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end. *After* equals one if the forecast used to compute scaled forecast error is made after the effective date of the merger, and zero otherwise. *Type 2* equals one if analyst *i* is of Type 2. *Type* 3 equals one if analyst *i* is of Type 3. Type-2 analysts are analysts working for a brokerage that employs an analyst covering the target firm prior to the merger. Type-3 analysts are analysts that cover both the acquirer- and the target firm prior to the merger. *Analyst Characteristics* are: the natural logarithm of 1 + the number of years the analyst has been covering the acquirer firm (Column 1); an indicator denoting whether the analyst is based in New York City, and zero otherwise (Column 2); an indicator denoting whether the analyst is female, and zero otherwise (Column 3). All coefficient estimates are multiplied by 100. *T*-statistics are based on standard errors clustered at the analyst-level and are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1) Firm-Specific Experience of Analyst	(2) Analyst Based in NYC?	(3) Analyst = Female?
After	0.148***	0.074***	0.068***
	(10.52)	(10.60)	(10.02)
*After * Type 2*	0.026	0.026	0.025
	(1.45)	(1.43)	(1.41)
*After * Type 3*	-0.067***	-0.071***	-0.072***
	(-6.44)	(-6.83)	(-6.92)
*After * Analyst Characteristic*	-0.050**	-0.029***	-0.008
	(-6.56)	(-3.40)	(-0.47)
Number of Observations	251,929	251,929	251,929
Adjusted R-Squared	0.412	0.412	0.412

Table 7
Real Firm Mergers: Analyst Type and Analyst Location

This table reports determinants of forecast errors for analysts around firm mergers. For each firm merger *m*, we consider the analysts *i* covering the acquirer firm prior to the merger and the newly merged firm after the merger. The dependent variable is the scaled forecast error (*FE*), for each (*m*,*i*), for quarterly earnings announced in the two year-window around the effective date of the firm merger. *FE* is defined as the absolute difference between the announced earnings-per-share (EPS) and analyst *i*'s most recent EPS forecast, divided by the stock price as of the corresponding fiscal quarter end. *After* equals one if the forecast used to compute scaled forecast error is made after the effective date of the merger, and zero otherwise. *Type 2* equals one if analyst *i* is of Type 2. *Type* 3 equals one if analyst *i* is of Type 3. Type-2 analysts are analysts working for a brokerage that employs an analyst covering the target firm prior to the merger. Type-3 analysts are analysts that cover both the acquirer- and the target firm prior to the merger. In Columns 2 and 3, we further subset Type-2 analysts into those where the analyst covering the acquirer firm and his/her colleague covering the target firm are based in the same locale (Column 2) and those where both analysts are based in New York City (Column 3). All coefficient estimates are multiplied by 100. *T*-statistics are based on standard errors clustered at the analyst-level and are reported in parentheses. *,**,*** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	(1)	(2)	(3)
After	0.067***	0.071***	0.071***
	(10.33)	(11.09)	(10.97)
*After * Type 2*	0.025		
	(1.41)		
*After * Type 2* (*same location*)		-0.040**	
		(-2.46)	
*After * Type 2* (*both in NYC*)			-0.027*
			(-1.80)
*After * Type 3*	-0.072***	-0.076***	-0.076***
	(-6.90)	(-7.21)	(-7.18)
Number of Observations	251,929	251,929	251,929
Adjusted R-Squared	0.412	0.412	0.412